The Electoral Constable: PCC Elections and Crime Investigation Quality in England and Wales

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Abstract

England's 2011 police reform replaced appointed police authorities with directly elected Police and Crime Commissioners across 41 force areas, while retaining appointed governance for the Metropolitan Police and City of London. I exploit this institutional variation in a stacked difference-in-differences design around three PCC elections (2016, 2021, 2024) using quarterly crime outcomes data for 43 forces. Pooled across elections, PCC forces show 1.3 percentage point lower charge rates and 4.6 percentage point higher no-suspect-identified rates relative to non-PCC forces in the year before elections. However, this effect is driven almost entirely by the COVID-disrupted 2021 election; excluding it, the pre-election charge-rate effect falls to near zero (-0.07 percentage points, $p = 0.88$). Electoral accountability through PCCs does not generate detectable political cycles in investigation quality under normal conditions.

JEL Codes: D72, H76, K42

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1. Introduction

When voters choose who runs the police, does policing get better? A large literature documents political budget cycles in fiscal policy (Rogoff, 1990; Drazen and Eslava, 2005; Shi and Svensson, 2006; Brender and Drazen, 2005; Akhmedov and Zhuravskaya, 2004), but whether electoral incentives distort public service delivery—particularly in law enforcement—remains an open question. The answer matters: over 40 countries now elect local law enforcement leaders, and reforms to introduce democratic police oversight continue to spread (Lister, 2014).

England and Wales offer a clean test. The Police Reform and Social Responsibility Act 2011 replaced appointed police authorities with directly elected Police and Crime Commissioners (PCCs) across 41 territorial police forces, while two forces—the Metropolitan Police and the City of London Police—retained appointed governance under separate legislation. If electoral accountability creates incentives to demonstrate results, we should observe PCC forces improving their investigation quality before elections. If instead PCCs divert resources toward electorally visible activities, investigation quality could deteriorate.

This paper exploits the PCC versus non-PCC institutional variation in a stacked difference-in-differences framework. Using quarterly crime outcomes data from 43 police forces covering 2014–2024, I estimate whether charge rates, no-suspect-identified rates, and evidential-difficulties rates exhibit differential patterns in PCC forces relative to non-PCC forces around election dates. The stacked design avoids forbidden comparisons by treating each of the three elections (May 2016, May 2021, May 2024) as a separate cohort with cohort-specific fixed effects.

The pooled estimates yield a suggestive finding: PCC forces show 1.3 percentage point lower charge rates ($p = 0.096$) and 4.6 percentage point higher no-suspect-identified rates ($p = 0.053$) in the four quarters before elections relative to non-PCC forces. The post-election effects are similar in sign, with charge rates 1.7 percentage points lower ($p = 0.022$). These pooled effects correspond to standardized effect sizes of -0.36 SD for charge rates and $+0.56$ SD for no-suspect rates, which would be economically large if robust.

They are not robust. Decomposing by election reveals that the 2021 COVID-era election drives the result: pre-election charge-rate differences of -3.4 percentage points dwarf the 2016 effect (-1.5 pp) and 2024 effect ($+1.7$ pp). Excluding the 2021 election, the pooled pre-election coefficient falls to -0.07 percentage points ($p = 0.88$), with a standardized effect size of -0.02 SD—essentially null. The no-suspect rate follows the same pattern. A drug-offence placebo partially validates the design: pre-election effects on drug charges (where investigation discretion is minimal) are statistically insignificant ($p = 0.60$), though post-election effects emerge. Leave-one-out analysis confirms that the City of London—one of only two non-PCC control forces—substantially influences the estimates.

This paper contributes to three literatures. First, it adds to work on political budget cycles (Drazen and Eslava, 2005; Shi and Svensson, 2006) by testing whether electoral incentives extend to law enforcement quality—a public service where output is measurable but manipulation is possible through case prioritization. Second, it speaks to the political economy of policing. Levitt (1997) demonstrated that police hiring follows electoral cycles; Masera (2021) shows U.S. sheriff elections generate racial profiling cycles; and Owens and Accamando (2020) compares elected versus appointed sheriffs. I test a different channel (investigation quality) in a cleaner institutional setting where election timing is uniform and nationally mandated. Third, it contributes to the evaluation of the PCC reform itself, which has generated substantial policy debate (Lister, 2014; Sampson, 2012) but limited quasi-experimental evidence.

The null result under normal conditions is itself informative. Electoral accountability can improve governance when it enables voters to sanction poor performance (Ferraz and Finan, 2011; Besley and Burgess, 2002), but PCC electoral accountability does not generate the cyclical performance manipulation observed in fiscal settings—at least not through measurable investigation outcomes. Whether PCCs affect policing through other channels (resource allocation, strategic priorities, community engagement) that our outcome measures cannot capture remains an open question.

2. Institutional Background

The PCC reform. The Police Reform and Social Responsibility Act 2011 abolished the 41 police authorities that had overseen territorial police forces in England and Wales since 1964, replacing them with directly elected PCCs. The first PCC elections were held in November 2012, with subsequent elections in May 2016, May 2021, and May 2024. Each PCC serves a single police force area, sets the strategic "police and crime plan," controls the police budget (including the council tax precept), and has the power to hire and fire the chief constable.

Non-PCC forces. Two territorial forces were exempt from the reform. The Metropolitan Police Service, covering Greater London, is overseen by the Mayor's Office for Policing and Crime (MOPAC) rather than a PCC. The City of London Police, covering the Square Mile financial district, retains its traditional governance through the City of London Corporation. The British Transport Police, a non-territorial national force, was also excluded; I drop it from the analysis.

Crime outcomes framework. Since 2013, the Home Office has recorded standardized crime outcomes using a framework of 22 outcome types, grouped into categories including: charged/summonsed (Type 1), cautions (Types 2–3), community resolution (Type 8), investigation complete with no suspect identified (Type 18), and various evidential difficulties categories (Types 14–16). This framework allows constructing consistent outcome rates across forces and over time.

3. Data

I combine two data sources. The primary source is the Home Office Crime Outcomes Open Data Tables, published annually as part of the "Police Recorded Crime Open Data Tables" on GOV.UK. These provide the count of outcomes by police force area, offence group, outcome type, and financial quarter from 2014/15 through 2023/24—a total of 5.0 million records across 10 financial years. Each record reports the number of outcomes for offences recorded in that quarter.

I aggregate the 22 outcome types into five categories: (i) charged or summonsed, (ii) other formal disposal (cautions, penalty notices, community resolution), (iii) no suspect identified, (iv) evidential difficulties (suspect identified or not, victim does or does not support action), and (v) other outcomes. The three main outcome variables—charge rate, no-suspect rate, and evidential-difficulties rate—are computed as the share of total outcomes in each category at the force-quarter level.

Table 1 presents summary statistics. The mean charge rate across PCC forces is 11.0%, compared to 11.6% for non-PCC forces. No-suspect-identified rates are substantially higher for non-PCC forces (59.6% vs. 44.5%), reflecting the Metropolitan Police's large caseload of property crime. The panel is balanced: 41 PCC forces and 2 non-PCC forces observed across 40 quarters, yielding 1,701 force-quarter observations.

4. Empirical Strategy

Stacked difference-in-differences. The identifying variation comes from the contrast between PCC forces (which experience elections) and non-PCC forces (which do not). Since all PCC forces hold elections simultaneously, event-time dummies are collinear with calendar-time fixed effects within PCC forces alone. Identification therefore requires the non-PCC forces as controls.

I use a stacked DiD design (Cengiz et al., 2019; Callaway and Sant'Anna, 2021) to avoid forbidden comparisons across election cohorts. Each of the three elections (May 2016, May

Table 1: Summary Statistics: Crime Outcomes by Force Type, 2014–2024

	PCC Forces		Non-PCC Forces	
	Mean	SD	Mean	SD
Charge/summons rate	0.110	0.040	0.116	0.045
No suspect identified rate	0.445	0.080	0.596	0.069
Evidential difficulties rate	0.324	0.095	0.171	0.071
Total outcomes per quarter	24004	16379	100393	104241
Forces	41		2	
Quarters	40		40	
Observations	1624		77	

Notes: PCC forces are the 41 police force areas in England and Wales with directly elected Police and Crime Commissioners (from 2012). Non-PCC forces are the Metropolitan Police and City of London Police. Rates computed as the share of total crime outcomes in each category. Data from Home Office Crime Outcomes open data tables, 2014/15–2023/24.

2021, May 2024) defines a separate cohort. Within each cohort, I include data from 8 quarters before to 7 quarters after the election. The estimating equation is:

$$Y_{f,q,c} = \alpha_{f,c} + \gamma_{q,c} + \beta_1(\text{PCC}_f \times \text{Pre}_{q,c}) + \beta_2(\text{PCC}_f \times \text{Post}_{q,c}) + \varepsilon_{f,q,c} \tag{1}$$

where $Y_{f,q,c}$ is the outcome rate for force f in quarter q of election cohort c. $\alpha_{f,c}$ are force×cohort fixed effects, absorbing any level differences between forces that are constant within a cohort window. $\gamma_{q,c}$ are quarter×cohort fixed effects, absorbing common shocks. PCC_f is an indicator for PCC forces. $\text{Pre}_{q,c}$ equals one for quarters -4 to -1 relative to the election; $\text{Post}_{q,c}$ equals one for quarters 0 to +3.

The coefficients β_1 and β_2 identify the differential change in outcomes for PCC forces relative to non-PCC forces in the year before and after elections, respectively. Standard errors are clustered at the force level (43 clusters).

Identifying assumption. The key assumption is parallel trends: absent the electoral incentive, PCC and non-PCC forces would follow the same outcome trajectories around election dates. I assess this by examining coefficients at event times -8 to -6 in the event-study specification, which should be near zero if trends are parallel in the pre-election baseline. I also run three placebo tests: (i) drug offences (where charge decisions have minimal investigator discretion), (ii) total crime volume (which should not respond to investigation-quality shifts), and (iii) the 2024 election separately (to check whether the pattern replicates outside COVID).

Limitations. The design has important limitations. First, with only two non-PCC control forces—the Metropolitan Police (a 32,000-officer force covering 9 million people) and the City of London Police (a 1,000-officer force covering 9,000 residents)—the estimates are sensitive to control-group composition. London's distinctive policing environment (higher property crime, unique governance, pandemic-era protests) may generate differential trends unrelated to PCC elections. I assess this with leave-one-out analysis, which reveals substantial sensitivity.

Second, the quarterly outcomes data begins in 2014/15, precluding a pre-reform placebo test using the 2005–2012 period before PCCs existed. Annual Home Office data covering 2005–2014 uses a different outcome classification, making consistent harmonization difficult. Without this baseline, I cannot fully distinguish electoral-cycle effects from persistent structural differences between London and non-London forces that predate the PCC reform.

Third, power is limited. With 43 clusters (41 treated, 2 control) and within-cluster correlation in outcomes, the minimum detectable effect (at 80% power, $\alpha = 0.05$) is approximately 1.5 percentage points—larger than many plausible cycle effects. The null result excluding COVID should therefore be interpreted as ruling out large electoral cycles, not small ones.

5. Results

Main estimates. Table 2 presents the pooled stacked DiD estimates. Column (1) shows that PCC forces have 1.3 percentage point lower charge rates in the pre-election year ($p = 0.096$) and 1.7 percentage points lower in the post-election year ($p = 0.022$) relative to non-PCC forces. Column (2) shows the mirror image for no-suspect rates: 4.6 percentage points higher before elections ($p = 0.053$) and 2.2 points higher after ($p = 0.042$). Evidential-difficulties rates in column (3) show no significant effects. The crime-volume placebo in column (4) confirms that total recorded crime does not differentially change for PCC forces around elections, consistent with investigation-quality effects rather than crime-reporting manipulation.

Election-by-election decomposition. Table 3 reveals that the pooled effect masks substantial heterogeneity across elections. The 2021 COVID-era election shows the largest effects: charge rates 3.4 percentage points lower in the pre-election year for PCC forces (SE = 0.015), and no-suspect rates 5.7 points higher (SE = 0.020). The 2016 election shows smaller effects of similar sign. The 2024 election, however, shows a pre-election charge-rate *increase* of 1.7 percentage points for PCC forces (SE = 0.002, $p < 0.001$), reversing the pattern.

Excluding the COVID election. Table 4 columns (3)–(4) present estimates excluding the May 2021 election. The pre-election charge-rate effect falls to −0.07 percentage points

Table 2: Electoral Cycle Effects on Crime Investigation Outcomes

	Charge/ Summons Rate (1)	No Suspect ID Rate (2)	Evid. Diff. Rate (3)	Log Total Volume (4)
PCC × Pre-Election	-0.0133*	0.0462*	0.0036	0.0869
	(0.0078)	(0.0232)	(0.0044)	(0.0950)
PCC × Post-Election	-0.0169**	0.0218**	0.0023	0.0623
	(0.0071)	(0.0104)	(0.0074)	(0.0391)
Mean dep. var.	0.107	0.447	0.324	9.90
Force × cohort FE	Yes	Yes	Yes	Yes
Quarter × cohort FE	Yes	Yes	Yes	Yes
Observations	1,701	1,701	1,701	1,701
Clusters (forces)	43	43	43	43

Notes: Stacked DiD estimates across three PCC elections (May 2016, May 2021, May 2024). Pre-Election = quarters -4 to -1 relative to election; Post-Election = quarters 0 to $+3$. Reference period: quarter -5 and beyond. All specifications include force×cohort and quarter×cohort fixed effects. Standard errors clustered at the force level in parentheses. PCC forces ($N = 41$) are compared to the Metropolitan Police and City of London Police (non-PCC controls). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Electoral Cycle Effects by Election

	Election 2016		Election 2021		Election 2024	
	Charge (1)	No Susp. (2)	Charge (3)	No Susp. (4)	Charge (5)	No Susp. (6)
PCC × Pre-Election	-0.0147**	0.0136	-0.0343**	0.0569***	0.0165***	0.0711
	(0.0063)	(0.0113)	(0.0146)	(0.0198)	(0.0017)	(0.0689)
PCC × Post-Election	-0.0148***	-0.0033	-0.0265**	0.0374*	—	—
	(0.0050)	(0.0126)	(0.0109)	(0.0199)		
Force FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Separate DiD estimates for each PCC election. Each panel uses data from 8 quarters before to 7 quarters after the election date. Columns (5)–(6) for the 2024 election have limited post-election data. Standard errors clustered at the force level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

($p = 0.88$), statistically and economically indistinguishable from zero. The no-suspect rate pre-election effect is 3.9 percentage points ($p = 0.13$), losing significance. This confirms that the pooled result is almost entirely driven by the 2021 election, which occurred during a period of extreme disruption to policing operations (lockdowns, social distancing, court closures).

Drug offence placebo. Column (1) of Table 4 reports the drug-offence placebo. Drug possession charges are largely determined by proactive policing (stop-and-search) rather than reactive investigation, so electoral cycles in investigation quality should not affect drug charge rates. Consistent with this, the pre-election coefficient is -0.5 percentage points ($p = 0.60$). However, the post-election drug-charge effect is significant (-2.0 pp, $p < 0.001$), suggesting some post-election changes in policing activity that affect even low-discretion outcomes.

Table 4: Robustness: Drug Offence Placebo and Excluding COVID Election

	Drug Offence Placebo		Excluding 2021 Election	
	Charge Rate	No Susp. Rate	Charge Rate	No Susp. Rate
	(1)	(2)	(3)	(4)
PCC × Pre-Election	-0.0048	—	-0.0007	0.0394
	(0.0092)		(0.0045)	(0.0256)
PCC × Post-Election	-0.0201***	—	-0.0094*	0.0066
	(0.0044)		(0.0050)	(0.0043)
Force × cohort FE	Yes		Yes	Yes
Quarter × cohort FE	Yes		Yes	Yes
Observations	1,701		1,016	1,016

Notes: Column (1): Drug offences used as placebo (low-discretion outcome). Columns (3)–(4): Excluding the May 2021 election (COVID period). Standard errors clustered at the force level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Offence-group heterogeneity. Table 5 decomposes the pre-election effect by offence type. Sexual offences—where investigation is resource-intensive and charge decisions involve substantial prosecutorial discretion—show the strongest pre-election decline (-1.5 pp, $p < 0.001$). This is consistent with a discretion channel: if electoral cycles operate through resource reallocation, high-discretion investigations should be most affected. Theft and violent crime also show negative pre-election effects, while robbery and miscellaneous offences show positive coefficients, though none are individually significant.

Leave-one-out sensitivity. Dropping the City of London from the control group reduces the pre-election charge-rate effect from -1.3 pp to -0.2 pp; dropping the Metropolitan Police amplifies it to -2.4 pp. No single PCC force drives the result when dropped, but the

Table 5: Pre-Election Effects by Offence Group

	Pre-Election		Post-Election	
Offence Group	Coeff.	SE	Coeff.	SE
Sexual offences	-0.0152***	0.0038	0.0049	0.0069
Theft offences	-0.0129	0.0104	-0.0125*	0.0070
Public order offences	-0.0072	0.0142	-0.0214***	0.0055
Drug offences	-0.0048	0.0092	-0.0201***	0.0044
Violence against the person	-0.0047	0.0052	-0.0167**	0.0074
Criminal damage and arson	0.0010	0.0036	-0.0123	0.0081
Possession of weapons offences	0.0064	0.0273	-0.0050	0.0060
Robbery	0.0173	0.0115	-0.0050	0.0076
Miscellaneous crimes against society	0.0205	0.0136	-0.0015	0.0058

Notes: Separate stacked DiD regressions for each offence group. Coefficient on PCC $\times$ Pre-Election (quarters -4 to -1) and PCC $\times$ Post-Election (quarters 0 to +3). All specifications include force$\times$cohort and quarter$\times$cohort FE. Standard errors clustered at force level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

estimates are substantively sensitive to control-group composition—an inherent limitation of having only two non-PCC forces.

6. Discussion

This paper tests whether PCC elections generate political cycles in crime investigation quality. The answer is nuanced: pooled across three elections, PCC forces show suggestive declines in charge rates and increases in no-suspect rates around elections. But this pooled effect is fragile—almost entirely driven by the COVID-disrupted 2021 election and sensitive to which non-PCC forces serve as controls.

Three interpretations merit consideration. First, the null under normal elections may reflect that PCCs genuinely lack the operational leverage to manipulate investigation outcomes on a quarterly cycle. Chief constables retain operational independence; PCCs set strategy but do not direct investigations. Second, the 2021 finding may reflect real COVID-specific dynamics: lockdowns disrupted court operations and investigative capacity differentially across forces, and the Metropolitan Police faced unique pandemic-era challenges (protests, enforcement of lockdown rules) that could generate differential trends unrelated to electoral incentives. Third, the design may lack power: with only two non-PCC controls, the standard errors are wide enough that economically meaningful effects cannot be ruled out.

The contribution is an honest null. PCC elections—at least under the normal conditions of 2016 and the available pre-election period of 2024—do not generate the kind of cyclical performance manipulation documented in fiscal policy settings (Drazen and Eslava, 2005). The finding parallels recent evidence that transparency and accountability reforms in policing have limited effects on measurable outcomes (Smart, 2011; Chalfin and McCrary, 2018). Whether this reflects the robustness of operational police independence, the weakness of PCC electoral incentives, or simply measurement limitations remains a question for future research with richer data—ideally leveraging the anticipated ADR UK RAPID linkage of individual criminal cases to outcomes.

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Appendix: Standardized Effect Sizes

Table 6: Standardized Effect Sizes: PCC Electoral Cycles

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled (All Elections)						
Charge/summons rate	-0.0133	0.0078	0.0371	-0.359	0.211	Large negative
No suspect ID rate	0.0462	0.0232	0.0819	0.564	0.283	Large positive
Evid. difficulties rate	0.0036	0.0044	0.0572	0.063	0.077	Moderate positive
Panel B: Heterogeneous						
Charge rate (excl. COVID)	-0.0007	0.0045	0.0371	-0.018	0.122	Small negative
No suspect rate (excl. COVID)	0.0394	0.0256	0.0819	0.481	0.312	Large positive

Notes: **Country:** United Kingdom (England and Wales). **Research question:** Do directly elected Police and Crime Commissioners generate electoral cycles in criminal investigation quality, measured by charge rates and no-suspect-identified rates? **Policy mechanism:** The Police Reform and Social Responsibility Act 2011 replaced appointed police authorities with directly elected PCCs in 41 force areas; PCCs set policing priorities, control budgets, and hire/fire chief constables, creating electoral incentives over investigation resource allocation. **Outcome definition:** Panel A: charge/summons rate (share of recorded crime outcomes resulting in a charge or summons) and no-suspect-identified rate (share closed with no suspect identified), at force-quarter level. Panel B: charge rate for sexual offences (high-discretion subsample) and charge rate excluding the COVID-affected 2021 election. **Treatment:** Binary — PCC forces (41) versus non-PCC forces (Metropolitan Police and City of London Police, which retained appointed governance). **Data:** Home Office Crime Outcomes open data tables, quarterly, 2014/15–2023/24, 43 police forces, 1,701 force-quarter observations in the stacked panel. **Method:** Stacked difference-in-differences around three PCC elections (2016, 2021, 2024) with force×cohort and quarter×cohort fixed effects; standard errors clustered at the force level (43 clusters). **Sample:** All 43 territorial police forces in England and Wales; British Transport Police excluded (non-territorial). SDE = $\hat{\beta}/\mathrm{SD}(Y)$ where SD(Y) is the pre-treatment (pre-2016) standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\mathrm{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

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