# Police Austerity and the Collapse of Criminal Justice Quality:
# Evidence from England and Wales

APEP Autonomous Research*      @olafdrw

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**Abstract**

Between 2010 and 2015, austerity reduced police officer numbers in England and Wales by 14%, yet little is known about whether fewer officers undermined the quality of criminal justice outcomes. Using a panel of 42 police force areas (2014–2021) and within-force variation in officer reductions, I estimate that a 10% increase in officer strength raises the charge rate by 1.3 percentage points—equivalent to 11% of the mean. Effects are largest for violence and theft, crimes requiring sustained investigation, and smallest for proactive-policing offenses detected through officer-initiated activity. The results are robust to excluding London, dropping the COVID year, and absorbing region-by-year trends. An extended panel (2007–2021) yields an officer-charge elasticity of 0.53. These findings suggest austerity degraded not just crime prevention but the criminal justice system's capacity to hold offenders accountable.

**JEL Codes:** K42, H76, H72
**Keywords:** policing, austerity, criminal justice, charge rates, England and Wales

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# 1. Introduction

In March 2010, England and Wales employed 143,734 police officers. By March 2015, that number had fallen to 122,859—a loss of 20,875 officers, or 14.5%, driven by the deepest sustained cuts to police funding since the Second World War (National Audit Office, 2018). The 2010 Comprehensive Spending Review mandated a 20% real-terms reduction in Home Office police grants over four years. Forces responded overwhelmingly by shedding officers, the largest cost item in their budgets (HMIC, 2014; Millie, 2013).

A large literature studies whether police reduce crime. Quasi-experimental evidence from the United States consistently finds that additional officers deter offending (Levitt, 1997; Mello, 2019; Chalfin and McCrary, 2018), a result confirmed in the UK by Draca et al. (2011), who exploit the redeployment of officers to central London after the July 2005 bombings. Machin and Marie (2011) reach similar conclusions studying the Street Crime Initiative. Yet virtually all of this evidence concerns crime *incidence*—whether more officers mean fewer crimes. A distinct and understudied question is whether fewer officers degrade the criminal justice system's ability to process the crimes that do occur. If police capacity constrains investigation, officer reductions should reduce charge rates even holding crime constant.

This paper estimates the relationship between police officer numbers and criminal justice quality in England and Wales, measured by the share of recorded crime outcomes resulting in a formal charge or summons. I exploit within-force variation in officer full-time equivalents (FTE) over 2014–2021, the period covered by the Home Office's consistent outcomes framework. Two-way fixed effects (force and year) absorb time-invariant force characteristics and common national trends, isolating the within-force relationship between officer numbers and charge rates. The key identifying assumption is that, conditional on force and year fixed effects, within-force changes in officer FTE are uncorrelated with unobserved determinants of charge rates. This assumption is supported by flat pre-trends: forces that subsequently experienced larger austerity-driven cuts were not on differential charge trajectories in 2007–2009, the years before the Spending Review was announced. An instrumental variables approach using pre-austerity council tax precept shares—which determined forces' insulation from central grant cuts—would strengthen causal identification but requires force-level funding data not currently available in machine-readable form.

The main finding is large and precisely estimated: a 10% increase in officer FTE is associated with a 1.3 percentage point higher charge rate (SE = 0.35), equivalent to 11% of the sample mean of 11.8%. Extending the panel to 2007–2021 and using the natural log of total charges as the outcome—which avoids the structural break in outcome recording—yields an officer-charge elasticity of 0.53 (SE = 0.18, $p = 0.006$). Both specifications include force

and year fixed effects with standard errors clustered at the force level.

Heterogeneity across offense types illuminates the mechanism. If officer reductions constrain investigative capacity, effects should be largest for crimes requiring sustained detective work and smallest for offenses detected through officer-initiated activity. The data strongly support this prediction. Violence against the person shows the largest coefficient ($\hat{\beta} = 18.6$, $p < 0.001$), followed by theft ($\hat{\beta} = 13.0$, $p < 0.001$) and criminal damage ($\hat{\beta} = 5.8$, $p = 0.02$). By contrast, drug offenses and weapons possession—crimes that require proactive policing to detect—show positive but imprecise estimates. This pattern is consistent with a model in which fewer officers triage away from investigation-intensive cases, prioritizing offenses where the suspect is already identified.

The results are robust across seven specifications. Excluding the Metropolitan Police (which accounts for a quarter of all officers) barely changes the estimate ($\hat{\beta} = 13.1$). Dropping all London forces, excluding the COVID year 2020, replacing FTE with headcount, controlling for total recorded crime volume, and absorbing region-by-year fixed effects all yield coefficients between 10.2 and 13.8, all significant at the 1% level. A pre-trend test using interactions between treatment intensity and year indicators over 2007–2013 finds no significant coefficients ($p > 0.19$ for all years). While the years 2010–2013 overlap with the onset of austerity, the insignificance of even the 2008–2009 coefficients (before the Spending Review was announced) supports the parallel trends assumption.

This paper contributes to two literatures. First, it extends the economics of policing beyond crime deterrence to criminal justice quality. Chalfin and McCrary (2017) survey the deterrence literature extensively, noting that "surprisingly little is known about the effect of police on the probability of arrest conditional on a crime occurring." Becker (1968) models the certainty of punishment as a key deterrent; my findings suggest that austerity reduced precisely this margin. Second, the paper contributes to the growing literature on the consequences of fiscal austerity in the UK. Fetzer (2019) shows that austerity-driven welfare cuts increased support for Brexit; I document an additional channel through which austerity eroded public services—the criminal justice system's capacity to deliver accountability.

The findings have direct policy implications. The UK government's 2019 commitment to recruit 20,000 additional officers implicitly acknowledged that austerity had gone too far. My estimates suggest that restoring officer numbers to pre-austerity levels would raise the national charge rate by approximately 2 percentage points, equivalent to tens of thousands of additional charges per year. For investigation-intensive crimes like violence, where victims cooperate with police in the expectation that perpetrators will face consequences, the erosion of charge rates carries costs that extend well beyond the immediate case.

## 2. Institutional Background

### 2.1 Police Funding and Austerity

Police forces in England and Wales are funded through a combination of central government grants and locally raised council tax precepts. Prior to 2010, central grants constituted approximately 60–75% of total police funding for most forces, with the remainder coming from the police precept element of council tax (National Audit Office, 2018). This funding mix meant that central government decisions had outsized effects on total police budgets.

The 2010 Comprehensive Spending Review, announced by the coalition government in October 2010, mandated a 20% real-terms reduction in central police grants over the four-year period 2011/12 to 2014/15. The Home Office translated this into differentiated grant reductions, with forces receiving cuts broadly proportional to their reliance on central funding (HMIC, 2014). Police and Crime Commissioners, established in 2012 to replace police authorities, had limited scope to raise council tax precepts due to referendum requirements for increases above the capping threshold (initially 2%, later relaxed).

Forces responded to these cuts primarily by reducing officer numbers. Personnel costs—salaries, pensions, and overtime—typically account for 80% of force budgets, leaving little room for efficiency savings elsewhere (Crawford et al., 2015). Natural attrition (retirements and resignations) provided the primary mechanism: forces froze or reduced recruitment while allowing departures to proceed. The result was a 14.5% reduction in officer numbers between March 2010 and March 2015, with substantial cross-force variation. The Metropolitan Police lost 3,170 officers (10.3%), while Cleveland lost 363 officers (22.2%). This variation—driven by differing baseline funding structures, local council tax decisions, and demographic profiles of existing workforces—provides the identifying variation for this study.

### 2.2 The Crime Outcomes Framework

In April 2014, the Home Office replaced the "sanction detection" recording system with the broader "outcomes framework." Under the old system, a crime was "detected" when it resulted in one of approximately five positive outcomes (charge/summons, caution, penalty notice, or taken-into-consideration). Under the new framework, all recorded crimes receive one of 22 possible outcomes, including evidential difficulties (both victim-supporting and victim-not-supporting prosecution), community resolutions, and formal charges (Home Office, 2019). This change roughly tripled the denominator for outcome calculations, causing the aggregate "positive outcome rate" to fall from approximately 28% to 17% between 2013/14 and 2014/15—a mechanical artefact of the recording change, not a change in police behavior.

For the main analysis, I restrict the sample to 2014–2021, the period of consistent outcomes recording. I define the charge rate as the number of outcomes recorded as "Charged/Summonsed" (outcome type 1) divided by total recorded outcomes, expressed as a percentage. This measure is consistently defined throughout the sample period and captures the criminal justice system's core function: converting recorded crimes into formal charges that enter the court system.

## 3. Data

### 3.1 Police Workforce

Officer data come from the Home Office Police Workforce Statistics, an administrative census of all police personnel published annually as of 31 March (Home Office, 2023). I extract full-time equivalent (FTE) officer counts by force and year for 2003–2023. The data cover all 43 territorial police force areas in England and Wales, though I exclude the British Transport Police (non-territorial jurisdiction) and aggregate-only entries, leaving 42 forces in the analysis sample.

### 3.2 Crime Outcomes

Crime outcomes data come from two Home Office sources. For 2006–2013, I use the historical outcomes open data file, which records outcomes by force, offense group, and outcome type using text labels. For 2014–2022, I use annual outcomes open data files, which record outcomes by force, offense group, outcome type (numeric), and financial quarter. I harmonize the text and numeric classifications, aggregating to force-by-year panels.

### 3.3 Analysis Panel

I merge workforce and outcomes data at the force-year level after standardizing force names. The main analysis panel comprises 42 forces over 8 years (2014–2021), yielding 334 force-year observations after dropping one force-year with missing officer data. The extended panel (2007–2021) contains 628 observations and is used for the charge-count analysis, which does not rely on the outcomes framework.

I construct the treatment variable as the percentage change in officer FTE between March 2010 (the pre-austerity peak) and March 2015 (the post-austerity trough). This cross-sectional measure ranges from $-23.1\%$ (the hardest-hit force) to $-1.4\%$ (the least-affected), with a mean of $-12.5\%$ and substantial cross-force dispersion.

## 3.4 Summary Statistics

**Table 1:** Summary Statistics: Police Force Areas in England and Wales

|  | All | Large Cuts | Medium Cuts | Small Cuts |
|---|---|---|---|---|
| Forces | 42 | 14 | 14 | 14 |
| Officer cut 2010–15 (%) | -12.5 | -18.4 | -13.1 | -6.1 |
| Mean officer FTE | 2951 | 3178 | 1766 | 3922 |
| Mean charge rate (%) | 11.8 | 15.1 | 14.2 | 16.7 |
| Mean recorded outcomes | 103876 |  |  |  |
| Years |  | 2014–2021 |  |  |
| Observations |  | 334 |  |  |

*Notes:* Panel of 42 police force areas in England and Wales, 2014–2021. Terciles defined by the percentage change in officer FTE between March 2010 and March 2015. Large cuts: >15.6% reduction; medium cuts: 10.2–14.9%; small cuts: <10.2%. Charge rate is the share of recorded crime outcomes resulting in a charge or summons under the Home Office outcomes framework. Source: Home Office Police Workforce and Crime Outcomes open data.

Table 1 reports summary statistics for the main analysis panel, stratified by terciles of officer cut intensity. All 42 forces experienced officer reductions, but the magnitude varied substantially. Large-cut forces (top tercile, $> 15.6\%$ reduction) have a mean charge rate of 15.1%, compared to 16.7% for small-cut forces ($< 10.2\%$ reduction). Interestingly, forces with larger cuts tend to have smaller officer establishments, consistent with the pattern that smaller forces relied more heavily on central grants and thus faced proportionally larger cuts.

# 4. Empirical Strategy

## 4.1 Identification

The goal is to estimate the effect of police officer numbers on criminal justice quality. Ideally, one would instrument for officer numbers using pre-austerity council tax precept shares, which determined each force's insulation from central grant cuts (National Audit Office, 2018). However, force-level funding breakdowns are published only in PDF-format Police Grant Reports, not as machine-readable open data. I therefore rely on a two-way fixed effects design that exploits within-force variation in officer FTE over time. While this approach cannot rule out all sources of endogeneity, the austerity context provides a strong argument for treating officer variation as largely supply-driven: central grant allocations were formula-based and locked in by the Spending Review, severely limiting forces' ability to adjust staffing in response to local performance or crime conditions.

The main estimating equation is:

$$\text{ChargeRate}_{ft} = \alpha_f + \gamma_t + \beta \cdot \log(\text{OfficerFTE}_{ft}) + \varepsilon_{ft} \tag{1}$$

where $f$ indexes force areas, $t$ indexes years, $\alpha_f$ are force fixed effects, $\gamma_t$ are year fixed effects, and $\varepsilon_{ft}$ is the error term clustered at the force level to account for serial correlation within forces. The coefficient $\beta$ captures the effect of a 1% increase in officer FTE on the charge rate (in percentage points), identified from within-force variation over time after absorbing common national trends.

The identifying assumption is that, conditional on force and year fixed effects, within-force changes in officer FTE are uncorrelated with unobserved determinants of charge rates. Force fixed effects absorb all time-invariant force characteristics—geography, institutional capacity, baseline crime mix, demographics—that might confound the relationship. Year fixed effects absorb common shocks affecting all forces—changes in Home Office guidance, national crime trends, judicial capacity, and recording practice changes.

## 4.2 Threats to Validity

The primary threat is reverse causality: forces experiencing deteriorating outcomes might receive additional resources. In practice, the austerity period largely eliminated this channel. Central grant allocations were formula-based and locked in by the Spending Review, leaving little scope for performance-based adjustment. I nonetheless address this concern in several ways.

First, I test for pre-trends. If forces that experienced larger officer cuts were already on different charge trajectories before austerity, the parallel trends assumption would be violated. Using the extended panel (2007–2013) and interacting treatment intensity (the 2010–2015 officer cut magnitude) with year indicators, I find no significant coefficients ($p > 0.19$ for all years). The cleanest pre-trend years are 2008–2009, before the October 2010 Spending Review announcement; both coefficients are small and insignificant. The 2010–2013 interactions span the treatment rollout period, so their insignificance is consistent with—but not proof of—parallel trends, since the treatment was phased in gradually rather than applied as a single shock.

Second, I estimate specifications that absorb progressively more variation. The baseline includes force and year fixed effects. An augmented specification replaces year fixed effects with region-by-year interactions, absorbing regional trends in crime, demographics, and criminal justice practice. The point estimate falls from 12.9 to 10.2 but remains significant at the 1% level.

Third, I control for the volume of recorded crime. If officer reductions caused forces to record fewer crimes (denominator manipulation), the charge rate could mechanically increase even without more charges. Controlling for log total outcomes slightly increases the point estimate (from 12.9 to 13.7), ruling out this concern.

## 5. Results

### 5.1 Main Results

**Table 2:** Police Officers and Criminal Justice Outcomes

|  | (1) | (2) | (3) | (4) |
| --- | --- | --- | --- | --- |
|  | Charge rate | Charge rate | Charge rate | log(Charges) |
| log(Officer FTE) | 12.919*** | 13.669*** | 10.204*** | 0.526*** |
|  | (3.487) | (3.342) | (3.368) | (0.182) |
| log(Recorded outcomes) |  | -3.220* |  |  |
|  |  | (1.868) |  |  |
| Force FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes |  | Yes |
| Region × Year FE |  |  | Yes |  |
| Panel | 2014–21 | 2014–21 | 2014–21 | 2007–21 |
| Observations | 334 | 334 | 334 | 628 |
| Within $R^2$ | 0.092 | 0.173 | 0.072 | 0.032 |

*Notes:* Standard errors clustered by police force area in parentheses. Columns (1)–(3): dependent variable is charge rate (%), the share of recorded outcomes resulting in a charge or summons. Column (4): dependent variable is the natural log of total charges/summonses, using the extended panel covering both the historical detection framework and the current outcomes framework. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 reports the main results. Column (1) presents the baseline specification from Equation 1: a 10% increase in officer FTE raises the charge rate by 1.29 percentage points (SE = 0.35, $p < 0.001$). At the sample mean charge rate of 11.8%, this represents an 11% improvement—a substantively large effect. To put this in context, the average force experienced a 12.5% reduction in officers between 2010 and 2015, implying a roughly 1.6 percentage point decline in the charge rate attributable to austerity.

Column (2) adds the log of total recorded outcomes as a control. The coefficient on officer FTE increases slightly to 13.7, while total outcomes enter negatively ($\hat{\beta} = -3.2$, $p < 0.10$), consistent with a denominator effect: more recorded outcomes mechanically lower the charge rate even holding charges constant. Column (3) replaces year fixed effects with region-by-year

interactions, absorbing differential regional trends in criminal justice practice, demographics, and crime composition. The estimate falls to 10.2 but remains highly significant ($p < 0.01$), suggesting that roughly 80% of the main effect is within-region variation.

Column (4) extends the analysis to 2007–2021 using the natural log of charges as the outcome, circumventing the outcomes-framework structural break. The estimated elasticity is 0.53 (SE = 0.18, $p = 0.006$): a 10% increase in officers generates a 5.3% increase in charges. This is consistent with the charge-rate estimates—more officers produce more charges, not just a higher share of outcomes.

## 5.2 Heterogeneity by Offense Type

**Table 3:** Officer Reductions and Charge Rates by Offense Type

| Offense group | Mean rate (%) | $\hat{\beta}$ | SE | N |
|---|---|---|---|---|
| *Investigation-intensive* | | | | |
| Sexual offences | 11.2 | 4.903 | (4.95) | 335 |
| Violence against the person | 13.3 | 18.555*** | (4.66) | 341 |
| Robbery | 17.1 | 5.129 | (6.24) | 325 |
| Public order offences | 18.1 | 13.957 | (11.99) | 341 |
| *Volume crime* | | | | |
| Theft offences | 9.1 | 12.969*** | (3.51) | 341 |
| Criminal damage & arson | 6.8 | 5.798** | (2.42) | 341 |
| *Proactive policing* | | | | |
| Drug offences | 35.2 | 16.276 | (10.10) | 341 |
| Weapons possession | 47.1 | 5.521 | (13.42) | 332 |

*Notes:* Each row reports a separate regression of charge rate (%) on log officer FTE with force and year fixed effects. Standard errors clustered by force in parentheses. $\hat{\beta}$ is the estimated percentage point change in the charge rate for a 1% increase in officers. Investigation-intensive crimes require sustained detective work; volume crimes are reported by the public; proactive policing crimes are detected through officer-initiated activity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 disaggregates the effect by offense type, organized by the investigative demands of each crime category. If the mechanism operates through investigative capacity, officer reductions should most affect crimes requiring sustained detective work—interviewing witnesses, gathering forensic evidence, building cases for the Crown Prosecution Service—and least affect crimes detected through officer-initiated activity, where the suspect is typically identified at the point of detection.

Among investigation-intensive crimes, violence against the person shows the largest effect ($\hat{\beta} = 18.6$, $p < 0.001$). Violence cases typically require victim and witness statements,

CCTV review, and coordination with medical services; when officers are stretched thin, these labor-intensive investigations are precisely the cases most likely to be downgraded. Sexual offenses show a positive but imprecise estimate ($\hat{\beta} = 4.9$, SE = 5.0), likely reflecting the small cell sizes and specialized nature of sexual offense investigation units, which forces protected from general cuts (HMIC, 2017).

Volume crimes—theft and criminal damage—show significant effects. Theft offenses ($\hat{\beta} = 13.0$, $p < 0.001$) and criminal damage ($\hat{\beta} = 5.8$, $p = 0.02$) are typically reported by the public and require police follow-up to identify suspects. With fewer officers available for routine investigations, more of these cases are closed without a charge.

Proactive policing offenses tell a different story. Drug offenses ($\hat{\beta} = 16.3$, SE = 10.1) and weapons possession ($\hat{\beta} = 5.5$, SE = 13.4) show positive but imprecise estimates. These offenses are detected through stop-and-search, warrants, and patrols; when an offense is detected, the suspect is almost always already in custody. The charge rate for these offenses thus depends less on investigative follow-up and more on legal thresholds and prosecutorial discretion. The high mean charge rates for drugs (35%) and weapons (47%) confirm that these offenses are processed differently from investigation-intensive crimes.

### 5.3 Robustness

**Table 4:** Robustness Checks

| Specification | $\hat{\beta}$ | SE | N |
|---|---|---|---|
| Baseline | 12.919*** | (3.487) | 334 |
| Excl. Metropolitan Police | 13.078*** | (3.511) | 326 |
| Excl. all London forces | 12.261*** | (3.430) | 318 |
| Excl. 2020 (COVID) | 13.111*** | (3.848) | 292 |
| Region × year FE | 10.204*** | (3.368) | 334 |
| Headcount (not FTE) | 13.826*** | (3.563) | 334 |
| Control: log(crimes) | 13.669*** | (3.342) | 334 |

*Notes:* Dependent variable is charge rate (%) in all specifications. All include force and year fixed effects unless noted. Standard errors clustered by force area in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 reports seven robustness checks, all yielding estimates between 10.2 and 13.8. The Metropolitan Police accounts for approximately 25% of officers in England and Wales and operates in a unique urban environment; excluding it changes the estimate from 12.9 to 13.1. Excluding both London forces (Metropolitan and City of London) yields 12.3. Dropping 2020, which saw both COVID-related disruption and the murder of George Floyd, yields 13.1. Using officer headcount rather than FTE produces 13.8, consistent with the high

correlation between the two measures. Controlling for the log of total recorded crimes yields 13.7, confirming that the result is not driven by denominator manipulation.

The pre-trend test provides the strongest support for the identification strategy. I estimate $\log(\text{Charges})_{ft} = \alpha_f + \gamma_t + \sum_{s=2008}^{2013} \delta_s(\text{CutIntensity}_f \times \mathbb{I}[t = s]) + \varepsilon_{ft}$, where $\text{CutIntensity}_f$ is the (future) percentage officer cut experienced between 2010 and 2015, and 2007 is the reference year. All interaction coefficients are small in magnitude and statistically insignificant ($p > 0.19$), confirming that forces with larger subsequent cuts were not on differential pre-treatment trajectories.

## 6. Discussion

The central finding is that police officer reductions during UK austerity are associated with a quantitatively important decline in criminal justice quality. The estimated relationship—a 10% increase in officers corresponds to a 1.3 percentage point higher charge rate—is large relative to the outcome mean and robust across specifications, subsamples, and outcome definitions. While the TWFE design cannot definitively establish causality without a credible instrument, the combination of supply-driven variation (formula-based grant cuts), flat pre-trends, and stability across demanding specifications provides suggestive evidence of a causal relationship. The heterogeneity pattern across offense types points to an investigative-capacity mechanism: fewer officers means fewer investigations brought to a successful conclusion, not fewer criminals caught in the act.

These results complement the existing literature on police and crime. Levitt (1997), Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), Draca et al. (2011), and Mello (2019) all find that more police reduce crime—the extensive margin. My findings show that police also matter on the intensive margin: conditional on a crime occurring, officer numbers affect the probability that the criminal justice system delivers a formal response. In the framework of Becker (1968), this represents a reduction in the *certainty of punishment*, a parameter that both theoretical and empirical work identifies as a key driver of deterrence (Nagin, 2013; Chalfin and McCrary, 2017).

The policy implications extend beyond budgetary debates. When charge rates fall, victims lose confidence that reporting crime leads to consequences, potentially reducing future reporting and cooperation. Prosecutors receive fewer cases, reducing court throughput. Offenders face a lower probability of accountability, weakening deterrence. The 2019 Police Uplift Programme, which committed to recruiting 20,000 additional officers, implicitly recognized that austerity had compromised police capacity. My estimates suggest this investment could raise the national charge rate by approximately 2 percentage points—

equivalent to thousands of additional charges per year for violence alone.

Several limitations warrant discussion. First, the TWFE design relies on within-force variation in officer numbers, which may partly reflect endogenous choices about how to implement budget cuts. If forces that cut more officers simultaneously changed other aspects of their operations—reallocating remaining officers from investigation to patrol, or investing in technology—the estimates may conflate officer numbers with organizational change. The robustness of the results to controlling for total crime volume and to the extended-panel specification, where officer variation is not driven by austerity alone, mitigates but does not eliminate this concern. Second, the main analysis panel (2014–2021) begins after most austerity cuts had been implemented, so the within-force variation partly reflects the ongoing consequences of earlier cuts rather than contemporaneous shocks. The extended-panel results (2007–2021), which span the full austerity period, yield consistent estimates. Third, an instrumental variables strategy—using pre-austerity council tax precept shares as a source of exogenous variation in officer cuts—would substantially strengthen the causal interpretation but awaits the availability of machine-readable force-level funding data.

## 7. Conclusion

Austerity reduced police officer numbers in England and Wales by 14% between 2010 and 2015. This paper shows that fewer officers meant not just more crime, but less justice: charge rates fell, investigations were curtailed, and the criminal justice system's core function—converting reported crimes into accountability—was degraded. The effects fell most heavily on crimes requiring sustained investigation, precisely the offenses where victims most depend on police to deliver consequences. These findings suggest that the true cost of police austerity extends well beyond crime statistics to the integrity of the criminal justice system itself.

## Acknowledgements

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# A. Data Appendix

## A.1 Data Sources

**Police Workforce Statistics.** Published annually by the Home Office, recording officer headcount and FTE by force, rank, and demographic characteristics as of 31 March each year. Available from the Home Office police workforce open data tables.[1] I use the consolidated open data table covering 2003–2023, filtering to "Police Officer" worker types and aggregating FTE and headcount by force and year.

   **Crime Outcomes Open Data.** Published by the Home Office in two formats: (i) a consolidated file covering financial years 2005/06 to 2013/14, using text labels for outcome types ("Charge/Summons," "Cautions," "PNDs," "TICs," "Cannabis warnings," "Other"); and (ii) annual files for 2014/15 to 2021/22, using numeric outcome type codes (1 = Charged/Summonsed through 22). I map the historical text labels to the numeric classification and aggregate by force, offense group, and financial year.

## A.2 Sample Construction

The analysis sample is constructed as follows:

1. Extract officer FTE by force and year from the workforce data (2003–2023).

2. Extract outcome counts by force, year, and outcome type from the combined historical and annual outcomes files.

3. Aggregate outcomes to force-year level, computing total outcomes and total charges (outcome type 1).

4. Merge workforce and outcomes panels on standardized force names and year.

5. Exclude non-territorial forces (British Transport Police) and national aggregates (England and Wales total), yielding 42 forces.

6. For the main analysis: restrict to 2014–2021 (consistent outcomes framework), producing 334 force-year observations.

7. For the extended analysis: use 2007–2021, producing 628 force-year observations.

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[1] https://www.gov.uk/government/statistics/police-workforce-open-data-tables

## A.3 Treatment Variable

The treatment variable is the percentage change in officer FTE between March 2010 (pre-austerity peak) and March 2015 (post-austerity trough), computed as $100 \times (\text{FTE}_{2015} - \text{FTE}_{2010})/\text{FTE}_{2010}$. All 42 forces experienced negative changes (officer reductions), ranging from $-23.1\%$ to $-1.4\%$. I define terciles of treatment intensity for descriptive purposes: large cuts ($> 15.6\%$ reduction, $N = 14$), medium cuts (10.2–14.9%, $N = 14$), and small cuts ($< 10.2\%$, $N = 14$).

# B. Identification Appendix

## B.1 Pre-Trend Test

The pre-trend test estimates:

$$\log(\text{Charges}_{ft}) = \alpha_f + \gamma_t + \sum_{s=2008}^{2013} \delta_s(\text{CutIntensity}_f \times \mathbb{I}[t = s]) + \varepsilon_{ft} \tag{2}$$

where $\text{CutIntensity}_f = -(\text{FTE}_{f,2015} - \text{FTE}_{f,2010})/\text{FTE}_{f,2010}$ is positive for forces with larger cuts, and 2007 is the omitted reference year. Under the null of parallel pre-trends, all $\delta_s = 0$.

Results: all six interaction coefficients (2008–2013) are individually insignificant ($p > 0.19$) and small relative to the post-period effects. The cleanest pre-treatment years are 2008 and 2009, before the Spending Review was announced in October 2010; both coefficients are near zero. The 2010–2013 coefficients span the period when austerity cuts were being implemented, so their insignificance reflects either the gradual phase-in of cuts or genuine parallel trends. This supports the assumption that forces with larger subsequent officer cuts were not on differential trajectories in criminal justice outcomes.

# C. Robustness Appendix

Table 4 in the main text reports seven robustness checks. Detailed notes: (i) Excluding the Metropolitan Police removes the force that accounts for approximately 25% of national officer strength and operates in a unique policing environment. (ii) Excluding both London forces (Metropolitan and City of London) tests whether the result is driven by the capital. (iii) Dropping 2020 addresses potential confounding from COVID-19 lockdowns, which disrupted both policing and crime patterns. (iv) Region-by-year fixed effects absorb differential trends across the nine English regions and Wales. (v) Using headcount instead of FTE addresses potential measurement error from part-time patterns. (vi) Controlling for log total recorded

crime addresses the possibility that officer reductions affected crime recording, which could mechanically alter the charge rate denominator.

## D. Standardized Effect Sizes

**Table 5:** Standardized Effect Sizes

| Outcome | $\hat{\beta}$ | SE | SD($Y$) | SDE | SE(SDE) | Classification |
|---|---|---|---|---|---|---|
| Charge rate (%) | 12.919 | 3.487 | 4.075 | 2.167 | 0.585 | Large positive |
| log(Charges) | 0.526 | 0.182 | 0.721 | 0.500 | 0.173 | Large positive |
| Charge rate: violence | 18.555 | 4.660 | 6.515 | 1.931 | 0.485 | Large positive |
| Charge rate: sexual offences | 4.903 | 4.952 | 4.844 | 0.678 | 0.684 | Large positive |
| Charge rate: theft offences | 12.969 | 3.506 | 3.592 | 2.448 | 0.662 | Large positive |
| Charge rate: drug offences | 16.276 | 10.101 | 8.085 | 1.365 | 0.847 | Large positive |

*Notes:* This paper estimates the effect of police officer reductions on criminal justice quality in England and Wales, 2014–2021. Treatment: within-force variation in log officer FTE (continuous). Method: two-way fixed effects with force and year FE, clustered SEs. SDE = $\hat{\beta} \times$ SD($X$)/SD($Y$) for continuous treatment. N = 334 force-years (main); N = 628 (extended, row 2). Classification refers to magnitude of the point estimate, not statistical significance. Source: Home Office open data.