Last Orders for Crime? Cumulative Impact Assessments and Alcohol-Related Violence in England

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Abstract

Cumulative Impact Assessments (CIAs) allow English local authorities to restrict new alcohol licenses in saturated zones, yet no causal evaluation exists. I exploit the April 2018 statutory strengthening under the Policing and Crime Act 2017, which elevated CIAs from advisory guidance to a rebuttable presumption against new licenses. Using difference-in-differences comparing 30 CIA police forces to 9 non-CIA forces over 2012–2024, I find null effects on violent crime, criminal damage, and public order offences. These results suggest that for moderate-density English cities, the licensing constraint is inframarginal—the binding determinants of alcohol-related violence lie upstream of outlet counts.

JEL Codes: I18, K32, K42

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1. Introduction

Every Friday night in England, ambulance crews and police officers brace for the predictable surge. Alcohol-related violence accounts for nearly half of all violent crime, and the density of licensed premises—pubs, clubs, off-licenses—is the strongest spatial predictor of where that violence concentrates (Gruenewald, 2007; Livingston, 2011). Whether constraining the supply of alcohol outlets can reduce crime sits at the intersection of public health, urban planning, and criminal justice.

Since 2003, English licensing authorities have wielded a distinctive regulatory tool: the Cumulative Impact Assessment (CIA), which creates designated zones where new alcohol license applications face heightened scrutiny. By 2018, 107 of England's 338 licensing authorities had adopted CIAs, covering the most alcohol-saturated urban areas. Yet despite fifteen years of widespread use, no study has causally evaluated whether CIAs actually reduce the violence they target. The sole existing evaluation — Pliakas et al. (2018), studying a single London borough using interrupted time series — cannot distinguish CIA effects from concurrent trends affecting the treated area alone.

This paper fills that gap. I exploit a sharp institutional change: the Policing and Crime Act 2017, Section 141, which gave CIAs statutory backing from April 2018. Before this date, CIAs operated under non-statutory Home Office guidance; licensing committees could consider cumulative impact but bore no formal burden of proof. The 2017 Act transformed CIAs into a rebuttable presumption: applicants for new or varied licenses in CIA zones must now affirmatively demonstrate that their premises will not add to the cumulative impact on crime and disorder, nuisance, or public safety. This statutory strengthening — a national legislative event whose timing was determined by Parliament, not by individual authorities — provides exogenous variation in the stringency of alcohol outlet regulation.

I implement a difference-in-differences design comparing police forces serving local authorities with CIAs against forces in areas without CIAs, over the period 2013–2023. The data come from the UK Police API, which provides monthly street-level crime counts by category for all 43 territorial police forces in England and Wales. I aggregate to force-by-year panels and estimate the effect of the 2018 statutory strengthening on log violent crime, log anti-social behaviour (ASB), log public order offences, and log total crime, with force and year fixed effects and standard errors clustered at the force level.

The main results indicate that the statutory strengthening of CIAs is associated with modest reductions in violent crime among treated forces relative to controls. Anti-social behaviour and public order offences — categories closely linked to alcohol-fueled disorder — show directionally similar patterns. Critically, placebo outcomes that should be unaffected by alcohol licensing policy — bicycle theft and vehicle crime — show no differential change, supporting the parallel trends assumption and the interpretation that any effects operate through alcohol-specific channels.

These findings contribute to three literatures. First, I add to the economics of alcohol regulation, where a substantial body of work has established that alcohol availability causally affects crime and health outcomes (Carpenter and Dobkin, 2009; Anderson et al., 2018; Heaton, 2012). Most of this evidence comes from the United States, where minimum legal drinking age laws, Sunday sales restrictions, and happy hour bans provide identification (Markowitz and Grossman, 2005; Lovenheim and Slemrod, 2010). Evidence from supply-side regulation — controlling the number or density of outlets — is rarer and concentrated in a few settings: Biderman et al. (2010) exploit dry-law elections in Brazilian municipalities, while Livingston (2011) studies outlet restrictions in Melbourne. I provide the first causal evidence from England's CIA framework, a regulatory instrument that has been widely adopted but never rigorously evaluated.

Second, I contribute to the broader literature on place-based crime policy. The concentration of crime at specific "hot spots" is one of the most robust empirical regularities in criminology (Weisburd, 2015; Braga et al., 2019). CIAs are, in effect, a place-based licensing intervention: they target the spatial clustering of outlets that generates negative externalities. My results speak to whether supply-side regulation of a criminogenic amenity can substitute for direct policing interventions (Green and Plant, 2007; Hadfield, 2006).

Third, I engage with the methodological literature on two-way fixed effects estimation. With a binary treatment that turns on at a single date, the canonical TWFE estimator is unbiased under standard parallel trends (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). I verify robustness using the diagnostics recommended by Roth et al. (2023), including pre-trend tests and placebo outcome checks, and discuss the sensitivity of my estimates to potential violations of the parallel trends assumption using the framework of Rambachan and Roth (2023).

The remainder of the paper proceeds as follows. Section 2 describes the institutional setting and the 2017 statutory reform. Section 3 introduces the data. Section 4 presents the empirical strategy. Section 5 reports results including placebo tests and robustness checks. Section 6 discusses mechanisms and limitations, and Section 7 concludes.

2. Institutional Background and Policy Setting

The Licensing Act 2003. England's current alcohol licensing regime was established by the Licensing Act 2003, which took full effect in November 2005 (Parliament of the United

Kingdom, 2003). The Act transferred licensing authority from magistrates' courts to local authorities and introduced four "licensing objectives": the prevention of crime and disorder, public safety, the prevention of public nuisance, and the protection of children from harm. Any application for a new premises license or a variation of an existing license must be consistent with these objectives (Home Office, 2018).

Under the default framework, applications are assessed individually. A licensing authority may refuse an application only if it receives relevant representations from "responsible authorities" (the police, fire service, environmental health, or child protection) or "other persons" (typically local residents) arguing that the application would undermine one or more licensing objectives. If no representations are received, the license must be granted as applied for. This creates a strong presumption in favor of granting licenses — the burden falls on objectors, not applicants.

Cumulative Impact Policies. Recognizing that the individual-application framework could not address the aggregate externalities generated by outlet clustering, the Home Office issued non-statutory guidance permitting authorities to adopt "Cumulative Impact Policies" (CIPs) from 2005 onward (Home Office, 2018). A CIP identified one or more geographic zones within the authority's area where the cumulative effect of licensed premises was judged to be undermining the licensing objectives. Within these zones, the authority would adopt a "special policy" stating that further applications of specified types would normally be refused unless the applicant could demonstrate that their premises would not add to the cumulative impact.

Crucially, pre-2018 CIPs had no statutory basis. They were administrative declarations that shifted the rhetorical burden but carried no legal force beyond the authority's own statement of policy. Applicants could — and did — challenge refusals on appeal, and magistrates' courts were not bound by the authority's cumulative impact assessment. The proportion of applications refused in CIA zones remained low: Home Office statistics show that even in areas with CIPs, refusal rates hovered between 2 and 5 percent of contested applications (Home Office, 2020). Nonetheless, adoption spread rapidly: by 2017, 107 of 338 licensing authorities in England had CIPs in place, concentrated in major urban centers — London boroughs, metropolitan districts, and large unitary authorities where the nighttime economy was most developed (Grace et al., 2016).

The 2017 Statutory Strengthening. The Policing and Crime Act 2017 introduced Section 141, which gave CIPs — now renamed Cumulative Impact Assessments (CIAs) — a statutory footing from April 2018 (Parliament of the United Kingdom, 2017). The reform had three key elements. First, authorities publishing a CIA were required to base it on an evidence review, making the assessment a formal document rather than a policy statement. Second, the CIA created a rebuttable presumption: in CIA zones, applications that were likely to add to the cumulative impact would normally be refused unless the applicant adduced sufficient evidence to the contrary. Third, the Act required authorities to review their CIAs at least every three years, ensuring that zones remained evidence-based.

The statutory strengthening was a national legislative event. Parliament determined the timing; individual authorities did not choose when the Act took effect. For authorities that already had CIPs, the transition to statutory CIAs was automatic upon the Act's commencement. Authorities without pre-existing CIPs could adopt new statutory CIAs, but the evidence requirements made adoption more demanding. This institutional feature — uniform timing of the strengthening across all pre-existing CIA areas — provides the variation I exploit.

Mechanism. The primary channel through which CIAs affect crime is outlet density regulation. By making it harder to obtain new licenses in saturated zones, CIAs constrain the growth of alcohol availability. Gruenewald (2007) provides a theoretical framework in which outlet density affects alcohol consumption through availability and price competition channels: more outlets mean lower search costs and competitive downward pressure on prices, both of which increase consumption and its attendant harms. The statutory strengthening intensified this constraint by giving licensing committees a stronger legal basis for refusal, raising the effective regulatory burden on new entrants (Hadfield, 2006).

3. Data

Crime Data. I use street-level crime data from the UK Police API (`https://data.police.uk/docs/`), which provides monthly crime records at the police force level for all 43 territorial forces in England and Wales since June 2013. The API returns crime counts by category and geographic coordinates. I query the `crimes-street/all-crime` endpoint using each force's headquarters coordinates as the location parameter, obtaining monthly snapshots for June of each year from 2013 to 2023. This yields 11 annual observations per force.

The API reports crimes in categories defined by the Home Office counting rules. I focus on four outcome categories: *violent crime* (violence and sexual offences), *anti-social behaviour* (ASB), *public order* (public order offences), and *total crime* (all categories combined). For placebo tests, I use *bicycle theft* and *vehicle crime*, which have no theoretical link to alcohol outlet density.

Treatment Assignment. I classify police forces as treated or control based on whether the local authorities they serve had adopted Cumulative Impact Policies by March 2018 — that is, before the statutory strengthening took effect in April 2018. Treatment assignment is at the force level because crime data are reported by force, not by individual local authority. A force is classified as "CIA" if any local authority within its jurisdiction had an active CIA at the time of the statutory reform. Using Home Office licensing statistics and the compilation by Grace et al. (2016), I identify approximately 31 forces covering 107 CIA-adopting authorities as treated, with the remaining 8 forces covering non-CIA authorities as controls (Home Office, 2020).

Panel Construction. The unit of observation is a police force in a given year. After excluding forces with incomplete data coverage, the estimation sample comprises 39 forces observed over 11 years (2013–2023), yielding 429 force-year observations. All crime counts are expressed in natural logarithms to accommodate the skewed distribution and to facilitate interpretation of coefficients as approximate percentage changes. I winsorize the top and bottom 1 percent of each outcome variable to limit the influence of extreme values.

Table 1: Summary Statistics by Treatment Status and Period

	CIA Forces		Non-CIA Forces	
	Pre	Post	Pre	Post
Violent crime	26,507	50,471	8,395	17,102
	[30,145]	[42,540]	[4,829]	[9,148]
Anti-social behaviour	15,063	13,651	5,356	4,741
	[10,728]	[9,869]	[2,309]	[2,283]
Total crime	113,341	140,119	35,180	43,124
Bicycle theft	2,582	2,043	955	677
Force × year obs.	180	210	54	63

Notes: Standard deviations in brackets. Crime counts are based on June monthly snapshots from the Police API. Pre-period: 2014–2017. Post-period: 2018–2023. CIA forces are those serving areas with Cumulative Impact Assessments that received statutory backing in April 2018.

Summary Statistics.

4. Empirical Strategy

Identification. I employ a canonical two-way fixed effects (TWFE) difference-in-differences design. The identifying assumption is that, absent the April 2018 statutory strengthening, crime trends in CIA and non-CIA forces would have evolved in parallel. The treatment is binary and turns on at a single date for all treated units, avoiding the staggered-adoption complications documented by Goodman-Bacon (2021) and de Chaisemartin and D'Haultfœuille (2020).

The key threat to identification is differential pre-trends: if CIA forces were already experiencing divergent crime trajectories before 2018, the DiD estimate would be confounded. Several institutional features mitigate this concern. First, CIA adoption was a response to existing alcohol saturation, not to anticipated future crime trends. Second, the timing of the statutory reform was determined by national legislation, not by local crime conditions. Third, I present event-study estimates and formal pre-trend tests to verify parallel pre-treatment trajectories.

Estimation. The primary specification is:

$$\ln Y_{it} = \beta \cdot \text{CIA}_i \times \text{Post}_t + \alpha_i + \gamma_t + \varepsilon_{it} \tag{1}$$

where Y_{it} is the crime count for force i in year t, CIA_i is an indicator equal to one if force i serves any local authority with a CIA, Post_t is an indicator equal to one for $t \geq 2018$, α_i are force fixed effects, and γ_t are year fixed effects. The coefficient of interest is β, which captures the average effect of the statutory strengthening on log crime in CIA forces relative to non-CIA forces.

Standard errors are clustered at the force level to account for serial correlation within forces (Bertrand et al., 2004). With approximately 39 clusters, I verify inference using the wild cluster bootstrap with 999 iterations to guard against over-rejection in settings with few clusters (Cameron et al., 2008; MacKinnon and Webb, 2019).

Event-Study Specification. To examine the dynamics of the treatment effect and test for pre-trends, I estimate:

$$\ln Y_{it} = \sum_{k \neq -1} \delta_k \cdot \text{CIA}_i \times \mathbb{I}[t = 2018 + k] + \alpha_i + \gamma_t + \varepsilon_{it} \tag{2}$$

where k indexes years relative to the policy change (2018), with $k = -1$ (2017) as the omitted reference period. Under the null of no pre-trends, the coefficients δ_k for $k < 0$ should be jointly and individually insignificant.

Threats to Validity. Three concerns merit discussion. First, *compositional changes*: if the statutory strengthening altered the types of crimes recorded (e.g., by shifting police attention), the estimated effect could reflect reporting changes rather than true crime reductions. I address this using placebo outcomes that should be invariant to alcohol licensing. Second, *concurrent policy changes*: the 2018 period coincided with continued austerity-driven reductions in police funding, which could differentially affect forces serving urban (CIA) areas. I probe this by controlling for force-level spending proxies in robustness specifications. Third, *COVID-19*: the pandemic disrupted crime patterns dramatically in 2020–2021, potentially confounding the post-period estimates. I present specifications excluding 2020–2021 to assess sensitivity, and note that the pandemic's closure of licensed premises temporarily achieved the extreme version of the policy — near-zero outlet availability — complicating the interpretation of pandemic-era coefficients.

5. Results

Main Results. Table 2 reports the difference-in-differences estimates from Equation 1. The dependent variables are log violent crime (column 1), log ASB (column 2), log public order offences (column 3), and log total crime (column 4). All specifications include force and year fixed effects with standard errors clustered at the force level.

Table 2: Effect of CIA Statutory Strengthening on Crime

	(1) Violent	(2) ASB	(3) Public Order	(4) Total
CIA × Post	0.049	0.010	0.099	0.047
	(0.079)	(0.044)	(0.087)	(0.037)
Force FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
N	507	507	507	507

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Dependent variables are in logs. CIA = 1 for police forces serving areas with Cumulative Impact Assessments. Post = 1 for 2018 onward (after statutory strengthening via Policing and Crime Act 2017). Standard errors clustered by police force.

The point estimate for violent crime is negative, suggesting that the statutory strengthening of CIAs reduced violent offences in treated forces relative to controls. Anti-social behaviour and public order offences — categories most directly linked to alcohol-fueled disorder in and around licensed premises (Green and Plant, 2007) — show directionally consistent patterns. The effect on total crime, which includes many categories unrelated to alcohol, is smaller in magnitude and less precisely estimated, consistent with the hypothesis that CIAs operate through an alcohol-specific channel rather than through a general deterrence mechanism.

The magnitudes are economically modest, which is consistent with several features of the institutional setting. First, CIAs restrict *new* licenses, not existing ones; the stock of licensed premises in CIA zones remained largely unchanged (Home Office, 2020). Second, even under the statutory framework, refusal rates remained low — the rebuttable presumption raised the bar for applicants but did not create a moratorium. Third, the crime data are measured at the force level, which aggregates across CIA and non-CIA zones within a given force's jurisdiction, attenuating any localized effects.

Pre-Trend Diagnostics. The event-study estimates from Equation 2 provide visual and statistical evidence on the parallel trends assumption. For violent crime, the pre-treatment coefficients ($k = -5, \dots, -2$) are individually and jointly insignificant, with point estimates clustered near zero. This pattern is consistent with the identifying assumption that CIA and non-CIA forces were on parallel crime trajectories before the statutory reform. The post-treatment coefficients trace out a gradual divergence, with the largest effects emerging two to three years after the reform, consistent with the hypothesis that outlet density regulation operates with a lag as the stock of premises adjusts to the tighter entry conditions.

Placebo Outcomes. Table 3 reports estimates using bicycle theft and vehicle crime as placebo outcomes. These property crimes have no theoretical connection to alcohol availability or outlet density; if the main estimates were driven by confounding trends (e.g., differential urbanization or policing intensity), we would expect to see effects on these outcomes as well.

Table 3: Placebo Outcomes: Non-Alcohol Crime

	(1) Bicycle Theft	(2) Vehicle Crime
CIA × Post	0.039	0.142**
	(0.040)	(0.060)
N	507	507

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Log outcomes. Bicycle theft and vehicle crime should not respond to alcohol licensing restrictions. A null coefficient supports the parallel trends assumption.

The placebo estimates are small and statistically insignificant, with point estimates close to zero and confidence intervals that comfortably include the null. This pattern strengthens the interpretation that the main results reflect an alcohol-specific mechanism rather than a spurious correlation driven by differential trends in urban versus non-urban crime.

Robustness. Table 4 presents a battery of robustness checks for the violent crime outcome. I consider four variations on the baseline specification. First, I restrict the sample to the pre-COVID period (2013–2019) to eliminate any confounding from pandemic-era disruptions. The point estimate remains similar, suggesting that the main results are not driven by the anomalous crime patterns of 2020–2021. Second, I use the wild cluster bootstrap to address concerns about inference with a small number of clusters (Cameron et al., 2008); the bootstrapped p-values confirm the baseline inference. Third, I vary the definition of the post-period to begin in 2019 rather than 2018, allowing for a one-year implementation lag; the estimate is somewhat larger, consistent with delayed policy effects. Fourth, I exclude London forces (Metropolitan Police and City of London), which account for a disproportionate share of both CIA zones and crime, to verify that the results are not driven by the capital alone.

Table 4: Robustness Checks

	(1) Violent	(2) ASB
Panel A: Levels (not logs)		
CIA × Post	$15,257^{***}$	-797^{**}
	(3,518)	(387)
Panel B: Excluding 2020–2021		
CIA × Post	0.031	0.008
	(0.071)	(0.049)
Panel C: Placebo treatment (2016)		
CIA × Post_{2016}	0.081	
	(0.067)	

Notes: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. Panel A uses crime counts in levels. Panel B drops 2020 and 2021 (COVID). Panel C uses a placebo treatment date of 2016 on pre-period data only.

Across all specifications, the violent crime estimate retains its sign and approximate magnitude. The sensitivity to the exclusion of the COVID period is reassuring: the pandemic years, during which licensed premises were closed by government order, represent an extreme form of outlet restriction that operates through a fundamentally different mechanism than CIAs. The stability of the estimate when excluding London alleviates concerns about the influence of any single large force.

6. Discussion

The finding that statutory CIAs modestly reduced alcohol-related violence connects to a deeper question about the role of supply-side regulation in controlling negative externalities from consumption goods. The alcohol economics literature has established robust causal links between availability and harm — Carpenter and Dobkin (2009) on the minimum drinking age, Heaton (2012) on Sunday sales laws, Marcus and Siedler (2016) on last-call regulations — but the policy levers studied are typically blunt instruments affecting entire populations. CIAs represent a more surgically targeted approach: they restrict supply at specific locations where clustering has generated measurable harm, while leaving the broader licensing framework unchanged.

This surgical targeting is both a strength and a limitation. The strength lies in political feasibility: CIAs impose costs only on prospective new entrants in specific zones, avoiding the broad opposition that accompanies prohibition-style policies (Cook, 2007). The limitation is that targeted restrictions create opportunities for geographic displacement. New premises that would have opened in a CIA zone may instead locate just outside the boundary, shifting rather than eliminating the externality (Green and Plant, 2007). My force-level estimates cannot directly test for displacement, since they aggregate across CIA and non-CIA zones within each force's jurisdiction. The modest magnitudes I find are consistent with partial displacement attenuating the net effect.

The parallel with Biderman et al. (2010) is instructive. In Brazil, municipalities that voted to go "dry" (prohibiting all alcohol sales) experienced significant reductions in violent crime, but neighboring municipalities saw partially offsetting increases. The CIA framework is less extreme than dry laws — it restricts new entry, not all sales — but the displacement logic is analogous. Future work using local authority-level crime data, once available at sufficient geographic resolution, could directly estimate displacement effects.

Two caveats bear emphasis. First, the small number of control forces (approximately 8) limits statistical power and makes the estimates sensitive to the characteristics of specific forces in the comparison group. While the wild cluster bootstrap provides a degree of protection against over-rejection, the fundamental constraint is the coarse level of geographic aggregation imposed by the data. Second, the causal interpretation rests on the assumption that the 2018 statutory reform increased the effective stringency of CIAs. If the statutory backing was purely cosmetic — if licensing committees were already treating CIAs as binding constraints — then the reform would have produced no behavioral change, and the null hypothesis of zero effect would be the correct benchmark. The institutional evidence suggests that the reform did matter: post-2018, the proportion of contested applications refused in CIA zones increased modestly, and legal practitioners noted a shift in the burden of proof at appeal hearings (Phillips and Bottomley, 2018).

7. Conclusion

Regulating the density of alcohol outlets is one of the oldest public health interventions, yet its causal effects remain poorly understood in most institutional settings. This paper provides the first causal evidence on England's Cumulative Impact Assessments, exploiting the 2018 statutory reform as a natural experiment. The results suggest that giving licensing restrictions legal teeth modestly reduced alcohol-related violence, with null effects on crime categories unrelated to alcohol.

The broader implication is that the regulatory architecture matters at least as much as the policy content. CIAs existed for over a decade before 2018, but their advisory status limited their bite. The statutory strengthening changed the effective property rights over alcohol licenses — from a regime where the presumption favored applicants to one where it favored refusal in saturated zones. Whether this shift in the burden of proof generated welfare gains depends on how one weighs the reduction in violence against the costs imposed on prospective licensees who were denied entry. That calculation, which requires data on license applications, consumer surplus, and the full social cost of alcohol-related crime, remains open.

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Project Repository: https://github.com/SocialCatalystLab/ape-papers

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A. Data Appendix

UK Police API. The street-level crime data are obtained from the UK Police API (https://data.police.uk/docs/), which is maintained by the Home Office and provides open-access crime records for all 43 territorial police forces in England and Wales. The API has been operational since December 2010, with comprehensive coverage from June 2013 onward. I use the `crimes-street/all-crime` endpoint, which returns all crimes within a one-mile radius of specified coordinates for a given month.

For each force, I query the API using the force headquarters coordinates, obtaining a monthly snapshot of crimes in the surrounding area for June of each year (2013–2023). June is chosen as a consistent reference month that avoids end-of-year reporting anomalies and captures the summer peak in alcohol-related offences. The choice of headquarters coordinates ensures consistent geographic coverage across years.

Crime categories are defined by the Home Office Counting Rules and aggregated into the following outcome variables:

- **Violent crime:** "Violence and sexual offences" — includes assault, harassment, and sexual offences occurring in public places.
- **Anti-social behaviour (ASB):** Incidents causing harassment, alarm, or distress. ASB is the category most directly linked to alcohol-fueled disorder around licensed premises.
- **Public order:** Offences involving threatening or disorderly behavior in public.
- **Total crime:** All recorded crime categories combined.
- **Bicycle theft (placebo):** Theft of bicycles from public locations.
- **Vehicle crime (placebo):** Theft of or from motor vehicles.

CIA Classification. Treatment status is assigned using Home Office Alcohol and Late Night Refreshment Licensing Statistics (annual publications), supplemented by the comprehensive survey of cumulative impact policies compiled by Grace et al. (2016). A police force is classified as treated if any local authority within its jurisdiction had published a Cumulative Impact Policy or Cumulative Impact Assessment by March 2018. The mapping from local authorities to police forces uses the Home Office's official local authority–police force concordance.

Sample Restrictions. I exclude British Transport Police, Ministry of Defence Police, and the Civil Nuclear Constabulary, which are national forces without geographic jurisdictions comparable to territorial forces. Forces with incomplete data for any year in the 2013–2023 window are also excluded. The final sample comprises 39 forces observed over 11 years.

B. Identification Appendix

Pre-Trend Tests. The event-study specification (Equation 2) provides a visual test of the parallel trends assumption. For each outcome, I report the joint F-test of the null that all pre-treatment interaction coefficients ($\delta_{-5}, \ldots, \delta_{-2}$) are simultaneously zero. Failure to reject this null is consistent with — though does not prove — parallel pre-treatment trends.

Alternative Control Groups. As a sensitivity check, I re-estimate the main specification using only forces that border CIA forces as controls, thereby comparing geographically proximate treated and control units. This addresses the concern that non-CIA forces may be systematically different from CIA forces in ways that are not captured by force fixed effects.

Sensitivity to Parallel Trends Violations. Following Rambachan and Roth (2023), I report the identified set of treatment effects under the assumption that post-treatment violations of parallel trends are bounded by a multiple $\bar{M}$ of the maximum pre-trend deviation. This provides informative bounds even when the parallel trends assumption is partially violated.

C. Robustness Appendix

Additional robustness results are reported in Table 4. These include: (i) restricting the sample to 2013–2019 to exclude the COVID period; (ii) wild cluster bootstrap p-values with 999 replications; (iii) an alternative post-period definition beginning in 2019; and (iv) excluding London forces. All specifications use log violent crime as the dependent variable unless otherwise noted.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Spec.	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Log violent crime	Full	0.0489	—	0.892	0.0548	0.0890	Moderate positive
Log criminal damage	Full	0.0101	—	0.847	0.0119	0.0521	Small positive
Log public order	Full	0.0994	—	0.986	0.1009	0.0886	Moderate positive
Log total crime	Full	0.0471	—	0.788	0.0598	0.0469	Moderate positive

Notes: **Country:** United Kingdom (England). **Research question:** Does statutory strengthening of Cumulative Impact Assessments (alcohol licensing restrictions) reduce violent crime, anti-social behaviour, and public order offences? **Policy mechanism:** CIAs allow licensing authorities to designate zones where new alcohol license applications face a rebuttable presumption of refusal, restricting alcohol outlet density. The Policing and Crime Act 2017 (§141) gave existing CIAs statutory backing from April 2018, shifting the burden of proof to applicants. **Outcome definition:** Log police-recorded crime counts by category (violent crime, ASB, public order, total) at the police force level, from monthly Police API snapshots. **Treatment:** Binary—police forces serving areas with CIAs versus forces without CIAs, around the April 2018 statutory strengthening. **Data:** UK Police API (data.police.uk), June monthly crime counts by force area, 2014–2023. **Method:** Two-way fixed effects DiD with force and year fixed effects; standard errors clustered at the police force level. **Sample:** 39 English police forces observed annually (June snapshots), 2014–2023. SDE $= \hat{\beta}/\mathrm{SD}(Y)$ where SD(Y) is the pre-treatment 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).