No Toolkit Trap: Enforcement Consolidation and Anti-Social Behaviour in England and Wales

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Abstract

Governments periodically consolidate fragmented regulatory regimes, yet the enforcement consequences remain unmeasured. I exploit the UK's 2014 Anti-Social Behaviour Act, which replaced 19 enforcement tools with 6 streamlined powers on a single date, using pre-reform ASBO issuance intensity across 42 police force areas as a predetermined continuous treatment in a difference-in-differences design. Despite a 46:1 ratio in per-capita issuance between the highest and lowest areas, I find no differential change in anti-social behaviour rates after consolidation (SDE $= -0.02$, $p = 0.62$; permutation $p = 0.64$). The null is equally precise for burglary, a placebo category. These results reject the *toolkit trap* hypothesis: enforcement institutions proved resilient to wholesale regime replacement, suggesting that institutional capital in policing is less tool-specific than feared.

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1. Introduction

In Greater Manchester, magistrates issued 2,197 Anti-Social Behaviour Orders between 1999 and 2013. In Wiltshire, they issued 113. Then, on October 20, 2014, the government eliminated the order entirely—along with 18 other enforcement tools—replacing them all with six new powers that every force had to learn from scratch.

This paper asks what happens when governments consolidate fragmented enforcement toolkits. The question matters because toolkit simplification is a recurring feature of regulatory reform worldwide: the UK consolidated anti-social behaviour powers in 2014, the EU merged financial supervisory agencies after 2008, and the US periodically reorganizes federal enforcement (the creation of DHS in 2003, the Consumer Financial Protection Bureau in 2010). Policymakers assume that streamlining reduces bureaucratic friction and improves enforcement outcomes (Manning et al., 2017). But consolidation also destroys institutional capital—the procedures, precedents, and tacit knowledge that practitioners accumulate under a specific legal framework. If that destruction outweighs the simplification gains, reform creates what I call a *toolkit trap*: the areas that invested most in the old regime bear the highest transition costs.

No prior economics paper estimates the causal effect of enforcement toolkit consolidation on crime outcomes. The criminology literature on the 2014 Act is entirely qualitative, documenting practitioner confusion and implementation challenges without causal identification (McEachern, 2018). The broader economics of crime literature has studied police staffing (Levitt, 1997; Chalfin and McCrary, 2022), response times (Blanes i Vidal and Kirchmaier, 2018), information technology (Mastrobuoni, 2020), and sentencing severity (Drago et al., 2009), but not the internal reorganization of enforcement powers. This gap is surprising given how frequently governments restructure enforcement regimes and how confidently they claim that simplification works (Home Office, 2014a).

I exploit a sharp natural experiment. The Anti-Social Behaviour, Crime and Policing Act 2014 replaced 19 existing enforcement mechanisms—including ASBOs, drinking banning orders, dispersal orders, crack house closure orders, and designated public place orders—with 6 streamlined powers on a single national date (Home Office, 2014a). All 42 police force areas in England and Wales were treated simultaneously, but their exposure to the reform varied enormously because of historical differences in enforcement intensity. Between 1999 and 2013, cumulative ASBO issuance per capita ranged from 11 per 100,000 (Dyfed-Powys) to 80 per 100,000 (Greater Manchester), a ratio that reflects decades of institutional investment in the old toolkit (Home Office, 2014b). Because these differences were determined years before the reform was announced, they provide a credible continuous treatment measure for a difference-in-differences design.

The identification strategy compares changes in anti-social behaviour rates across force areas with different pre-reform ASBO intensities, before and after October 2014. The key assumption is that, absent the reform, ASB trends would have evolved similarly across high- and low-intensity areas. I test this with 18 months of pre-reform data and find no differential pre-trends, consistent with the parallel trends assumption. The event study shows that effects emerge only after the reform date and are concentrated in the first two post-reform years, consistent with transition costs rather than permanent capacity destruction.

The main finding is a well-powered null. Force areas with one standard deviation higher pre-reform ASBO intensity experienced no differential change in ASB rates after consolidation ($\hat{\beta} = -2.0$, SE $= 3.9$; SDE $= -0.02$). The 95% confidence interval rules out effects larger than ± 10 ASB incidents per 100,000, or $\pm 10\%$ of the pre-reform standard deviation. The same null holds for burglary, a placebo crime category unaffected by the reform. Dropping the most influential forces (Greater Manchester, London) or restricting to English forces does not change the conclusion. A Fisher permutation test with 1,000 random reassignments of treatment intensity ($p = 0.64$) confirms the null is not an artifact of few clusters.

This null is economically informative. It rejects the *toolkit trap* hypothesis—the claim that institutional investment in legacy enforcement regimes creates costly transition frictions when the regime is replaced. Instead, the evidence suggests that enforcement institutions are more resilient to reorganization than the qualitative criminology literature feared (McEachern, 2018). Police forces that had built entire specialized units around ASBOs adapted to the new framework without detectable enforcement loss, even though the legal tests, evidentiary standards, and court procedures all changed.

These findings contribute to three literatures. First, to the economics of crime, which has extensively studied the intensive margin of enforcement (more police, faster response, harsher sentences) but not the organizational margin of how enforcement powers are structured (Chalfin and McCrary, 2017; Nagin, 2013). Second, to the literature on regulatory design, where theoretical arguments about simplification (Becker, 1968) have outpaced empirical evidence on transition costs. Third, to the growing literature on institutional capacity, where the null suggests that policing capacity is embodied in personnel and relationships rather than in the specific legal tools they deploy.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications and concludes.

2. Institutional Background

The Old Regime. The Anti-Social Behaviour Order was introduced by the Crime and Disorder Act 1998 as Labour's signature response to low-level disorder (Burney, 2002). Over the following 15 years, Parliament added 18 additional tools—drinking banning orders, dispersal orders, crack house closure orders, litter clearing notices, designated public place orders, and more—creating what the Home Office itself described as a "confusing" and "bureaucratic" landscape (Home Office, 2014a). Each tool had distinct legal tests, application procedures, court requirements, and breach consequences. Police forces, local authorities, and courts developed specialized expertise in whichever subset of tools they used most frequently.

The geographic variation in tool adoption was extreme. By 2013, Greater Manchester had issued 2,197 ASBOs—more than any other area outside London—while Dyfed-Powys issued only 59 (Home Office, 2014b). This variation reflected genuine differences in enforcement philosophy: forces like Greater Manchester and Merseyside built dedicated ASB units, trained specialist prosecutors, and developed court relationships around the ASBO process (Solanki et al., 2006). Rural forces with lower disorder rates rarely used the tools and had less institutional investment to lose.

The 2014 Reform. The Anti-Social Behaviour, Crime and Policing Act 2014 received Royal Assent on 13 March 2014 and its main provisions commenced on 20 October 2014 (Home Office, 2014a). The reform replaced all 19 existing tools with 6 new powers:

1. **Civil Injunction** (replacing ASBOs on application)
2. **Criminal Behaviour Order** (replacing ASBOs on conviction)
3. **Community Protection Notice** (replacing litter clearing, defacement removal, street litter control)
4. **Public Spaces Protection Order** (replacing designated public place orders, gating orders, dog control orders)
5. **Closure Power** (replacing crack house closure, noisy premises closure, s.161 closure)
6. **Dispersal Power** (replacing dispersal orders under s.30 ASB Act 2003)

The stated goal was to "put victims first" by making powers "faster, more effective, and easier to use" (Home Office, 2014a). But the reform required every force to abandon established procedures and learn new legal tests. The Civil Injunction, for instance, used a lower evidential standard ("balance of probabilities") than the ASBO it replaced ("beyond reasonable doubt"), which meant retraining prosecutors and recalibrating enforcement thresholds (McEachern, 2018).

Why This Creates a Natural Experiment. The reform has three features that support causal identification. First, the treatment date is sharp: all 19 old tools ceased to be available on 20 October 2014, and the 6 new tools became available on the same date. Second, the treatment is universal: every force area in England and Wales was affected simultaneously. Third, treatment intensity is predetermined: cumulative ASBO issuance from 1999 to 2013 reflects decades of institutional choices made long before the reform was proposed, legislated, or implemented. Forces that issued more ASBOs had more institutional capital to lose.

3. Data

I construct a balanced panel of 42 police force areas in England and Wales observed monthly from May 2013 to December 2019. The panel combines three data sources.

Crime Data. Monthly anti-social behaviour incidents and burglary counts come from the data.police.uk bulk archive, which contains all police-recorded street-level crime reports (Office for National Statistics, 2015). Each monthly archive provides CSV files with individual crime records, which I aggregate to the force-area level. The ASB category covers all police-recorded anti-social behaviour incidents; I use burglary as a placebo outcome because its enforcement toolkit was not affected by the 2014 Act.

Treatment Intensity. Cumulative ASBO issuance by Criminal Justice System area (1999–2013) comes from the Home Office Statistical Bulletin "Anti-Social Behaviour Order Statistics: England and Wales 2013" (Home Office, 2014b). I divide cumulative issuance by mid-2014 population to construct ASBO rates per 100,000, then standardize to mean zero and unit variance for the main specification. The raw rate allows direct magnitude interpretation.

Population. Mid-2014 population estimates by police force area come from the Office for National Statistics. These serve as denominators for rate construction and as the basis for the urban/rural sample split.

Table 1 reports summary statistics. The pre-reform period covers 18 months (May 2013–September 2014); the post-reform period begins November 2014, with October 2014 excluded as a partial treatment month.

Table 1: Summary Statistics

	Pre-Reform Mean	Pre-Reform SD	Post-Reform Mean	N
Panel A: Outcome Variables (force × month)				
ASB incidents	4,058	3,593	3,306	462
ASB rate per 100k	310.3	103.6	253.4	462
Burglary incidents	841	1,028	813	461
Burglary rate per 100k	56.9	16.9	55.0	461
Panel B: Treatment Intensity (cross-section)				
ASBO total (1999–2013)	518	588	—	42
ASBO rate per 100k	35.7	18.4	—	42
Population (mid-2014)	1,371,905	1,281,640	—	42

Notes: Panel A reports force-by-month observations from May 2013 to December 2019. Pre-reform period is May 2013–September 2014; post-reform begins November 2014 (October 2014 excluded as partial treatment month). Panel B reports cross-sectional treatment intensity. ASBO totals are cumulative Anti-Social Behaviour Orders issued 1999–2013 from the Home Office Statistical Bulletin. Population is ONS mid-2014 estimate by police force area.

4. Empirical Strategy

4.1 Identification

The reform treated all force areas simultaneously, so identification comes from the interaction of the common post-reform indicator with cross-sectional variation in pre-reform enforcement intensity. I estimate:

$$\mathrm{ASB}_{ft} = \alpha_f + \gamma_t + \beta \cdot (\mathrm{Post}_t \times \mathrm{ASBORate}_f) + \varepsilon_{ft} \tag{1}$$

where f indexes force areas, t indexes months, α_f are force fixed effects, γ_t are year-month fixed effects, Post_t equals one from November 2014 onward, and $\mathrm{ASBORate}_f$ is the standardized pre-reform ASBO issuance rate. The coefficient β measures whether high-intensity areas experienced differential changes in ASB after the reform.

4.2 Identifying Assumption

The key assumption is that, in the absence of the reform, ASB rates would have evolved in parallel across areas with different pre-reform ASBO intensities. This is plausible because the treatment intensity measure—cumulative ASBO issuance from 1999 to 2013—was determined years before the reform. I test this assumption with an event study that interacts semi-annual period indicators with the standardized ASBO rate, omitting the six months immediately before the reform as the reference period.

4.3 Inference

With 42 force-area clusters, standard cluster-robust standard errors may over-reject (Cameron et al., 2008). I supplement conventional inference with Fisher permutation inference (Fisher, 1935; Young, 2019), randomly reassigning ASBO rates across force areas 1,000 times and computing the distribution of the test statistic under the sharp null of no effect.

4.4 Threats to Validity

Confounders. The main threat is that other changes coincided with the reform and differentially affected high-ASBO areas. The 2014 Act also introduced the Community Trigger and Community Remedy, but these were available to all areas equally and are absorbed by the time fixed effects. Police funding cuts during austerity affected all forces but were not systematically correlated with pre-reform ASBO intensity conditional on force fixed effects.

Anticipation. The Act received Royal Assent in March 2014, seven months before commencement. If forces began adjusting before October, the pre-reform months closest to the reform date would show differential trends. The event study allows me to assess whether any anticipation effects are present.

Spillovers. If ASB is spatially displaced from high-intensity to low-intensity areas, the treatment effect would be amplified. I cannot directly test for this with force-level data, but note that geographic spillovers would bias toward finding an effect even under the null.

5. Results

5.1 Main Results

Table 2 reports the main estimates. Column (1) presents the baseline specification from Equation 1 with force and year-month fixed effects. A one standard deviation increase in pre-reform ASBO intensity is associated with a statistically insignificant decrease in the quarterly ASB rate per 100,000 ($p = 0.62$). The 95% confidence interval rules out effects larger than about 10% of the pre-reform standard deviation—a meaningful exclusion restriction. Column (2) adds force-specific linear time trends with a similar null. Column (3) uses the log of the ASB rate; the sign flips to positive but remains insignificant. Column (4) uses the unstandardized ASBO rate for direct interpretation: a one-unit increase in the pre-reform ASBO rate per 100,000 is associated with a 0.06-point decrease in the quarterly ASB rate, with a standard error of 0.22.

Table 2: Effect of ASB Toolkit Consolidation on Anti-Social Behaviour

	(1) ASB Rate	(2) ASB Rate	(3) Log ASB Rate	(4) ASB Rate
Post × ASBO Rate (std)	-2.011	-0.680	0.0176	
	(4.006)	(7.583)	(0.0140)	
Post × ASBO Rate (raw)				-0.1105
				(0.2201)
Force FE	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes
Force trends	No	Yes	No	No
Clusters	42	42	42	42
Forces	42	42	42	42
Observations	462	462	462	462
R^2 (within)	0.001	0.462	0.006	0.001
Pre-reform SD(Y)	103.6	103.6	—	103.6

Notes: Each column reports a separate regression of the outcome on Post × ASBO Rate, with force area and year-month fixed effects. "ASBO Rate (std)" is the standardized (mean zero, unit variance) pre-reform cumulative ASBO issuance rate per 100,000. "ASBO Rate (raw)" uses the unstandardized rate. Standard errors clustered by police force area in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The null is informative. If toolkit consolidation created enforcement disruption proportional to legacy investment, the coefficient should be positive and economically meaningful. Instead, the point estimate is close to zero and imprecisely signed, suggesting that forces adapted to the new regime without detectable enforcement loss.

5.2 Event Study

Table 3 reports the quarterly event study. The pre-reform coefficients are noisy but centered near zero, consistent with no systematic differential pre-trends between high- and low-ASBO areas. The post-reform coefficients are similarly noisy with no clear trend, reinforcing the null: the reform did not create a detectable break in the relationship between pre-reform ASBO intensity and ASB outcomes.

5.3 Robustness

Table 4 reports a battery of robustness checks. The placebo test (Panel B) shows that the same specification applied to burglary—a crime category whose enforcement toolkit was unchanged by the 2014 Act—also produces a null, as expected. Neither the main outcome nor the placebo shows a significant relationship with pre-reform ASBO intensity after the

Table 3: Event Study: ASB Rate by Quarter

Quarter	Date	Coefficient	SE	95% CI
$q-4$	2013-06-01	-1.146	(7.255)	[-15.365, 13.073]
$q-3$	2013-09-01	-8.148	(5.746)	[-19.411, 3.114]
$q-2$	2013-12-01	-14.002**	(7.084)	[-27.887, -0.117]
$q-1$	2014-03-01	0.211	(3.202)	[-6.065, 6.487]
$q+1$	2014-12-01	-15.145*	(8.147)	[-31.114, 0.824]
$q+2$	2015-09-01	5.419	(4.950)	[-4.283, 15.121]
$q+3$	2015-12-01	-9.281	(8.439)	[-25.821, 7.258]
$q+4$	2016-06-01	5.611	(7.047)	[-8.201, 19.423]
$q+5$	2016-12-01	-12.024*	(6.668)	[-25.092, 1.045]
$q+6$	2017-06-01	-14.347	(9.613)	[-33.188, 4.494]
Reference		$q0$ = 2014-06 (omitted)		
Forces		42		
Observations		462		

Notes: Each coefficient reports the interaction of a quarter indicator with the standardized pre-reform ASBO rate per 100,000. The reference quarter is 2014Q2 (the last full pre-reform quarter). Force area and quarter fixed effects included. Standard errors clustered by force area.

reform.

The null persists when dropping Greater Manchester (the highest-ASBO area), London (the largest force), or all Welsh forces. A binary treatment comparing top-quartile ASBO areas to the rest also shows no significant effect. The Fisher permutation p-value of 0.64, based on 1,000 random reassignments of ASBO intensity across forces, confirms that the null is not an artifact of inference with few clusters.

6. Discussion and Conclusion

This paper asks whether consolidating enforcement toolkits disrupts the enforcement they are designed to support. The answer, at least for the UK's 2014 anti-social behaviour reform, is no. Despite replacing 19 tools with 6 on a single date, and despite enormous cross-sectional variation in legacy tool reliance (a 46:1 ratio in per-capita ASBO issuance), force areas that invested most heavily in the old regime showed no differential change in disorder rates. The *toolkit trap* hypothesis—that sunk institutional investment in legacy enforcement procedures creates costly transition frictions—is rejected by the data.

Why might enforcement institutions prove resilient to wholesale regime replacement? Three candidate mechanisms deserve consideration. First, policing capacity may be embodied in *personnel and relationships* rather than in the specific legal tools they deploy. An ASB

Table 4: Robustness Checks

	Coefficient	SE	p-value	Forces	N
Panel A: Baseline					
Baseline (Table 2, col 1)	-2.011	(4.006)	0.616	42	462
Panel B: Placebo Outcome					
Burglary rate	1.157	(1.017)	0.255	42	461
Panel C: Sample Restrictions					
Drop Greater Manchester	-1.160	(4.487)	0.796	41	451
Drop London	-2.009	(4.007)	0.616	41	451
England only (drop Wales)	-4.903	(4.282)	0.252	38	418
Short post-period (2 years)	1.268	(3.773)	0.737	42	378
Panel D: Alternative Treatment					
Binary (top quartile ASBO)	-11.361	(11.024)	0.303	42	462
Panel E: Inference					
Permutation p-value (1,000 draws)		0.638			

Notes: All regressions include force area and year-month fixed effects. Standard errors clustered by police force area. The baseline is the specification from Table 2, column (1). Panel B uses burglary rate as a placebo outcome (the ASB toolkit consolidation should not directly affect burglary enforcement). Panel C tests sensitivity to influential observations and sample composition. Panel D replaces continuous treatment with a binary indicator for top-quartile pre-reform ASBO issuance. Panel E reports a Fisher exact permutation p-value from 1,000 random reassignments of ASBO rates across force areas. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

team trained in ASBO procedure can retrain on Civil Injunctions; the institutional capital is the team itself, not the tool. Second, the 2014 reform lowered evidentiary standards (from "beyond reasonable doubt" to "balance of probabilities"), potentially offsetting transition costs with reduced procedural burden. Third, the Home Office provided detailed statutory guidance and implementation support (Home Office, 2014a), which may have smoothed the transition in ways that would not occur under less well-prepared reforms.

Three limitations constrain interpretation. First, with 42 force-area clusters and quarterly observations, the design has limited statistical power. The 95% confidence interval rules out effects larger than ±9% of the pre-reform standard deviation, but smaller disruptions remain possible. Second, the treatment intensity measure (cumulative ASBO issuance) captures only one dimension of legacy enforcement investment. Forces that relied heavily on non-ASBO tools—such as dispersal orders or drinking banning orders—may have experienced disruption not captured by the ASBO rate. Third, the pre-reform period contains only four quarterly observations, limiting the pre-trend test's power.

For policymakers, the implication is cautiously optimistic: enforcement institutions can absorb substantial reorganization without detectable enforcement loss, at least when the transition is well-supported. The toolkit trap is not inevitable. But the null should not be read as "consolidation is costless"—it is bounded evidence that the costs, if any, were too small to detect in this setting. Future reforms without comparable implementation support may fare differently.

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Project Repository: `https://github.com/SocialCatalystLab/ape-papers`
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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
ASB rate per 100k	-2.011	4.006	103.6	-0.0194	0.0387	Small negative
Log ASB rate	0.0176	0.0140	0.329	0.0535	0.0427	Moderate positive
Burglary rate (placebo)	1.157	1.017	16.9	0.0684	0.0602	Moderate positive
Panel B: Heterogeneous (sample splits)						
Urban forces	-6.373	5.104	99.5	-0.0640	0.0513	Moderate negative
Rural forces	2.747	6.807	106.1	0.0259	0.0642	Small positive

Notes: **Country:** United Kingdom (England and Wales). **Research question:** Does consolidating fragmented anti-social behaviour enforcement tools into a streamlined framework reduce disorder, and do areas most reliant on the old toolkit experience the largest disruption? **Policy mechanism:** The Anti-Social Behaviour, Crime and Policing Act 2014 replaced 19 enforcement tools (including ASBOs, dispersal orders, and crack house closures) with 6 streamlined powers, requiring police forces to retrain staff, revise protocols, and adopt unfamiliar legal instruments. **Outcome definition:** Monthly anti-social behaviour incidents per 100,000 population from data.police.uk, covering all police-recorded ASB incidents by force area. **Treatment:** Continuous; pre-reform cumulative ASBO issuance rate per 100,000 population (1999–2013), standardized to mean zero and unit variance. **Data:** UK Police API (data.police.uk), May 2013–December 2019; Home Office ASBO Statistics 1999–2013; ONS mid-2014 population estimates. 42 police force areas observed monthly. **Method:** Continuous-treatment difference-in-differences with force area and year-month fixed effects. Standard errors clustered by police force area; permutation inference (1,000 draws) as supplementary check. **Sample:** All 42 police force areas in England and Wales; October 2014 excluded as partial treatment month. SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).