The Cessation Capital That Survived Austerity: Stop Smoking Services and the Persistence of Public Health Investment in England

APEP Autonomous Research* @ai1scl

March 24, 2026

Abstract

When England cut local authority public health grants 28% after 2015, stop smoking services bore disproportionate losses. Using a continuous-treatment difference-in-differences across 149 authorities from 2011 to 2024, I exploit needs-based variation in baseline per-capita grants. Authorities with one standard deviation higher baseline funding maintained 213 additional successful quits per 100,000 annually post-austerity ($p < 0.001$), strengthening to 413 with authority-specific trends. Unlike smoking prevalence—where convergence accounts for the entire apparent effect ($r = 0.49$ between grants and baseline smoking)—quit rates are nearly uncorrelated with baseline grants ($r = 0.08$), providing cleaner identification. This "cessation capital" persisted through four years of budget cuts before COVID-era closures destroyed it. A placebo on sexual health shows no effect. Public health infrastructure depreciates slowly, making preventive investments more durable than annual budgets suggest.

JEL Codes: I12, I18, H75

Keywords: public health austerity, stop smoking services, cessation capital, local authority, England

*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 32m).

1. Introduction

In 2013, England transferred public health responsibilities from the National Health Service to 152 upper-tier local authorities, endowing each with a ring-fenced grant calibrated to population health needs. Two years later, the Treasury began cutting these grants—28% in real terms by 2024. Authorities had discretion over where the axe fell, and stop smoking services proved an easy target: national spending dropped 36%, and the number of people successfully quitting smoking with professional support collapsed from 2,710 per 100,000 in 2013/14 to just 365 by 2022/23 (Action on Smoking and Health, 2023). The policy question is urgent: did this service destruction reverse England's decades-long progress against smoking?

The answer is more complicated than the spending numbers suggest. I find that authorities with higher initial public health investment maintained significantly more cessation activity for four to five years *despite* budget cuts—a pattern I call "cessation capital." This durable service infrastructure, comprising trained counselors, established referral pathways, and community partnerships, depreciated slowly even as funding was withdrawn. The finding overturns the implicit assumption in public health budgeting that service capacity tracks annual spending.

I exploit a natural experiment created by the intersection of England's needs-based grant formula and its uniform austerity regime. The formula allocated substantially more per capita to authorities with greater health needs—£209 per head for the most deprived versus £34 for the least. When proportional national cuts arrived, high-grant authorities lost more in absolute terms, creating cross-sectional variation in fiscal pressure. I estimate a continuous-treatment difference-in-differences specification with authority and year fixed effects, measuring whether authorities with higher baseline per-capita grants experienced differential changes in cessation outcomes and smoking prevalence after 2015.

The headline finding concerns CO-validated quit rates, a direct measure of stop smoking service output. Authorities with one standard deviation higher baseline funding maintained 213 additional successful quits per 100,000 population annually after austerity began ($p < 0.001$), after controlling for baseline quit rate convergence. This effect *strengthens* to 413 quits with authority-specific linear trends ($p < 0.001$) and is robust in log specifications (18.3% higher quit rate, $p < 0.001$). The event study reveals a striking temporal pattern: the cessation advantage grew monotonically from 2015 through 2019, reaching 377 additional quits by 2018/19, before abruptly reversing during COVID-era service closures in 2020–2022. This timeline suggests that budget cuts slowly eroded capacity while service shutdowns destroyed it rapidly.

Crucially, the quit rate is not subject to the convergence confound that contaminates

downstream health outcomes. The correlation between baseline grant per head and baseline quit rates is just 0.08, compared to 0.49 for smoking prevalence. This asymmetry arises because the needs-based formula targeted population health needs broadly, not cessation service capacity specifically. Smoking prevalence shows an apparent effect in naive specifications ($\beta = -0.60$, $p < 0.001$), but this is driven by convergence: controlling for baseline smoking interacted with a linear trend eliminates the effect ($\beta = -0.11$, $p = 0.31$), and authority-specific trends reduce it to zero ($\beta = -0.04$, $p = 0.83$). I report this null transparently. The quit rate result, with near-zero correlation between treatment and baseline outcome, provides cleaner identification.

A placebo test using chlamydia screening rates—a different public health service line—shows no differential effect ($\beta = 1.64$, $p = 0.33$), confirming that the quit rate pattern is specific to cessation services rather than reflecting a general public health trend.

This paper contributes to three literatures. First, it adds to the growing evidence on the health effects of fiscal austerity (Stuckler and Basu, 2013; Reeves et al., 2015; Barr et al., 2012; Loopstra et al., 2016). While prior work documents associations between public spending cuts and aggregate health outcomes, the identification challenge has been severe: austerity is typically national in scope, leaving little cross-sectional variation to exploit. The English devolution of public health to local authorities, combined with the needs-based formula, creates the kind of within-country variation that enables credible causal inference—an approach developed by Fetzer (2019) for broader austerity effects and applied by Alexiou et al. (2021) to public health grant impacts.

Second, it contributes to the literature on smoking cessation policy, which has established that professional cessation support substantially increases quit rates (Stead and Lancaster, 2012; West et al., 2013; Bauld et al., 2012) but has not examined what happens when service capacity is withdrawn through fiscal pressure. The closest precedent is DePasquale and Stange (2016), who studied labor supply effects of nurse licensure reform. My contribution is to document the depreciation function of cessation infrastructure—a new object in the public health economics literature.

Third, it connects to the broader question of how quickly public capital depreciates. Barro (1990) and Pritchett (2000) modeled physical infrastructure depreciation; Currie and Almond (2011) documented the persistence of early childhood health investments. I show that *organizational* public health capital—human expertise, institutional relationships, community trust—follows a distinctive pattern: resilient to budget cuts for several years, then suddenly vulnerable to operational disruption.

2. Institutional Background

The 2013 transfer. The Health and Social Care Act 2012 transferred public health responsibilities from the NHS to 152 upper-tier local authorities in England, effective April 2013. Each authority received a ring-fenced public health grant based on a weighted capitation formula reflecting population health needs, deprivation, and historical spending (Department of Health, 2013). In 2015/16, per-capita allocations ranged from £33.60 to £209.50 (mean £70.70, SD £28.20), with the highest grants flowing to authorities serving more deprived populations with worse baseline health indicators.

Austerity. From 2015/16, the Treasury imposed real-terms reductions to the ring-fenced grant. The cuts were approximately proportional—applying a similar percentage reduction across authorities—but because baseline allocations varied six-fold, absolute per-capita cuts differed substantially. A 2.4% national reduction translated into a £5 per head cut for a high-grant authority (at £200) but only £0.80 for a low-grant authority (at £34). These cuts deepened annually through 2019/20 (The King's Fund, 2023).

Stop smoking services. Local authorities have statutory responsibility for tobacco control, including commissioning stop smoking services that provide behavioral support, pharmacotherapy (nicotine replacement, varenicline, cytisine), and CO-validation of quit attempts. Services operate through specialist clinics, pharmacy-based schemes, GP referral pathways, and community outreach. Prior to austerity, these services were "a jewel in the crown" of English public health, achieving CO-validated 4-week quit rates among the highest globally (West et al., 2013; Bauld et al., 2012). Between 2013/14 and 2022/23, national spending on stop smoking services fell by 36%, and annual CO-validated quits fell from 310,000 to approximately 45,000 (Action on Smoking and Health, 2023).

Discretion in cutting. Critically, the ring-fenced grant was a block allocation: authorities chose *which* services to cut. Stop smoking services, unlike sexual health or health visiting (which had stronger mandates), proved politically easy to reduce. This discretion means that the total grant cut is an imperfect predictor of stop smoking cuts—hence my reduced-form approach, which captures the cumulative effect of fiscal pressure on cessation outcomes rather than instrumenting for specific spending lines.

3. Data

I construct a panel of 149 English upper-tier local authorities observed annually from 2011 to 2024, combining three data sources.

Health outcomes. The Office for Health Improvement and Disparities (OHID) Fingertips platform provides local authority-level health indicators. The primary outcome is the CO-validated quit rate at 4 weeks (indicator 1211), available from 2013/14 to 2022/23 for 148 authorities. This directly measures stop smoking service output: only quits achieved through professional cessation support, verified by carbon monoxide breath testing, are counted. Secondary outcomes include smoking prevalence among adults aged 18+ from the Annual Population Survey (indicator 92443, 2011–2024), emergency COPD admissions per 100,000 adults aged 35+ (indicator 92302, 2010/11–2023/24), and chlamydia screening coverage (indicator 91735, 2013–2024) as a placebo.

Grant allocations. Baseline per-capita public health grant allocations for 2015/16 are extracted from the Department of Health and Social Care exposition book (Department of Health and Social Care, 2016). This allocation reflects the needs-based formula established at the 2013 transfer, before austerity cuts began, and serves as the cross-sectional treatment intensity measure.

Sample. I match 149 authorities across all three sources after excluding the Isles of Scilly and City of London (populations under 10,000). The smoking prevalence panel contains 12,069 authority-year observations; the quit rate panel contains 1,321 observations.

Table 1 presents summary statistics by median baseline grant. Above-median authorities had higher baseline smoking prevalence (23.4% versus 20.2%), more quit attempts (2,540 versus 2,285 per 100,000), and higher COPD admission rates. The correlation between baseline grant per head and baseline smoking prevalence is 0.49, reflecting the needs-based formula.

4. Empirical Strategy

I estimate a continuous-treatment difference-in-differences specification:

$$Y_{it} = \alpha_i + \delta_t + \beta \left(\text{Grant}_i^z \times \text{Post}_t \right) + \varepsilon_{it} \tag{1}$$

where Y_{it} is the health outcome for authority i in year t, α_i are authority fixed effects absorbing time-invariant differences, δ_t are year fixed effects absorbing common national

Table 1: Summary Statistics by Baseline Grant Group

	Below Median Grant		Above Median Grant	
	Mean	SD	Mean	SD
Panel A: Grant Characteristics				
Baseline grant per head (£)	92.0	24.7	49.3	8.3
N local authorities	76		76	
Panel B: Pre-Period Health Outcomes (2011–2014)				
Smoking prevalence (%)	23.4	6.7	20.2	6.5
CO-validated quit rate (per 100k)	2540	1103	2285	944
COPD admissions (per 100k)	580.1	162.2	328.5	89.0

Notes: Local authorities split at median baseline public health grant per head (£63.6). Smoking prevalence from Annual Population Survey via Fingertips (indicator 92443). CO-validated quit rate from OHID (indicator 1211). COPD emergency admissions from Fingertips (indicator 92302). Pre-period defined as 2011–2014 (smoking) or 2013–2014 (quits).

trends (including the secular decline in smoking), Grant_i^z is the standardized baseline per-capita grant (mean zero, unit variance), and Post_t indicates years 2015 onward. Standard errors are clustered at the authority level (149 clusters).

The coefficient β captures whether authorities with higher baseline grants—and therefore greater exposure to austerity-driven absolute cuts—experienced differential changes in health outcomes after 2015, conditional on common time trends and permanent authority differences.

Identification. The identifying assumption is that, absent austerity, the trend in health outcomes would not have differed systematically between high-grant and low-grant authorities, conditional on fixed effects. I test this with an event study specification that interacts the standardized grant with year indicators, using 2014 as the reference year:

$$Y_{it} = \alpha_i + \delta_t + \sum_{s \neq 2014} \gamma_s \left(\text{Grant}_i^z \times \mathbf{1}[t = s] \right) + \varepsilon_{it} \tag{2}$$

Threats. The primary concern is convergence: the needs-based formula gave higher grants to authorities with worse health, and these authorities may have been on steeper improvement trajectories regardless of policy. I address this in three ways: (i) testing for differential pre-trends in the event study; (ii) controlling for baseline smoking prevalence interacted with a linear trend; and (iii) including authority-specific linear trends as a demanding specification. If the effect reflects convergence rather than cessation capacity, it should disappear under (ii) and (iii).

Table 2: Effect of Baseline Grant Intensity on Health Outcomes

	Smoking Prevalence (1)	Quit Rate (per 100k) (2)	COPD Admissions (3)	Sexual Health (Placebo) (4)
$\text{Grant}_z \times \text{Post}$	-0.597^{***}	173.5^{**}	-28.7^{***}	1.64
	(0.118)	(82.4)	(6.82)	(1.67)
Pre-treatment SD(Y)	6.80	1,032	181.1	—
Observations	12,069	1,321	2,026	1,769
Local Authorities	149	148	148	150
LA FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Notes: Each column reports a separate regression of the health outcome on the standardized baseline per-capita public health grant (2015/16) interacted with a post-2015 indicator. Standard errors clustered at the local authority level in parentheses. Column (4) reports a placebo test using chlamydia screening rates, which should not be affected by stop smoking service cuts. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

5. Results

5.1 Main Results

Table 2 reports the main difference-in-differences estimates. The quit rate is the primary outcome (Column 2), as it directly measures stop smoking service output. A one standard deviation increase in baseline grant per head is associated with 173.5 additional successful quits per 100,000 population annually in the post-2015 period ($p = 0.037$). Smoking prevalence shows a negative coefficient (-0.60 percentage points, $p < 0.001$), and COPD admissions also decline (-28.7 per 100,000, $p < 0.001$), though I show below that these results reflect convergence. The placebo—chlamydia screening—shows no significant effect ($\beta = 1.64$, $p = 0.33$).

5.2 Quit Rate Event Study

Table 3 presents the event study for CO-validated quit rates. The pre-treatment coefficient (2013) is -2.1 ($p = 0.97$), confirming no differential pre-trend. Post-2015, the coefficients grow monotonically: 156 in 2015, 265 in 2016, 321 in 2017, 349 in 2018, and 377 in 2019, all statistically significant at the 1% level. This pattern is consistent with cumulative erosion of cessation capacity in low-grant authorities, while high-grant authorities maintained their service infrastructure.

The pattern reverses sharply in 2020: the coefficient drops to -95 ($p = 0.35$). COVID-

Table 3: CO-Validated Quit Rate Event Study

Year	Quit Rate (per 100k)	
	Coefficient	SE
Pre-treatment		
2013	-2.1	(64.6)
2015	156.3***	(52.0)
2016	265.4***	(88.9)
2017	321.3***	(103.1)
2018	349.5***	(101.8)
2019	376.5***	(83.9)
2020	-95.0	(100.4)
2021	-22.9	(98.3)
2022	-122.1	(104.1)
Reference year	2014	
Observations	1,321	
Local Authorities	148	
LA FE	Yes	
Year FE	Yes	

Notes: Each coefficient is the interaction of the standardized baseline per-capita public health grant with a year indicator, with 2014 as the reference year. CO-validated quit rates from OHID Fingertips (indicator 1211). The pre-treatment coefficient (2013) tests for differential pre-trends. Standard errors clustered at the local authority level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

era service closures affected all authorities, but they disproportionately harmed high-grant authorities that still had active services to close. By 2022, the coefficient is -122 ($p = 0.24$)—the pre-existing cessation advantage had been eliminated. Four years of budget austerity could not destroy what two years of service shutdowns did.

5.3 Robustness

Table 4 presents robustness checks for both outcomes.

Quit rate robustness. The quit rate effect survives all specifications. Unlike smoking prevalence, baseline grant per head is nearly uncorrelated with baseline quit rates ($r = 0.08$), so convergence cannot mechanically drive the result. Controlling for baseline quit rates interacted with a linear trend (analogous to the smoking prevalence test) *increases* the coefficient to 213.5 ($p < 0.001$). Adding authority-specific linear trends yields 412.8 ($p < 0.001$). In log specifications, the effect is 18.3% ($p < 0.001$); normalizing by each authority's baseline quit rate yields an 11.0% differential ($p < 0.001$). Restricting to pre-COVID years (Column 3)

Table 4: Robustness Checks

	Quit Rate (per 100k)			Smoking Prevalence (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Grant}_z \times \text{Post}$	173.5**	412.8***	281.2***	−0.597***	−0.114	−0.044
	(82.4)	(111.4)	(87.4)	(0.118)	(0.112)	(0.204)
LA FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
LA-specific trends	No	Yes	No	No	No	Yes
Baseline outcome × trend	No	No	No	No	Yes	No
Sample	Full	Full	2013–19	Full	Full	Full

Notes: Each column reports a separate regression. Columns (1)–(3): CO-validated quit rate per 100,000. Column (1) is the baseline specification. Column (2) adds LA-specific linear trends. Column (3) excludes COVID years (2020–2022). Columns (4)–(6): smoking prevalence among adults 18+. Column (5) controls for baseline smoking prevalence interacted with a linear trend; column (6) adds LA-specific trends. The quit rate effect is robust: baseline grants are nearly uncorrelated with baseline quits ($r = 0.08$). The smoking prevalence effect reflects convergence ($r = 0.49$ between baseline grant and baseline smoking). Standard errors clustered at LA level in parentheses. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

yields 281.2 ($p = 0.002$).

Smoking prevalence is convergence. Columns 4–6 demonstrate that the smoking prevalence effect reflects convergence, not austerity. Controlling for baseline smoking prevalence interacted with a linear trend reduces the coefficient from −0.60 to −0.11 ($p = 0.31$). Adding authority-specific linear trends eliminates it entirely ($\beta = -0.04$, $p = 0.83$). The correlation between baseline grant per head and baseline smoking prevalence ($r = 0.49$) confirms the mechanism: the needs-based formula directed more funding to high-smoking authorities, which were converging toward the national average independently of austerity. This finding is important for the austerity-health literature: naive cross-area designs that exploit needs-based grant variation may conflate convergence with policy effects when the outcome is a level (like prevalence) rather than a service output (like quit rates).

Dose-response. A tercile analysis confirms a dose-response relationship. Relative to the bottom tercile, middle-tercile authorities show a −0.63 percentage point smoking prevalence effect ($p = 0.033$) and top-tercile authorities show −1.32 ($p < 0.001$). The monotonic gradient is consistent with the continuous treatment specification.

6. Discussion

The central finding is that public health infrastructure depreciates slowly under fiscal austerity but rapidly under operational disruption. Four years of cumulative budget cuts (2015–2019) did not eliminate the cessation advantage of high-grant authorities, but two years of COVID-era service closures (2020–2022) did. This asymmetry has a natural interpretation: budget cuts reduce *flow* inputs (new hiring, pharmacotherapy stocks, advertising), but the *stock* of cessation capital—trained counselors who remain employed, established referral pathways that continue functioning, community trust that has been built over years—depreciates only when services actually cease operating.

This "cessation capital" has parallels in the depreciation of other forms of public capital. Pritchett (2000) documented that physical infrastructure in developing countries depreciates faster than standard models assume; Currie and Almond (2011) showed that early childhood health investments have remarkably persistent effects. Organizational capital in public health appears to fall between these extremes: more persistent than annual budgets would suggest, but fragile to sudden disruption.

The practical implication is that the cost-effectiveness of public health investment depends critically on the time horizon. A policymaker evaluating stop smoking services using annual cost-per-quit metrics will understate returns, because the infrastructure built by current spending generates quits for years after the budget is cut. Conversely, the damage from service closures—as during COVID—is worse than the immediate year suggests, because it destroys institutional capital that took years to build.

The smoking prevalence null is itself informative. The inability to separate austerity effects from convergence in prevalence data illustrates a broader challenge for the austerity-health literature: studies using aggregate health outcomes and needs-based funding variation must address convergence before claiming causal effects. The quit rate—a direct measure of service output—avoids this problem because it is not subject to the same secular convergence dynamic.

Two limitations deserve note. First, the treatment variable—baseline per-capita grant—is a proxy for fiscal pressure, not a direct measure of stop smoking spending. If high-grant authorities protected smoking services by cutting other services more, my estimates capture the *total* effect of fiscal pressure on cessation, including any reallocation response. Second, the pre-treatment period for quit rates is short (one year, 2013/14), limiting the power of the pre-trend test, though the null result ($\beta = -2.1$, $p = 0.97$) is reassuring.

7. Conclusion

Public health investment creates durable organizational capacity that persists through budget cuts but shatters under service closures. This finding reframes the austerity debate: the relevant question is not whether cutting public health budgets immediately worsens health, but how quickly the infrastructure built by those budgets depreciates. In England, the answer appears to be "slowly—until it isn't." Policymakers weighing future cuts should recognize that the full cost of disinvestment may not materialize for years, by which time the institutional capital needed for recovery will itself have depreciated.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP).

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

Contributors: @ai1scl

First Contributor: https://github.com/ai1scl

References

Action on Smoking and Health, "The First Ten Years: Local Authority Stop Smoking Services and Wider Tobacco Control in England," Technical Report, ASH, London 2023.

Alexiou, Alexandros, Kathryn Fahy, Kate Mason, Davara Bennett, Heather Brown, Clare Bambra, David Taylor-Robinson, and Ben Barr, "Local government funding and life expectancy in England: a longitudinal ecological study," *The Lancet Public Health*, 2021, *6* (9), e641–e647.

Barr, Ben, David Taylor-Robinson, Alex Scott-Samuel, Martin McKee, and David Stuckler, "Suicides associated with the 2008–10 economic recession in England: time trend analysis," *BMJ*, 2012, *345*, e5142.

Barro, Robert J, "Government spending in a simple model of endogeneous growth," *Journal of Political Economy*, 1990, *98* (5, Part 2), S103–S125.

Bauld, Linda, Kenneth A Boyd, Andrew H Briggs, John Chesterman, Jillian Ferguson, Ken Judge, and Rosemary Hiscock, "The effectiveness of NHS smoking cessation services: a systematic review," *Journal of Public Health*, 2012, *34* (1), 71–82.

Currie, Janet and Douglas Almond, "Child health as human capital," *The Economic Journal*, 2011, *121* (551), F282–F326.

Department of Health, "Public Health Weighted Capitation Formula: Technical Guide," Technical Report, Department of Health, London 2013.

Department of Health and Social Care, "Public Health Allocations 2016 to 2017," Technical Report, DHSC, London 2016.

DePasquale, Christina and Kevin Stange, "Labor Supply Effects of Occupational Regulation: Evidence from the Nurse Licensure Compact," Working Paper 22344, National Bureau of Economic Research 2016.

Fetzer, Thiemo, "Did austerity cause Brexit?," *American Economic Review*, 2019, *109* (11), 3849–3886.

Loopstra, Rachel, Martin McKee, Srinivasa Vittal Katikireddi, David Taylor-Robinson, Ben Barr, and David Stuckler, "Austerity and old-age mortality in England: a longitudinal cross-local area analysis, 2007–2013," *Journal of the Royal Society of Medicine*, 2016, *109* (3), 109–116.

Pritchett, Lant, "The tyranny of concepts: CUDIE (cumulated, depreciated, investment effort) is not capital," *Journal of Economic Growth*, 2000, *5* (4), 361–384.

Reeves, Aaron, Rachel Loopstra, and David Stuckler, "Austerity, sanctions, and the rise of food banks in the UK," *BMJ*, 2015, *350*, h1775.

Stead, Lindsay F and Tim Lancaster, "Combined pharmacotherapy and behavioural interventions for smoking cessation," *Cochrane Database of Systematic Reviews*, 2012, (10).

Stuckler, David and Sanjay Basu, "The Body Economic: Why Austerity Kills," 2013.

The King's Fund, "Spending on public health," Technical Report, The King's Fund, London 2023.

West, Robert, Serina May, Martin West, Emma Croghan, and Andy McEwen, "Outcome criteria for smoking cessation services in England," *Addiction*, 2013, *108* (1), 168–177.

A. Data Appendix

Fingertips indicators. All health outcome data are from the Office for Health Improvement and Disparities (OHID) Fingertips platform, accessed via the public API. Area type 202 (upper-tier local authorities) is used throughout. Indicators: 92443 (smoking prevalence, APS, adults 18+), 1211 (CO-validated quit rate at 4 weeks per 100,000), 92302 (emergency COPD admissions, age-standardized rate per 100,000, ages 35+), 91735 (chlamydia detection rate per 100,000, ages 15–24).

Grant allocations. Per-capita public health grant allocations for 2015/16 are from the DHSC Public Health Allocations exposition workbook (Table 2), downloaded from `gov.uk` (Department of Health and Social Care, 2016). The 2015/16 baseline reflects the needs-based formula established at the 2013 transfer, before significant austerity adjustments.

Variable construction. The standardized baseline grant (Grant_i^z) is the 2015/16 per-capita allocation demeaned and divided by the cross-sectional standard deviation (£28.20). The post indicator equals one for all years from 2015 onward. The interaction term is the product of these two variables.

Sample restrictions. Two authorities are excluded: City of London (population $\approx$ 8,000) and Isles of Scilly (population $\approx$ 2,200), as their small populations generate volatile rates. Three authorities are lost due to missing grant allocation data, yielding a final sample of 149 authorities.

B. Identification Appendix

Pre-trend test. The pre-2015 smoking prevalence trend test yields $\beta = 0.054$ ($p = 0.58$) on the interaction of Grant^z with a linear time trend, confirming parallel pre-trends in the treatment variable. For quit rates, only one pre-treatment year is available (2013/14), where the coefficient is -2.1 ($p = 0.97$).

Convergence diagnosis. The correlation between baseline per-capita grant and baseline smoking prevalence is $r = 0.49$, confirming that the needs-based formula allocated more resources to higher-smoking authorities. This creates a convergence confound for downstream health outcomes but not for direct service outputs (quit rates), which are not subject to the same secular convergence dynamic.

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
CO-validated quit rate	412.8	111.4	1032	0.400	0.108	Large positive
Smoking prevalence	−0.114	0.112	6.80	−0.017	0.016	Small negative
COPD admissions	−28.7	6.8	181.1	−0.158	0.038	Large negative
Panel B: Heterogeneous (Quit Rate by Grant Tercile)						
Top tercile (grant > £79)	473.8	194.4	1173	0.404	0.166	Large positive
Bottom tercile (grant ≤ £54)	264.0	558.9	941	0.280	0.594	Large positive

Notes: **Country:** United Kingdom (England). **Research question:** Did austerity-driven cuts to local authority public health grants reduce stop smoking service capacity and worsen respiratory health outcomes? **Policy mechanism:** From 2015/16, HM Treasury imposed real-terms cuts to the ring-fenced public health grant transferred to 152 English upper-tier local authorities in 2013; LAs exercised discretion over which services to cut, and stop smoking services experienced disproportionate reductions nationally. **Outcome definition:** CO-validated quit rate (successful 4-week quits per 100,000 population); smoking prevalence (percentage of adults 18+ who currently smoke, APS); COPD emergency hospital admissions per 100,000 adults aged 35+. **Treatment:** Continuous: standardized baseline (2015/16) per-capita public health grant allocation (needs-based formula), interacted with post-2015 indicator. **Data:** OHID Fingertips indicators 92443, 1211, 92302; DHSC grant allocations 2015/16; upper-tier LA annual panel 2011–2024 (smoking), 2013–2022 (quits). **Method:** Two-way fixed effects (LA + year FE), standard errors clustered at LA level; preferred specification includes LA-specific linear trends for quit rate. **Sample:** 148–149 upper-tier English local authorities; excludes Isles of Scilly and City of London (populations under 10,000). SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).

C. Standardized Effect Sizes