

# Price Floors and Poison: The Effect of Minimum Unit Pricing on Alcohol-Specific Mortality

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## Abstract

Does minimum unit pricing (MUP) for alcohol reduce mortality? We exploit the staggered adoption of MUP in Scotland (May 2018) and Wales (March 2020)—with England as the never-treated control—using difference-in-differences on annual country- and region-level mortality panels, 2013–2023. Scotland’s age-standardised alcohol-specific death rate held flat at 22.3 per 100,000 after MUP, while England’s surged 24 percent to 30.0. Our preferred regional TWFE estimate is  $-2.1$  deaths per 100,000 ( $p < 0.01$ ); the Callaway–Sant’Anna staggered estimator gives  $-1.1$  ( $p < 0.10$ ). Pre-trends pass an F-test and the placebo outcome is insignificant. Translating Scotland’s estimate implies roughly 140 deaths averted per year. England’s deprivation gradient widened in the absence of MUP, underscoring distributional stakes.

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# 1. Introduction

Alcohol causes more than three million deaths globally each year, with harm concentrated among heavy drinkers of the cheapest products ([World Health Organization, 2018](#)). Price is one of the most powerful levers available to policymakers: a large meta-analysis of 112 studies finds that a 10 percent increase in the price of alcoholic beverages reduces consumption by 4–6 percent on average, with larger elasticities among heavy and hazardous drinkers ([Wagenaar et al., 2009](#)). Yet translating this elasticity into lives saved requires a policy that specifically raises the floor price of the cheapest alcohol—the products consumed disproportionately by dependent drinkers—rather than general taxes that raise prices across the board. Minimum unit pricing (MUP) is precisely such a policy: it sets a per-unit-of-alcohol price floor that binds almost exclusively on the cheapest products.

The debate over whether MUP saves lives has been contentious. Before adoption, the Sheffield Alcohol Policy Model projected that a 50p MUP in Scotland would reduce alcohol-specific deaths by 121 per year and generate £942 million in social benefits over 20 years ([Holmes et al., 2014](#)). Critics argued that the price-mortality link might be attenuated by substitution toward other substances, that dependent drinkers would cut food and other essentials rather than alcohol consumption, and that the policy amounted to a regressive tax on a population already facing multiple deprivation ([Anderson et al., 2012](#)). The alcohol industry challenged the policy legally for five years, and it was not until 2018 that Scotland became the first country in the world to implement MUP at 50 pence per unit. Wales followed in 2020 at the same floor price. England, despite substantial political debate, has not implemented MUP, creating a natural comparative experiment.

This paper provides the first difference-in-differences estimates of MUP’s causal effect on alcohol-specific mortality using the full post-implementation record through 2023—six years for Scotland and four for Wales. We use annual, age-standardised alcohol-specific mortality rates from official statistical agencies for all three nations and exploit both country-level variation and a finer regional panel of eleven units (nine English regions plus Scotland and Wales) to maximise statistical power. Our main findings are as follows. England’s alcohol-specific mortality rate rose from a pre-period mean of 10.5 deaths per 100,000 to 13.0 by 2023—a 24 percent increase. Scotland’s rate, by contrast, held virtually flat, moving from 22.4 to 22.3. Wales saw a more modest increase of 1.2 deaths per 100,000, consistent with its later MUP date and shorter post-period. A two-way fixed effects regression on the regional panel yields an average treatment effect of  $-2.1$  deaths per 100,000 (standard error 0.6,  $p < 0.01$ ). The Callaway and Sant’Anna ([2021a](#)) staggered estimator, which is robust to heterogeneous treatment effects across the two adoption cohorts, gives  $-1.1$  (SE 0.6,  $p < 0.10$ ).

Our event-study estimates show that the Scotland–England differential was small and stable in the four years before MUP and turned sharply negative in 2019—the first full post-implementation year—and has widened since. An F-test for differential pre-trends (2013–2017) yields  $p = 0.619$ , supporting the parallel-trends assumption. A placebo test using crude all-cause mortality yields a coefficient of  $-0.7$  that is not statistically significant, showing that the result is specific to alcohol-attributable causes rather than a common mortality shock. Excluding the two COVID-dominated years 2020–2021 changes the point estimate only modestly to  $-1.4$ .

Translating the Scotland-only estimate of  $-2.6$  deaths per 100,000 into a population figure: Scotland’s population is approximately 5.4 million, implying roughly 140 fewer alcohol-specific deaths per year (95% CI: approximately 50–230, based on event-study standard errors). [Giles et al. \(2024\)](#) report that 156 deaths were averted in Scotland in the first year alone, a figure closely aligned with our estimate. Using a value of a statistical life of £1.8 million ([Department for Transport, 2023](#)), the annual social value of avoided mortality exceeds £250 million—substantially larger than the costs of implementation. We treat the Scotland-only estimate as our primary result because Scotland’s six-year post-period is uncontaminated by COVID timing, while Wales’s March 2020 implementation coincides almost exactly with the first UK lockdown.

The paper makes three contributions to the literature. First, it provides the most up-to-date causal evaluation of MUP in Scotland, extending the record by three years beyond [O’Donnell et al. \(2019\)](#) and [Giles et al. \(2024\)](#) and incorporating the Welsh quasi-experiment as a second treated unit. Second, it demonstrates that the mortality benefit of MUP appears durable, not a one-time level shift: event-study coefficients remain negative and grow larger through 2023. Third, it documents the distributional stakes using England as a counterfactual: in the absence of a price floor, the mortality gradient between the most and least deprived deciles of the English population widened from 2.0 to 2.1 between 2013 and 2023, while the absolute rate in the most deprived areas rose by 27 percent.

The remainder of the paper proceeds as follows. [Section 2](#) describes the MUP policy and its legislative history. [Section 3](#) introduces the data. [Section 4](#) lays out the empirical strategy. [Section 5](#) presents main results, event-study, and heterogeneity analysis. [Section 6](#) discusses robustness. [Section 7](#) interprets the findings and [section 8](#) concludes.

## 2. Institutional Background

**The minimum unit pricing policy.** Minimum unit pricing establishes a statutory floor price per unit of alcohol (one unit = 10 ml or 8 g of pure ethanol). At the Scottish and

Welsh rate of 50 pence per unit, a typical bottle of wine (75 cl, approximately 9 units) must retail for no less than £4.50, and a 70 cl bottle of spirits (approximately 28 units) for no less than £14.00. The floor binds primarily on the cheapest supermarket and off-licence products—three-litre cider containers, low-cost white cider, and economy spirits—which are purchased disproportionately by heavy drinkers and those on low incomes (Stockwell et al., 2012; Griffith et al., 2022). Standard on-trade (pub and restaurant) prices already exceed the floor by a large margin, so the policy has essentially no first-order effect on on-trade consumption.

The theoretical mechanism connecting MUP to mortality operates through three channels. First, a direct consumption-reduction channel: higher prices reduce the quantity of alcohol purchased, particularly by price-sensitive heavy drinkers (Wagenaar et al., 2009; Meng et al., 2014). Second, a substitution-within-product channel: heavy drinkers may switch from high-strength, cheap products to lower-strength alternatives whose effective per-unit price rises less (Pryce et al., 2019). Third, a dependence-moderation channel: for alcohol-dependent individuals, consumption reductions reduce acute and chronic physiological harm including liver disease and pancreatitis, which are the primary causes of alcohol-specific death (Shield et al., 2013).

**Scotland: the world’s first MUP..** Minimum unit pricing in Scotland was first proposed by the Scottish National Party government in 2010. The Alcohol (Minimum Pricing) (Scotland) Act received Royal Assent in June 2012 but was immediately challenged by the Scotch Whisky Association (SWA), which argued the policy violated EU free-trade rules. Litigation proceeded through Scottish courts and the Court of Justice of the European Union, which ruled in December 2015 that MUP could be compatible with EU law if no less trade-restrictive measure would achieve the same public health objective. The Scottish Court of Session ultimately upheld the Act in November 2017, and MUP came into force on 1 May 2018. After a scheduled review, the Scottish Government raised the MUP to 65 pence per unit in September 2024, beyond our study window.

**Wales: a second natural experiment..** The Welsh Government enacted The Minimum Price for Alcohol (Wales) Order 2019, bringing MUP into force on 2 March 2020—a date that coincidentally occurred two weeks before the first UK COVID-19 lockdown. This timing complicates identification for Wales: the immediate post-implementation period was dominated by pandemic-era behavioral changes and pub closures that affected all nations. We therefore treat Wales as an additional treated unit but report its simple DiD and event-study coefficients with appropriate caveats about COVID confounding, and conduct sensitivity analysis excluding 2020–2021.

**England: the control..** England conducted a public consultation on MUP in 2013 but the Cameron government shelved the policy after industry lobbying, citing evidence that was “not yet strong enough” (HM Government, 2013). Subsequent commitments to revisit the decision have not led to legislation. England thus serves as the never-treated comparison throughout our study, complemented by its nine constituent statistical regions which we use as individual donor units in the regional panel and synthetic control analyses.

### 3. Data

Our primary outcome is the age-standardised alcohol-specific mortality rate per 100,000 population, using the European Standard Population 2013 for standardisation. We compile annual series from three official sources. For England, we use Office for Health Inequalities and Disparities (OHID) Fingertips indicator 91380, available at national and regional level from 2001. For Scotland, we use the National Records of Scotland (NRS) annual Alcohol-Specific Deaths bulletin, available from 2000. For Wales, we use the Office for National Statistics (ONS) Alcohol-Specific Deaths in the UK release, available from 2001. All series are based on ICD-10 codes for causes considered wholly attributable to alcohol (the “alcohol-specific” definition: F10, G31.2, G62.1, G72.1, I42.6, K29.2, K70, K85.2, K86.0, R78.0, X45, X65, Y15, and related codes), following the definitions used by the Office for National Statistics (Office for National Statistics, 2023). K70 (alcoholic liver disease) is by far the largest component, accounting for approximately 80 percent of all alcohol-specific deaths in the UK, followed by F10 (mental and behavioral disorders due to alcohol). Our primary outcome therefore predominantly reflects chronic liver damage among heavy drinkers—the mechanism most directly targeted by MUP’s floor-price channel.

We construct an eleven-unit annual panel covering 2013–2023 (eleven years, 121 observations), comprising Scotland, Wales, and nine English Government Office Regions (North East, North West, Yorkshire and The Humber, East Midlands, West Midlands, East of England, London, South East, South West). Regional-level mortality rates are available from OHID Fingertips at the same specification as the national England series. The country-level panel reduces to three units and thirty-three observations. For the deprivation analysis, we use OHID Fingertips data on alcohol-specific mortality by Index of Multiple Deprivation (IMD 2019) decile for England, available for County and Unitary Authority areas.

For robustness, we also compile the crude (not age-standardised) all-cause mortality rate per 100,000 for England and Scotland from ONS and NRS respectively, to serve as a placebo outcome for the primary DiD specification.

Table 1 presents summary statistics. Pre-MUP means (2013–2017) are 10.5 for England,

22.4 for Scotland, and 16.5 for Wales. Post-MUP means (2018–2023) are 13.0, 22.3, and 17.8 respectively. The divergence in trends is apparent even in these simple averages: Scotland’s mean was essentially unchanged while England’s rose by 2.5 deaths per 100,000.

**Table 1:** Summary Statistics: Alcohol-Specific Mortality Rates

Country	Period	Years	Mean Rate	SD	Min	Max
England	Post-MUP (2018–23)	6	13.0	1.8	10.7	15.0
England	Pre-MUP (2013–17)	5	10.5	0.3	10.2	11.1
Scotland	Post-MUP (2018–23)	6	22.3	1.7	19.2	23.7
Scotland	Pre-MUP (2013–17)	5	22.4	0.8	21.4	23.3
Wales	Post-MUP (2018–23)	6	17.8	2.1	15.4	20.5
Wales	Pre-MUP (2013–17)	5	16.5	0.4	16.2	17.2

*Notes:* Age-standardised alcohol-specific mortality rates per 100,000 (ESP 2013). England: OHID Fingertips indicator 91380. Scotland: NRS Alcohol-Specific Deaths. Wales: ONS Alcohol-Specific Deaths. Pre-MUP: 2013–2017 for Scotland/England; 2013–2019 for Wales (Scotland MUP: May 2018; Wales MUP: March 2020; England: no MUP).

## 4. Empirical Strategy

**DiD framework..** Our identification strategy compares changes in alcohol-specific mortality rates in treated nations (Scotland from 2018, Wales from 2020) to concurrent changes in England, which never adopted MUP. The key identifying assumption is that, absent MUP, the treated nations would have followed parallel mortality trends to England. We motivate this assumption through the event-study estimates in section 5 and test it formally using an F-test for differential pre-period linear trends.

Our baseline specification is a two-way fixed effects (TWFE) regression:

$$Y_{it} = \alpha_i + \lambda_t + \delta \cdot \text{MUP}_{it} + \varepsilon_{it} \quad (1)$$

where  $Y_{it}$  is the age-standardised alcohol-specific mortality rate for unit  $i$  in year  $t$ ;  $\alpha_i$  are unit fixed effects;  $\lambda_t$  are year fixed effects; and  $\text{MUP}_{it} = \mathbf{1}[\text{unit } i \text{ has MUP in force in year } t]$ . The coefficient  $\delta$  is the average treatment effect on the treated (ATT) in the post-MUP period.

**Addressing heterogeneous treatment effects..** A well-known limitation of TWFE with staggered adoption is that under heterogeneous treatment effects the coefficient may be a weighted average that assigns negative weight to some units (Goodman-Bacon, 2021; Callaway and Sant’Anna, 2021a; Sun and Abraham, 2021). With only two treated units and two

adoption dates (2018 and 2020), the bias from “forbidden comparisons” is mechanically limited, but we nonetheless report the Callaway and Sant’Anna (2021a) estimator as our preferred robustness specification. This estimator constructs group-time average treatment effects using only clean comparisons—treated units against never-treated controls in the pre-period of each cohort—and then aggregates them to an overall ATT. We implement this on the eleven-unit regional panel with bootstrap standard errors.

**Event study..** We augment equation (1) with leads and lags of the treatment indicator, using 2017 as the reference year for Scotland (one period before implementation) and 2019 as the reference year for Wales (one period before implementation). This produces year-specific treatment-effect estimates that allow us to inspect the temporal pattern of divergence and test for pre-treatment parallel trends.

**Synthetic control..** As a complementary approach, we construct a synthetic Scotland by combining English regions as donor units, weighted to minimise the pre-period (2013–2017) mean squared error in the alcohol-specific mortality rate (Abadie et al., 2010). We compute a permutation p-value by applying the same procedure to each English donor region and comparing Scotland’s post-treatment gap to the placebo distribution. We report the synthetic control results but note they face a structural-level problem: Scotland’s pre-treatment mortality rate (around 22 per 100,000) is substantially higher than any English region, limiting the quality of the pre-fit and the reliability of the permutation inference.

**Inference at the country level..** The country-level panel has only three units. Standard cluster-robust standard errors are unreliable with so few clusters. We therefore use a permutation-based p-value for the country-level TWFE: we randomly reassign the treatment indicator across all permutations of the three units and compute the fraction of permutation draws that yield a coefficient more negative than the observed  $\hat{\delta}$ . The regional panel (eleven units) supports cluster-robust standard errors clustered at the unit level, which we use for columns (3) and (4) of the main results table.

**Threats to validity..** Three concerns merit discussion. First, the COVID-19 pandemic from March 2020 caused substantial disruption to drinking behaviour, mortality coding, and health service access across all three nations simultaneously. To the extent that the pandemic affected all nations symmetrically, TWFE absorbs the shock through year fixed effects. We test sensitivity by excluding 2020–2021. Second, Scotland may have experienced other alcohol-relevant policy changes over the period—for example, changes in minimum legal drinking age enforcement or licensing hours—that confound the MUP effect. We are

not aware of any major co-occurring alcohol policy reform in Scotland but cannot rule out minor changes. Third, the pre-trend test, while passing the parallel-trends F-test, does show some noise in the pre-period (particularly a spike in 2014–2015) that warrants caution. We note these limitations explicitly in the discussion.

## 5. Results

### 5.1 Main Difference-in-Differences Estimates

Table 2 reports our main estimates. Column (1) shows the country-level TWFE estimate of  $-1.7$  deaths per 100,000 (SE 0.7). The permutation-based p-value is 0.164, reflecting the imprecision inherent in a three-unit panel. Column (2) provides simple pre-post comparisons: Scotland’s alcohol-specific mortality fell by 2.6 deaths per 100,000 relative to England’s change, and Wales’s fell by 1.2.

The regional panel provides considerably more power. Column (3), the TWFE on eleven units, yields  $-2.1$  (SE 0.6,  $p < 0.01$ ). This is our preferred specification because it uses nine English regions as distinct control units, improving the precision of the counterfactual and enabling cluster-robust inference. Column (4), the Callaway–Sant’Anna estimator, gives an overall ATT of  $-1.1$  (SE 0.6,  $p < 0.10$ ). The slight attenuation relative to column (3) is consistent with heterogeneous treatment timing: Wales’s effect, which is confounded by COVID, pulls the aggregate ATT toward zero. When we restrict to Scotland-only comparisons, the estimate rises to  $-2.9$ .

### 5.2 Event Study

Table 3 presents event-study coefficients. For Scotland versus England, using 2017 as the omitted reference year, the pre-period coefficients (2013–2016) are positive and noisy. The elevated 2014–2015 differentials ( $+1.9$ ) reflect a temporary spike in Scottish alcohol deaths documented by NRS that had fully reversed by 2017; crucially, this spike predates any MUP-related policy activity and shows no monotonic trend toward the post-period divergence. The 2018 coefficient ( $+0.1$ ) is near zero: MUP came into force in May 2018, so this year captures at most eight months of treatment and we would expect little effect. By 2019—the first full calendar year of MUP—the coefficient is  $-2.6$  (SE 0.3,  $p < 0.01$ ). It remains negative and significant through 2023, with the most recent estimate being  $-2.7$  (SE 0.9). The pattern suggests a persistent, not merely transitory, mortality reduction.

For Wales versus England, using 2019 as the reference year, the post-2020 pattern is more mixed: 2020 and 2021 show negative coefficients ( $-1.0$ ,  $p < 0.05$  and  $-1.0$ ,  $p < 0.01$ ), but 2022

**Table 2:** Main Difference-in-Differences Estimates

	Country-Level		Regional-Level	
	(1) TWFE	(2) Simple DiD	(3) TWFE	(4) CS-DiD
ATT (MUP treatment)	-1.7 (0.7)	–	-2.1*** (0.6)	-1.1* (0.6)
Scotland simple DiD	–	-2.6	–	–
Wales simple DiD	–	-1.2	–	–
Country FE	Yes	–	Yes	Yes
Year FE	Yes	–	Yes	Yes
Cluster	Country	–	Unit	Unit
Inference	Permutation	–	Cluster SE	Bootstrap
Permutation p-value	0.164	–	–	–
Observations	33	33	121	121

*Notes:* Dependent variable is age-standardised alcohol-specific mortality rate per 100,000. Col. (1): two-way fixed effects (country + year) with permutation-based p-value (10,000 draws). Col. (2): simple pre-post DiD using 2013–2017 pre-period. Col. (3): TWFE on 11-unit regional panel (9 English regions + Scotland + Wales), cluster-robust SEs. Col. (4): Callaway-Sant’Anna (2021) staggered DiD with never-treated English regions as controls, bootstrap SEs. Stars: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

shows an unexpected positive coefficient of +1.3, possibly reflecting a COVID rebound or compositional changes in the Welsh mortality denominator. The Wales evidence is therefore less decisive, as anticipated given COVID contamination of the initial post-MUP years.

### 5.3 Deprivation Heterogeneity

Table 4 presents the deprivation gradient for England—where no MUP was implemented—over 2013–2023. The data are striking. The most deprived decile (D1) saw alcohol-specific mortality rise from 15.5 to 19.8 per 100,000, a 27 percent increase. The least deprived decile (D10) rose from 7.8 to 9.5, a 22 percent increase. The rate ratio (D1/D10) widened marginally from 2.0 to 2.1, but the absolute gap between D1 and D10 grew from 7.7 to 10.3 deaths per 100,000.

This gradient is directly relevant to the MUP mechanism. The Sheffield Alcohol Policy Model predicts that MUP’s benefits are concentrated in the heaviest and most deprived drinkers because they are most price-sensitive and consume the most cheap, high-strength alcohol (Holmes et al., 2014; Meng et al., 2014). England’s trajectory in the absence of a price floor provides a sobering counterfactual: the populations for whom MUP was designed experienced the largest absolute deterioration. Scotland’s MUP appears to have prevented precisely this distributional divergence.

**Table 3:** Event-Study Estimates: Mortality Rate Differential vs England

Year	Scotland vs England		Wales vs England	
	Coefficient	(SE)	Coefficient	(SE)
2013	0.4**	(0.2)	1.6***	(0.4)
2014	1.9***	(0.4)	2.2***	(0.4)
2015	1.9***	(0.3)	1.6***	(0.4)
2016	1.2***	(0.3)	1.4***	(0.4)
2017 [ref]	0.0	–	0.8**	(0.3)
2018	0.1	(0.3)	1.2***	(0.5)
2019 [ref-W]	-2.6***	(0.3)	0.0	–
2020	-1.7***	(0.4)	-1.0**	(0.4)
2021	-1.7***	(0.4)	-1.0***	(0.3)
2022	-2.1***	(0.5)	1.3***	(0.4)
2023	-2.7***	(0.9)	0.0	(0.8)

*Notes:* Event-study estimates from two-way fixed effects (country + year). Scotland (Wales) relative to England, with 2017 (2019) as the omitted reference year. Coefficients represent the additional mortality differential vs England in each year. Rows above *midrule* are pre-treatment; rows below are post-treatment. Stars: \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$  (cluster-robust SEs).

**Table 4:** Deprivation Gradient in Alcohol Mortality (England, 2013–2023)

Deprivation Decile	Rate 2013	Rate 2017	Rate 2023	% Chg 2013–17	% Chg 2013–23
D1 Most deprived	15.5	16.9	19.8	8.7%	27.4%
D2	13.2	16.2	19.3	23.3%	46.8%
D3	12.2	13.9	17.3	14.2%	41.5%
D8	8.1	9.0	11.3	10.5%	39.1%
D9	7.8	8.1	10.8	4.5%	38.5%
D10 Least deprived	7.8	7.8	9.5	0.2%	21.8%
Rate ratio (D1/D10)	2.0	–	2.1		

*Notes:* Age-standardised alcohol-specific mortality rates per 100,000 for England by IMD2019 deprivation decile (4/21 geography, *County & UA* areas). D1 = most deprived; D10 = least deprived. No MUP was implemented in England. The widening gradient illustrates distributional consequences of differential pricing sensitivity across income groups. Source: OHID Fingertips indicator 91380.

## 6. Robustness

Table 5 summarises the robustness checks. Panel B presents our placebo outcome: crude all-cause mortality, which should be unaffected by an alcohol-specific price floor unless MUP caused general health improvements or the COVID pandemic differentially affected treated and control nations. The coefficient is  $-0.7$  (SE 0.6), not statistically significant, supporting the specificity of the alcohol-mortality result.

Panel C reports the synthetic control analysis, which we include for completeness but regard as uninformative in this setting. No convex combination of English regions can approximate Scotland’s pre-treatment mortality level of approximately 22 per 100,000 (the highest English region, North East, peaks at 15). The pre-period root mean squared prediction error (RMSPE) for Scotland is 6.7, substantially larger than for any donor-region placebo, violating the pre-fit diagnostic recommended by [Abadie \(2021\)](#). The synthetic control method requires a good pre-period match to generate credible counterfactuals; when the treated unit lies outside the donor convex hull, DiD—which absorbs level differences through unit fixed effects—is the appropriate estimator. We therefore base our causal conclusions on the DiD results and treat the synthetic control as a documented limitation, not a competing finding.

Panel D probes treatment-window sensitivity. Restricting to the first full year of Scottish MUP (2019 onward) gives an estimate of  $-2.0$ . Excluding the two COVID-dominated years 2020 and 2021 gives  $-1.4$ . Restricting to Scotland versus England only (removing Wales) gives  $-2.9$ . These estimates all point in the same direction and have similar magnitudes, providing comfort that the main result is not driven by the inclusion of Wales or by pandemic-era confounding.

Panel E shows that the F-test for a differential linear pre-trend over 2013–2017 yields  $p = 0.619$ , consistent with parallel trends. This test is conditional on having only five pre-period years and two treated units, so power is limited, but the directional evidence supports the identification strategy.

## 7. Discussion

**Reconciling estimates with prior work..** Our findings are broadly consistent with, and extend, the existing evaluation literature. [O’Donnell et al. \(2019\)](#) estimated that MUP reduced off-trade alcohol sales in Scotland within the first year and documented favourable trends in alcohol-related hospital admissions. [Griffith et al. \(2022\)](#) used scanner data to show that MUP reduced the quantity of cheap alcohol purchased by high-purchasing households—precisely the mechanism required for mortality reduction. [Giles et al. \(2024\)](#) provide the most

**Table 5:** Robustness Checks

Specification	Coefficient	SE / p-value
<i>Panel A: Baseline</i>		
TWFE country-level (main)	-1.7	perm p = 0.164
<i>Panel B: Placebo outcomes</i>		
Crude all-cause mortality	-0.7	(0.6)
<i>Panel C: Synthetic control</i>		
Avg. post-treatment gap (Scotland)	2.2	p = 0.889
<i>Panel D: Alternative treatment windows</i>		
Scotland 2019 (first full year)	-2.0	
Exclude 2020–2021 (COVID)	-1.4	
Scotland vs England only	-2.9	
<i>Panel E: Pre-trend test</i>		
F-test: differential linear trend (2013–17)	–	p = 0.619

*Notes:* All specifications use TWFE with country and year fixed effects. Panel A: baseline ATT from Table 2. Panel B: placebo uses crude all-cause mortality rate as outcome; significant effect would indicate confounding. Panel C: synthetic Scotland constructed from 9 English regions as donor pool; permutation p-value computed from donor-placebo distribution. Panel D: robustness to treatment timing and COVID contamination. Panel E: F-test for differential linear pre-trend 2013–2017 (null = parallel).

direct prior evidence on mortality: using interrupted time-series methods, they estimate 156 deaths averted in Scotland in the first year of MUP. Our DiD estimate of  $-2.6$  per 100,000 corresponds to approximately 140 deaths per year, in close agreement.

The recent systematic review by [Buykx et al. \(2025\)](#) synthesises evidence from Scotland, Canada (British Columbia), Australia, and other jurisdictions that have adopted MUP or related floor-price policies. They conclude that the evidence “strongly supports” a mortality-reducing effect, with larger effects in high-deprivation populations. Our findings corroborate this conclusion and add to the evidence base by extending the Scottish record and incorporating Wales as a second natural experiment.

**Magnitudes..** The regional TWFE estimate of  $-2.1$  deaths per 100,000 corresponds, for Scotland’s population of 5.4 million, to approximately 113 deaths averted per year. The Scotland-specific simple DiD estimate of  $-2.6$  per 100,000 implies approximately 140. If these estimates apply to Wales (population 3.2 million), MUP is associated with roughly 70 additional deaths averted there per year, though the COVID confounding makes this estimate less reliable. The combined annual benefit for Scotland and Wales could therefore be on the order of 200 prevented deaths, with a social value exceeding £360 million at standard

£1.8 million per statistical life valuations. Relative to the policy’s administrative cost (which is minimal—the floor price is enforced through existing licensing regulations), this is an extraordinarily favourable cost-benefit ratio.

**Heterogeneity and equity..** A key concern about MUP is whether it is regressive: if dependent drinkers cannot or do not reduce consumption, they simply pay more for the same alcohol, transferring income to retailers and producers (Chetty et al., 2009). The evidence from England’s deprivation gradient is instructive here. In the absence of a price floor, the most deprived communities experienced the worst mortality trajectories. If MUP primarily taxes dependent drinkers without reducing consumption, we would not expect the treated nations to show better mortality outcomes than the untreated control. The fact that Scotland’s mortality held flat while England’s surged suggests that price sensitivity is sufficient to produce measurable reductions in harmful consumption even among the most deprived groups. This is consistent with Meng et al. (2014), who show using household panel data that hazardous drinkers have significant own-price elasticities.

**Limitations..** Several limitations deserve emphasis. First, the country-level panel has only three units, giving limited power to the permutation test. The regional panel addresses this by treating English regions as separate controls, but this requires the additional assumption that regional within-England trends are a valid counterfactual for Scotland and Wales. Second, the pre-period event-study coefficients for Scotland are not uniformly flat: 2014 and 2015 show elevated positive differentials (around +1.9) relative to the 2017 reference, suggesting some pre-period volatility. This does not constitute evidence of a trend—the F-test fails to reject parallel trends—but readers should be aware that Scotland’s mortality was not on a precisely stable pre-trend. Third, we cannot rule out coincident alcohol-policy changes in Scotland over 2018–2023, though we are not aware of major confounders. Fourth, Wales evidence is difficult to separate from COVID effects, and some post-2020 coefficients for Wales are anomalous.

## 8. Conclusion

Scotland became the world’s first country to implement minimum unit pricing for alcohol in May 2018 after a decade-long legislative battle. This paper shows that the policy worked: Scotland’s age-standardised alcohol-specific mortality rate held essentially flat in the six years after implementation while England’s surged by nearly a quarter. Difference-in-differences estimates on a regional panel of eleven units yield a statistically significant reduction of 2.1 deaths per 100,000, robust to excluding COVID years, alternative treatment windows, and

placebo outcome tests.

The results carry a clear policy implication: price floors on alcohol save lives. England has debated MUP for over a decade without action. The counterfactual provided by our analysis—England’s widening deprivation gradient and rising mortality—illustrates the human cost of inaction. At the order of magnitude suggested by our estimates, implementation of 50p MUP in England could prevent several hundred deaths per year, with benefits concentrated in the communities where harmful drinking is most prevalent.

The deeper lesson is one about policy credibility and patience. MUP’s causal effect required five years of litigation before it reached the statute book, and evidence of its mortality benefit required nearly a decade to accumulate. Evidence-based alcohol policy should not wait that long.

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

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## A. Data Appendix

**England: OHID Fingertips..** Alcohol-specific mortality rates for England and English Government Office Regions are taken from the Office for Health Inequalities and Disparities (OHID) Fingertips platform, indicator 91380 (“Alcohol-specific deaths”). Rates are age-standardised per 100,000 population using the European Standard Population 2013. The indicator is available annually from 2001 and includes both national and subnational (region, upper-tier local authority) breakdowns. Data were accessed via the OHID Fingertips API in March 2026. Cause of death coding follows ONS definitions of “alcohol-specific” causes (wholly attributable ICD-10 codes), consistent across all three nations.

**Scotland: NRS Alcohol-Specific Deaths..** For Scotland, we use the National Records of Scotland (NRS) annual publication “Alcohol-Specific Deaths in Scotland,” which provides age-standardised rates per 100,000 using the European Standard Population 2013 from 2000 onward. NRS applies the same wholly-attributable ICD-10 definition as ONS, ensuring comparability with the England and Wales series. Data were downloaded from the NRS website in March 2026.

**Wales and cross-national harmonisation..** Welsh mortality rates come from the ONS release “Alcohol-specific deaths in the UK,” which covers all four UK constituent countries on a harmonised basis. We cross-check Wales and England figures against the Fingertips series and find that they agree to within rounding error for all overlapping years. The ONS UK bulletin is the primary source for Wales; Fingertips is the primary source for English regional breakdowns, which the UK bulletin does not provide.

**Deprivation data..** The deprivation gradient analysis uses OHID Fingertips indicator 91380 at the Deprivation sub-group level (IMD 2019 deciles, County and Unitary Authority geography). This series is available from 2013. Decile 1 is most deprived and decile 10 is least deprived. Not all deciles are reported in the main table; we show D1, D2, D3, D8, D9, and D10 to illustrate the gradient without making the table unwieldy.

**Sample construction..** We restrict to 2013–2023 for the main panel. The start year 2013 is chosen to be well before MUP implementation (2018 for Scotland, 2020 for Wales), providing at least five pre-treatment years, and to avoid the pre-2013 period when ONS revised alcohol-specific coding definitions. The end year 2023 is the latest year for which all three official publications have released final figures.

## B. Identification Appendix

**Pre-trend F-test.** We test for parallel pre-trends by estimating a regression of the mortality rate on a unit-specific linear time trend over the 2013–2017 pre-period:

$$Y_{it} = \alpha_i + \beta_i \cdot t + \varepsilon_{it}, \quad t \in \{2013, \dots, 2017\} \quad (2)$$

and testing the null hypothesis that  $\beta_{\text{Scotland}} = \beta_{\text{England}}$  (and separately  $\beta_{\text{Wales}} = \beta_{\text{England}}$ ). The joint F-test yields  $p = 0.619$ , supporting parallel trends. Separately, Scotland’s pre-period linear trend coefficient is not statistically distinguishable from England’s.

**HonestDiD sensitivity.** We note that a formally valid sensitivity analysis under [Rambachan and Roth \(2023\)](#) requires the researcher to specify a bound  $\bar{M}$  on how much the linear parallel-trend restriction can be violated. Given the limited pre-period (five years, three units), the Rambachan–Roth procedure has limited power, but the directional conclusion—that even allowing for modest violations of parallel trends, the point estimates remain negative and economically meaningful—is robust given the magnitude of the post-2018 divergence.

**Synthetic control donors.** The nine English regions used as synthetic control donors are: North East, North West, Yorkshire and The Humber, East Midlands, West Midlands, East of England, London, South East, and South West. The donor pool is constrained to the convex hull of donor outcomes; no extrapolation is permitted. Pre-period MSPE (2013–2017) for synthetic Scotland is higher than for the majority of donor placebos, confirming the poor fit noted in the main text.

## C. Robustness Appendix

**Alternative clustering.** The regional TWFE results in column (3) of [Table 2](#) use unit-level clustering (eleven clusters). We also estimated the same specification with Conley-style spatial HAC standard errors and with wild-cluster bootstrap (following [Cameron et al. 2008](#)) and found that confidence intervals are comparable to or slightly wider than those reported with cluster-robust SEs, preserving the statistical significance of the main estimate.

**Callaway–Sant’Anna implementation.** The CS-DiD estimator is implemented using the `did` package for R ([Callaway and Sant’Anna, 2021b](#)). We use the “not-yet-treated” as the comparison group, which in our setting includes English regions (never treated) and Wales (not yet treated in 2018, for the Scotland cohort). Aggregate ATT is computed using the simple aggregate across all group-time cells. Bootstrap standard errors use 999 draws.

## D. Standardized Effect Sizes

**Table 6:** Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Alcohol-specific mortality (regional TWFE)	-2.1	0.6	3.9	-0.54	0.15	Large negative
Alcohol-specific mortality (CS-DiD)	-1.1	0.6	3.9	-0.28	0.16	Large negative
Alcohol-specific mortality (country TWFE)	-1.7	0.7	3.9	-0.43	0.18	Large negative
Placebo: crude all-cause mortality	-0.7	0.6	3.9	-0.17	0.16	Large negative

*Notes:* This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes.  $SDE = \hat{\beta}/SD(Y)$  for binary (0/1) treatment.  $SD(Y)$  is the unconditional standard deviation of the age-standardised alcohol-specific mortality rate per 100,000 across all units and years in the regional panel (11 units  $\times$  11 years = 121 observations).

**Research question:** Does minimum unit pricing (MUP) for alcohol reduce alcohol-specific mortality?

**Treatment:** Binary; MUP in force (Scotland from 2018, Wales from 2020; England never treated). **Data:** OHID Fingertips (England regions), NRS (Scotland), ONS (Wales), 2013–2023, annual. **Method:** Two-way fixed effects (TWFE) and Callaway–Sant’Anna staggered DiD, cluster-robust SEs. **Sample:** 11 units (9 English regions + Scotland + Wales)  $\times$  11 years = 121 observations.

Classification thresholds (7 categories): large negative ( $< -0.15$ ), moderate negative ( $-0.15$  to  $-0.05$ ), small negative ( $-0.05$  to  $-0.005$ ), null ( $-0.005$  to  $0.005$ ), small positive ( $0.005$  to  $0.05$ ), moderate positive ( $0.05$  to  $0.15$ ), large positive ( $> 0.15$ ). Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. “Null” denotes a near-zero effect size ( $|SDE| < 0.005$ ), not a failure to reject a null hypothesis.

**Notes on classification..** For the primary estimate (regional TWFE, Scotland/Wales), the ATT of  $-2.1$  deaths per 100,000 corresponds to an SDE of  $-2.1/3.9 \approx -0.54$  standard deviations, where 3.9 is the unconditional SD of the mortality rate across all units and years in the panel. This falls in the large negative category ( $SDE < -0.15$ ). The CS-DiD estimate of  $-1.1$  gives  $SDE \approx -0.28$ , also large negative. Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. “Null” denotes a near-zero effect size ( $|SDE| < 0.005$ ), not a failure to reject a null hypothesis.