Booze and Bruises: A Century-Old Natural Experiment on the Causal Link Between Pubs and Crime

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Abstract

This paper provides causal estimates of how pub density affects crime in New South Wales, Australia. We exploit a century-old natural experiment. The 1907 Local Option referendum closed around ten per cent of pubs in electorates that voted against renewal, and a cascade of subsequent licensing reforms locked the closures into modern pub geography. We use the 1907 No Licence vote share to instrument modern hotel density across 546 Statistical Area Level 2 regions and estimate effects on sixteen pre-committed offence categories. An additional hotel per 1,000 residents raises non-domestic assault by 5.25 per 1,000 annually, more than the sample mean. Liquor offences, disorderly conduct, drug offences, malicious damage and weapons offences show comparable effects. Theft rises by 18.8, plausibly through foot-traffic exposure rather than alcohol-induced behaviour. Sexual offences, arson, pornography offences and coercive control show no robust causal effect; homicide is marginal, surviving naive and Romano-Wolf inference but not the weak-instrument correction. Domestic assault is essentially zero in metropolitan SA2s but sizeable and significant in country areas, consistent with a single on-premises-drinking mechanism playing out through different post-pub geographies. Seven outcomes survive both Romano-Wolf step-down correction and the Lee et al. [2022] tF adjustment for weak instruments. The differential pattern across crime types is difficult to reconcile with omitted-variable bias and is consistent with on-premises drinking and post-pub behaviour as the channel. Blanket caps on alcohol licences are blunt; targeted, regionally differentiated regulation focused on the specific harms causally linked to pubs would be more effective.

Keywords: Alcohol availability, crime, violence, instrumental variables, liquor licensing, Local Option Poll.

JEL Codes: I18, K42, R58, C26, H75.

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1 Introduction

The relationship between alcohol availability and crime is one of the most studied questions in criminology and public health, and one of the most resistant to causal interpretation. Cross-sectional studies consistently find positive correlations between alcohol outlet density and violent crime across geographies from city blocks to metropolitan regions [Scribner et al., 1995, Gruenewald et al., 2006, Kearns et al., 2015], and the medical literature documents plausible behavioural channels through alcohol's effects on cognition and self-control [Bechara, 2005, Moeller and Dougherty, 2001]. The policy implication, were the correlations causal, would be straightforward: restricting pub licences should reduce alcohol-related harm. Australian state governments have repeatedly acted on exactly this premise through licensing caps and trading-hour restrictions. Whether the premise is correct, and across which categories of crime, remains an open question.

This paper provides causal estimates of the effect of pub density on crime across sixteen offence categories in New South Wales, Australia. We exploit a historical natural experiment that produced exogenous, persistent variation in the spatial distribution of pubs. The Liquor Amendment Act of 1905 created a Local Option mechanism that allowed each NSW electorate to vote on whether to retain, reduce or eliminate pub licences within its boundaries. The first poll was held in 1907 alongside state elections. Sixty-five of ninety electorates voted to reduce, and Local Option Courts subsequently closed around 293 pubs, about ten per cent of those then operating. A cascade of subsequent regulation, including the 1905 Act's statewide cap on new licences, the 1919 Licence Reduction Board, and the mid-twentieth-century Tooths and Tooheys duopoly [Stubbs, 1999], locked the 1907 closures into modern pub geography. Electorates that voted heavily against renewal in 1907 have meaningfully fewer pubs today. We use the 1907 No Licence vote share as an instrument for modern hotel density across 546 Statistical Area Level 2 regions covering populated NSW.

The previous literature on alcohol and crime divides into two main strands. Observational studies of outlet density and crime are abundant but cannot separate selection from causation

[Scribner et al., 1995, Kearns et al., 2015]. Causal estimates rely either on aggregate policy changes [Luca et al., 2015] or on shocks to nighttime transit access [Phillips and Sandler, 2015, Jackson and Owens, 2011]. The aggregate policy designs identify total effects of large interventions but cannot recover the marginal contribution of a single outlet. The transit designs identify changes in accessibility rather than the supply of pubs. Neither speaks directly to the question that licensing authorities actually face: what is the causal effect of one more pub in a given neighbourhood, and how does it vary across crime types?

This paper makes three contributions. First, we deliver what is, to our knowledge, the largest set of causal estimates of pub-density effects on crime in any jurisdiction, covering sixteen offence categories under a pre-committed five-tier classification of theoretical priors and a Romano-Wolf step-down correction for joint testing. We additionally report Lee et al. [2022] tF-adjusted confidence intervals; the two corrections, conceptually independent, identify the same set of survivors. Second, we demonstrate a clean differential pattern across crime types that identifies the alcohol-behaviour channel and rules out generic neighbourhood-quality explanations. Non-domestic assault, liquor offences, disorderly conduct, drug offences, malicious damage, weapons offences and theft all respond causally to pub density. Sexual offences, arson, pornography offences and coercive control do not. Domestic assault decomposes into a near-zero metropolitan effect and a sizeable rural one, a pattern that fits a single on-premises-drinking mechanism playing out through different post-pub settlement geometries. Theft is consistent with foot-traffic exposure rather than alcohol-induced behaviour. Third, the policy implication is that blanket caps on alcohol licences mistake correlation for causation across most crimes. Targeted regulation focused on the specific harms causally linked to pubs, and regionally differentiated to recognise that rural and metropolitan pub-going have different downstream consequences, would be both more effective and less costly than the current uniform approach.

The remainder of the paper is structured as follows. Section 2 describes the 1905 Liquor Amendment Act and the institutional cascade that converted the 1907 vote into a hundred-

year shock to pub geography. Section 3 describes the data and the SA2-level harmonisation of voting records, licensing data and crime counts. Section 4 sets out the empirical strategy, defends the instrument against the standard relevance, exclusion and monotonicity threats, and introduces the pre-committed tier classification. Section 5 presents the headline IV estimates and the Romano-Wolf correction. Section 6 reports alternative instrument encodings, metro and country subsamples, a panel IV, and the Lee tF adjustment. Section 7 discusses the mechanism and the theft puzzle. Section 8 draws out the policy implications and limitations.

2 Institutional Context

Pubs have been a major part of the social and economic life of New South Wales since the founding of the colony in 1788. Rum served as a de-facto currency during the early colonial period and provoked the only military coup in Australian history with the Rum Rebellion of 1808. By 1837, Sydney's 23,000 residents supported 224 licensed taverns, roughly twelve times the modern prevalence per capita [Hanna, 2016]. By 1880, NSW contained nearly 4,000 pubs.

In the early twentieth century a growing temperance movement combined with concern about the social costs of heavy drinking led to the Liquor Amendment Act of 1905. The Act capped the number of liquor licences in each electorate at the pre-Act level and created a Local Option mechanism that allowed voters in each electorate to determine how many pubs would operate locally. The poll was held alongside state elections.

Voters chose among three options. *Continuance* retained existing licences. *Reduction* required a court to close up to twenty per cent of licences in the electorate. *No Licence* prohibited all liquor licences in the electorate. The No Licence outcome required at least sixty per cent of the votes cast and required that the voters comprise at least thirty per cent of registered voters. If No Licence failed, the votes cast for it were added to those for

Reduction. No compensation was payable to licensees in the event of either outcome.

Three Local Option polls were held in NSW, in 1907, 1910 and 1913, before the mechanism was suspended in 1928. The 1907 poll produced the strongest result in favour of restriction. Sixty-five of the ninety electorates carried Reduction. Twenty-five carried Continuance. No electorate met the threshold for a No Licence outcome at any of the three polls. At the state aggregate, Continuance received 45.1 per cent of votes, Reduction 16.3 per cent, and No Licence 38.5 per cent.

Where Reduction carried, a local court was established to identify pubs for closure. The court was required to reduce licences by up to twenty per cent, with priority given to pubs that had previous records of disruptive behaviour. Pubs with two or more convictions were allowed one year to close. Pubs with one conviction were allowed two years. Where insufficient pubs had convictions to meet the target, the court closed clean-record pubs with regard to local public convenience, allowing up to three years for closure [Arnott, 1934]. The final closures resulting from the 1907 vote took effect in 1913. In total 293 pubs across NSW were closed, approximately ten per cent of the pubs in operation in 1907. Of the sixty-five electorates that carried Reduction, only three failed to carry out any closures [NSW Government, 1954].

3 Data

The analysis draws on four main data sources: voting data from the 1907 Local Option poll, modern liquor licensing data, modern crime statistics, and sociodemographic data on local communities. These sources are originally published using different geographic classifications, including 1907 state electorates, modern suburbs and various census units. We harmonise everything to a common geography known in Australia as the Statistical Area Level 2, or SA2. SA2s are the official ABS unit designed to represent communities that interact socially and economically, roughly corresponding to suburbs in cities and small towns or districts in

regional areas. They typically have populations between 3,000 and 25,000.

3.1 1907 Voting Data

The 1907 election results, including vote counts and registered voters, were provided by the Parliament of New South Wales [Parliament of NSW, 2007]. The Local Option poll results were sourced from the New South Wales Government Gazette of 1907 [NSW Government, 1907]. Of the ninety electorates, eighty-nine had Local Option results reported in the Gazette. The electorate of Tenterfield had no published result and so we drop it from the analysis. Electorate boundary maps were digitised from Pernica [1958, pp. 142–143], which reports 1910 boundaries. Additional research confirmed no significant changes in electorate boundaries between 1907 and 1910.

Figure 1 shows the spatial pattern of 1907 outcomes at both the 1907 electorate level and the modern SA2 level after reconciliation. The eastern coastal strip and parts of the inner west voted Reduction while the western inland and most of inner Sydney voted Continuance.

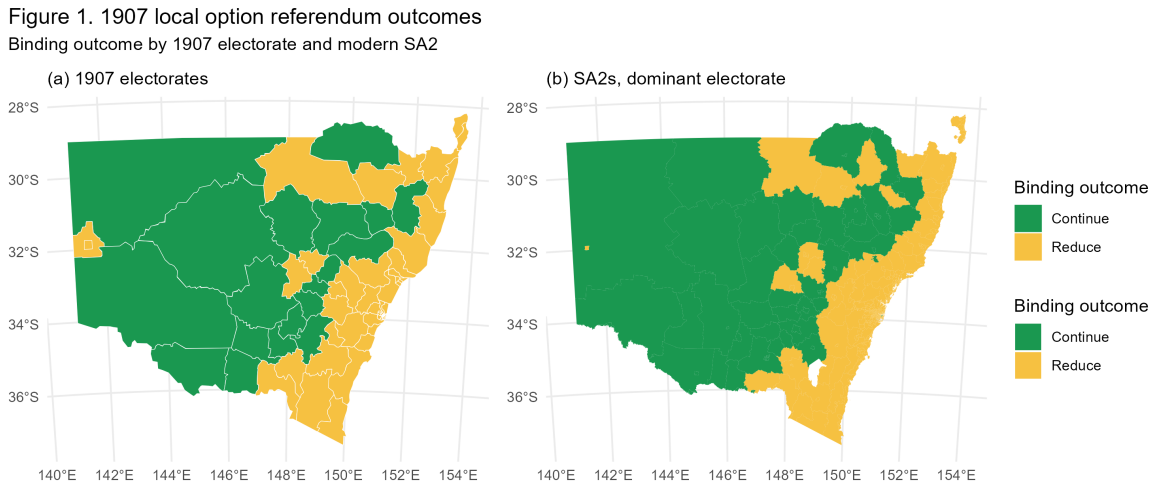

Figure 1: 1907 Local Option Referendum Outcomes

Note: Binding outcome by 1907 electorate (left panel) and by 2016 Statistical Area Level 2 region (right panel). SA2s are assigned to their dominant 1907 electorate by area weight. No electorate voted for No Licence at the 1907 poll. SA2s with less than 80 per cent area coverage by a single 1907 electorate are shown in grey.

3.2 Liquor Licensing Data

We identify current pubs using the liquor licensing database maintained by the NSW Office of Liquor, Gaming and Racing [OLGR, 2019]. The database lists every active liquor licence in NSW with its address, type and operating status. We restrict attention to active hotel licences,[1] which are the closest modern analogue to the on-premises drinking establishments targeted by the 1907 vote. We geocoded all licence addresses and assigned each pub to its containing SA2 by spatial join. Of approximately 4,000 hotel licences in NSW, 3,975 were successfully geocoded. The licensing snapshot is from 2019, while our crime window runs from 2015 to 2024. We treat the modern pub stock as effectively fixed over this window: NSW hotel licence numbers have been broadly stable for decades and the small year-on-year churn is negligible relative to the cross-sectional variation our instrument moves.

3.3 Crime Data

Crime counts come from the NSW Bureau of Crime Statistics and Research's Recorded Criminal Incidents dataset [BOCSAR, 2025], which reports monthly counts by suburb. BOCSAR organises offences into 21 main categories such as assault, theft, malicious damage and drug offences, with most categories further broken down into more specific subcategories such as steal from motor vehicle or steal from dwelling under theft. The full dataset covers 62 subcategories in total. We work at the category level for most outcomes to retain interpretability and statistical power, but we promote two subcategories of assault to their own first-class outcomes: assault flagged by police as domestic-violence-related, and assault not so flagged. We refer to these throughout as domestic assault and non-domestic assault. Both are highly policy-relevant and there is substantial uncertainty in the existing literature about whether pubs causally affect either, both, or neither.

We aggregate monthly counts to annual totals, then aggregate suburbs to SA2s using area-

[1] In NSW nomenclature, "hotel" refers to what is colloquially called a pub. NSW pubs were historically required to provide accommodation and food alongside alcohol service, and the two terms became synonymous. Modern hotel licences no longer require accommodation but the name has stuck.

weighted allocation. We compute average annual crime rates per 1,000 residents over 2015 to 2024 using ABS estimated resident population. We use a ten-year window to smooth year-on-year variation while keeping the period contemporaneous with our licensing snapshot.

3.4 Sociodemographic Controls

We obtain SA2-level sociodemographic data from the ABS Regional Profiles series [Australian Bureau of Statistics, 2020]. Our parsimonious specification uses median age and population density. The full specification adds median household income and the SEIFA Index of Relative Socio-Economic Disadvantage. SEIFA is an ABS-constructed composite score that aggregates around twenty census variables related to income, education, employment, housing and household composition into a single decile ranking for each area.[2] A single SEIFA decile therefore captures a large share of the cross-sectional variation in neighbourhood socioeconomic status that would otherwise have to be picked up by multiple separate controls.

3.5 Final Analysis Sample

We restrict the analysis sample to SA2s with at least 1,000 residents and at least eighty per cent area coverage by a single 1907 electorate. This yields 546 SA2s across 80 dominant electorates. A subset of these have zero hotels per 1,000 residents; these are genuine zero-pub SA2s identified after spatial joining the geocoded licence file to the SA2 layer, rather than unmatched observations. Descriptive statistics appear in Table 1, grouped by data source.

Table 2 compares means across SA2s grouped by their dominant 1907 outcome. Continue SA2s have around four times the pub density of Reduce SA2s. They are less population-dense on average and more disadvantaged on the SEIFA index. Crime rates are substantially higher in Continue SA2s across most categories, including non-domestic assault (7.3 versus

[2]SEIFA stands for the Socio-Economic Indexes for Areas. The ABS publishes four such indexes; we use the Index of Relative Socio-Economic Disadvantage (IRSD), which is constructed so that lower deciles indicate greater disadvantage.

Table 1: Descriptive statistics for the cross-sectional analysis sample

	N	Mean	SD	Min	Median	Max
Liquor licensing						
Hotels per 1,000	546	0.34	0.47	0.00	0.16	5.23
1907 voting						
No Licence share	546	0.43	0.09	0.09	0.43	0.62
Reduce share	546	0.15	0.05	0.03	0.16	0.31
Continue share	546	0.42	0.07	0.31	0.40	0.69
Sociodemographic						
Population	546	14,014	7,099	2,420	13,167	35,154
Population density (per km^2)	546	1,792	2,321	0	884	15,608
Median age	546	39.8	5.9	26.5	39.0	61.6
Median income (AUD)	546	47,463	8,469	27,324	47,066	77,807
SEIFA IRSD decile	529	5.3	2.9	1.0	5.0	10.0
Crime rates (per 1,000)						
Non-domestic assault	546	4.32	4.69	0.12	3.12	58.39
Liquor offences	546	1.20	2.37	0.00	0.52	36.40
Homicide	546	0.013	0.016	0.000	0.009	0.148
Disorderly conduct	546	2.84	4.01	0.10	1.63	44.43
Malicious damage	546	7.63	7.88	0.22	5.85	114.55
Drug offences	546	5.80	6.43	0.14	3.90	56.94
Intimidation, stalking, harassment	546	5.26	5.35	0.17	4.21	83.61
Sexual offences	546	2.08	1.64	0.05	1.74	22.97
Weapons offences	546	2.05	2.12	0.06	1.45	27.75
Betting and gaming offences	546	0.016	0.073	0.000	0.000	1.089
Theft	546	26.36	24.82	0.55	19.98	375.67
Robbery	546	0.25	0.30	0.00	0.15	2.83
Arson	546	0.79	1.38	0.01	0.43	21.31
Pornography offences	546	0.117	0.100	0.000	0.101	1.514
Domestic assault	546	4.41	4.61	0.17	3.51	67.61
Coercive control	546	0.002	0.006	0.000	0.000	0.055

Notes: Sample is 546 SA2s in NSW with population $\geq$ 1,000 and instrument coverage $\geq$ 80%. Crime rates are annual averages 2015 to 2024.

3.9 per 1,000), liquor offences, disorderly conduct, drug offences and theft. The two placebo outcomes, arson and pornography offences, show much smaller differences. These differences foreshadow the reduced-form effects we estimate in Section 5.

Table 2: Means by dominant 1907 outcome

	Continue ($n = 67$)	Reduce ($n = 479$)	Difference (Reduce − Continue)
Liquor licensing			
Hotels per 1,000	1.02	0.24	-0.78
Sociodemographic			
Population density (per km^2)	732	1,940	1,208
Median age	40.5	39.7	-0.8
Median income (AUD)	44,017	47,945	3,929
SEIFA IRSD decile	3.6	5.6	2.0
Crime rates (per 1,000)			
Non-domestic assault	7.28	3.90	-3.38
Liquor offences	2.75	0.98	-1.76
Homicide	0.019	0.012	-0.006
Disorderly conduct	7.00	2.26	-4.74
Malicious damage	13.25	6.85	-6.40
Drug offences	9.38	5.29	-4.09
Intimidation, stalking, harassment	8.91	4.75	-4.16
Sexual offences	3.26	1.91	-1.34
Weapons offences	4.30	1.73	-2.57
Betting and gaming offences	0.075	0.008	-0.068
Theft	39.22	24.57	-14.65
Robbery	0.31	0.24	-0.06
Arson	1.73	0.65	-1.08
Pornography offences	0.179	0.108	-0.071
Domestic assault	7.39	3.99	-3.40
Coercive control	0.006	0.002	-0.004

Notes: SA2s grouped by their dominant 1907 electorate outcome. No electorate ever voted No Licence so all SA2s fall in Continue or Reduce.

4 Empirical Strategy

4.1 Specification

The estimand of interest is the causal effect of an additional pub on the local crime rate. We pool BOCSAR data over the period 2015 to 2024 into a single cross-sectional measure of average annual crime rates rather than running a panel. The reason is that the variation we want to exploit, namely persistent differences in pub density induced by the 1907 vote, operates over the long run rather than month to month. A panel specification would absorb most of our identifying variation into fixed effects, while introducing the noise of short-run fluctuations in policing, reporting and seasonal patterns that have nothing to do with pub density. The pooled cross-section produces a long-run representative measure of crime that is appropriate for identifying a long-run causal effect.

Let i index SA2s. We estimate:

$$\left(\frac{\text{Crime}}{\text{1,000 residents}}\right)_i = \beta \cdot \left(\widehat{\frac{\text{Pubs}}{\text{1,000 residents}}}\right)_i + X_i' \boldsymbol{\phi} + \nu_i \tag{1}$$

$$\left(\frac{\text{Pubs}}{\text{1,000 residents}}\right)_i = \alpha \cdot \text{NoLicence}_i + X_i' \boldsymbol{\lambda} + \varepsilon_i \tag{2}$$

The dependent variable in the structural equation is the annual offence rate per 1,000 residents averaged over 2015 to 2024. The endogenous regressor, pubs per 1,000 residents, is instrumented in the first stage by NoLicence_i, the 1907 No Licence vote share assigned to SA2 i by area-weighting. The coefficient β is the parameter of interest. To make it concrete, consider an SA2 with 10,000 residents and one pub. A value of $\beta = 5$ would mean that adding a second pub to this SA2, which doubles the pub density from 0.1 to 0.2 per 1,000 residents, would raise the annual non-domestic assault rate by $5 \times 0.1 = 0.5$ per 1,000 residents, or five additional assaults per year in absolute terms.[3] The vector X_i

[3]For an elasticity interpretation at the sample mean, the relevant quantity is $\eta_{c,p} = \beta \cdot \bar{p}/\bar{c}$, where $\bar{p}$ is the mean of pubs per 1,000 residents and $\bar{c}$ is the mean of the crime rate of interest. With our headline estimate of $\beta = 5.25$ for non-domestic assault, $\bar{p} = 0.34$ and $\bar{c} = 4.32$, the implied elasticity is approximately 0.41. A one per cent increase in pub density raises non-domestic assault by about 0.4 per cent at the sample mean.

contains demographic controls. The coefficient vectors $\boldsymbol{\phi}$ and $\boldsymbol{\lambda}$ collect the partial effects of those controls on the crime rate and on pub density respectively. We do not interpret them directly. Standard errors are clustered at the level of the SA2's dominant 1907 electorate to allow for within-electorate correlation in unobservables.

The preferred specification controls only for median age and log population density. The choice is partly substantive and partly econometric. Substantively, age and density are basic demographic determinants of crime that should be included in any reasonable model. Econometrically, adding additional socioeconomic controls such as income and SEIFA risks post-treatment bias because both are plausibly affected by long-run pub density itself. We also report a robustness specification that adds log median income and SEIFA disadvantage decile, which trades off some risk of post-treatment bias against tighter control for socioeconomic confounders. Section 6 shows the headline result is essentially unchanged across both specifications.

4.2 Identification: Relevance

For the 1907 vote to be a useful instrument for modern pub density it must predict the modern pub count, more than a century after the vote took place. The persistence is not coincidental. Rather, it reflects a cascading sequence of institutional choices that, taken together, locked the closures resulting from the 1907 vote into place. The 1905 Act capped pub numbers in each electorate at the pre-Act level, so closed pubs could not be replaced. By 1919, the NSW government established a Licence Reduction Board with statutory authority to remove licences directly and to make new licences difficult to obtain [Ross, 2020]. Through the mid-twentieth century, the Sydney pub trade was dominated by the brewery duopoly of Tooths and Tooheys, which used tied-house arrangements to suppress new entry [Stubbs, 1999]. Even after liberalisation of liquor licensing in the late twentieth century, opening a new pub in NSW continued to require extensive consultation with local government, police, health and community stakeholders. Each layer of regulation preserved what came before

and ensures that the reduction following the 1907 vote has survived into the 21st century.

Figure 2 confirms that the 1907 vote remains a strong predictor of modern pub density. The figure plots the residualised relationship after partialling out the preferred controls. The slope coefficient is -1.84 with a cluster-robust standard error of 0.46. The negative slope says that SA2s whose constituent 1907 electorates voted more strongly against pub licences have fewer pubs today. Specifically, an SA2 whose 1907 No Licence share was 0.6 has, all else equal, about 0.4 fewer pubs per 1,000 residents today than one whose share was 0.4. For an SA2 of 10,000 residents this is roughly four fewer pubs in absolute terms. The cluster-robust first-stage F-statistic is 16.1, comfortably above the Stock-Yogo critical value of 10. We discuss instrument strength in light of the more demanding Lee et al. [2022] effective F threshold in Section 6.

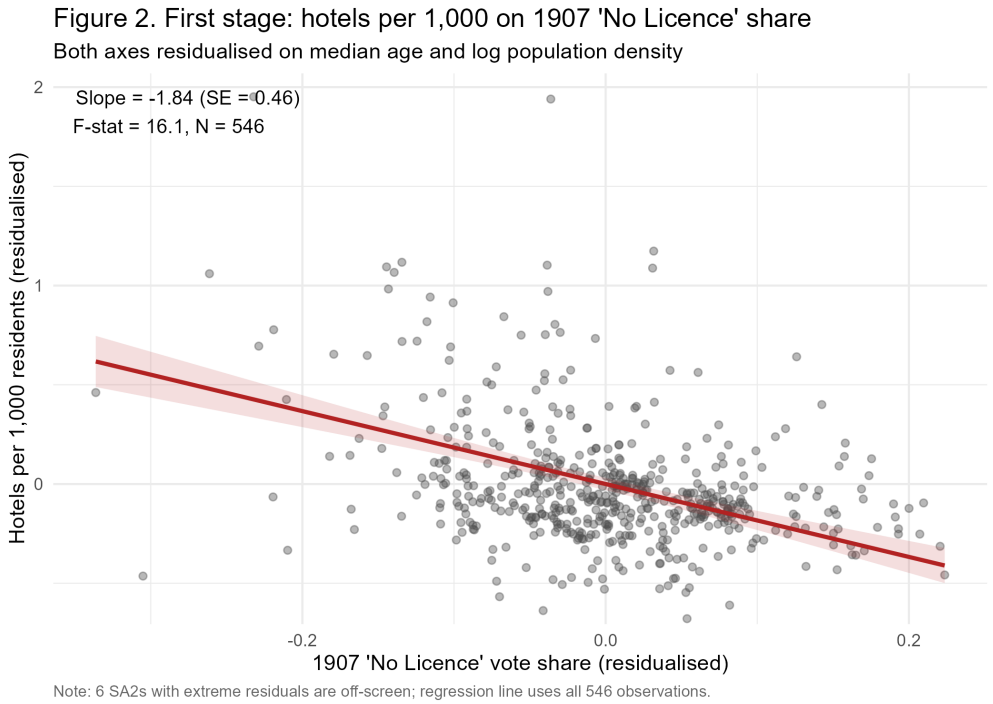

Figure 2: First Stage: Pubs per 1,000 on 1907 No Licence Share

Note: Each point is an SA2. Both axes residualised on median age and log population density. The fitted line and 95 per cent confidence band are from a felm regression with electorate-clustered standard errors. The slope coefficient is -1.84 with cluster-robust SE 0.46 and cluster-robust F of 16.1. Sample is 546 SA2s.

The first-stage table (Table 3) reports estimates with and without controls. The coeffi-

Table 3: First Stage: Effect of 1907 'No Licence' Share on Modern Hotel Density

	Dependent variable: hotels per 1,000 residents		
	Bivariate	Parsimonious	Full controls
	(1)	(2)	(3)
share_no_licence	-2.400^{***}	-1.836^{***}	-1.975^{***}
	(0.524)	(0.458)	(0.558)
median_age		-0.005	-0.008
		(0.005)	(0.007)
log_pop_density		-0.082^{***}	-0.075^{***}
		(0.011)	(0.015)
log_income			-0.352
			(0.423)
seifa_irsd			0.005
			(0.012)
Constant	1.360^{***}	1.806^{***}	5.703
	(0.242)	(0.298)	(4.783)
First-stage F (cluster-robust)	21	16.1	12.5
SE clustering	Electorate	Electorate	Electorate
Observations	546	546	529
R^2	0.200	0.384	0.388
Adjusted R^2	0.199	0.381	0.382
Note:	Standard errors clustered at 1907 electorate level.		

cient on No Licence share is fairly stable across specifications. Adding income and SEIFA modestly reduces the coefficient and the F-statistic from 16.1 to 12.5, indicating that some of the first-stage variation operates through socioeconomic channels, but the bulk of the variation reflects the institutional lock-in.

Overall, the institutional history establishes a credible mechanism for the persistence, and the empirical first stage is strong by conventional standards which indicates that the instrument is relevant.

4.3 Identification: Exclusion

The instrument must affect modern crime only through its effect on pub density. We argue that this assumption is plausible on four grounds.

First, the elapsed time has extinguished most of the channels through which 1907 voting could affect crime today: The voters are now all dead; the population of NSW has turned over completely through one of the highest migration rates in the developed world; personal and social characteristics that influenced voting in 1907 are wildly different at the electorate level.

Second, the 1907 electorate boundaries no longer correspond to any administrative or political unit. They do not coincide with modern state electorates, local government areas or police command areas. There is no contemporary decision-maker whose authority is bounded by the 1907 electorates. The boundaries do not affect any modern policy lever.

Third, the 1907 Local Option mechanism was abandoned within twenty years and has been largely forgotten. Most modern NSW residents are not aware that this vote took place, let alone its outcome. The vote cannot affect modern beliefs or behaviour through awareness because awareness is essentially absent.

Fourth, the social geography of NSW has changed substantially since 1907. The inner Sydney electorates that voted strongly for Continuance in 1907, including Darling Harbour, Pyrmont, Surry Hills, Redfern and Camperdown, were then working-poor neighbourhoods.

They are now among the most affluent SA2s in Australia. Whatever socioeconomic correlates of 1907 voting persisted into the mid-twentieth century, the persistence has been disrupted by population growth, urban renewal, migration and gentrification.

The most credible threat to the exclusion restriction is that 1907 voting reflects persistent local attitudes that affect crime through channels other than pubs. Temperance voting in 1907 was associated with Methodist, Baptist and Presbyterian religious adherence. Modern religious composition still correlates with 1907 patterns, though weakly. Persistent religious or cultural attitudes could plausibly affect modern crime through community norms, social capital or family structure. We address this threat directly through three complementary tests. First, we conduct placebo regressions on crimes for which an alcohol-behavioural channel is unlikely (arson and pornography offences). A failure of the exclusion restriction through cultural-attitude persistence would tend to produce significant effects on these placebos. Second, we conduct a falsification test on the differential between domestic and non-domestic violence. Both crimes involve violence and would respond similarly to any general violence-prone area channel. Third, we conduct subsample analyses to test whether the effect is concentrated in places where pub-going is most plausible as the channel.

4.4 Identification: Monotonicity

Under monotonicity, the 1907 vote must either reduce or have no effect on pub density. That is, in the historical data, there must be no *defiers*, defined as electorates that voted Reduction but ended up with more pubs as a result. The institutional context strongly supports monotonicity. Local Option Courts were established by statute and tasked with closing pubs in Reduction electorates. The closure procedure followed a structured priority order based on prior convictions and so sixty-two of sixty-five Reduction electorates carried out closures. There is no mechanism by which a vote for Reduction could plausibly increase pub density.

4.5 Multiple Testing

We estimate IV regressions on sixteen offence categories. The two assault subcategories described in Section 3 provide two of these. The remainder are the BOCSAR main categories for which an alcohol-related mechanism is either plausible or implausible enough to serve as a placebo. We exclude six BOCSAR categories that are either ill-defined catch-alls (Other offences, Other offences against the person), regulatory in nature with no plausible link to pub-going, or so rare in our sample that estimation is uninformative. To control the family-wise error rate, we apply the Romano and Wolf [2005] step-down procedure with cluster bootstrap. For each outcome and each bootstrap iteration, we draw 80 dominant 1907 electorates with replacement, build the bootstrap sample from all SA2s assigned to the drawn electorates, and re-estimate the IV; this respects the within-electorate dependence assumed in our clustered standard errors. The step-down procedure produces adjusted p-values that account for joint testing across all sixteen outcomes.

4.6 Outcome Classification

Testing sixteen outcomes against an instrument raises the obvious question of how to interpret the results. The behavioural literature offers strong priors about which crimes should respond to pub density and which should not. A blanket significance test across sixteen outcomes ignores these priors and treats every result as equally surprising. We instead pre-commit to a five-tier classification of the sixteen outcomes based on the strength of the theoretical link between pub-going and the crime in question. Pre-commitment was completed before we ran the IV regressions. The classification serves two purposes. First, it makes our priors explicit so readers can judge the results against them. Second, it gives the empirical pattern a meaningful structure: confirmation of strong priors, support for moderate priors, null results on placebos, and a within-category falsification test that we view as the paper's most informative piece of identifying evidence.

Table 4 sets out the classification.

Table 4: Pre-committed outcome classification

Tier	Outcomes	Reasoning
Tier 1: Strong prior	Non-domestic assault, Liquor offences, Homicide	Crimes where alcohol's effect on cognition, inhibition and aggression provides the most direct causal pathway from a pub to an offence.
Tier 2: Moderate prior	Disorderly conduct, Malicious damage, Drug offences, Intimidation, Sexual offences, Weapons offences, Betting and gaming	Crimes plausibly affected by alcohol-induced behaviour but where other channels also operate.
Tier 3: Ambiguous	Theft, Robbery	Property crimes whose connection to pub-going could operate through exposure (foot traffic, intoxicated victims) rather than alcohol-induced behaviour itself.
Tier 4: Placebo	Arson, Pornography offences	Crimes for which no plausible alcohol-behaviour channel exists. While intoxication can plausibly precipitate isolated arson incidents, the deliberate planning typically involved makes arson at the SA2 average a poor candidate for an alcohol-driven response. Significant effects here would point to a failure of the exclusion restriction.
Tier 5: DV comparison	Domestic assault, Coercive control	Crimes that share violent or controlling content with non-domestic assault but occur primarily in private settings. The contrast with Tier 1 isolates the on-premises drinking channel.

Notes: Tier classification was pre-committed before IV estimation. Each tier identifies a different prior about how pub density should affect the outcome.

5 Results

5.1 Headline Results

Table 5 reports OLS and IV estimates for each of the sixteen crime outcomes, ordered by the pre-committed tier classification described in Section 4.6. Each row is a separate pair of regressions of the crime rate on pubs per 1,000 residents. Column 1 reports the OLS estimate. Column 2 instruments pubs per 1,000 using the 1907 No Licence vote share. Both specifications include median age and log population density as controls. Standard errors are clustered at the 1907 electorate level. The full coefficient detail is reported in Appendix Tables A1 and A2.

The first message of the table is robustness. For most outcomes where alcohol is a plausible behavioural channel, the OLS and IV estimates point in the same direction and remain statistically significant under both estimators. The systematic pattern across the table is that the IV estimate exceeds the OLS estimate, modestly for liquor and drug offences, more substantially for disorderly conduct, malicious damage and intimidation. This direction is consistent with negative selection in pub location. Operators tend to open pubs in neighbourhoods with lower baseline propensity for these offences, conditional on demographics; absent the instrument, OLS therefore understates the causal effect. The IV strips out that selection and recovers a larger marginal effect of an additional pub. Non-domestic assault and homicide are exceptions to this pattern, with OLS and IV estimates close to one another. We return to non-domestic assault below.

The most striking feature of the table is the contrast between non-domestic and domestic assault. The OLS coefficient on non-domestic assault is 5.51 while the OLS coefficient on domestic assault is 1.34, even though the two crime types have similar sample means of around 4.4 per 1,000. The IV estimates preserve this contrast: 5.25 for non-domestic assault, robustly significant at the 1 per cent level, against 1.96 for domestic assault, significant only at the 10 per cent level. Pubs are causally responsible for roughly two and a half times as

Table 5: OLS and IV estimates by outcome

	OLS (1)	IV (2SLS) (2)
Non-domestic assault	5.507***	5.249***
	(0.862)	(1.070)
Liquor offences	4.357***	4.516***
	(0.904)	(1.065)
Homicide	0.007***	0.011**
	(0.002)	(0.004)
Disorderly conduct	4.385***	5.739***
	(0.405)	(0.803)
Malicious damage	4.159***	5.879***
	(0.936)	(1.475)
Drug offences	8.376***	8.992***
	(1.052)	(2.217)
Intimidation, stalking, harassment	2.191***	2.893**
	(0.489)	(1.172)
Sexual offences	0.972***	0.682**
	(0.196)	(0.337)
Weapons offences	1.634***	1.990***
	(0.180)	(0.347)
Betting and gaming offences	0.042***	0.063**
	(0.010)	(0.027)
Theft	21.330***	18.817***
	(3.576)	(5.031)
Robbery	0.264***	0.122
	(0.043)	(0.103)
Arson	-0.072	0.158
	(0.123)	(0.238)
Pornography offences	0.010	0.010
	(0.008)	(0.025)
Domestic assault	1.344***	1.964*
	(0.476)	(1.043)
Coercive control	0.001	0.001
	(0.001)	(0.002)
Observations	546	546
Clusters (electorate)	80	80
First-stage F	–	16.1
Controls	Yes	Yes

Notes: Each cell reports a separate regression. Column (1) is OLS of the crime rate on pubs per 1,000 residents. Column (2) instruments pubs per 1,000 with the 1907 No Licence vote share. All specifications include median age and log population density as controls (estimates suppressed; full coefficient table in Appendix Table A2). Cluster-robust standard errors at the 1907 electorate level in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

much non-domestic assault per pub as they are for domestic assault, both in absolute terms and relative to base rates. We return to this contrast in Section 7.

Homicide, coercive control, arson and pornography offences show small or null effects under both estimators, consistent with their low base rates or with the absence of a plausible alcohol-behaviour mechanism.

For our headline outcome of non-domestic assault, the closeness of the OLS and IV estimates (5.51 versus 5.25) is the table's one anomaly relative to the negative-selection pattern. Non-domestic assault is plausibly the outcome where pub location and crime are most jointly determined: operators avoid the rowdiest precincts while patrons generate disorder where they drink, so positive and negative selection forces can offset. Either way, the IV result for non-domestic assault rests on essentially the same evidence as a careful OLS analysis would have produced, and our identification strategy serves more clearly to discipline the estimates for the other outcomes in the table where OLS understates the causal effect.

5.2 Performance Against Pre-Committed Priors

The headline table shows that for our main outcomes, the IV estimates are broadly consistent with OLS. The natural follow-up question is how the IV performs against the tier classification we pre-committed to in Section 4.6. Figure 3 plots OLS and IV coefficients for all sixteen outcomes, colour-coded by tier.

The pattern in the figure matches the priors closely. **Tier 1** outcomes are all positive and statistically significant. The IV estimate for non-domestic assault is 5.25, for liquor offences 4.52, and for homicide 0.011. Non-domestic assault and liquor offences are large in absolute terms and tightly estimated. Homicide is significant only marginally, but the point estimate is in the expected direction and homicide is rare enough that power is the binding constraint.

Tier 2 outcomes are positive almost without exception. Disorderly conduct, malicious damage, drug offences and weapons offences are all large and significant. Intimidation, sexual offences and betting and gaming offences are positive but smaller. No Tier 2 outcome

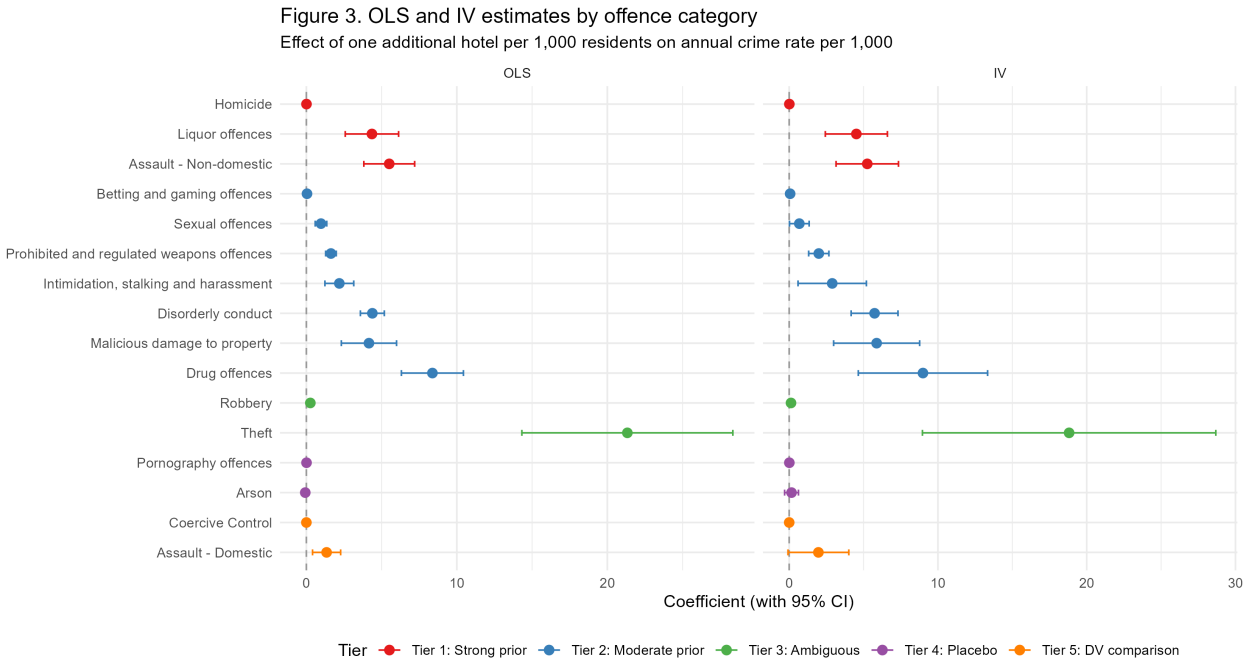

Figure 3: OLS and IV Estimates by Offence Category, with Tier Classification

Note: Effect of one additional pub per 1,000 residents on annual crime rate per 1,000. Bars are 95 per cent confidence intervals using electorate-clustered standard errors. IV uses 1907 No Licence share. Tier classifications were pre-committed before estimation.

is negative or insignificantly close to zero.

Tier 3 is the substantive surprise. Theft is large and significant with an IV estimate of 18.8 per 1,000. Robbery shows no effect. The contrast within Tier 3 itself is informative and we return to it in Section 7, where we argue that the theft response reflects an exposure mechanism rather than alcohol-induced behaviour.

Tier 4 placebos behave as a falsification test should. Both arson and pornography offences are statistically indistinguishable from zero, with point estimates close to zero. A failure of the exclusion restriction through generalised cultural or socioeconomic omitted variables would tend to produce significant placebo effects. We do not see them.

Tier 5 provides the paper's clearest within-design falsification test. Domestic assault and non-domestic assault have similar sample means of around 4.4 per 1,000. They share the underlying physical act and would respond similarly to any general violence-prone area omitted variable. Yet the IV estimate for non-domestic assault is more than two and a half times the IV estimate for domestic assault, and the OLS estimate for non-domestic assault is more than four times the OLS estimate for domestic assault. Coercive control, the other Tier 5 outcome, is null. The combined Tier 5 evidence points to a causal mechanism that operates through on-premises drinking and post-premises behaviour, not through neighbourhood-level characteristics that would affect violence inside and outside the home equally.

5.3 Multiple Testing Correction

The discussion so far has used naive cluster-robust p-values. Testing sixteen outcomes against an instrument inflates the false-discovery rate, so the naive p-values overstate statistical significance to some degree. As set out in Section 4, we apply the Romano and Wolf [2005] step-down procedure with $B = 1{,}000$ cluster bootstrap iterations to compute multiple-testing-adjusted p-values.

Table 6 compiles the IV estimates, the Romano-Wolf adjusted p-values, and effect sizes relative to sample means, grouped by tier. The pattern is striking. All Tier 1 outcomes

survive joint correction. All Tier 2 outcomes are positive and either significant or marginally significant. All Tier 4 placebos remain null. The DV comparison continues to show a sharp differential between non-domestic and domestic assault. The conclusions drawn from the figure are unchanged once we account for multiple testing.

Table 6: IV estimates with Romano-Wolf adjusted p-values, by tier

Tier	Outcome	Estimate	SE	% of mean	p (naive)	p (RW)
Tier 1: Strong prior						
	Assault - Non-domestic	5.249***	(1.070)	122%	<0.001	<0.001
	Homicide	0.011**	(0.004)	83%	0.011	0.049
	Liquor offences	4.516***	(1.065)	376%	<0.001	0.006
Tier 2: Moderate prior						
	Betting and gaming offences	0.063*	(0.027)	395%	0.020	0.072
	Disorderly conduct	5.739***	(0.803)	202%	<0.001	<0.001
	Drug offences	8.992***	(2.217)	155%	<0.001	0.006
	Intimidation, stalking and harassment	2.893*	(1.172)	55%	0.014	0.059
	Malicious damage to property	5.879***	(1.475)	77%	<0.001	0.006
	Prohibited and regulated weapons offences	1.990***	(0.347)	97%	<0.001	<0.001
	Sexual offences	0.682*	(0.337)	33%	0.044	0.076
Tier 3: Ambiguous						
	Robbery	0.122	(0.103)	48%	0.236	0.422
	Theft	18.817***	(5.031)	71%	<0.001	0.007
Tier 4: Placebo						
	Arson	0.158	(0.238)	20%	0.507	0.646
	Pornography offences	0.010	(0.025)	9%	0.677	0.646
Tier 5: DV comparison						
	Assault - Domestic	1.964*	(1.043)	45%	0.060	0.097
	Coercive Control	0.001	(0.002)	63%	0.346	0.526

Notes: Each row is a separate IV regression of the offence rate on pubs per 1,000 residents, instrumented by the 1907 No Licence vote share. Controls are median age and log population density. Standard errors are clustered at the 1907 electorate level. "% of mean" is the IV estimate divided by the sample mean of the outcome. Romano-Wolf step-down p-values use cluster bootstrap with $B = 1{,}000$. Stars on the estimate column reflect the RW-adjusted p-value: $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$. Tier classifications were pre-committed before estimation.

6 Robustness

6.1 Alternative Instrument Specifications

There are two reasonable encodings of the 1907 vote as an instrument. Our preferred specification uses the continuous No Licence vote share, which captures the intensity of temperance sentiment: voters had to actively support a complete ban for their preference to register. The alternative is a binary indicator for whether the SA2's dominant 1907 electorate voted Reduction, which captures the discrete binding outcome and ignores the intensive margin entirely. There is no strong theoretical reason to prefer one to another, and if the result is sensitive to this choice the underlying mechanism is less robust than we have claimed.

Table 7 re-estimates the second stage under each encoding across all sixteen outcomes. The first stage is strong in both: F-statistics of 16.1 and 17.7, both comfortably above the Stock-Yogo threshold of 10. The second-stage estimates are broadly consistent across the two instruments. Point estimates agree in sign for every outcome, and the headline magnitudes are very close: non-domestic assault is 5.25 under the continuous instrument and 4.66 under the binary, liquor offences 4.52 and 3.64, disorderly conduct 5.74 and 5.87, theft 18.8 and 18.2. The Binary Reduce estimates carry larger standard errors on average, reflecting the loss of information from the discretisation, and a small number of outcomes that are significant under the continuous instrument lose significance under the binary one. The placebos remain null under both, and the Tier 5 differential between non-domestic and domestic assault is preserved. The result is not an artefact of how we operationalise the historical record.[4]

Because both encodings of the 1907 vote produce strong first stages and similar second-stage estimates, we can also use them jointly as an overidentified specification. With two

[4]A third potential encoding, the continuous Reduce vote share, is not a valid instrument. The 1907 ballot rules allocated No Licence votes to Reduce when No Licence failed to clear the 60 per cent threshold, with the consequence that the highest pure Reduce shares appear in electorates with the weakest temperance sentiment. A high Reduce share therefore signals more pubs today, not fewer. The first stage on Reduce share is positive (coefficient 1.34) and weak (F = 4.0), confirming this institutional reading. We use the No Licence share and the binary Reduce indicator throughout the paper.

Table 7: Alternative instrument specifications across all outcomes

Tier	Outcome	(1) No Licence share	(2) Binary Reduce
Tier 1	Assault - Non-domestic	5.249***	4.656***
		(1.070)	(1.650)
	Homicide	0.011**	0.004
		(0.004)	(0.005)
	Liquor offences	4.516***	3.637***
		(1.065)	(0.833)
Tier 2	Betting and gaming offences	0.063**	0.105*
		(0.027)	(0.055)
	Disorderly conduct	5.739***	5.867***
		(0.803)	(1.370)
	Drug offences	8.992***	7.122***
		(2.217)	(2.083)
	Intimidation, stalking and harassment	2.893**	2.534
		(1.172)	(2.031)
	Malicious damage to property	5.879***	5.732*
		(1.475)	(3.048)
	Prohibited and regulated weapons offences	1.990***	2.087***
		(0.347)	(0.647)
	Sexual offences	0.682**	0.651
		(0.337)	(0.561)
Tier 3	Robbery	0.122	0.198**
		(0.103)	(0.093)
	Theft	18.817***	18.196*
		(5.031)	(9.392)
Tier 4	Arson	0.158	-0.030
		(0.238)	(0.487)
	Pornography offences	0.010	0.021
		(0.025)	(0.036)
Tier 5	Assault - Domestic	1.964*	1.825
		(1.043)	(1.733)
	Coercive Control	0.001	0.005**
		(0.002)	(0.002)
First-stage F (cluster-robust)		16.1	17.7
Observations		546	546

Notes: Each cell reports a separate IV estimate of the effect of pubs per 1,000 residents on the crime rate (per 1,000 residents) for the offence in the row, using the instrument in the column. Column (1) uses the continuous 1907 No Licence vote share (our preferred specification). Column (2) uses a binary indicator for whether the SA2's dominant 1907 electorate voted Reduction. All specifications include median age and log population density as controls. Cluster-robust standard errors at the 1907 electorate level in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

instruments and one endogenous regressor, the Sargan test of overidentifying restrictions provides a formal test of joint validity. Estimating the headline non-domestic assault IV with both the No Licence share and the binary Reduce indicator as instruments, the Sargan statistic is 0.31 with a p-value of 0.58. The test fails to reject the null of joint validity at any conventional significance level, consistent with both instruments isolating the same underlying institutional variation.

6.2 Subsample Analysis

A robust causal estimate should not turn on idiosyncratic features of a single sample. We split the 546 SA2s by population density: 257 metropolitan SA2s above 1,000 per km^2 and 289 country SA2s at or below that threshold. The first-stage F-statistic is 16.6 in the country subsample and 5.0 in the metropolitan subsample. The metropolitan F is below the Stock-Yogo threshold of 10, reflecting reduced within-sample variation in the instrument when the sample is restricted to high-density SA2s. The asymmetry is important to keep in mind throughout this subsection: country estimates are identified at conventional strength, while metropolitan estimates should be read as informative about the qualitative pattern rather than as precise quantities.

Table 8 reports IV estimates across all sixteen outcomes for each subsample alongside the full sample for reference.

Tier 1 holds in both subsamples but each outcome carries a different geographic signature. Non-domestic assault is significant and similar in magnitude in both, with point estimates of 7.20 in metro and 5.93 in country against the full-sample 5.25. Liquor offences are concentrated in metropolitan SA2s, where the estimate is 7.10 against 1.48 in country areas, consistent with the urban concentration of late-night drinking precincts. Homicide goes the other way, with a metro point estimate close to zero and a country estimate of 0.025 significant at the 1 per cent level. The small-numbers nature of homicide makes us hesitant to read too much into the geographic split, but the direction is preserved.

Table 8: Subsample robustness across all outcomes

Tier	Outcome	(1) Full	(2) Metro	(3) Country
Tier 1	Assault - Non-domestic	5.249***	7.202***	5.928**
		(1.070)	(0.859)	(2.420)
	Homicide	0.011**	0.003	0.025***
		(0.004)	(0.004)	(0.009)
	Liquor offences	4.516***	7.095***	1.477*
		(1.065)	(1.130)	(0.843)
Tier 2	Betting and gaming offences	0.063**	0.065*	0.051
		(0.027)	(0.036)	(0.048)
	Disorderly conduct	5.739***	5.008***	7.464***
		(0.803)	(0.929)	(2.309)
	Drug offences	8.992***	15.972***	3.299
		(2.217)	(3.564)	(3.906)
	Intimidation, stalking and harassment	2.893**	0.931	8.135***
		(1.172)	(1.161)	(3.101)
	Malicious damage to property	5.879***	4.090**	13.021***
		(1.475)	(1.696)	(3.888)
	Prohibited and regulated weapons offences	1.990***	1.546**	3.282***
		(0.347)	(0.614)	(0.767)
	Sexual offences	0.682**	1.143***	0.999
		(0.337)	(0.382)	(0.778)
Tier 3	Robbery	0.122	0.171	0.202*
		(0.103)	(0.147)	(0.121)
	Theft	18.817***	24.302***	25.511**
		(5.031)	(6.643)	(11.457)
Tier 4	Arson	0.158	-0.062	0.326
		(0.238)	(0.196)	(0.612)
	Pornography offences	0.010	0.013	0.057
		(0.025)	(0.024)	(0.062)
Tier 5	Assault - Domestic	1.964*	-0.009	6.484**
		(1.043)	(0.960)	(2.670)
	Coercive Control	0.001	-0.001	0.004
		(0.002)	(0.001)	(0.004)
First-stage F (cluster-robust)		16.1	5.0	16.6
Observations		546	257	289

Notes: Each cell reports a separate IV estimate of the effect of pubs per 1,000 residents on the crime rate (per 1,000 residents) for the offence in the row, estimated on the subsample in the column. Column (1) is the full sample of 546 SA2s. Column (2) restricts to metropolitan SA2s, defined as population density above 1,000 per km^2. Column (3) restricts to country SA2s, the complement. All specifications instrument pubs per 1,000 with the 1907 No Licence vote share and include median age and log population density as controls. Cluster-robust standard errors at the 1907 electorate level in parentheses. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

Tier 2 splits along urban and rural lines without overturning any of the headline conclusions. Drug offences are concentrated in metropolitan SA2s at 16.0 against an insignificant 3.30 in country areas. Several other Tier 2 outcomes are larger in country SA2s. Malicious damage is 13.0 against 4.09, intimidation is 8.14 against an insignificant 0.93, and weapons offences are 3.28 against 1.55. These are outcomes for which rural pubs may play an outsized role as the principal site of public social mixing, with fewer alternative venues than in urban areas. Disorderly conduct is large in both subsamples.

The Tier 3 theft response carries through in both subsamples at similar magnitudes of 24.3 in metro and 25.5 in country. The Tier 4 placebos remain null in both: arson and pornography offences are indistinguishable from zero everywhere.

The Tier 5 contrast is the most informative single result. The marginal full-sample effect on domestic assault of 1.96 is essentially zero in metropolitan SA2s and a sizeable 6.48 in country SA2s, significant at the 5 per cent level. The qualitative reading is consistent with the broader paper: where pubs are the dominant social setting and alcohol consumption is concentrated within them, spillover from public drinking to private violence may be larger than in cities where pub-going is one option among many. We are cautious about reading too much into the absolute magnitude given that this estimate is identified from rural variation in pub density. Coercive control remains null in both subsamples.

Overall, the subsample analysis confirms the broad pattern of the headline results. The Tier 1 effects survive, the Tier 4 placebos remain null, and the Tier 5 differential is preserved on the margin. Variation in magnitudes across subsamples maps onto plausible substantive readings of how pubs operate in different settlement patterns rather than to a single-channel mechanism.

6.3 Panel IV: Exploiting Year-to-Year Variation

The cross-sectional design that produces our headline estimates is deliberate. The identifying variation in pub density is long-run and stable, induced by a vote that took place in 1907

and locked in by subsequent regulation. Averaging crime rates over 2015 to 2024 smooths year-to-year noise from policing intensity, reporting practices and short-run shocks, leaving the cross-sectional signal that the instrument actually moves. Running a panel with year fixed effects on the same data carries an extra cost. SA2-level fixed effects would absorb the instrument entirely because the 1907 vote share is fixed for each SA2. Without SA2 fixed effects the panel adds little beyond efficiency from extra observations, and with them it cannot be estimated.

The panel design is not, however, useless. Its strength is in absorbing time-varying unobservables that affect all SA2s equally, such as state-wide policing reforms, alcohol consumption trends and broad reporting changes. Estimating IV on an SA2-year panel with year fixed effects is therefore a meaningful check on whether the cross-sectional estimates are picking up something that varies systematically over time and that the cross-section happens to align with.

Table 9 reports cross-sectional and panel IV estimates side by side for all sixteen outcomes. The qualitative pattern is the same. Every outcome that is significant in the cross-section is significant in the panel, every placebo remains null in both designs, and the Tier 5 differential between domestic and non-domestic assault is preserved. For non-domestic assault the panel estimate is 7.32 against a cross-sectional 5.25; for liquor offences 5.47 against 4.52; for homicide 0.021 against 0.011.

Quantitatively, the panel estimates are systematically larger than the cross-sectional estimates for most outcomes. The most striking case is theft, where the panel estimate of 38.8 is more than twice the cross-sectional 18.8. Robbery moves from an insignificant cross-sectional 0.12 to a panel estimate of 1.19 significant at the 1 per cent level. These shifts deserve caution rather than reassurance. Two features of the panel design account for them. First, without SA2 fixed effects the panel pools cross-sectional and within-SA2 variation in a way that, mechanically, places more weight on years and SA2s where the relationship between the instrument and crime is tightest. Second, we cluster panel standard errors at the

Table 9: Cross-sectional and panel IV estimates by outcome

Tier	Outcome	Cross-section (1)	Panel (year FE) (2)
Tier 1	Assault - Non-domestic	5.249***	7.324***
		(1.070)	(1.232)
	Liquor offences	4.516***	5.471***
		(1.065)	(1.125)
	Homicide	0.011**	0.021***
		(0.004)	(0.004)
Tier 2	Malicious damage to property	5.879***	9.182***
		(1.475)	(1.999)
	Drug offences	8.992***	6.684***
		(2.217)	(2.103)
	Disorderly conduct	5.739***	6.552***
		(0.803)	(0.991)
	Intimidation, stalking and harassment	2.893**	2.109***
		(1.172)	(0.744)
	Prohibited and regulated weapons offences	1.990***	1.738***
		(0.347)	(0.257)
	Sexual offences	0.682**	0.839***
		(0.337)	(0.215)
	Betting and gaming offences	0.063**	0.080***
		(0.027)	(0.028)
Tier 3	Theft	18.817***	38.758***
		(5.031)	(6.903)
	Robbery	0.122	1.193***
		(0.103)	(0.343)
Tier 4	Arson	0.158	-0.178
		(0.238)	(0.204)
	Pornography offences	0.010	0.011
		(0.025)	(0.011)
Tier 5	Assault - Domestic	1.964*	1.788*
		(1.043)	(0.917)
	Coercive Control	0.001	0.000
		(0.002)	(0.001)

Notes: Column (1) reports the cross-sectional IV estimate using crime rates averaged over 2015-2024. Column (2) reports the panel IV estimate using annual crime rates from 1995-2024 with year fixed effects. SA2 fixed effects are not feasible because the 1907 vote share is fixed within each SA2 and would absorb the instrument. Both specifications include median age and log population density as controls. Cluster-robust standard errors at the 1907 electorate level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

1907 electorate level following the cross-sectional specification; given likely serial correlation in crime rates over a thirty-year window, two-way clustering on SA2 and year would be a more cautious choice and would widen the panel CIs. Our reading is that the panel confirms the qualitative cross-sectional pattern but is less useful for pinning down magnitudes; we report it because the agreement on sign and significance is reassuring, while noting that the absolute panel estimates should not be treated as superseding the cross-sectional headline numbers.

6.4 Lee et al. (2022) Effective F

Our first-stage F of 16.1 comfortably exceeds the Stock and Yogo [2005] 10 per cent bias threshold but falls well short of the Lee et al. [2022] threshold of 104.7 for honest 5 per cent inference. Lee et al. argue that with first stages of moderate strength, standard t-statistics are anti-conservative and confidence intervals must be widened to recover correct coverage. Their tF procedure does this by applying critical values that depend on the realised first-stage F. At F = 16.1 the tF critical value is approximately 2.79, widening the standard 95 per cent CI by a factor of about 1.42 relative to the 1.96 critical value used in conventional inference, without changing the point estimate. Applying this correction to all sixteen outcomes provides a stress test of the headline pattern against the most demanding weak-instrument standard currently in the literature.

Table 10 reports the naive and tF-adjusted 95 per cent confidence intervals for each outcome.

Tier 1 splits. Non-domestic assault survives comfortably: the naive 95 per cent CI of $[3.15, 7.35]$ widens to $[2.26, 8.24]$ but remains well away from zero. Liquor offences also survive, with the tF-adjusted CI of $[1.54, 7.49]$ staying clear of zero. Homicide does not survive: the tF-adjusted CI of $[-0.001, 0.023]$ just barely crosses zero. Homicide was always going to be the marginal Tier 1 case given its low base rate, and the tF correction tips it from a 1.1 per cent naive significance to a null.

Table 10: IV estimates with tF-adjusted 95% confidence intervals

			95% confidence interval	
Tier	Outcome	Estimate	Naive	tF-adjusted
Tier 1	Assault - Non-domestic	5.249†	[3.151, 7.347]	[2.258, 8.240]
	Liquor offences	4.516†	[2.429, 6.603]	[1.541, 7.491]
	Homicide	0.011	[0.003, 0.019]	[-0.001, 0.023]
Tier 2	Drug offences	8.992†	[4.647, 13.338]	[2.796, 15.188]
	Malicious damage to property	5.879†	[2.988, 8.770]	[1.757, 10.000]
	Disorderly conduct	5.739†	[4.165, 7.314]	[3.494, 7.984]
	Intimidation, stalking and harassment	2.893	[0.596, 5.191]	[-0.382, 6.169]
	Prohibited and regulated weapons offences	1.990†	[1.310, 2.670]	[1.021, 2.959]
	Sexual offences	0.682	[0.021, 1.343]	[-0.260, 1.624]
	Betting and gaming offences	0.063	[0.010, 0.116]	[-0.012, 0.139]
Tier 3	Theft	18.817†	[8.957, 28.677]	[4.760, 32.875]
	Robbery	0.122	[-0.080, 0.325]	[-0.166, 0.411]
Tier 4	Arson	0.158	[-0.308, 0.624]	[-0.506, 0.822]
	Pornography offences	0.010	[-0.039, 0.060]	[-0.060, 0.081]
Tier 5	Assault - Domestic	1.964	[-0.080, 4.008]	[-0.950, 4.878]
	Coercive Control	0.001	[-0.002, 0.005]	[-0.003, 0.006]
First-stage F (cluster-robust)			16.1	
Lee et al. threshold for 5% inference			104.7	
Outcomes surviving tF at 5%			7 of 16	
Observations			546	

Notes: Each row reports an IV estimate from Table 5 with two 95% confidence intervals. The naive CI uses standard ±1.96 critical values. The tF-adjusted CI applies the Lee et al. [2022] procedure, which widens the critical value as a function of the realised first-stage F to recover correct 5% coverage under weak instruments. At our F of 16.1 the tF widening factor is approximately 1.42. † indicates that the tF-adjusted 95% CI excludes zero. All specifications instrument pubs per 1,000 with the 1907 No Licence vote share and include median age and log population density as controls. Cluster-robust standard errors at the 1907 electorate level.

Tier 2 holds for four of seven outcomes. Drug offences, malicious damage, disorderly conduct and weapons offences all survive correction with tF-adjusted CIs comfortably away from zero. Intimidation, sexual offences and betting and gaming offences do not. These were the outcomes with naive p-values between 0.01 and 0.05, and the tF widening is enough to flip them. The same three outcomes lose significance under Romano-Wolf correction, so the tF result here reinforces a signal that multiple-testing correction was already sending.

In Tier 3, theft survives despite the widest absolute CI in the table, with the tF-adjusted range of $[4.76, 32.88]$ staying well above zero. Robbery is null in both naive and tF inference. The Tier 4 placebos remain null in both. Tier 5 domestic assault was the marginal result in the headline table at $p = 0.060$ under naive inference, and the tF-adjusted CI of $[-0.95, 4.88]$ now comfortably includes zero. Coercive control was null in the cross-section and remains null.

Of the sixteen outcomes, seven survive tF inference at the 5 per cent level: non-domestic assault, liquor offences, disorderly conduct, drug offences, malicious damage, weapons offences and theft. These are the same outcomes that survive Romano-Wolf step-down correction at the 5 per cent level, except that homicide is marginal under RW at $p = 0.049$ and just fails under tF. The agreement between two completely different statistical corrections, one targeting multiple testing and the other targeting weak-instrument bias, is reassuring evidence that the headline results are not artefacts of either.

7 Discussion

7.1 Mechanism and the Theft Puzzle

The differential pattern across crime types points strongly to on-premises drinking and post-pub behaviour as the channel through which pubs affect crime. The two clearest pieces of evidence are the placebo results, where alcohol-implausible crimes such as arson and pornography show precise nulls in every specification, and the within-violence differential

between non-domestic and domestic assault. A generalised neighbourhoods-with-pubs-are-worse channel would not produce these patterns.

The subsample analysis refines the mechanism in a useful direction. The full-sample marginal effect on domestic assault of 1.96 decomposes into essentially nothing in metropolitan SA2s and a sizeable 6.48 in country SA2s. In dense urban areas with many venues and short paths home, post-pub aggression plays out in public space, on footpaths, at food stops, around transit, in queues for taxis. The drinker's destination from the pub is more public space. In country areas the pub is often the only on-licence in the township, drinking is concentrated rather than dispersed, and the drinker's destination is home. The same alcohol-behaviour mechanism may then register as different offence classifications: non-domestic assault in cities, domestic assault in country. The Tier 2 pattern in the subsample table fits this reading. Drug offences are concentrated in metropolitan SA2s and consistent with the urban geography of late-night drinking precincts. Malicious damage, intimidation and weapons offences are larger in country SA2s, where the pub may play an outsized role as the principal site of public social mixing with few alternative venues.

Theft is the apparent exception within the cross-sectional results. The IV estimate of 18.8 per 1,000 is large and robust across both subsamples, at 24.3 in metro and 25.5 in country. The theft response is difficult to reconcile with an alcohol-behaviour channel alone. Pubs do not obviously alter the disposition of patrons to commit theft, and most patrons are not committing theft themselves. The more plausible mechanism is exposure. Pubs concentrate foot traffic, parked vehicles, unattended phones and wallets, and people in temporarily-reduced-vigilance states. Stealing from a drunk patron walking home from a pub is easier than stealing from a sober person on a residential street. The theft response is therefore consistent with our overall interpretation that pubs increase crime in their vicinity, but through channels that are operationally specific to the pub environment rather than diffuse.

We cannot distinguish exposure from alcohol-behaviour mechanisms within our identification strategy. Future work using more granular data on the timing and location of

theft incidents, perhaps from BOCSAR's incident-level data, could potentially separate these channels.

A second caveat applies specifically to the Tier 2 betting and gaming result. NSW pubs are a major venue for electronic gaming machines, with most hotel licences holding gaming authorisations. Our IV identifies the causal effect of pub density, not of on-premises drinking in isolation; for betting and gaming offences and arguably also for some property offences in pub vicinity, gambling availability is a parallel channel running through the same fitted variable. We are content to label this as "the effect of pubs" since pubs and pub-pokies are jointly determined in NSW, but readers interested specifically in the alcohol channel should treat the gambling-adjacent estimates with that caveat.

7.2 Comparison with the Literature

The closest economics paper to ours is Luca et al. [2015] on Indian alcohol prohibition. Their identification is at the policy level and identifies aggregate effects of large changes in alcohol availability. We identify the marginal effect of a single outlet, which is the policy-relevant quantity for licensing decisions facing regulators in most developed countries, where blanket prohibition is not on the table. Our estimates are broadly consistent with theirs in the sense that alcohol availability raises violence but not all categories of crime, and we extend the finding by showing the differential is sharp enough to identify the operative channel.

Papers using public transport closures [Phillips and Sandler, 2015, Jackson and Owens, 2011] identify changes in nighttime accessibility to drinking areas rather than the supply of pubs. Their estimates are not directly comparable to ours but the qualitative direction is consistent. Our design holds accessibility fixed and varies the supply of outlets, which speaks to the licence-cap policy lever those papers cannot directly address.

The literature on outlet density and domestic violence has produced mixed results [Kearns et al., 2015]. Our cross-sectional finding of a small and statistically insignificant marginal effect of pub density on domestic assault, together with the subsample finding that this

average masks a near-zero metropolitan effect and a sizeable rural one, may help reconcile some of that mixed evidence. Studies based on metropolitan samples should expect to find little, while studies that include rural areas or that are dominated by them may pick up substantial effects. Off-premises liquor availability, through bottle shops and packaged liquor, may still play an independent role for domestic violence; our data identify pubs only and cannot address that question.

8 Conclusion

This paper has provided causal estimates of how pub density affects crime in New South Wales, using a 1907 Local Option referendum as an instrument for modern hotel density across 546 SA2s. The headline finding is a clean differential structure. Pubs cause non-domestic assault, public disorder, liquor offences, drug offences, malicious damage and weapons offences. They cause increased theft through exposure channels rather than alcohol-induced behaviour. They do not cause sexual offences, arson or pornography offences, and they do not generally cause homicide outside rural areas. Domestic violence is a more nuanced case: the marginal effect is essentially zero in metropolitan SA2s but sizeable in country areas, where the pub is often the dominant site of public alcohol consumption. The differential pattern across crime types is hard to reconcile with omitted-variable bias and is consistent with on-premises drinking and post-pub behaviour as the operative channel.

The standard policy response to alcohol-related harm in Australia is to impose blanket caps on new licences and operating hours. Our results suggest this is a blunt instrument. Pubs cause public-space violence, disorder and liquor offences. They do not cause most other crimes for which they are sometimes blamed. They cause increased theft through exposure channels that licence caps may not affect.

A more effective policy response would target the specific harms causally linked to pubs. Late-trading restrictions, lock-out laws and density limits in identified violence hotspots are

likely to reduce non-domestic assault. The 2014 Sydney lockout laws produced reductions in non-domestic assault in the affected areas that are broadly consistent with our IV estimates. Targeting blanket reductions across NSW would be substantially less efficient.

Our results refine, rather than reject, the role of pub regulation in domestic violence policy. In metropolitan areas, restricting pub density appears to have little marginal effect on domestic assault, and DV reduction strategies in cities should focus on other levers including off-premises alcohol availability, perpetrator interventions, family supports and crisis services. In country areas, where the pub is often the dominant site of public alcohol consumption, restrictions on pub density may meaningfully reduce domestic assault as well as public assault. This points towards a regionally differentiated approach rather than a uniform state-wide policy. A blanket licence cap is too blunt; a pure metropolitan focus on lockouts and density would miss a substantial rural channel.

Our estimates are local average treatment effects on SA2s whose modern pub density was influenced by the 1907 vote. The LATE may differ from the average treatment effect across NSW. In particular, the SA2s for which our instrument has bite are those where the 1907 vote was decisive and the historical lock-in mechanisms operated as expected. The estimates may not generalise to areas where pub density is determined primarily by post-1907 forces such as planning decisions or new urban developments. The policy implications we draw, in particular the case for regionally differentiated regulation, rest on the assumption that the compliant subpopulation is informative about the broader response. We view this as reasonable since the historical lock-in operated state-wide, but readers should treat extrapolation to non-compliant areas, including newer growth corridors and recently rezoned precincts, with appropriate caution.

We identify the effect of pubs, not all alcohol outlets. Clubs, restaurants with liquor licences, bottle shops and packaged liquor outlets are excluded. Pubs are the natural object of the 1907 vote and the most policy-salient category for on-premises drinking, but the total contribution of all alcohol outlets to NSW crime is likely larger than what we estimate.

The instrument is moderately strong by conventional standards but well short of the Lee et al. [2022] threshold for 5 per cent honest inference. We address this directly by reporting tF-adjusted confidence intervals for all sixteen outcomes alongside Romano-Wolf step-down corrected p-values. The two corrections, one targeting weak-instrument bias and the other targeting multiple testing, are conceptually independent but agree on the same set of seven survivors with the single exception of homicide, which is marginal under RW and just fails under tF. The robustness of the headline pattern under both procedures is reassuring, but readers should treat outcomes that pass only one of the two corrections as suggestive rather than definitive.

The instrument is specific to NSW. Other Australian states held their own Local Option polls and may produce useful comparative estimates. The methodological approach generalises to any jurisdiction that experienced a sufficiently durable historical shock to alcohol-outlet density.

Finally, our estimates capture average effects across a long time period. Effects may vary across the business cycle, between weekends and weekdays, and at different times of night. Disaggregating the response by these dimensions would refine the policy implications.

Pub density has a measurable causal effect on a specific and identifiable subset of crime in New South Wales, and very little effect on the rest. A century-old referendum that none of the affected parties can remember provides the variation that lets us see the pattern. The empirical content of that pattern is that pubs are not a generalised crime nuisance. They are a specific cause of a specific set of harms, and they should be regulated as such.

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A Full Coefficient Tables

The main results in Table 5 suppress the coefficients on the control variables to keep the table readable. This appendix reports the same regressions with the full coefficient detail visible. Table A1 reports OLS estimates for all sixteen outcomes; Table A2 reports the corresponding 2SLS estimates. In both tables the columns are outcomes and the rows are regressors. Coefficients on the controls are not the parameter of interest but are reported so readers can confirm that median age enters negatively and log population density enters with the expected sign across most specifications.

Table A1: OLS estimates with full coefficients (appendix)

	Dependent variable:															
	Non-domestic assault	Liquor offences	Homicide	Disorderly conduct	Malicious damage	Drug offences	Intimidation, stalking, harassment	Sexual offences	Weapons offences	Betting and gaming offences	Theft	Robbery	Arson	Pornography offences	Domestic assault	Coercive control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
hotels_per_1k	5.507***	4.357***	0.007***	4.385***	4.159***	8.376***	2.191***	0.972***	1.634***	0.042***	21.330***	0.264***	−0.072	0.010	1.344***	0.001
	(0.862)	(0.904)	(0.002)	(0.405)	(0.936)	(1.052)	(0.489)	(0.196)	(0.180)	(0.010)	(3.576)	(0.043)	(0.123)	(0.008)	(0.476)	(0.001)
median_age	−0.099**	0.014	−0.0002	−0.080**	−0.211***	−0.104**	−0.170***	−0.037***	−0.072***	−0.001*	−0.729***	−0.014***	−0.055***	−0.003***	−0.192***	0.00000
	(0.044)	(0.013)	(0.00002)	(0.040)	(0.081)	(0.049)	(0.055)	(0.013)	(0.015)	(0.001)	(0.236)	(0.004)	(0.016)	(0.001)	(0.054)	(0.0001)
log_pop_density	0.138	0.374***	−0.001*	−0.206	−0.730**	0.512***	−0.668***	−0.205***	−0.317***	−0.003	0.044	0.029***	−0.319***	−0.017***	−0.630***	−0.0003
	(0.162)	(0.112)	(0.0004)	(0.131)	(0.310)	(0.187)	(0.196)	(0.051)	(0.061)	(0.002)	(0.929)	(0.010)	(0.053)	(0.003)	(0.174)	(0.0002)
Constant	5.574**	−3.017***	0.023***	5.769**	18.875***	4.123	15.179***	4.407***	6.216***	0.067*	47.906***	0.569***	4.852***	0.315***	15.270***	0.004
	(2.506)	(1.248)	(0.008)	(2.314)	(5.008)	(2.624)	(3.191)	(0.765)	(0.915)	(0.038)	(14.384)	(0.198)	(0.971)	(0.049)	(3.051)	(0.005)
SE clustering	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate
Observations	546	546	546	546	546	546	546	546	546	546	546	546	546	546	546	546
R^2	0.266	0.538	0.069	0.329	0.155	0.295	0.171	0.240	0.368	0.099	0.168	0.240	0.270	0.167	0.158	0.040
Adjusted R^2	0.262	0.535	0.064	0.325	0.151	0.291	0.166	0.236	0.365	0.094	0.163	0.236	0.266	0.162	0.153	0.035

Note: Standard errors clustered at 1907 electorate level.

Table A2: IV estimates with full coefficients (appendix)

	Dependent variable:															
	Non-domestic assault	Liquor offences	Homicide	Disorderly conduct	Malicious damage	Drug offences	Intimidation, stalking, harassment	Sexual offences	Weapons offences	Betting and gaming offences	Theft	Robbery	Arson	Pornography offences	Domestic assault	Coercive control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
median_age	−0.102**	0.016	−0.0002	−0.064*	−0.191**	−0.097*	−0.162***	−0.040***	−0.068***	−0.001	−0.759***	−0.016***	−0.052***	−0.003***	−0.185***	0.00001
	(0.045)	(0.016)	(0.0002)	(0.039)	(0.080)	(0.050)	(0.058)	(0.013)	(0.015)	(0.001)	(0.229)	(0.004)	(0.016)	(0.001)	(0.056)	(0.0001)
log_pop_density	0.112	0.291***	−0.0003	−0.067	−0.552*	0.576**	−0.596***	−0.232***	−0.281***	−0.001	−0.215	0.014	−0.295***	−0.016***	−0.566***	−0.0003
	(0.169)	(0.129)	(0.001)	(0.134)	(0.300)	(0.280)	(0.209)	(0.053)	(0.063)	(0.004)	(0.922)	(0.014)	(0.049)	(0.004)	(0.184)	(0.0002)
`hotels_per_1k(fit)`	5.249***	4.516***	0.011**	5.739***	5.879***	8.992***	2.893**	0.682**	1.990***	0.063**	18.817***	0.122	0.158	0.010	1.964*	0.001
	(1.070)	(1.065)	(0.004)	(0.803)	(1.475)	(2.217)	(1.172)	(0.337)	(0.347)	(0.027)	(5.031)	(0.103)	(0.238)	(0.025)	(1.043)	(0.002)
Constant	5.938**	−3.242**	0.018*	3.863*	16.454***	3.256	14.190***	4.815***	5.714***	0.037	51.442***	0.768***	4.528***	0.314***	14.398***	0.003
	(2.672)	(1.568)	(0.010)	(2.272)	(4.830)	(3.512)	(3.549)	(0.783)	(0.955)	(0.049)	(13.904)	(0.255)	(0.891)	(0.059)	(3.283)	(0.004)
First-stage F	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1	16.1
SE clustering	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate
Observations	546	546	546	546	546	546	546	546	546	546	546	546	546	546	546	546
R^2	0.266	0.537	0.062	0.311	0.148	0.294	0.168	0.235	0.364	0.086	0.166	0.206	0.266	0.167	0.155	0.040
Adjusted R^2	0.262	0.535	0.056	0.307	0.143	0.290	0.163	0.231	0.360	0.081	0.161	0.201	0.262	0.162	0.150	0.035

Note: Instrument: 1907 No Licence vote share. SEs clustered at electorate level.

B Reduced-Form Estimates

The reduced form regresses each crime rate directly on the 1907 No Licence vote share, with the parsimonious controls. The reduced-form coefficient is the product of the first-stage coefficient and the IV coefficient, and serves as a useful diagnostic: it isolates the relationship between the instrument and the outcome without imposing the structural model. Table B3 reports reduced-form estimates for a selection of outcomes spanning the tier classification. Sign and significance line up with the IV estimates in Table 5: outcomes that respond causally to pub density show a negative reduced-form coefficient on the No Licence share, reflecting the fact that higher No Licence sentiment in 1907 translates into fewer pubs today and consequently lower crime rates. The placebo outcomes show small and statistically insignificant reduced-form coefficients.

Table B3: Reduced-Form Estimates: Effect of 1907 'No Licence' Share on Crime Rates

	Dependent variable:							
	Non-domestic assault	Domestic assault	Liquor offences	Disorderly conduct	Malicious damage	Drug offences	Theft	Coercive control
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
share_no_licence	−9.640***	−3.607**	−8.294**	−10.540***	−10.796***	−16.514**	−34.558**	−0.003
	(3.435)	(1.812)	(3.522)	(2.794)	(2.650)	(6.755)	(13.608)	(0.003)
median_age	−0.129***	−0.195***	−0.007	−0.094**	−0.221***	−0.143***	−0.855***	−0.00000
	(0.047)	(0.056)	(0.020)	(0.044)	(0.084)	(0.052)	(0.246)	(0.0001)
log_pop_density	−0.321**	−0.728***	0.018	−0.540***	−1.037***	−0.166	−1.767**	−0.0004**
	(0.148)	(0.175)	(0.076)	(0.138)	(0.287)	(0.187)	(0.846)	(0.0002)
Constant	15.416***	17.945***	4.914***	14.227***	27.070***	19.494***	85.422***	0.006
	(2.994)	(3.339)	(1.497)	(3.026)	(5.319)	(3.633)	(16.014)	(0.005)
SE clustering	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate	Electorate
Observations	546	546	546	546	546	546	546	546
R^2	0.078	0.149	0.092	0.188	0.124	0.073	0.065	0.035
Adjusted R^2	0.073	0.144	0.087	0.183	0.120	0.068	0.059	0.029

Note: Standard errors clustered at 1907 electorate level.

C Robustness to Full Controls

Section 4 argued for a parsimonious specification controlling only for median age and log population density, on the grounds that adding income and SEIFA risks post-treatment bias since both are plausibly affected by long-run pub density itself. This appendix demonstrates that the headline pattern is essentially unchanged when the full control set is used instead. Table C4 reports OLS and IV estimates for all sixteen outcomes with median age, log population density, log median income and SEIFA IRSD decile as controls. The sample drops

slightly because income and SEIFA are not observed for every SA2.

The first-stage F-statistic falls from 16.1 under parsimonious controls to 12.5 under the full set, indicating that some of the first-stage variation operates through socioeconomic channels. The bulk of the variation remains, however, and the headline IV coefficients are quantitatively close to the parsimonious estimates. Non-domestic assault, liquor offences, disorderly conduct, malicious damage, drug offences, weapons offences and theft remain significant under both specifications. The Tier 4 placebos remain null. The Tier 5 differential between non-domestic and domestic assault is preserved. We conclude that the parsimonious specification's case for avoiding post-treatment bias does not come at the cost of misrepresenting the empirical pattern.

Table C4: OLS and IV estimates by outcome, full controls

	OLS (1)	IV (2SLS) (2)
Non-domestic assault	5.513***	6.354***
	(0.695)	(0.825)
Liquor offences	4.376***	4.788***
	(0.845)	(1.009)
Homicide	0.007***	0.015***
	(0.002)	(0.005)
Disorderly conduct	4.414***	6.042***
	(0.400)	(0.797)
Malicious damage	4.177***	6.503***
	(1.008)	(1.601)
Drug offences	8.355***	10.786***
	(0.984)	(1.860)
Intimidation, stalking, harassment	2.184***	3.816***
	(0.556)	(1.228)
Sexual offences	0.977***	0.967***
	(0.169)	(0.278)
Weapons offences	1.632***	2.321***
	(0.192)	(0.388)
Betting and gaming offences	0.043***	0.065**
	(0.011)	(0.029)
Theft	21.462***	22.683***
	(3.137)	(4.197)
Robbery	0.264***	0.238***
	(0.035)	(0.078)
Arson	-0.065	0.157
	(0.126)	(0.210)
Pornography offences	0.009	0.021
	(0.009)	(0.023)
Domestic assault	1.333**	2.756**
	(0.541)	(1.081)
Coercive control	0.001	0.002
	(0.001)	(0.002)
Observations	529	529
Clusters (electorate)	80	80
First-stage F	–	12.5
Controls	Full	Full

Notes: Each cell reports a separate regression. Column (1) is OLS of the crime rate on pubs per 1,000 residents. Column (2) instruments pubs per 1,000 with the 1907 No Licence vote share. Both specifications include the full control set: median age, log population density, log median income and SEIFA IRSD decile. Sample size drops relative to Table 5 because log income and SEIFA are not observed for every SA2. Cluster-robust standard errors at the 1907 electorate level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.