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The impact of restricted alcohol availability on alcohol-related violence in Newcastle, NSW

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In March 2008, the New South Wales Liquor Administration Board (LAB) introduced significant restrictions on hotel trading hours for a number of licensed premises in the Newcastle CBD. We sought to determine whether this reduced the incidence of assault in the vicinity of these premises. Three sources of police data were employed to address this research question: recorded crime data, last-place-of-consumption data from the Alcohol Linking Program and police call-out data. Recorded crime and Linking data revealed a significant reduction in alcohol-related assaults in the intervention site but not the comparison site. These two data sources revealed no evidence of any geographic displacement of assaults to other licensed premises or neighbouring areas. There was no evidence of any decrease in the total number of calls for service in either the intervention or comparison sites but this is most likely due to limitations inherent in the call-out data. All three data sources revealed a significant decrease in the proportion of assaults occurring after 3am in the intervention site but not in the comparison sites. Collectively, the data provide strong evidence that the restricted availability of alcohol reduced the incidence of assault in the Newcastle CBD.

KEYWORDS: alcohol, availability hypothesis, assault, violence, interrupted time series analysis.

BACKGROUND

In July 2007 the NSW Police Force lodged a complaint with the NSW Liquor Administration Board (LAB)³ against four Newcastle licensed premises on the grounds that they were causing "undue disturbance of the quiet and good order of the neighbourhood". This complaint was made against a backdrop of considerable community dissatisfaction with high levels of alcohol-related violence in and around the Newcastle CBD (ABC 2008).

Following individual conferences with each of the four licensed premises, the LAB adjourned the matter and re-convened a joint conference in November 2007. During this joint conference, the Director of the Office of Liquor and Gaming and the NSW Police Force requested that a further 11 licensed premises be added to the complaint.

Following a further adjournment to allow the 15 licensed premises to lodge any factual material and/or submissions for consideration by the Board, a joint conference was held under s104 of the (now repealed) Liquor Act 1982. The Board reached its decision on 14 March 2008 and imposed significant restrictions on 14 of the 15 premises.⁴ The most significant of these restrictions were, effective from 21 March 2008:

1. Imposition of a lockout from 1am for all 14 hotels, whereby patrons who were still on the premises could continue to drink alcohol but no patron would be allowed to enter after the lockout; and
2. Bringing forward the closing time to 3am for the 11 premises that were previously licensed to trade until 5am⁵, and to 2:30am for the three premises that had previously been licensed to trade until 3am.

Eleven of the licensees subsequently appealed to the Licensing Court and on 29 July 2008, an out-of-court agreement was reached to relax the lockout and closing times by 30 minutes. While this relaxation may have weakened the effect of the intervention, it still represented a significant reduction in overall trading hours for most of the 14 licensed premises for the entire follow-up period.⁶

The following additional restrictions were imposed on all 14 premises:

3. Licensees were required to produce a Plan of Management within six weeks of the ruling;
4. Licensees were to arrange for independent audits to be carried out on a quarterly basis to ensure compliance with this Plan of Management;

5. Licensees were to ensure that a supervisor be on the premises from 11pm until closing with the sole purpose of monitoring responsible service of alcohol;
6. From 10pm there was to be: no sale of shots, no sale of mixed drinks with more than 30mls of alcohol, no sale of ready mixed drinks stronger than five per cent alcohol by volume, no sale of more than four drinks to any patron at one time and for there to be free water stations on every bar;
7. Licensees were to ensure patrons did not stockpile drinks;
8. The sale of alcohol had to cease 30 minutes prior to closing time;
9. Licensees were to notify all staff members of these restrictions within 14 days;
10. Each of the licensees was required to enter into an agreement to share a radio network to enable management and security of each hotel to communicate with one another.

The application of such uniform restrictions on the sale of alcohol was unprecedented in NSW at that time. Indeed, as will be discussed below, such restrictions have been rare both in Australia and internationally. Importantly, because these restrictions only applied to licensed premises in a confined geographic region, they constitute a unique natural experiment in which to observe the impact of such restrictions on alcohol-related violence.

The aim of the current research was to determine whether the decreased availability of on-licence alcohol sales had any impact on alcohol-related violence in the Newcastle CBD. Before describing the aim, method and results of this study in more detail, we first review previous research that has examined the impact of variations in the availability of alcohol on rates of crime.

PRIOR RESEARCH

Put simply, the availability hypothesis suggests that an increase in the availability in alcohol will lead to an increase in alcohol consumption and

a corresponding increase in alcohol-related harm. In contrast, decreases in alcohol availability will result in decreased consumption and reductions in alcohol-related harm. Various empirical approaches have been adopted to test the validity of the availability hypothesis. Studies have found, for example, that there is a strong positive correlation between total alcohol sales and alcohol-related crime (e.g. Gruenewald et al. 1999; Midford et al. 1998; Stevenson 1996). A large number of studies have now shown a positive correlation between alcohol outlet density and alcohol-related crime (e.g. Donnelly et al. 2006; Gyimah-Brempong & Racine 2006; Livingston 2008; National Drug Research Institute 2007; Nielsen & Martinez 2003; Nielsen, Martinez & Lee 2005; Stevenson 1996). There is also a lot of evidence to suggest that reducing the 'economic availability' of alcohol by increasing alcohol prices is a highly effective means of reducing alcohol consumption and related harms (see Wagenaar et al. 2009 for a review).

The following review is limited to those studies that have directly tested the impact of varying the hours or the days upon which alcohol can be sold on alcohol-related crime. Previous research can be broadly delineated into 'liberalisation' and 'restriction' studies. Liberalisation studies examine the effect of increasing trading hours (usually by permitting premises to stay open longer) while restriction studies examine the impact of reducing trading hours (usually by requiring premises to close earlier). Searches for the keywords 'alcohol' and 'availability' were conducted on PubMed, CINCH (the Australian criminology database) and Criminal Justice Abstracts to identify relevant literature. The reference lists of each identified publication were then searched to identify relevant literature that did not appear on the database searches.

We review both peer-reviewed journal articles and evidence from government and academic reports if those reports were based on Australian studies. We also include international evidence that has been published in peer-reviewed journals, although we made no attempt

to identify 'grey literature' from other countries. We have deliberately omitted studies that explored the impact of sudden decreases in alcohol availability, such as those resulting from prohibition in the United States, or sudden prohibitions on alcohol sales during Mexican elections (e.g. Baker et al. 2000). This is because interventions such as these are not directly comparable with the intervention under examination here. For the same reason, we also only review studies examining the effect of variations in on-licence (as opposed to takeaway) alcohol sales. While the following review is therefore not exhaustive, it covers most Australian studies and some of the international research.

LIBERALISATION STUDIES

Stockwell and Chikritzhs (2009) recently undertook a comprehensive review of the international literature on the impact of varying licensed premises trading hours. Of the 14 peer-reviewed studies that had both baseline measurements and a suitable comparison group, 13 involved liberalisation of trading hours and one (reviewed in the ensuing section) assessed the impact of restricting trading hours. Eleven of the 14 studies reported at least one negative outcome (e.g. an increase in assaults or drink-driving offences relative to control sites). While less methodologically rigorous, the studies that either had no baseline measurements or no suitable control group also generally tended to find adverse effects following variations in trading hours.

In Australia, a series of early studies focussing on the relationship between liberalisation of trading hours and road traffic accidents found increases in casualties in Perth, Brisbane and Victoria after liquor laws were changed to allow trading on Sundays (Smith 1978, 1988c, 1990). While the overall level of traffic casualties did not change when alcohol sales in Victoria were relaxed by four hours each day (to 10pm; Raymond 1969), there was an increase in casualties between 6pm and 10pm, relative to other times of day (Smith 1988a). There was

no evidence of any increase in traffic casualties following the enactment of more liberal trading hours in Tasmania in the 1980s but most hotels elected not to increase their trading hours when given the opportunity to do so (Smith 1988b).

Research in Perth, Western Australia, examined the relationship between liberalisation of liquor trading and alcohol-related violence. Using interrupted time series analyses, Chikritzhs and Stockwell (2002) found increases in assaults on licensed premises that were granted extended trading permits relative to those that continued to trade within normal trading hours. Subsequent research using 'last-place-of-consumption' data also revealed an increase in the number of alcohol-related traffic accidents that were attributable to hotels that had extended trading hours, relative to control sites (Chikritzhs & Stockwell 2006). A more recent analysis of breath alcohol concentration (BAC) levels among apprehended drivers in Perth found differential effects of extended trading for men and women. Apprehended male drivers who had their last drink at a hotel granted an extended trading permit were found to have higher concentrations of alcohol than apprehended male drivers who were stopped after drinking at a hotel that did not have extended trading. Apprehended female drivers, on the other hand, had lower breath alcohol concentrations than women who had been drinking at hotels that did not have extended trading (Chikritzhs & Stockwell 2007).

Few studies have found no effect of more liberal licensed premises trading hours. One survey-based study of young male drinkers found no effect on self-reported alcohol consumption following a short-term liberalisation of trading hours in Fremantle, Western Australia, during the 1987 America's Cup yachting regatta (McLaughlin & Harrison-Stewart 1992). A more recent quasi-experimental analysis of alcohol use derived from survey data revealed that repealing a ban on Sunday trading in Canada significantly increased drinking on Sundays. However, there was also evidence of a decrease in

self-reported alcohol consumption on Saturdays and no evidence of an overall increase in drinking across the week, which led the authors to conclude that repealing such laws need not result in overall increases in alcohol consumption (Carpenter & Eisenberg 2009).

Stockwell and Chikritzhs (2009) argue that the differences between studies that find increases in anti-social behaviour and those that find negligible effects can be largely attributed to methodological differences between studies. For example, many studies fail to control for other aspects of the regulatory environment, or have inadequate baseline or follow-up periods. When these methodological weaknesses are accounted for, they argue, the weight of evidence strongly suggests that liberalising alcohol sales results in increased levels of alcohol-related crime.

RESTRICTION STUDIES

The literature examining the impact of restrictions in licensed premises trading hours is much more sparse than evidence bearing on the liberalisation of trading hours. This is, in large part, due to the fact that the post-World War II trend has overwhelmingly been toward more liberalised trade in all commodities, with alcohol being no exception (Babor et al. 2003). This trend toward increasing competition and reducing trade barriers has afforded few opportunities to study the impact of restrictions in the trading hours of licensed premises.

Tables 1a and 1b summarise all of the Australian restriction studies that were identified using our search criteria. Table 1a summarises those studies that had both baseline measures and a comparison site while Table 1b summarises studies that had either limited baseline data and/or no comparison group. As can be seen in Table 1a, very few studies have rigorously explored the impact of restrictions on licensed premises trading hours. Only two Australian studies were identified, one of which examined the impact of restricted trading hours in the Darwin CBD and the other of which examined restricted hotel

opening hours in the remote Northern Territory town of Tennant Creek.

d'Abbs, Forner and Thomsen (1994) conducted a pre-post analysis of the impact of a two-hour restriction in nightclub closing times in the Darwin CBD (closing times were brought forward from 6am to 4am). Rather than finding a decrease in assaults, d'Abbs and colleagues found that assaults increased in the intervention site after these restrictions were imposed. However, this result appears to have been due to an unintended increase in actual licensed premises opening hours prior to 4am with a negligible decrease in actual opening hours after 4am. As such, an overall increase in exposure to late night drinking in licensed premises is a plausible explanation for those results.

In the only other identified Australian study, Gray and colleagues (2000) examined the impact of restrictions imposed in the Northern Territory town of Tennant Creek. In comparison to the remainder of the Northern Territory, reductions in overall per-capita alcohol consumption and declines in several measures of alcohol-related harm were observed following restrictions in hotel opening hours and restrictions on takeaway sales. Positive outcomes attributed to the intervention included decreases in hospital admissions, police arrests and admissions to women's refuges. While these results are consistent with the availability hypothesis, the intervention included restrictions on takeaway alcohol sales, wine container sizes and a number of other measures, which makes casual interpretations difficult. Moreover, the generalisability of the results to more densely populated urban areas such as Newcastle is unclear.

In the United States, Voas and his colleagues examined the impact of restrictions on licensed premises opening hours (from 5am to 2am) in the Mexican border town of Juarez. The restrictions arose out of concerns that many young American residents were crossing from El Paso, Texas into Mexico to take advantage of longer licensed premises

Table 1a. Restriction studies that had both baseline measures and a comparison group

<i>Author</i>	<i>Location</i>	<i>Intervention</i>	<i>Research design</i>	<i>Findings</i>	<i>Consistent with availability hypothesis?</i>
d'Abbs et al. 1994	Darwin, NT, Australia	Restricted nightclub opening hours to 4am for several late night venues (previously 6am).	Pre-post analysis of computer aided dispatch data (with control site, although control site not replete with similar late night drinking venues)	Increase in assault and disorder reports in intervention sites but not control sites. Note: restrictions caused increase in venue/hours between 2pm and 4pm	No
Gray et al. 2000	Tennant Creek, NT, Australia	Restricted hotel opening hours and restricted takeaway sales (including hotel front bars and all takeaway sales being prohibited on Thursdays)	(a) ITS of total volume of liquor sold (b) pre-post analysis of recorded crime and admissions to hospitals, women's refuges and sobering up shelters (used rest of NT as control)	Declines in: per capita consumption of pure alcohol, alcohol-related (AR) hospital admissions, number of admissions to women's refuges and police arrests (on Thursdays)	Yes
Voas et al. 2002	Mexico / USA	Restricted licensed premises opening hours from 5am to 2am in Juarez, Mexico	Pre-post analysis of positive BACs among pedestrians crossing Juarez/EI Paso border	Decrease in total number of positive BACs after 3am. No short-term change between midnight and 3am in the intervention site or during either period in the control site.	Yes
Voas et al. 2006	Mexico / USA	Restricted licensed premises opening hours from 5am to 2am in Juarez, Mexico	Pre-post analysis of positive BACs among pedestrians crossing Juarez/EI Paso border – seven year follow-up	Original decreases after 3am sustained but increases in alcohol-positive border crossings between midnight and 3am in the intervention site	Yes but temporal shift

opening hours and the younger drinking age in that country. Voas, Lange and Johnson (2002) examined changes in positive alcohol breath tests among pedestrians crossing the Juarez / El Paso border into the United States and found significant reductions after 3am relative to a comparison border crossing region. No changes were observed in the hours between midnight and 3am. These findings were sustained seven years later, although at that time significant increases were observed between the hours of midnight and 3am (Voas et al. 2006). Voas et al. interpreted this to mean that, over the longer term, there had been a temporal displacement of heavy episodic drinking towards earlier in the night.

Table 1b summarises the remaining identified restriction studies. Three of the five Australian interventions summarised in Table 1b involved restrictions in regional or remote Aboriginal communities and all three also included restrictions on

takeaway alcohol sales (Crundall & Moon 2003; d'Abbs et al. 1999, 2003, cited by National Drug Research Institute 2007; Gray 2003). While all three evaluations found reductions in alcohol-related problems, none of these studies was able to employ a suitable comparison site.

Duailibi and colleagues (2007) estimated that prohibition of on-premises alcohol sales after 11pm in Diadema, Brazil, reduced homicides by nine per month. There was also a trend towards a reduction in recorded assaults against women, although this was not statistically significant. Graham, McLeod and Steadman (1998), on the other hand, found no reduction in assault or alcohol-related presentations to an inner-city emergency department following uniform restrictions on extended trading in the city of Edinburgh, Scotland. Walker and Biles (1997) also found no effect of a uniform restriction in licensed premises trading hours to 4am in the Australian Capital Territory. Among a number of measures

collected, these researchers found no changes in police-recorded anti-social behaviour or community feelings of safety following the intervention. There was a reduction in recorded drink-driving incidents, although the authors suggested that it was difficult "to tie this success specifically to the 4am closure of taverns and nightclubs" (p.3). While this evidence is contrary to the availability hypothesis, the intervention only involved a small number of premises, was run in the middle of a major economic recession, employed very short baselines and no control group was available for the analysis. All of these factors may have impacted on the outcomes.

While not technically assessing the effect of restricted opening hours, two further Australian studies have relevance for the current research because they examined the impact of a lockout on markers of alcohol-related problems. Researchers from the Centre for Health Research and Practice at the University

Table 1b. Restriction studies that had short baseline measures and/or no comparison group

<i>Author</i>	<i>Location</i>	<i>Intervention</i>	<i>Research design</i>	<i>Findings</i>	<i>Consistent with availability hypothesis?</i>
Crundall & Moon 2003; Gray 2003	Alice Springs, NT, Australia	Reduction in hours upon which takeaway sales could be purchased, restrictions on container sizes (nothing greater than 2 litres) and nothing other than light beer to be consumed on-premises prior to 11.30am	Pre-post analysis of recorded crime, ambulance call-outs (short baseline and follow-up, no control group)	Decreases in protective custody orders issued, number of people placed in police cells, AR assaults, AR ambulance call-outs, AR presentations to hospital EDs and AR presentations to Aboriginal health clinics. Increases in alcohol-related 'disturbances', 'criminal damage' and 'unlawful entry' incidents (although possibly due to enforcement) and acute AR hospital admissions	Yes
d'Abbs et al. 2003, cited by National Drug Research Institute 2007	Mt Isa, Queensland, Australia	No on-premises alcohol sales before 9am and no takeaway sales before 10am each day	Pre-post, no control	Reduction in alcohol consumption and hospital admissions although evidence of an increase in drinking in a nearby town	Yes
d'Abbs et al. 1999, cited by National Drug Research Institute 2007	Curtin Springs Roadhouse, NT, Australia	Ban on on-premise alcohol sales to Indigenous people and restrictions on takeaway sales	Pre-post, no control	Reduction in purchases of full-strength alcohol and alcohol-related events at local health clinics	Yes
Duailibi et al. 2007	Diadema, Brazil	On-premises alcohol sales prohibited after 11pm	ITS, no control site	Reduction in homicides by 9 per month. No significant change in assaults against women	Yes
Graham et al. 1998	United Kingdom	Restriction in extended trading hours for Edinburgh licensed premises	Pre-post, no control	No evidence of any reduction in the number of assault or alcohol-related emergency department presentations in weeks following the restriction	No
Walker & Biles 1997	ACT, Australia	Restriction in licensed premises trading hours beyond 4am	Pre-post analysis of: police-recorded anti-social behaviour and drink-driving incidents; survey of public feelings of safety	No change in anti-social behaviour. Possible decrease in drink-driving incidents. No detectable difference in feelings of safety	No

of Ballarat examined the impact of a 3am lockout for all late night entertainment venues in the town of Ballarat, Victoria and found reductions in assaults in the year following the lockout as well as decreases in residential property damage (Centre for Health Research and Practice 2004). Palk, Davey and Freeman (2008) found a small reduction in the proportion of all recorded incidents deemed to be alcohol-related following the introduction of a lockout policy on the Gold Coast, Australia, but little or no impact when introduced in two other late night entertainment districts. Unfortunately, neither of these studies was able to construct a suitable comparison group so causal inferences were limited.

THE CURRENT STUDY

The evidence generally indicates that liberalising the availability of alcohol increases alcohol-related crime while restricting the availability of alcohol tends to reduce alcohol-related crime. The volume of literature bearing on this issue, however, is relatively small and of limited generalisability to densely populated urban Australian cities where the bulk of the population live and where there is significant public debate about the impacts of licensed premises. The restrictions applied to the 14 Newcastle licensed premises therefore provide a timely natural experiment in which to observe the effect of restricting alcohol availability on alcohol-related crime.

Reducing the availability of alcohol sales might affect violent crime in a number of ways. The intended effect is to reduce the total volume of assaults in the CBD by reducing the number occurring early in the morning (i.e. after 3am). An unintended effect might be to geographically displace assaults from the CBD into neighbouring areas without having an effect on the aggregate level of crime. It is also possible that the intervention could produce a shift in the temporal distribution of assaults towards earlier times when alcohol is still available. The current evaluation therefore aimed to assess:

- (1) Whether the trading hour restrictions had any impact on the total number

of alcohol-related assaults in and around the Newcastle CBD;

- (2) Whether there was any geographic displacement of alcohol-related assaults to areas neighbouring the Newcastle CBD; and
- (3) Whether the restrictions had any impact on the time of day upon which assaults were recorded as occurring in the Newcastle CBD.

It is difficult to obtain good measures of trends in assault. Recorded crime data are one useful marker but they are subject to measurement error because seven in ten victims do not report the offence to the police (Australian Bureau of Statistics 2008). In addition, staff of licensed premises that stand to have their trade restricted if found to be contributing to alcohol-related crime may have a strong incentive to not report assaults to the police. Furthermore, because recorded crime data are a primary performance indicator for police, there is always a risk that those data might be manipulated to misrepresent what has actually happened (Smith 1995).

Three sources of police data were therefore used to measure trends in assault: (1) recorded crime data; (2) last-place-of-consumption data from the NSW Police Force Alcohol Linking Program; and (3) call-out data from the NSW Police Force Computer Aided Dispatch (CAD) system. Each of these datasets is described in more detail in the Method section.

METHOD

STUDY PERIOD

For the analysis of changes in the number of recorded offences (aims 1 and 2), the intervention start date was defined as 1 April 2008. While this was 11 days after the restrictions were enacted, it was considered to be preferable because it allowed the statistical analyses to be carried out using whole months. The entire study period for the interrupted time series analysis (see below) extended from 1 April 2004 to 31 March 2009, which provided 48 months of pre-intervention

and 12 months of post-intervention data. For the analysis of whether there was any change in the temporal distribution of assaults (aim 3), the post-intervention period extended from the date that the restrictions were applied (21 March 2008) to 20 March 2009. The pre-intervention time period extended from 21 March 2004 to 20 March 2008.

DATA SOURCES

Recorded crime data

The Bureau receives a monthly extract of recorded crime data directly from the NSW Police Force Computerised Operational Policing System (COPS). This extract includes all criminal incidents that have been 'accepted and verified' by a supervising officer as a genuine COPS entry.⁷ The Bureau also receives information on incidents that have been 'rejected' as invalid entries, recorded as 'occurrences only' or as a 'personal violence' incident. None of these rejected, occurrence or personal violence incidents are included in the Bureau's published crime statistics. A criminal incident on COPS is an activity that is either detected by or reported to police that involved the same victim and offender, occurred at one location and during one uninterrupted period of time, fell into one offence category and one incident type (e.g. 'actual' or 'attempted', see Goh & Moffatt 2009 for more detail). As such, a criminal incident can be interpreted as a single occurrence of an alleged offence.

When entering the details of a criminal incident, the police officer must make a determination as to whether an incident was related to domestic violence. For the purposes of assessing whether there was any impact on the total number of alcohol-related assaults (aims 1 and 2), all assaults that occurred in the Newcastle Local Government Area (LGA) that were deemed to be unrelated to domestic violence and were recorded as occurring between the hours of 10pm and 6am were extracted from the Bureau's recorded crime database. Domestic violence related assaults were excluded from the primary analysis because the restrictions were intended to reduce pub

violence, which is more likely to involve victims and offenders who are not in a domestic relationship. Similarly, limiting the analyses to night-time assaults reduced the likelihood of falsely attributing an assault to alcohol consumption where alcohol, in fact, had no bearing on that assault (Stockwell et al. 1998). In 2008, for example, more than three-quarters of non-domestic assaults occurring between the hours of 10pm and 6am were deemed by police to be alcohol-related, compared with 26 per cent of assaults occurring at other times of day. For the purposes of assessing whether the temporal distribution of assaults changed after the intervention began (aim 3), all non-domestic assaults that occurred in the Newcastle LGA were included in the analysis irrespective of the time of day on which they occurred.

While it is possible to identify from recorded crime data whether an incident

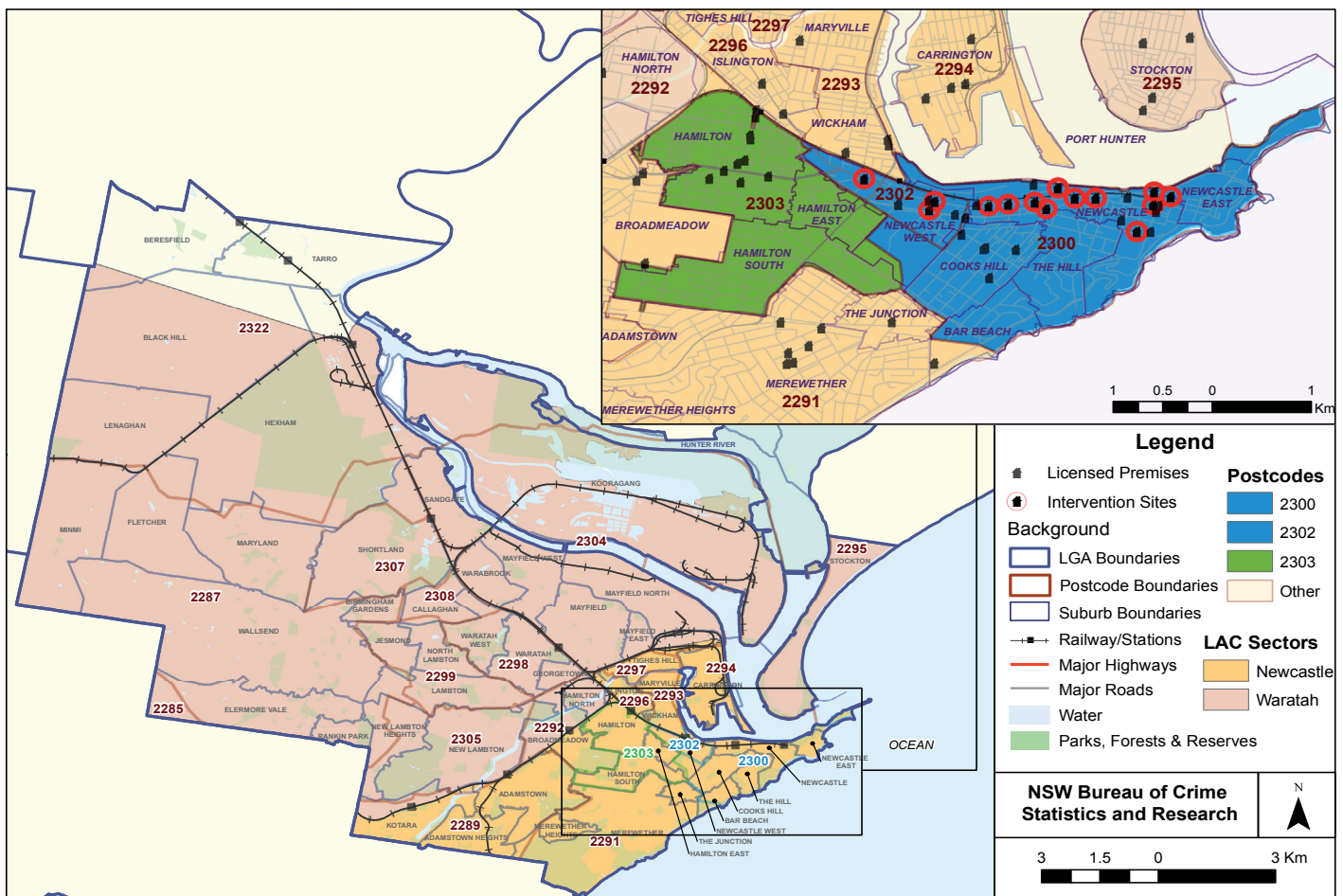
took place on a particular licensed premises, unpublished BOCSAR data reveal that only around one in six assaults were recorded as occurring on licensed premises in 2008. Many more alcohol-related assaults may have occurred in the area surrounding the licensed premises (e.g. on street fronts or in alley ways) among patrons who had recently been drinking at those premises. For the purposes of this part of the analysis, assaults were recorded as occurring in the intervention site if the location of the assault occurred anywhere in either the 2300 or 2302 postcode. As shown in Figure 1, all of the hotels that were subject to restrictions under the LAB ruling fell within one of these two postcodes. The adjacent suburb of Hamilton (postcode 2303) constituted the comparison site. Hamilton is another popular late night entertainment district that has a high density of licensed premises. A number of these premises

were licensed to trade until 3am and one could trade beyond 3am. Some Hamilton hotels also have a high number of assaults recorded on their licensed premises. For example, in the year prior to the restrictions Hamilton contained three hotels that ranked in the top 100 hotels for assaults on licensed premises in NSW. None of these premises were subject to the LAB restrictions (see Figure 1).

Last-place-of-consumption data

As an intelligence gathering exercise, the NSW Police Force has since mid-2004 been determining for all suspected offenders, victims and drivers: (a) whether they consumed alcohol prior to the incident, (b) their apparent level of intoxication, (c) where the alcohol was consumed, (d) who purchased the alcohol and (e) from where the alcohol was purchased. This data collection – known as the Alcohol Linking Program – aims

Figure 1. Geographic breakdown of intervention and comparison sites used in the analysis of recorded crime data



ANALYSIS

Interrupted time series analyses were employed to assess aims (1) and (2). Separate models were estimated for the intervention and comparison sites for each of the three outcomes.⁹ Correlograms were first observed to determine whether there was any evidence of serial autocorrelation in the series. Where autocorrelation was found to be present, autoregressive terms at the appropriate lag were fitted to the models.¹⁰ Once serial autocorrelation had been appropriately accounted for, terms were added to the model to adjust for seasonality.¹¹ In addition to this seasonal adjustment, a dummy variable was fitted to the models to adjust for months that had a greater number of Saturday nights to account for the fact that alcohol-related assaults peak on weekends (Briscoe & Donnelly 2001). This variable took the value one if the month had five Saturday nights and zero otherwise. This variable was only retained in the model if it significantly improved the fit of the model. A monotonically increasing trend term was also fitted to each model to account for any underlying trend in the series.

Once serial autocorrelation, seasonality, the underlying assault trend and the number of weekends in each month had been accounted for, an intervention effect was tested for by assessing whether there was:

- (a) any change in the level of crime following the introduction of the restrictions; and/or
- (b) any change in the trend in assault following the introduction of the restrictions.

We tested for (a) by fitting a dummy variable that took the value zero before the intervention and one after the intervention. We tested for (b) by fitting a term that took the value zero before the intervention and then a monotonically increasing trend term (1,2,3...12) after the intervention. Figure 3 illustrates what these hypothetical intervention effects might look like. The graph on the left shows what we would expect if the intervention brought about a decrease in the level of assault but had no impact on the trend in assault. The graph on the right of Figure 3 shows what the pattern of assault would look like if the effect of the intervention were to change the slope of the trend in assault but have no immediate impact on the level of assault. We made no a priori assumption about which of these possible intervention effects is most likely. Instead, the decision was made to first fit a change in level term to the model, followed by separately fitting a change in trend term. The term that provided the best fit to the data was retained as the intervention term. If there were any evidence of a geographic displacement of crime to neighbouring areas (aim 2), on the other hand, we

would expect to see either an increase in the level or an increase in the slope in assault in the comparison series.

Aim (3) was assessed by cross-tabulating the time of day of the incident by the intervention time point (pre vs post). All analyses were conducted using Stata v10.1.

RESULTS

RECORDED CRIME

Table 2 shows the final models estimating whether there were any significant changes in the number of police-recorded night-time non-domestic assault incidents following the introduction of the restricted trading hours. The model shown in the top half of Table 2 gives the results for the Newcastle CBD, while the model shown in the bottom half of Table 2 gives the results for Hamilton. The critical terms in these tables are the 'change in level' or the 'change in trend' terms because they indicate whether there was any significant change in the number of assaults after the intervention began. The other terms in the model simply adjust for the underlying trend, seasonality, the number of Saturday nights in the month and serial autocorrelation.

In Table 2, the 'change in trend' term in the Newcastle model was statistically significant (p=0.011), while the 'change in level' term in the Hamilton model was not statistically significant (p=0.547). This suggests that there was a significant change in the trend in night-time assaults in Newcastle following the LAB decision but not in Hamilton. After adjusting for seasonal variation, the slope of the in night-time assault series decreased by 1.7 assaults per month in the year following the LAB ruling. This might not sound large in absolute terms but translates into an overall decrease of approximately 133 recorded assaults in the year following the intervention (i.e. 1.7 fewer assaults in month one + 3.4 fewer assaults in month two + 5.1 fewer assaults in month three etc.). Figure 4 illustrates the observed number of incidents in each site as well as the number estimated by the

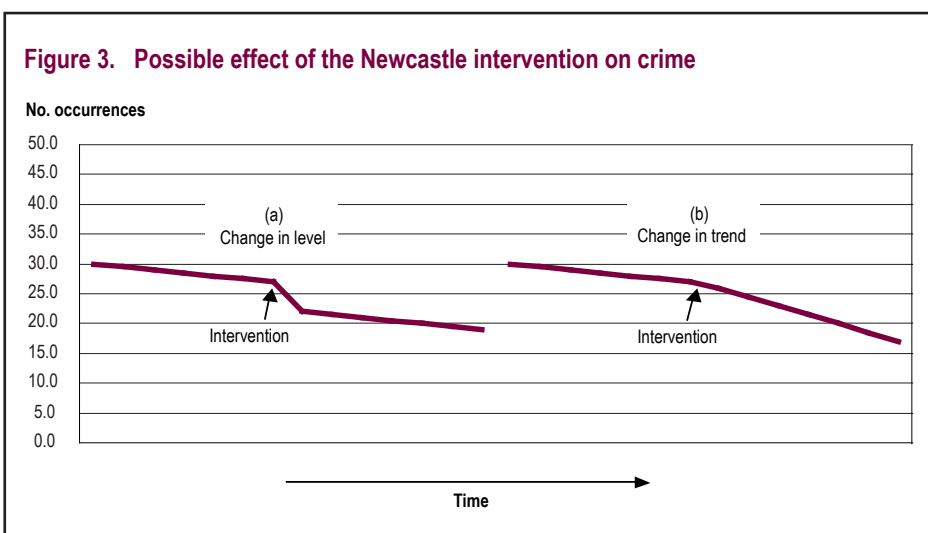


Table 2. ARIMA models estimating change in recorded number of incidents of night-time non-domestic assault incidents in the Newcastle CBD (postcode 2300/2302) and Hamilton (postcode 2303) following LAB ruling

<i>Newcastle (postcode 2300/2302)</i>		
<i>Variable</i>		<i>Coefficient (std. error) p-value</i>
Constant		38.17 (8.31) <0.001
Underlying trend		-0.01 (0.09) 0.872
Change in trend		-1.69 (0.67) 0.011
Month	January	-0.65 (8.26) 0.937
	February	-1.50 (8.41) 0.859
	March	4.62 (10.55) 0.662
	April	-10.01 (8.74) 0.252
	May	-10.27 (9.21) 0.265
	June	-6.31 (8.88) 0.477
	July	-4.36 (8.65) 0.614
	August	-5.21 (8.25) 0.528
	September	-8.06 (8.75) 0.357
	October	-7.30 (9.11) 0.423
	November	-5.15 (8.38) 0.538
AR(1)		0.02 (0.15) 0.906
<i>Hamilton (postcode 2303)</i>		
<i>Variable</i>		<i>Coefficient (std. error) p-value</i>
Constant		12.69 (1.63) <0.001
Underlying trend		-0.02 (0.03) 0.402
Change in level		0.82 (1.37) 0.547
Saturday		0.96 (1.04) 0.355
Month	January	-4.00 (2.52) 0.112
	February	-5.29 (1.92) 0.006
	March	-4.58 (2.39) 0.056
	April	-4.17 (1.86) 0.025
	May	-2.89 (2.19) 0.188
	June	-2.82 (3.02) 0.351
	July	-1.58 (2.24) 0.479
	August	-5.68 (2.19) 0.009
	September	-6.72 (2.36) 0.004
	October	-4.44 (2.44) 0.068
	November	-4.47 (2.72) 0.1
AR(1)		-0.17 (0.15) 0.263
AR(3)		-0.17 (0.17) 0.312

respective models. The fitted trend line is relatively close to the observed line, which suggests that the model provided a good fit to the data.¹² The fact that there was no increase in night-time non-domestic assaults in Hamilton suggests that there

was no geographic displacement into the neighbouring entertainment district following the restrictions.

Figure 5 shows the temporal distribution of assaults in the intervention and control sites. In the Newcastle CBD, there was

a clear decrease in the proportion of non-domestic assaults between the hours of 3am and 6am and a corresponding increase in the proportion occurring between 9pm and midnight ($\chi^2_7 = 58.3$, $p < 0.001$). In the four years prior to the LAB restrictions, 17 per cent of all non-domestic assault incidents were recorded as occurring between the hours of 3am and 6am. In the year following the intervention, this was reduced to six per cent. In contrast, there was no significant shift in the time of day upon which assaults occurred in Hamilton following the start of the intervention ($\chi^2_7 = 4.4$, $p = 0.733$).

There was no evidence to suggest that the decrease in recorded assaults observed in the Newcastle CBD was due to a change in willingness to report assaults to police or to a change in police recording practices. There was also no evidence of any displacement into other types of offences (see Appendix for details).

LAST-PLACE-OF-CONSUMPTION DATA

Table 3 shows the final models estimating changes in assaults linked to the restricted premises and to the remaining, non-restricted Newcastle licensed premises. After adjusting for seasonal effects, there was a significant change to the underlying trend in assaults linked to intervention premises following the imposition of these restrictions but not for incidents linked to other Newcastle licensed premises. The model in the top half of Table 3 estimates that the trend in assaults linked to intervention hotels decreased by 1.1 incidents per month following the onset of the restrictions. This translates into approximately 83 fewer assaults linked to intervention hotels in the year following the restrictions. Figure 6 illustrates the change graphically and, again, shows that there was a close fit between the observed and predicted values.¹³ As with the recorded crime data, the fact that there was no significant increase in assaults linked to non-intervention premises suggests that there was no geographic displacement of assaults.

Figure 4. Observed and predicted number of night-time non-domestic assault incidents recorded in the Newcastle CBD (postcode 2300/2302) and Hamilton (postcode 2303)

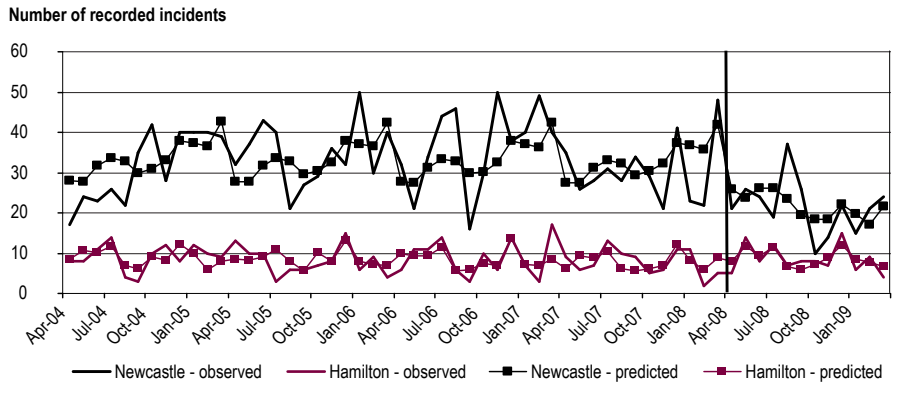
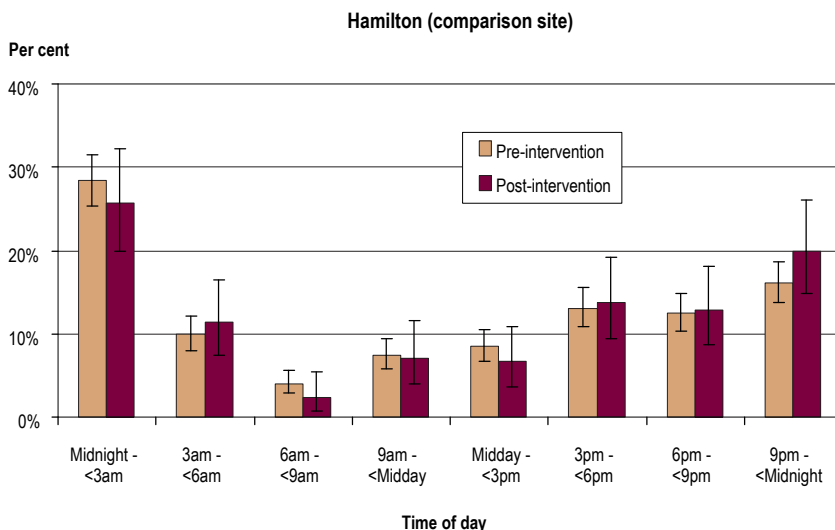
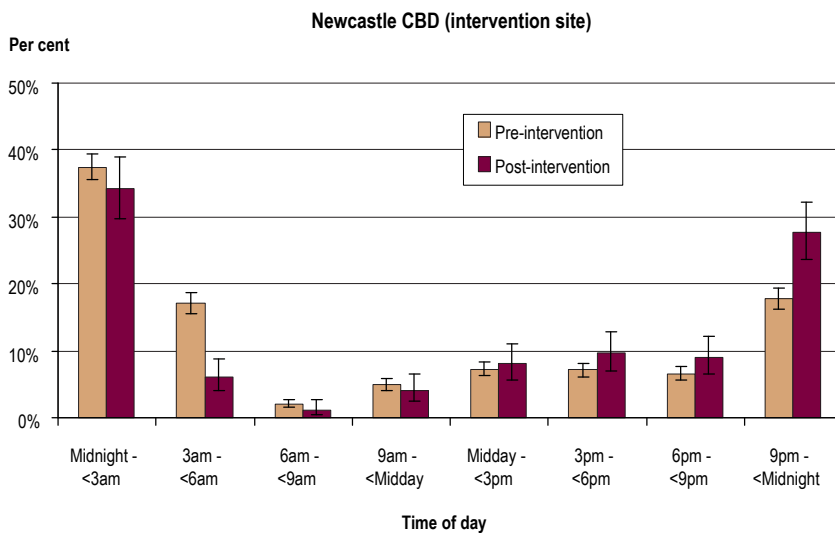


Figure 7 shows the temporal distribution of recorded assault incidents where the alleged victim and/or the alleged offender reported having their last drink in a Newcastle licensed premises. Incidents recorded between 6am and 6pm are grouped together because very few incidents occurred during these time periods. As with the recorded crime data, there was a significant decrease in the proportion of incidents between the hours of 3am and 6am where the victim and/or offender reported having their last drink in one of the premises subject to the restrictions ($\chi^2_4 = 17.4, p=0.002$) but not for those who reported having their last drink at another Newcastle licensed premises ($\chi^2_4 = 1.0, p=0.917$). Prior to the restrictions, 27.7 per cent of all assaults linked to intervention premises occurred between the hours of 3am and 6am. In the year following the intervention, this decreased to 14.6 per cent. For assaults linked to the hotels subject to the intervention, there were small increases in the proportion of assaults recorded at each other time period. However, the overlapping error bars indicate that these increases were not statistically significant for any one period.

Figure 5. Temporal distribution of recorded assault incidents in the intervention and comparison sites prior to and subsequent to the intervention



As with the recorded crime data, there was no evidence of any displacement into other types of offending following the onset of the restrictions and, in fact, the intervention appears to have had an ancillary effect on other types of offending (see Appendix for details).

CAD DATA

Table 4 shows the final models estimating whether there were any significant changes in the number of night-time calls to assault or brawl incidents in the Newcastle/City or Waratah LAC sectors following the introduction of the restrictions. Contrary to the recorded crime and Linking data, after adjusting for seasonal effects and for the number of Saturdays in a given month, there was no significant change in the number of calls for service in the Newcastle/City sector following the imposition of the restrictions. There was also no significant change in the Waratah sector after the restrictions

Table 3. ARIMA models estimating change in number of assault incidents following LAB ruling where last place of consumption was one of the intervention hotels or another Newcastle licensed premises

<i>Intervention hotel</i>		
<i>Variable</i>		<i>Coefficient (std. error) p-value</i>
Constant		16.98 (3.75) <0.001
Underlying trend		0.07 (0.07) 0.332
Change in trend		-1.06 (0.43) 0.013
Month	January	-1.93 (3.54) 0.586
	February	-1.95 (3.53) 0.581
	March	1.37 (3.16) 0.665
	April	-3.33 (4.03) 0.409
	May	-2.97 (4.18) 0.476
	June	-1.53 (3.67) 0.677
	July	-3.58 (6.50) 0.581
	August	-3.77 (4.39) 0.391
	September	-4.97 (3.36) 0.139
	October	-3.86 (3.83) 0.314
	November	-2.29 (3.94) 0.562
AR(1)		0.04 (0.13) 0.728
AR(3)		0.30 (0.15) 0.041
<i>Other Newcastle licensed premises</i>		
<i>Variable</i>		<i>Coefficient (std. error) p-value</i>
Constant		21.71 (3.78) <0.001
Underlying trend		0.07 (0.08) 0.37
Change in trend		-0.06 (0.32) 0.849
Month	January	-9.83 (3.31) 0.003
	February	-9.09 (4.68) 0.052
	March	-1.13 (3.13) 0.717
	April	-3.15 (4.25) 0.458
	May	-4.97 (4.41) 0.26
	June	0.51 (6.40) 0.937
	July	-0.12 (3.69) 0.975
	August	1.24 (3.05) 0.683
	September	-5.82 (3.51) 0.098
	October	-6.47 (4.92) 0.189
	November	-4.31 (3.35) 0.198
Saturday		1.09 (1.57) 0.486
AR(1)		0.24 (0.20) 0.215
AR(2)		-0.17 (0.18) 0.347

began. These findings are reflected in Figure 8, which also shows no evidence that the number of call-outs changed following the LAB ruling. Again, Figure 8 shows that there was a relatively close fit between the observed and predicted values, which suggests that the model provided a good fit to the data.¹⁴

Figure 9 shows the temporal distribution of calls to assault or brawl incidents in the Newcastle/City and Waratah police sectors before and after the LAB ruling. As with the recorded crime and Linking data, there was a significant decrease in the proportion of calls to these incidents between the hours of 3am and 6am in the Newcastle sector ($\chi^2_7 = 49.6, p < 0.001$) but not for the Waratah sector ($\chi^2_7 = 7.3, p = 0.395$). Prior to the restrictions, 19.1 per cent of all calls to assault or brawl incidents in the Newcastle sector were recorded between the hours of 3am and 6am. In the year following the intervention, this decreased to 11.3 per cent. For the Newcastle sector, there was a corresponding increase in the proportion of calls to assault and brawl incidents recorded between 3pm and 6pm and a smaller (non-significant) increase between midnight and 3am.

As with recorded crime and Linking data, there was no evidence of any increases in calls for service to other types of incidents that could be attributed to the restrictions (see Appendix for details).

DISCUSSION

The current study aimed to assess whether the decision by the LAB to restrict the availability of on-licence alcohol sales in the Newcastle CBD brought about: (1) any change in the overall number of alcohol-related assaults recorded in and around the CBD, (2) any geographic displacement of alcohol-related assault, and/or (3) any temporal shift in the distribution of alcohol-related assaults. In relation to aim (1), two of the three data sources converged to suggest that the number of assaults decreased in the intervention site following the restrictions. In relation to aim (2), none of the data sources

provided any evidence to suggest that there was any geographic displacement of assaults following the intervention. In relation to aim (3), all three data sources showed evidence of a decrease in the proportion of assaults occurring between the hours of 3am and 6am (i.e. the time during which the late-trading premises were ordered to be closed). There were corresponding increases in the proportion of assaults recorded earlier in the night but, with the exception of CAD data, these did not wholly offset the reduction occurring between 3am and 6am. This is an important point to note because one argument against restrictions in hotel trading hours is that assaults will simply be temporally displaced toward earlier times in the evening. The current study provides strong evidence against such a conclusion.

There was strong evidence of a decrease in assault following the onset of the intervention from both recorded crime data and last-place-of-consumption data from the Alcohol Linking Program. There were estimated to be 133 fewer recorded assaults in postcode 2300 or 2302 and 83 fewer assaults linked specifically to the intervention premises in the year following the intervention. There was no significant increase or decrease in recorded assaults in Hamilton or in the premises that were not subject to the intervention, which suggests that there was no evidence of any displacement to the other late-night entertainment areas in Newcastle. While the absolute magnitude of these decreases might not sound large, it must be kept in mind that recorded crime data only capture those incidents that are reported to police. We know from crime victim surveys that only 31 per cent of assault victims report the incident to police (Australian Bureau of Statistics 2008). If we were to scale up on this basis, the restrictions may have prevented as many as 429 actual assaults in the year following the intervention. Indeed, the reduction may have been much larger than this if there were flow-on reductions in domestic assault or to assaults that would have happened in geographic areas not included in this evaluation.¹⁵

Figure 6. Observed and predicted number of assault incidents where alleged victim and / or offender had their last drink at an hotel subject to the intervention or another hotel

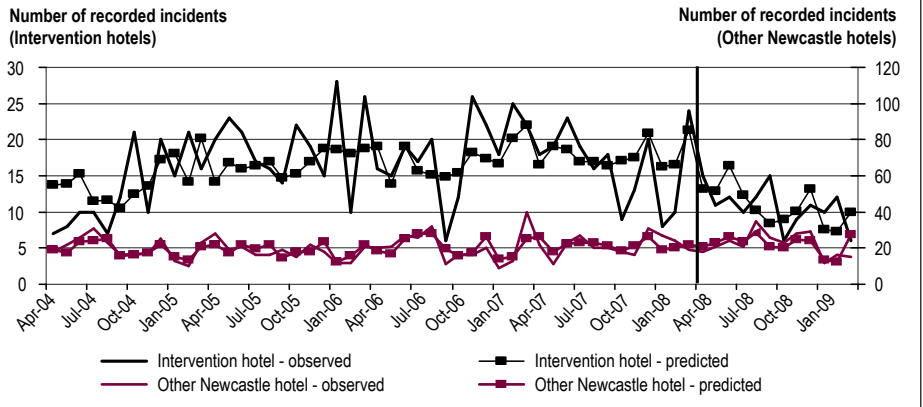


Figure 7. Temporal distribution of recorded assault incidents where the alleged victim and / or the alleged offender was deemed to be affected by alcohol and had their last drink in a Newcastle licensed premises

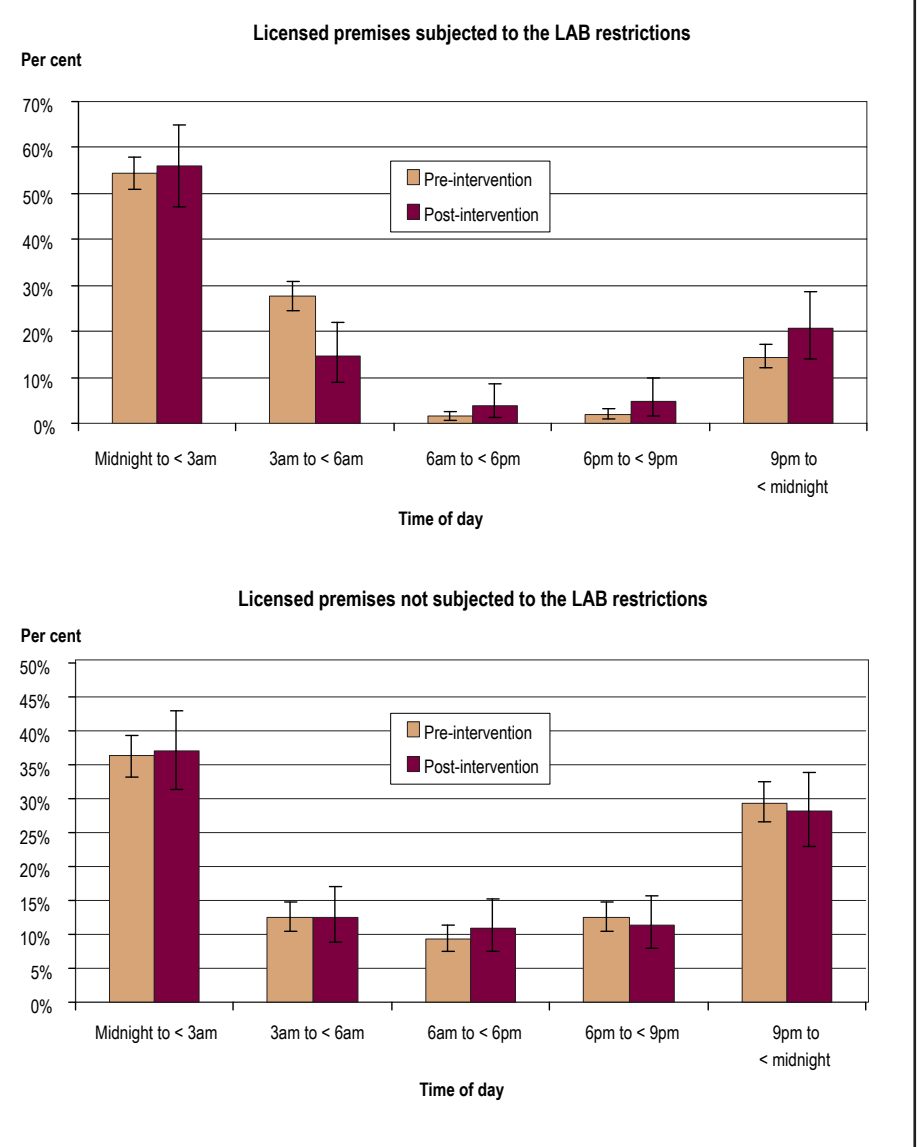


Table 4. ARIMA models estimating change in number of calls to night-time assault and brawl incidents in the Newcastle and Waratah police sectors following LAB ruling

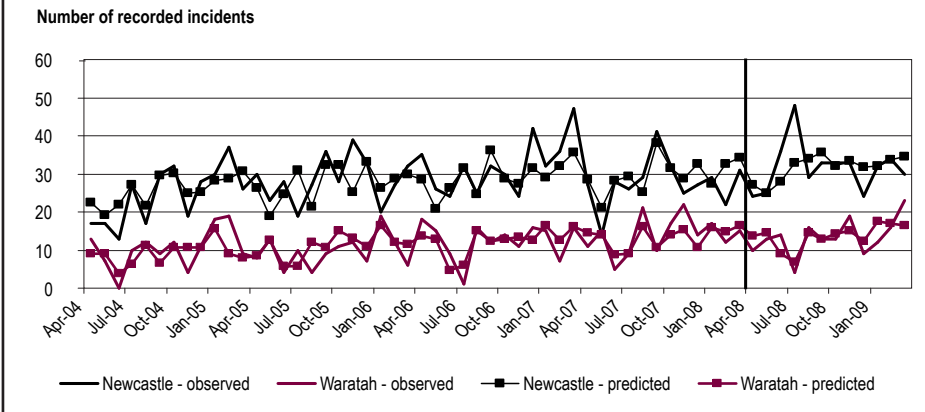
<i>Newcastle/City</i>		
<i>Variable</i>	<i>Coefficient (std. error)</i>	<i>p-value</i>
Constant	25.23 (3.75)	<0.001
Underlying trend	0.11 (0.10)	0.278
Change in level	0.38 (5.48)	0.944
Saturday	3.19 (2.52)	0.207
Month	January -1.68 (5.21)	0.747
	February 2.06 (4.38)	0.638
	March 2.51 (3.60)	0.485
	April -2.86 (4.03)	0.477
	May -8.43 (4.59)	0.066
	June -3.08 (3.84)	0.422
	July -0.14 (3.39)	0.966
	August -3.69 (4.05)	0.362
	September 4.56 (5.01)	0.362
	October 1.06 (6.56)	0.872
	November -1.42 (3.51)	0.687
AR(1)	0.18 (0.23)	0.424
<i>Waratah</i>		
<i>Variable</i>	<i>Coefficient (std. error)</i>	<i>p-value</i>
Constant	8.05 (2.21)	<0.001
Underlying trend	0.11 (0.03)	<0.001
Change in level	-1.71 (1.26)	0.175
Month	January 4.72 (3.31)	0.154
	February 1.31 (3.00)	0.662
	March 1.98 (2.42)	0.414
	April 0.95 (2.82)	0.737
	May 1.62 (4.38)	0.711
	June -4.12 (2.77)	0.136
	July -4.05 (2.85)	0.156
	August 2.73 (2.74)	0.319
	September -0.48 (4.27)	0.91
	October 2.20 (3.97)	0.579
	November 2.23 (2.84)	0.432
AR(1)	-0.26 (0.20)	0.176
AR(2)	-0.29 (0.18)	0.101

There was also strong evidence of a decrease in the proportion of assaults recorded between the hours of 3am and 6am in the year following the intervention. Recorded crime data, calls for police attendance at crime scenes and last-place-of-consumption data all converged in suggesting that a greater proportion of assaults occurred earlier in the evening in the intervention site following the restrictions. For recorded crime and Linking data, this did not wholly offset the reduction in assaults between 3am and 6am. On the other hand, the increase in earlier call-outs observed in CAD data offset the reductions observed between 3am and 6am.

There are four prima facie explanations for the discrepancy between police call-outs and the other two data sources: (1) the Newcastle/City police sector encompassed a geographic region that was too large to detect subtle changes in calls for police attendance; (2) any decrease in calls for service as a result of an actual reduction in assault may have been offset by an increase in willingness to call for police attendance at the scene of alleged assaults; (3) the decrease in recorded crime and Linking data did not reflect a real decrease in assault but was instead an artefact of reporting or recording bias; and/or (4) there may be qualitative differences between the types of assaults that get recorded on these different databases and the intervention may have differentially impacted on assaults of the type that end up as recorded criminal incidents. We consider each of these possibilities in turn below.

The Newcastle/City police sector, which constituted the intervention site for the call-out analysis, is a broadly defined geographic region. Indeed, this sector also includes the other late night entertainment district of Hamilton, which was the comparison site for the recorded crime analysis. This broad definition might have obscured any actual reduction in calls to attend assaults in the CBD by one of two means. It is possible that subtle changes in the CBD were swamped by a large volume of calls for service in other parts of the sector. While possible,

Figure 8. Observed and predicted number of calls to night-time assault or brawl incidents in the Newcastle and Waratah police sectors

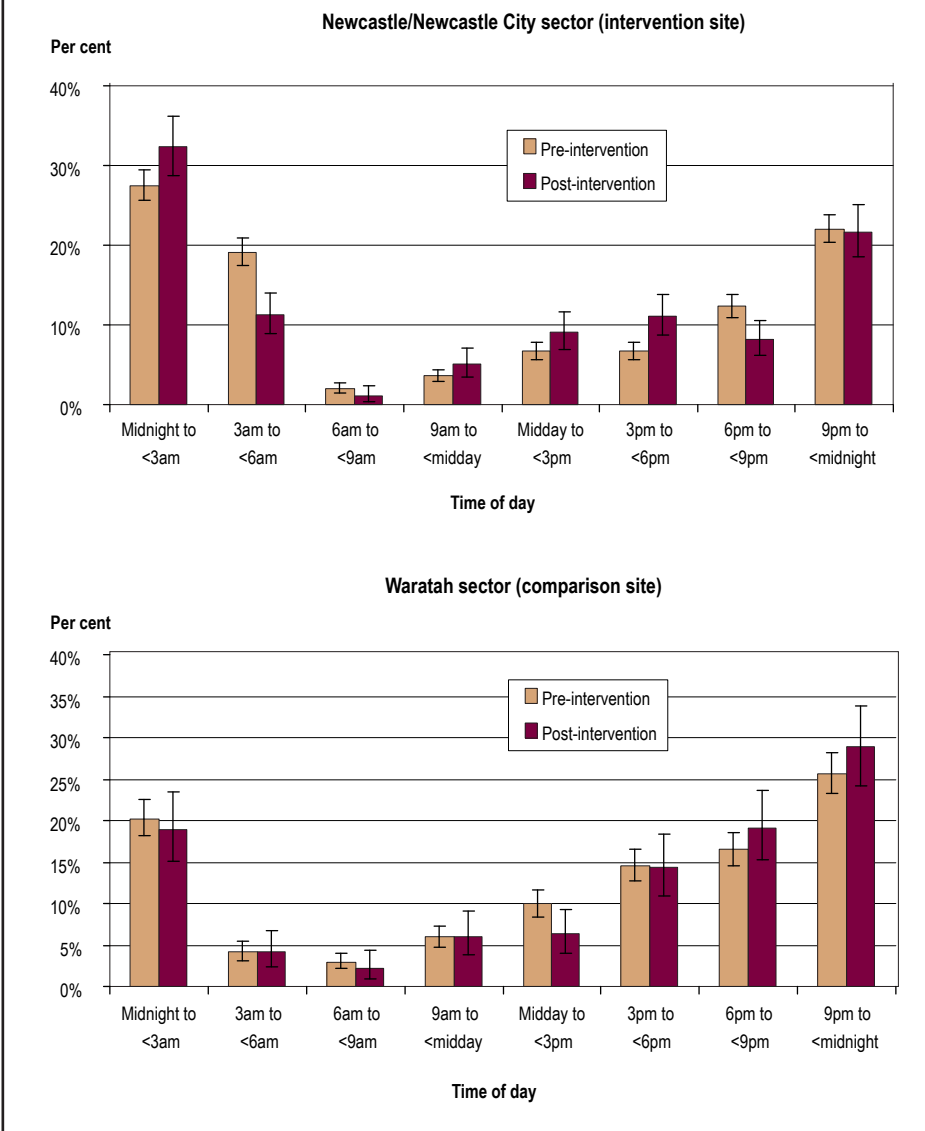


this interpretation is unlikely in light of the fact that the reduction in recorded criminal incidents is still apparent when aggregated to the Newcastle/City sector level (see Appendix Table A3). Alternatively, it is possible that any real decrease in calls to attend assaults in the CBD was displaced by an increase in calls to other parts of the police sector. If we accept this latter possibility, we must also accept that the displaced assaults happened earlier than they otherwise would have because there was a decrease in the proportion of calls for service between 3am and 6am.

The second possibility (i.e. that an actual decrease was offset by an increase in willingness to call police) has merit for two reasons. First, there was a lot of media publicity about alcohol-related crime in the months leading up to and following the onset of the restrictions. People who frequented the Newcastle CBD following the intervention may have had a heightened sensitivity to alcohol-related crime in the months following the intervention and this heightened sensitivity may have led to an increase in willingness to call for police assistance. Secondly, anecdotal reports suggest that many licensees feared that they would lose their licence if they were found to have high rates of assaults on their premises. In fact, the restrictions themselves may have made licensees more inclined to call for police assistance when presented with minor incidents on their premises. There is no way of testing the validity of either of these propositions but this latter explanation would also explain the increase in calls for service earlier in the night when premises were still permitted to sell alcohol.

While recorded crime data are always potentially susceptible to changes in reporting or recording biases, the evidence provided in the Appendix casts doubt on the validity of the third possible explanation for the discrepancy between CAD and recorded crime data. Most recorded crime incidents are reported either by the victim or another witness. It is not clear why either of these groups would have a greater incentive to stop

Figure 9. Temporal distribution of calls to assault or brawl incidents in the intervention and comparison sites prior to and subsequent to the intervention



reporting assaults to police following the introduction of the restrictions. Indeed, there was no evidence that any party changed their willingness to report assaults to police, nor was there evidence that police detected offences at a lower rate after the restrictions (see Appendix for details). As for changes in police recording practices, there was no evidence that a greater number of incidents were rejected, recorded as occurrence only or as personal violence incidents. There was also no evidence of any increases in other incident categories in the year following the restrictions. Therefore the only conceivable way that the decrease in assaults evident in the recorded crime and Alcohol Linking data could reflect a change in recording is if police simply stopped recording some assaults altogether. This seems to be an unlikely possibility because the COPS entry can be an important evidentiary component of any subsequent legal proceedings. It seems particularly unlikely that police would stop recording assaults linked to the hotels that had restrictions imposed but not other premises in the area. The fact that there were no increases in other offence categories also illustrates that a decrease in assaults in and around licensed premises need not lead to increases in other types of violence (e.g. domestic violence).

The final explanation is also potentially informative if CAD and recorded crime data capture different types of assaults (bearing in mind that Linking data are essentially a subset of recorded crime data). In other words, one of these data sources might act as a better proxy for actual rates of alcohol-related assault than the other. As a post hoc test of this hypothesis, the monthly number of CAD incidents recorded in the Newcastle LAC was correlated with the monthly number of COPS incidents recorded in that area. There was a modest correlation between the two measures over the entire study period (Spearman's $\rho = 0.427$, $p < 0.001$), which suggests that only 18 per cent of the variance in recorded crime can be explained by trends in calls for service. This leaves a lot of unexplained variance in recorded crime.

Furthermore, CAD data reveal that only around one per cent of incidents logged over the study period had an associated event recorded on the COPS database. It is highly likely, therefore, that the types of incidents recorded on each database are very different. However, at present, the nature of these differences is not well understood.

We are not currently able to identify which of these explanations, or which combination of explanations, accounts for the lack of any change in calls for service. However, the three most likely explanations all suggest that there was an actual change in assaults that was not manifested in the call-out data. We can therefore conclude with some certainty that there was evidence of a decrease in assaults following the onset of the restricted trading hours. A priority for future research should be to ascertain whether these reductions are sustained beyond the 12-month follow-up that we were able to observe.

One threat to causal inference is that we had no way of accounting for external factors that could also have influenced recorded crime in the CBD area. In the same month that the Newcastle intervention began, the Bureau publicly released a list of the top 100 hotels and clubs for assaults on licensed premises. Similarly, in mid-2008 the regulatory environment was significantly reshaped by the enactment of the Liquor Act 2007, and police and licensing authorities appear to have significantly increased their focus on liquor licensing activities as a result. For example, the NSW Police Force established a specific command – the Alcohol and Licensing Enforcement Command (ALEC) – to focus solely on liquor licensing enforcement. In October 2008, the Premier of NSW also announced that the top 48 hotels for assaults on licensed premises would be subject to more stringent licensing conditions. Rather than the LAB restrictions, it is possible that this negative media publicity and other licensing activity may have pressured hotels in the Newcastle CBD to more rigorously enforce responsible service of alcohol.

Similarly, even if the reductions were due to the restrictions imposed by the LAB, we cannot say for certain that it was the reduction in trading hours, the lockout, the other restrictions or simply better management practices that produced the effect.

Such risk of bias will always be a possibility in the absence of a randomised experimental research design. However, there are good reasons for believing that the effects observed here were, in fact, due to the restrictions imposed by the LAB and due to the restriction in trading hours in particular. Unlike most of the current literature, the present study had good quality baseline measures and a suitable comparison site. Critically, while the intervention site for the recorded crime analysis (postcodes 2300 and 2302) contained five hotels on the 'top 100' list released by the Bureau in March 2008, the comparison site (postcode 2303) also contained three licensed premises that were on that list. This representation of high-risk hotels in both the intervention and comparison sites suggests that the observed decreases cannot be attributed to adverse publicity alone. There is also not a lot of evidence to suggest that factors such as adverse publicity can bring about meaningful reductions in violence, whereas there is now a growing body of evidence showing that reducing the availability of alcohol can produce such effects (Chikritzhs & Stockwell 2007). Nevertheless, there would be value in observing other sources of data such as ambulance attendances or emergency department presentations to ascertain whether there were comparable reductions in those data. As to whether the reductions were due to the trading hours or the other aspects of the restrictions, the temporal shift in assaults towards earlier times in the evening strongly suggests that the reduced trading hours drove the observed reductions in violence.

One further cautionary note must be added in relation to the analyses based on Alcohol Linking Data. This data collection is based on a subjective attribution by a sworn police officer that

alcohol was involved in an incident. It is possible that some incidents were not attributed to alcohol where it was, in fact, involved and it is possible that there are missing data in this collection for other reasons as well. Perhaps more importantly, it is possible that some patrons would be reluctant to name particular licensed premises as the place where they had their last drink in order to protect the licensee from prosecution. These limitations are only likely to affect the current conclusions if there is reason to believe that this measurement error (a) differentially affected the pre- and post-intervention series and/or (b) differentially affected the hotels that had the restrictions as opposed to hotels that did not have any restrictions. There is no prima facie reason to suspect that either of these was apparent in the data.

In summary, there is sufficient evidence on the basis of the results reported here to encourage licensing authorities to adopt similar restrictions in other areas. Should such measures be implemented, causal interpretations of their effects would be greatly enhanced by also establishing a rigorous evaluation framework at the outset.

ACKNOWLEDGEMENTS

The authors gratefully acknowledge the assistance of Jim Baldwin and Param Sivasubramani from the NSW Police Force for their assistance with CAD and Linking data, and Derek Goh and Neil Marrot for provision of recorded crime data. We thank Amanda Mason for her assistance in coding the assault narratives, Maria Pavlou for her invaluable assistance with the literature review and Florence Sin for desktop publishing this report. We also thank Dr Don Weatherburn and our anonymous reviewers for their comments on an earlier draft of this paper.

NOTES

1. NSW Bureau of Crime Statistics and Research
2. School of Medicine and Public Health, University of Newcastle
3. Members of the (since abolished) Liquor Administration Board were Magistrates of the Licensing Court of New South Wales as well as of the Local Court of New South Wales. The Board were at that time responsible for granting and transferring liquor licences in New South Wales, as well as dealing with prosecutions and disciplinary complaints against licensees. Following the passing of the Liquor Act 2007, these responsibilities were assumed by the Casino, Liquor and Gaming Control Authority (CLGCA).
4. No ruling was made against one hotel on the basis that it was prohibited by development consent and strata title conditions from trading beyond midnight.
5. While 11 hotels were licensed to trade until 5am, the LAB noted that four of those hotels were not actually trading until that time on most weekends. However, the LAB ruling formally removed this discretionary arrangement.
6. The out-of-court agreement relaxed the lockout to 1:30am for all 11 appellant hotels and relaxed the closing times to 3am for the two appellant hotels that had been restricted to 2:30am closing and to 3:30am for the nine premises that had been ordered to close at 3am. The three hotels that did not appeal to the Licensing Court were subject to the initial LAB ruling until a review in the latter part of 2008 also relaxed their lockout and closing times by 30 minutes.
7. This does not necessarily mean that an offence took place, only that the entry on COPS was valid.
8. It is important to note that very few calls for police attendance that are logged on the CAD system result in a criminal incident being recorded on COPS. This is because, by the time police arrive, the incident has often already been resolved or the parties are no longer at the scene. Likewise, only a small proportion of incidents recorded on COPS would have a corresponding record in the CAD system because a car is only called to a small proportion of all recorded criminal incidents. In addition, the incident types recorded on CAD do not correspond exactly with the offence categories recorded on COPS because, at the time a call is received, the nature of the offence is often not clear.
9. Repeated Phillips-Perron tests for a unit root returned MacKinnon approximate p-values of <0.001 for each series on each outcome. The null hypothesis of non-stationarity was therefore rejected in each series, which allowed modelling to be undertaken in levels rather than differences.
10. While the autoregressive terms were not statistically significant in the adjusted models, there was evidence of serial autocorrelation in each of the unadjusted models and it was deemed more appropriate to use ARIMA models. As suggested by one of the reviewers, we also fit the same models using multiple linear regression with the monthly number of assaults in the control sites as covariates in the analyses. The Durbin Watson d-statistics were between 1 and 2 in each model, which suggests that MLR was also appropriate when seasonal terms were fitted to the models. Critically, the coefficients on the intervention term were indistinguishable when the two approaches were used.
11. Seasonality was adjusted for in two ways. The first was to fit a dummy variable that took the value one during the warmer months (November to April) and zero otherwise. The second was to fit dummy variables for each month, although one month is necessarily omitted from the model and each of the remaining months is compared against that reference

month. One or both of these seasonal adjustments were included in the final models if they were found to significantly improve the fit of the model (as indicated by higher log likelihood statistics).

12. For the Newcastle model, the log likelihood statistic for the unadjusted series was -218.5 (Wald $\chi^2_1 = 5.4$, $p=0.020$) and for the adjusted model the statistic was -206.5 (Wald $\chi^2_{14} = 23.5$, $p=0.053$). The p-value on the Portmanteau test for white noise at $Q(28) = 0.626$. For the Hamilton model, the log likelihood statistic for the unadjusted series was -158.1 (Wald $\chi^2_2 = 3.2$, $p=0.205$) and for the adjusted model the statistic was -147.5 (Wald $\chi^2_{16} = 25.7$, $p=0.058$). The p-value on the Portmanteau test for white noise at $Q(28) = 0.209$.
13. For the intervention series, the log likelihood statistic for the unadjusted model was -183.2 (Wald $\chi^2_2 = 12.1$, $p=0.002$) and for the adjusted model the statistic was -176.6 (Wald $\chi^2_{15} = 20.9$, $p=0.141$). The p-value on the Portmanteau test for white noise at $Q(28) = 0.605$. For the non-intervention premises, the log likelihood statistic for the unadjusted series was -193.5 (Wald $\chi^2_2 = 3.3$, $p=0.193$) and for the adjusted model the statistic was -176.7 (Wald $\chi^2_{16} = 97.0$, $p<0.001$). The p-value on the Portmanteau test for white noise at $Q(28) = 0.139$.
14. For the Newcastle model, the log likelihood statistic for the unadjusted series was -210.1 (Wald $\chi^2_1 = 1.6$, $p=0.214$) and for the adjusted model the statistic was -187.2 (Wald $\chi^2_{15} = 38.3$, $p<0.001$). The p-value on the Portmanteau test for white noise at $Q(28) = 0.648$. For the Waratah model, the log likelihood statistic for the unadjusted series was -181.1 (Wald $\chi^2_2 = 0.1$, $p=0.959$) and for the adjusted model the statistic was -161.7 (Wald $\chi^2_{15} = 89.8$, $p<0.001$). The p-value on the Portmanteau test for white noise at $Q(28) = 0.610$.
15. We thank our anonymous reviewer for pointing this out.
16. We thank our anonymous reviewer for pointing this out.

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APPENDIX

ASSESSMENT OF REPORTING BIAS

One threat to valid causal inference in the current study was that licensed premises staff may have become less inclined to report assaults to police over time through fear of having more severe restrictions imposed on their licence. In order to test this possibility, the narrative descriptions of all non-domestic assault incidents recorded as occurring in the intervention and comparison sites in the fourth quarters of 2007 and 2008 were coded to determine how the incident came to the attention of police. The assumption underpinning this analysis was that staff of licensed premises (or any other reporting entity) would report a smaller proportion of assaults over time if they had become less willing to report assaults.

Table A1 shows the number and proportion of incidents recorded in the last quarters of 2007 and 2008 broken down by the person who reported or detected the assault. It is clear that most recorded assaults (70%) are reported to police by the victim or by a member of the public. Smaller proportions are detected by police during routine patrols (8.8%), or reported by staff of licensed premises (7.6%), transport workers (3%) or by suspected offenders (0.9%). The likelihood that the reporting practices of licensed premises would influence the current results is therefore very low.

Table A2 breaks down the proportion of incidents reported by these different entities prior to and subsequent to the intervention. There was no significant decrease in the proportion of assaults reported or detected by these different groups following the start of the intervention. This analysis provides no evidence to suggest that the observed decrease in recorded incidents of assault was due to a change in willingness to report assaults to police.

ASSESSMENT OF RECORDING BIAS

Another threat to valid causal inference is that police changed the way they recorded assaults following the imposition of the restrictions. One possibility is that assaults could be recorded on COPS but not as ‘accepted and verified’ non-domestic assault incidents. One way would be for supervising officers to ‘reject’ a greater number of incidents as invalid COPS entries. Another way would be for attending officers to enter a greater number of incidents as ‘occurrence-only’ or ‘personal violence’ incidents. None of these incident categories are included in the Bureau’s recorded crime data because they are not considered by police to constitute criminal offences. Figure A1 shows trends in rejected, occurrence-only and personal violence incidents occurring between the hours of 10pm and 6am in the months before and after the intervention. There is no evidence of any increase in any of these incident categories in the year following the LAB restrictions.

Table A1. Number and proportion of non-domestic violence related assault incidents recorded as occurring in postcodes 2300, 2302 and 2303 during the fourth quarter of 2007 and the fourth quarter of 2008 (combined)

<i>Reported / detected by...</i>	<i>N (%)</i>
Victim	129 (39.2)
Public / other witness	102 (31.0)
Police	29 (8.8)
Licensed premises staff	25 (7.6)
Transport worker	10 (3.0)
Suspected offender	3 (0.9)
Unknown	31 (9.4)
Total	329 (100.0)

Table A2. Number and proportion of non-domestic violence related assaults recorded in postcodes 2300/2302 (Newcastle) and 2303 (Hamilton), by person reporting the assault, location of the incident and year (Q4 of 2007 or Q4 of 2008)

<i>Location / time period</i>	<i>Reported / detected by...</i>				
	<i>LP staff N (%)</i>	<i>Police N (%)</i>	<i>Victim N (%)</i>	<i>Other N (%)</i>	<i>Unclear N (%)</i>
Newcastle					
Q4 2007	13 (9.9)	16 (12.2)	51 (38.9)	40 (30.5)	11 (8.4)
Q4 2008	7 (7.7)	6 (6.6)	35 (38.5)	35 (38.5)	8 (8.8)
$\chi^2_4=3.0, p=0.554$					
Hamilton ^a					
Q4 2007	4 (8.2)	1 (2.0)	19 (38.8)	20 (40.8)	5 (10.2)
Q4 2008	1 (1.7)	6 (10.3)	24 (41.4)	20 (34.5)	7 (12.1)
$\chi^2_4=5.6, p=0.234$					

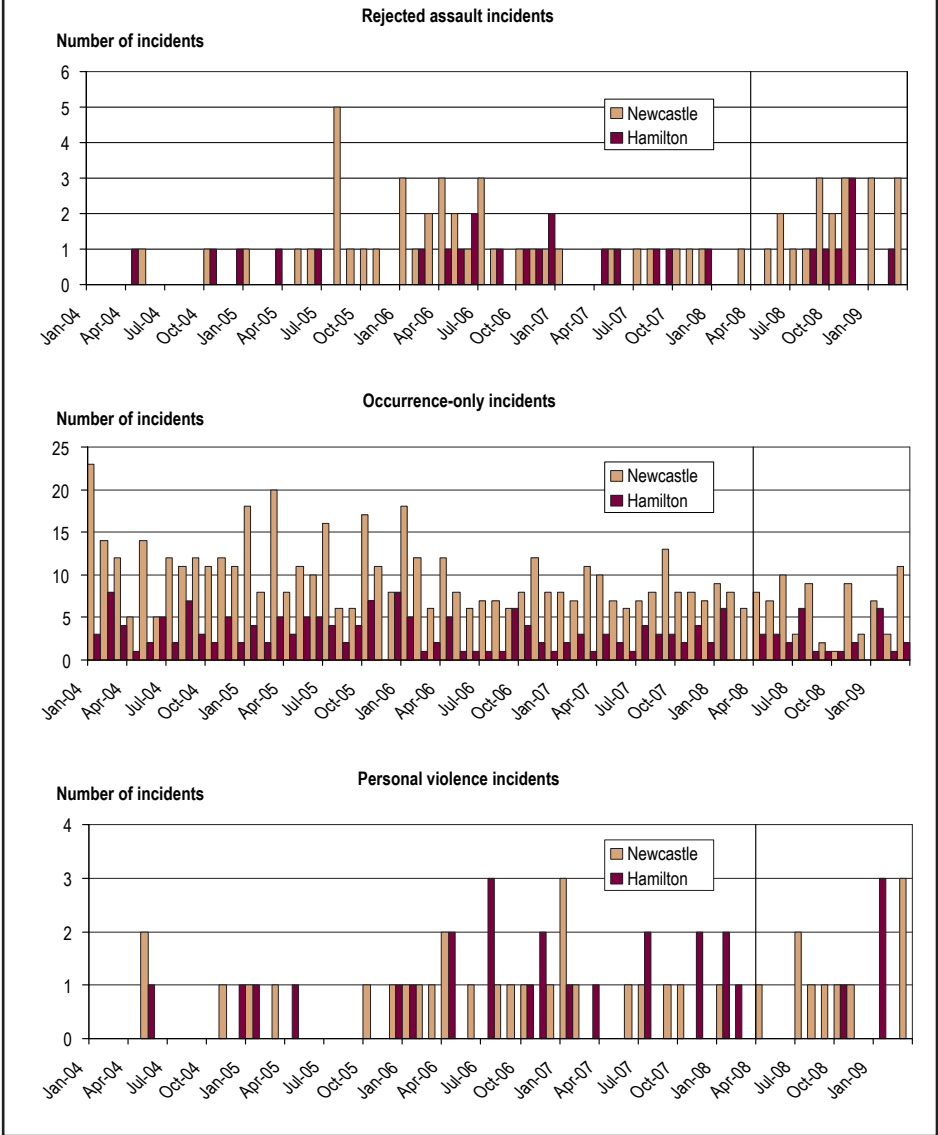
^a Caution should be used when interpreting the results of this chi-square test as four cells had very low counts

IMPACT ON OTHER OFFENCE CATEGORIES

It is possible that the LAB restrictions may have had an unintended impact on other offence categories. For example, if the effect of the intervention were to encourage people to purchase off-licence alcohol and drink at home, this may put upwards pressure on rates of domestic violence.¹⁶ It is also possible that the intervention could have had an impact (adverse or otherwise) on levels of disorderly conduct, property damage and other offences that are highly related to alcohol consumption (e.g. Stevenson 1996).

Table A3 summarises the annual number of night-time assault, malicious damage and disorderly conduct incidents recorded in the Newcastle area prior to and subsequent to the LAB restrictions. There is little evidence to suggest that any other offence categories increased in the year following the intervention. There was a slight increase in the number of domestic violence related assault incidents recorded in the Waratah sector, from a high of 82 in 2006-07 to 100 in 2008-09. However, after adjusting for the underlying trend in assault and for seasonality, ordinary least squares regression indicated that this increase was not statistically significant (p=0.091).

Figure A1. Trends in the number of rejected, occurrence-only and personal violence incidents in the Newcastle CBD (postcodes 2300/2302) and Hamilton (postcode 2303)



The only other incident category to increase was malicious damage in the Waratah sector, although these incidents also increased across the entire state over this time period (NSW Bureau of Crime Statistics & Research 2009).

Table A4 summarises the annual number of assault, traffic, malicious damage and other incidents linked to the intervention hotels under the Alcohol Linking Program. If anything, the intervention would appear to have had an ancillary impact on traffic and other offence categories as opposed to an adverse impact on other offence categories.

Table A5 summarises the annual number of calls to attend night-time incidents of assault/brawl, domestic, hoodlum, intoxicated persons, malicious damage and other incidents in the Newcastle and Waratah police sectors. The only incident category that showed any sign of increasing was the domestic incidents in both the Newcastle and Waratah sectors. Figure A2 suggests that this increase was due to a higher number of calls in the latter part of 2008. Fitted ARIMA models revealed that, after adjusting for seasonality and the number of weekends in the month, these apparent increases were not statistically significant ($p=0.361$ and $p=0.632$ for the Newcastle and Waratah series, respectively).

Figure A2. Number of calls to night-time domestic incidents in the Newcastle and Waratah police sectors

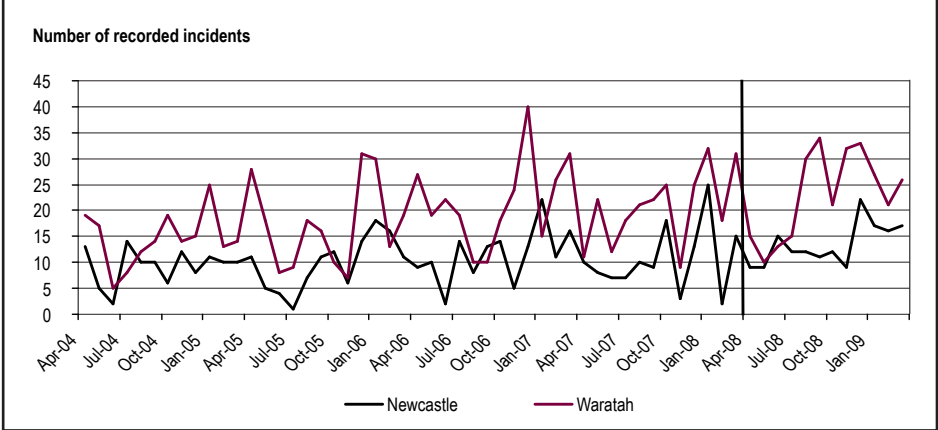


Table A3. Annual number of night-time non-domestic violence (DV) related assault, DV related assault, assault police, malicious damage and disorderly conduct incidents recorded in the Newcastle area prior to and subsequent to the intervention

Location / offence	Pre				Post
	Apr-04 to Mar-05	Apr-05 to Mar-06	Apr-06 to Mar-07	Apr-07 to Mar-08	Apr-08 to Mar-09
Newcastle CBD					
Non-DV assault	376	414	445	361	258
DV assault	29	33	31	28	19
Assault police	13	15	9	23	19
Total assault ^a	418	462	485	412	296
Malicious damage	411	272	244	241	263
Disorderly conduct	154	127	189	412	224
Hamilton					
Non-DV assault	110	96	107	94	103
DV assault	27	28	23	28	18
Assault police	1	4	2	5	8
Total assault ^a	138	128	132	127	129
Malicious damage	86	82	89	102	100
Disorderly conduct	26	36	26	60	59
Rest Newcastle/City sector					
Non-DV assault	80	76	78	86	81
DV assault	36	46	51	39	34
Assault police	8	5	4	4	3
Total assault ^a	124	127	133	129	118
Malicious damage	150	158	184	159	159
Disorderly conduct	25	36	23	41	43
Total Newcastle/City sector					
Non-DV assault	566	586	630	541	442
DV assault	92	107	105	95	71
Assault police	22	24	15	32	30
Total assault ^a	680	717	750	668	543
Malicious damage	647	512	517	502	522
Disorderly conduct	205	199	238	513	326
Waratah sector					
Non-DV assault	127	115	129	164	152
DV assault	81	68	82	73	100
Assault police	11	10	7	7	6
Total assault ^a	219	193	218	244	258
Malicious damage	320	388	345	420	506
Disorderly conduct	58	63	46	45	68
Newcastle LGA					
Non-DV assault	693	701	759	705	594
DV assault	173	175	187	168	171
Assault police	33	34	22	39	36
Total assault ^a	899	910	968	912	801
Malicious damage	967	900	862	922	1028
Disorderly conduct	263	262	284	558	394

^a Note: Total assault is the sum of non-domestic assault, domestic assault and assault police incidents

Table A4. Annual number of assault, traffic, malicious damage and other incidents linked to restricted licensed premises and other Newcastle licensed premises prior to and subsequent to the intervention

<i>Location / offence</i>	<i>Pre</i>				<i>Post</i>
	<i>Apr-04 to Mar-05</i>	<i>Apr-05 to Mar-06</i>	<i>Apr-06 to Mar-07</i>	<i>Apr-07 to Mar-08</i>	<i>Apr-08 to Mar-09</i>
Intervention hotel					
Assault	157	231	218	197	129
Traffic	190	238	213	211	166
Malicious damage	35	38	27	18	22
Other	285	347	449	611	326
<i>Total</i>	<i>675</i>	<i>858</i>	<i>913</i>	<i>1043</i>	<i>653</i>
Other Newcastle licensed premises					
Assault	241	219	251	258	268
Traffic	447	459	358	507	439
Malicious damage	59	52	59	55	51
Other	385	425	473	565	520
<i>Total</i>	<i>1132</i>	<i>1155</i>	<i>1141</i>	<i>1385</i>	<i>1278</i>

Table A5. Annual number of calls to night-time assault/brawl, domestic, hoodlums, intoxicated persons, malicious damage and other incidents in the Newcastle and Waratah police sectors prior to and subsequent to the intervention

<i>Location / offence</i>	<i>Pre</i>				<i>Post</i>
	<i>Apr-04 to Mar-05</i>	<i>Apr-05 to Mar-06</i>	<i>Apr-06 to Mar-07</i>	<i>Apr-07 to Mar-08</i>	<i>Apr-08 to Mar-09</i>
Newcastle/City					
Assault/brawl	293	342	385	331	381
Domestic	111	116	137	127	161
Hoodlums	147	209	219	176	121
Intox. persons	176	213	230	197	171
Malicious damage	84	135	104	95	124
Other	2762	3207	3469	3431	3204
<i>Total</i>	<i>3573</i>	<i>4222</i>	<i>4544</i>	<i>4357</i>	<i>4162</i>
Waratah					
Assault/brawl	124	115	150	168	162
Domestic	175	207	261	246	277
Hoodlums	154	188	223	230	170
Intox. persons	96	144	139	142	103
Malicious damage	54	46	102	93	81
Other	2851	2962	3270	3344	2889
<i>Total</i>	<i>3454</i>	<i>3662</i>	<i>4145</i>	<i>4223</i>	<i>3682</i>

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