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Mats Ekman & Niklas Jakobsson

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RESEARCH ARTICLE

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The impact of earlier pub closing hours on emergency calls to the police during the COVID-19 pandemic in Sweden

Mats Ekman and Niklas Jakobsson

Karlstad Business School, Karlstad University, Karlstad, Sweden

ABSTRACT

On 20 November 2020, the government of Sweden banned on-premise alcohol sales after 10:30 p.m. and then after 8 p.m. on December 24. This study aims to estimate the impact of earlier pub closing hours on emergency calls to the police. We use a quasi-experimental hybrid differences-in-differences design, drawing on data for emergency calls in Sweden. The primary outcome measure is the daily number of emergency calls to the police in Sweden 70 days before the intervention and 70 days after the intervention. The primary control series is the daily number of emergency calls to the police in Sweden during the preceding year, 70 days before the intervention and 70 days after the intervention. We fail to find an effect on daily emergency calls, or nighttime emergency calls to the police, from the restrictions on the sale of alcohol. There is, however, some evidence indicating that weekend emergency calls may have been affected, but that potential effect does not translate into an overall effect. While our study is limited in its focus, it contributes to using a wide range of time windows and a large geographical area (the whole of Sweden) to inform on displacement effects, as well as in considering a broader set of robustness checks. We suggest that our results and future work should be seen in light of our limitations and our contribution, respectively.

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Alcohol policy; closing hours; natural experiments; crime

1. Introduction

Crime related to drinking in pubs and bars is a well-known problem. To the extent that inebriation reduces decision-making abilities and inhibitions, restrictions on alcohol sales should decrease alcohol consumption and thus crime. On the other hand, to the extent that criminal activity is the result of a rational weighing of pros and cons (Becker 1968), alcohol on its own does not impact crime (but might increase it if alcohol and crime are complements). On 20 November 2020, the government of Sweden restricted on-premise alcohol sales to before 10:30 p.m. and 8 p.m. on December 24, as a way to combat the spread of COVID-19. This study estimates the impact of earlier closing hours on crime as measured by emergency calls to the police. In so doing, it joins a growing number of studies exploiting the impacts of non-pharmaceutical interventions (NPIs) on non-Covid outcomes, such as Mulligan and Arnott (2022), who examine death certificates to argue that NPIs made it more costly to maintain health and that this change is responsible for at least a part of 171 000 excess deaths in the US between early 2020 and late 2021. Hansen et al. (2022) use variations in the timing of suicides over the academic year, in combination with the disruptions to in-person schooling in 2020 and 2021, to estimate that suicides occur more frequently with in-person schooling. An advantage of studying non-Covid outcomes due to NPIs is that the NPI is unlikely to be motivated by a desire to change the outcome measure, making exogeneity more plausible.

There are some quasi-experimental studies on the effects of on-premise alcohol trading hours on crime, physical violence and other outcomes thought to be affected by the consumption of alcohol. Duffy and Pinot de Moira (1996) study a one-hour extension of pub opening hours in England and Wales in 1988 and find no effect on violence, with Scotland as a control. Green et al. (2014) study the same intervention, finding a reduction in traffic accidents. Chikritzhs and Stockwell (2002) find increases in violence following a one-hour extension of on-premise openings in Perth, Australia, with pubs not getting a prolonged permit as controls. Kypri et al. (2011) find reductions in violence in the Central Business District in Newcastle, Australia, following a 2008 up-to-two-hour reduction in on-premise opening hours, with the bordering district of Hamilton as control. Rossow and Norström (2012) find more violence following small increases in opening hours in 18 Norwegian cities from 2000–2011. Additionally, a positive alcohol-violence correlation is the predominant finding in recent systematic reviews (Wilkinson et al. 2016; Sanchez-Ramirez and Voaklander 2018; Nepal et al. 2020). In a recent study, Gerell et al. (2022) find that the COVID-19 ban on selling alcohol in pubs and restaurants in Oslo, Norway, reduced

CONTACT Niklas Jakobsson niklas.jakobsson@kau.se Karlstad Business School, Karlstad University, Karlstad 651 88, Sweden

reported crime, using an interrupted time series design. However, banning sales at midnight increased crime, while they find no effect on crime by banning sales after 9 p.m.

Norström et al. (2018) study the effect of a one-hour increase in nightclub opening hours in Visby, Sweden, a popular tourist destination. This is the only quasi-experimental study that finds large reductions in violence following extended on-premise hours. The authors discuss two possible reasons for their findings. First, the extension was coupled with measures that curb alcohol-related violence: responsible-server training, cooperation between restaurants and police, and the nightclub owners' knowledge that they were evaluated for the possibility of a prolonged extension. Second, extended opening hours were not granted to all restaurants, which may have led to less congestion in the streets. A differences-in-differences study on alcohol availability and crime in Sweden by Grönqvist and Niknami (2014) utilizes the fact that certain regions of Sweden were testing grounds for introducing Saturday opening hours at the Swedish alcohol monopoly. Examining criminal sentencing, the authors find that greater alcohol availability increases crime in general, but has no impact on violent crime in particular.

While most evidence indicates that restricting on-premise alcohol sales decreases crime and violence, a concern is that most studies do not consider the overall effect of on-premise opening hours. Most studies show changes in crime or violence in affected smaller areas and during certain hours, but they cannot say if there is an overall effect; crime may be merely displaced, rather than removed. Spatial and intertemporal substitution may also combine to affect reporting, e.g. violence at home may not be reported as quickly or frequently as street violence. Moreover, many studies test multiple hypotheses or specifications, such as the effect of drinking on various types of crime, yet fail to correct their confidence intervals for this fact or even discuss the issue, despite the fact that the likelihood of false positives rises with the number of specifications. The borderline-significant effects on crime that we find are statistically indistinguishable from zero with a Bonferroni correction. A further shortcoming of previous research is the lack of control for weather conditions. While it remains uncertain whether this effect comes from heat-induced social interaction or aggression, ambient temperature has been found to be positively (and mostly linearly) correlated with violent crime in numerous studies (e.g. Card and Dahl 2011; Tiihonen et al. 2017; Mišák 2022). Any study of restricted alcohol availability is sensitive to confounding effects due to weather to the extent that its observations are collected at different times between which the weather might change.

2. Method and materials

We adopt a hybrid differences-in-differences design in which the outcome measure is the daily number of emergency calls to the police in Sweden before and after the restrictions on on-premise alcohol sales (it is a *hybrid* DiD because the treated group and the control group are temporally separated, *cf.* Nazif-Munoz et al. 2020). We use such calls exactly one year earlier as our control. The identifying assumption is that the trend in the outcome variable would have been the same as the trend in the control, without treatment, after controlling for confounding variables.

On-premise alcohol sales were banned in Sweden after 10:30 p.m. on 20 November 2020 and after 8 p.m. on 24 December 2020. Guests needed to leave the premises 30 min after those times (SFS 2020). Before the intervention, alcohol could be served until at least 1 a.m., with some municipalities permitting sales until 5 a.m. (SFS 2010). Compared to previous research, we study a big change in on-premise alcohol sales. The intervention was in response to the COVID-19 pandemic, and already before its implementation, bars and restaurants were supposed to follow recommendations regarding social distancing (Public Health Agency of Sweden 2020). With cases rising throughout the autumn, the pandemic itself also implied that fewer people may have frequented bars and restaurants, even before the restrictions on alcohol sales (Goolsbee and Syverson 2021). At the time of the first restriction, there were over 30,000 weekly COVID-19 cases, and around 40,000 by the time of the second restriction (Public Health Agency of Sweden 2021) (see Figure 1). This number then declined rapidly in January, going below 25,000 by the third week. To the extent that these figures influence people's willingness to drink alcohol and socialize, this willingness is reduced before the restrictions and increased afterwards, so voluntary changes may take away some of the effects of the restrictions. To account for this mechanism, we control for weekly COVID-19 cases in all estimations.

The primary outcome measure is the daily number of emergency calls (we do not have information about the exact nature of these emergencies) to the police in Sweden 70 days before the intervention (11 September to 19 November 2020), and 70 days after (24 December 2020 to 3 March 2021). The primary comparison is the daily number of emergency calls to the police during the preceding year, 70 days before the intervention (11 September to 19 November 2019), and 70 days after (24 December 2019 to 2

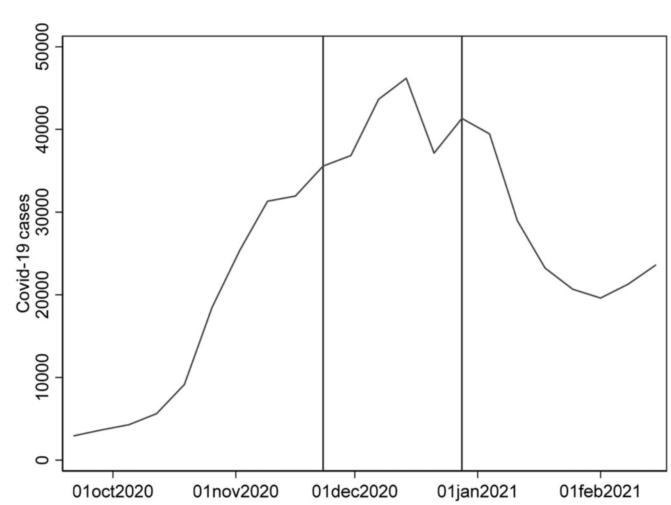

Figure 1. Weekly number of covid-19 cases in the treatment period. Vertical lines indicate the implementation of restrictions.

March 2020). With this outcome variable, we assess if there is an overall effect of the intervention on emergency calls to the police. As secondary outcome measures we include only nighttime data, and only weekend data, in order to assess if there are effects on emergency calls where they might be most expected. Since crime and temperature are correlated, a control variable is the average daily temperature, obtained from the Swedish Meteorological and Hydrological Institute. Since our emergency-call data are at the national level, the temperature data are population-weighted by the three major cities in Sweden: Stockholm, Gothenburg, and Malmö (more than 85% of all Swedes live within 250 km (155 miles) of these three cities and 40% live within these cities).

We use hybrid differences-in-differences regressions to estimate the effect of closing hours on emergency calls using OLS (Nazif-Munoz et al. 2020). The estimated regression is

$$Y_{ip} = \alpha + \beta_0 \mathrm{Treated}_{ip} + \beta_1 \mathrm{Post}_{ip} + \beta_2 \mathrm{Treated}_{ip} \times \mathrm{Post}_{ip} + \mathbf{X}_{ip} + \varepsilon_{ip},$$

where Y_{ip} is the number of emergency calls to the police in group i (the time-period during which treatment occurred or the time-period one year earlier), before or after the intervention period p. α is the constant term, the variable $\mathrm{Treated}_{ip}$ is a dummy for the treated and equals 1 for the outcome series and 0 for the control series. Post_{ip} is a dummy that equals 1 in the intervention period, and 0 pre-intervention, $\boldsymbol{X}_{ip}$ is relevant group-specific controls, and ε_{ct} is an error term. Given the identifying assumptions, the estimate of β_2 is the impact of the policy change on emergency calls to the police. All regressions also include controls for the week of the year and weekday, as well as the number of weekly COVID-19 cases, and the temperature. Since we have data for many days both before and after the policy change, we follow Bertrand et al. (2004) to address serial correlation in differences-in-differences analyses by collapsing the data in pre- and post-period to produce consistent standard errors. Additionally, we control for week of the year and day of week effects in all regressions, to handle seasonality.

We run a total of six different specifications, the first one (1) including 70 days before the first restriction and 70 days after the second restriction, in total 140 days in the treatment year and 140 days in the control year. In the second one (2), we use the same time window but only include emergency calls from 6 p.m. to 6 a.m.; in (3) we use the same time window but only include emergency calls during weekends. In specifications 4–6 we use the same outcomes as in specifications 1–3 but a time window of 35 days before and after.

3. Results

The top panel of Figure 2 shows the treatment period; the number of daily emergency calls to the police and when the restrictions were implemented, illustrated by the first vertical line on 20 November 2020. It also shows the period between the two restrictions (November 20 to December 23), and the period after, to the right of the second vertical line. The second panel shows the same period the year before, without restrictions (the control period). The tops mark weekends and holidays and the troughs weekdays. There is a clearer downward trend in emergency calls in 2020 than in 2019, indicating that the COVID-19 pandemic may have decreased emergency calls.

Table 1 presents the results of the hybrid differences-in-differences estimations, with the corresponding period the year before as control, following Eq 1. In Column 1, daily emergency calls to the police fell by 29, a non-statistically significant decrease of 1.6% (28.934/(1845.542–47.191) ≈0.016). Column 2 includes only emergency calls from 6 p.m. to 6 a.m., and the non-statistically significant point estimate implies a small increase in calls (2.7%). Column 3 includes only weekends. Even though the point estimate is not statistically significant at the 5% level, it indicates a decrease in calls of 10.1%. In Columns 4–6 the window is 35 days, and the point estimate for the weekend estimation (6) implies a 13.0% decrease in calls, statistically significant at the 5% level. However, the test for parallel trends is rejected for the weekend data, so the estimate must be interpreted with care. Overall, we fail to find an effect on emergency calls to the police, and nighttime emergency calls, but we find an effect on weekend emergency calls. However, since we have tested several specifications, a marginally statistically significant measure in one specification should not be seen as strong evidence for an effect; imposing a Bonferroni correction eliminates the statistical significance of weekend emergency calls on crime.

4. Discussion

The main finding is that the restrictions of on-premise alcohol sales were associated with no change in the number of emergency calls to the police. The only non-zero effects that our results indicated come from specifications including only weekend emergency calls. Most previous studies that find a reduction in violence from restricted alcohol sales restrict the outcome variable to a few hours during the evening or night, and/or to a small area around certain locations (e.g. Chikritzhs and Stockwell 2002; Kypri et al. 2011; Rossow and Norström 2012; Norström et al. 2018). That is preferable if one wants to study the direct effect of violence around pub closing hours, but it is also important to know if there is an overall effect on crime. If the outcome is restricted to a small geographical area, there is a possibility that violence moves from one spot to another. For example, in the studies by Chikritzhs and Stockwell (2002), and by Kypri et al. (2011), there may be displacement effects drawing violence from pubs with no extensions to pubs with extensions, since the treated areas are located close to the control areas. The problem is similar if one restricts the hours of the day during which one measures the outcome; violence may move to other times of the day. Spatial and intertemporal substitution may also combine in ways that affect reporting, e.g. violence at home may not be reported as quickly or frequently as street violence. Since we are considering restrictions implemented country-wide and use data covering all 24 h of the day, we minimize the problems with

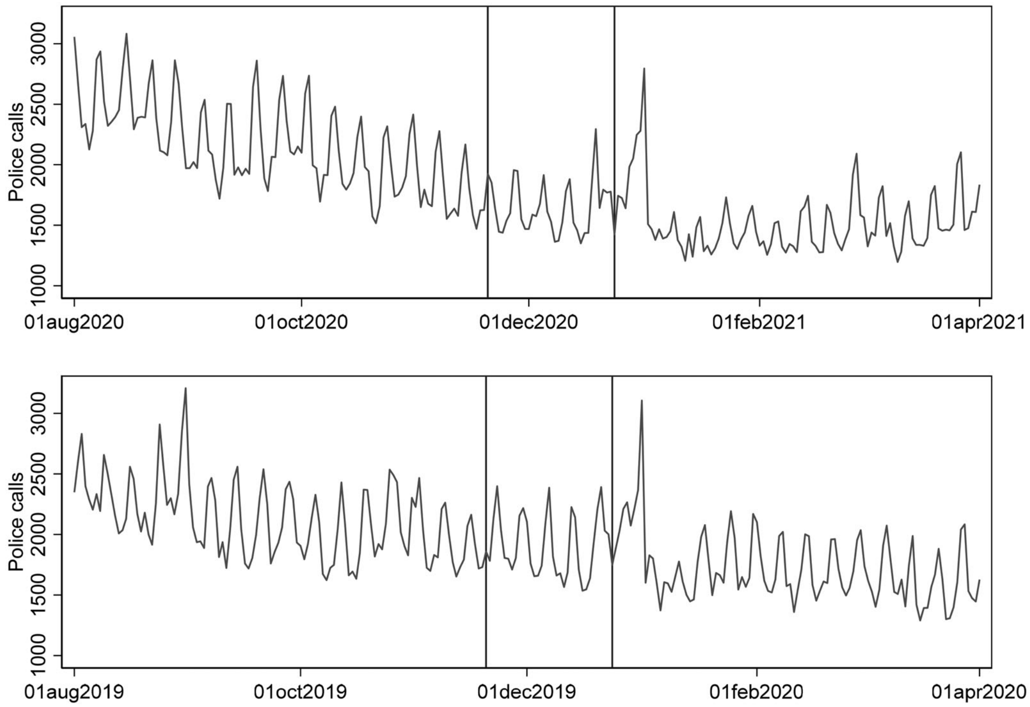

Figure 2. Daily emergency calls to the police in the treatment period (top) and the year before (below). vertical lines indicate the implementation of restrictions.

Table 1. Differences-in-differences estimations of emergency calls, a year before were the control series.

	1 140 days	2 140 days, night	3 140 days, weekend	4 70 days	5 70 days, night	6 70 days, weekend
Treated	47.191	44.390	27.727	−183.726	−98.916	−283.302*
	(43.215)	(35.915)	(61.291)	(108.416)	(92.243)	(123.575)
Post	9.455	−11.407	63.464	47.581	250.165	−58.217
	(61.926)	(53.273)	(105.370)	(159.705)	(150.971)	(200.291)
Treated × Post	−28.934	31.606	−167.960	−54.533	5.705	−273.470*
	(54.197)	(45.613)	(89.876)	(74.209)	(66.332)	(104.907)
Constant	1845.542**	1224.353**	1685.239**	1772.729**	938.620**	1816.145**
	(146.265)	(139.378)	(226.311)	(75.801)	(66.703)	(92.927)
Temperature	16.122**	13.540**	15.334*	13.845*	11.911	4.428
	(4.430)	(3.656)	(7.238)	(6.962)	(5.940)	(8.729)
COVID-19	−7.667**	−5.422*	−3.160	0.684	−0.111	9.446
	(2.802)	(2.589)	(3.960)	(5.000)	(4.493)	(6.499)
R^2	0.772	0.728	0.826	0.640	0.593	0.750
Parallel trends *p*	0.068	0.060	0.063	0.365	0.318	0.005
N	280	280	120	140	140	60

Regressions include indicators for week, and day of the week as control variables. Heteroscedasticity-robust standard errors in parentheses. $^{**}p < 0.01$, $^{*}p < 0.05$ without correcting for multiple specifications. Columns 1 and 4 include data covering the full 24 h of each day, Columns 2 and 4 include only nighttime data, 5 p.m. to 6 a.m., and Columns 3 and 6 include only data for Fridays, Saturdays and Sundays.

substitution. Indeed, substitution (from consumption of alcohol bought at pubs to consumption of alcohol bought elsewhere) may be a reason for our findings. There may be fixed costs to substituting alcohol consumption away from restaurants and bars (by arranging parties at home), so that small restrictions on the hours of sale reduce consumption (as in Rossow and Norström 2012) because the fixed costs are only worth incurring for large restrictions. Our results are similar to Gerell et al. (2022) who study several restrictions on alcohol sales during the COVID-19 pandemic in Oslo. They find no effects on the crime of banning sales after 9 p.m., which is close to the sales ban of the Swedish restrictions, but clear effects of banning sales altogether.

While we attempt to control for the effect of the pandemic, it is important to exercise caution when generalizing our results to non-pandemic circumstances. The Swedish government implemented a host of restrictions on large gatherings and issued advice on physical distance in 2020. Persons likely to be the cause of emergency calls to the police may also be likely to be younger and less heedful of government-issued advice, so it is not clear that the non-pharmaceutical interventions should significantly impact how these individuals behave. However, we cannot know this and our attempts to control the spread of the virus may fail to capture all the ways in which the pandemic may have affected on-premise drinking habits.

Our outcome measure includes all emergency calls directed to the police. These calls include a wide range of incidents related to crime; thus, our measure is crude. There are also benefits to our measure since it includes calls related to other issues that may be related to on-premise alcohol sales, such as disturbing noise in the streets, and crime-related activities that would not necessarily be captured if we used reported crimes as the outcome variable. As Grönqvist and Niknami (2014) show, other types of crime than violence may very well be affected by more alcohol availability. Future research should use individual-level data from crime registers to elaborate on the findings in this article.

5. Conclusion

The restrictions on on-premise alcohol sales are associated with a small (–1.6% to 2.8%) and not statistically significant effect on daily emergency calls to the police. There are, however, indications of an effect on emergency calls during weekends of up to 13%, but that potential effect does not translate into an overall effect of the restricted on-premise sales. In considering country-wide restrictions and using data covering all 24 h of the day, we complement previous research by controlling for weather conditions and minimizing the problems with spatial and temporal substitution of crime. Our findings indicate that it is important to consider the effects of alcohol sales on crime and violence using a wide range of time windows and a large geographical area in order to inform on displacement effects, as well as to use a broader set of robustness checks.

Ethical approval

The research in this paper does not require ethics board approval.

Disclosure statement

No potential conflict of interest was reported by the author(s).

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