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Abstract

Excessive alcohol use is associated with a wide range of adverse outcomes that inflict large societal costs. This paper investigates the impacts of increases in regulated opening hours of Swedish alcohol retailers on alcohol purchases, health and crime outcomes by relating changes in these outcomes in municipalities that increased their retail opening hours to those in municipalities whose opening hours remained unchanged. We show that extended opening hours led to statistically and economically significant increases in alcohol purchases by around two percent per weekly opening hour, but find no corresponding increases in adverse outcomes related to the consumption of alcohol. We study potential mechanisms, such as consumption spillovers and on and off-premise substitution, and we discuss policy implications of our findings.

Keywords: alcohol policy; alcohol availability; health effects; crime
JEL classification: I12; I15

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1 Introduction

Excessive consumption of alcohol is a major public health problem that causes a significant burden of disease and substantial economic and societal costs. It has been estimated that 4–6% of all ill health and premature death worldwide is due to alcohol consumption (WHO, 2019). Excessive use of alcohol increases mortality (Koski *et al.*, 2007; Bhattacharya *et al.*, 2013) and is linked to a wide range of health conditions and diseases (Rehm *et al.*, 2009). Alcohol consumption has also been related to many adverse economic and social outcomes, such as workplace accidents (Bassols and Castello, 2018), absenteeism (Johansson *et al.*, 2014; Green and Navarro Paniagua, 2016), crime (Chikritzhs and Stockwell, 2002; Dualibi *et al.*, 2007; Hough and Hunter, 2008; Biderman *et al.*, 2010; Humphreys and Eisner, 2010; Carpenter and Dobkin, 2010), motor vehicle accidents (Smith, 1990; Vingilis *et al.*, 2005; Carpenter and Dobkin, 2011; Green *et al.*, 2014) and risky sexual behavior (Kerr *et al.*, 2015). The US Center for Disease Control estimated the total cost of excessive alcohol use in the country to be a quarter trillion dollars each year, or about US$807 per person (Sacks *et al.*, 2015).[1]

This paper exploits regional variation in mandated opening hours among alcohol retailers in Sweden to estimate the causal effect of extending availability of alcohol on a set of policy-relevant outcomes. Due to the Swedish nationwide retail monopoly for strong alcoholic drinks[2], so-called *Systembolaget*, the institutional context avoids several econometric issues, such as policy non-compliance or mis-measurement of alcohol sales, which may otherwise lead to biased effect estimates, without necessarily sacrificing external validity. Moreover, the empirical variation in retail opening hours is large; between 2008 and 2015, more than half of all alcohol retailers in Swedish municipalities extended their weekly opening hours, with 19% of municipalities increasing between two and eight hours per week. We first show that the extended opening hours had both economically and statistically significant impacts on alcohol sales. We subsequently explore whether these effects were associated with changes in adverse outcomes generally related to alcohol consumption. Finally, we consider a set of robustness and sensitivity tests to gauge the stability of our findings with respect to model specification, estimator and effect heterogeneity.

The economic literature that explores intended and unintended consequences of alcohol policies include those investigating the effects of increasing the number of establish-

[1] The costs of alcohol abuse in Sweden – the setting for our analysis – are relatively large and have been even larger historically, which is one of the main reasons for having an alcohol monopoly (see Section 2). A government inquiry estimated the total costs of alcohol abuse to be 66 billion Swedish crowns annually, equivalent to roughly 7 billion Euros (or approximately 1.6% of GDP; Stanens Offentliga Utredningar, 2011). A more recent analysis commissioned by the Swedish alcohol monopoly (2017) estimated a total cost of 107 billion Swedish crowns (2.1% of GDP; Systembolaget, 2019). This implies a cost for Sweden that is approximately similar to that in the US.

[2] This includes all drinks stronger than 3.5% by volume

ments licensed to sell alcohol (Anderson *et al.*, 2018) and changing the price of or tax on alcohol (Mohler-Kuo *et al.*, 2004; Helakorpi *et al.*, 2008; Mäkelä *et al.*, 2010; Gehrsitz *et al.*, 2020). Studies that explore the effects of changes in opening hours of alcohol retail outlets mainly relate to the Nordic countries. Norström and Skog (2005) investigate the effects of allowing alcohol retail outlets to open on Saturdays on assaults and drink driving, and Ben-David and Bos (2020) study the effects of the same policy on the credit behavior of low-income households. Our paper differs from these in that we look at *marginal* changes in opening hours that vary across municipalities, rather than a uniform increase of a full day that affects all outlets across the country. This feature allows us to explore the effect of marginal changes in opening hours along several novel dimensions, such as heterogeneity in demand by time and day and to analyze spillover effects across neighboring municipalities, to provide evidence on possible behavioral responses underlying our estimated effects.

Our results suggest that a one hour increase in weekly opening hours increases annual per capita purchases of strong alcohol in the municipality by 2–3%, depending on alcohol type. These estimates imply an elasticity of demand with respect to opening hours of between 0.85–1.11, significantly higher than other estimates reported in the literature (see, e.g., Pryce *et al.*, 2019). However, we show that this difference is mainly driven by cross-border substitution. Indeed, taking this into account, the elasticity of demand reduces to 0.21–0.47, similar to the literature as well as Euromonitor International's total price elasticity of demand for alcoholic drinks.[3]

Hence, even when accounting for cross-border substitution, we find a significant increase in alcohol purchases in response to longer opening hours of alcohol retailers. Despite this, we find no evidence of corresponding impacts on a wide range of alcohol-related incidents, such as motor vehicle accidents, criminal charges from assault or drink-driving or alcohol-related hospital admissions and mortality. These findings are robust to the inclusion of potential confounding factors, including county-level trends, municipality-level trends in pre-determined socioeconomic and demographic characteristics, and to the choice of treatment effect estimator; difference-in-differences or synthetic control.

Our research speaks to a large literature that aims to evaluate the effects of alcohol policy on alcohol consumption as well as on individuals' health and social outcomes. A systematic review concluded that legislative measures and policies that regulate the environment in which alcohol is marketed (e.g., price and availability) are effective in reducing alcohol-related harm, while policies that provide education or information are less effective (Anderson *et al.*, 2009). For example, Norström and Skog (2003, 2005) find an increase in alcohol sales in response to lifting the ban on Saturday sales in 2000 for some areas in Sweden, with no changes to alcohol availability in the control areas. Stehr (2007) finds an increase in liquor purchases following the lifting of the ban on Sunday

[3]See https://blog.euromonitor.com/price-elasticities-in-alcoholic-drinks/.

sales in the USA, and Carpenter and Eisenberg (2009) exploit a liberalization of Sunday sales in Ontario, showing a substitution away from drinking on Saturdays to increased drinking on Sundays, but with no changes in overall drinking.

Individuals' time consistency potentially plays an important role in their alcohol consumption. Indeed, forward-looking consumers can buy alcohol in advance, meaning that increasing retailers' opening hours does not necessarily affect consumption (see also Hinnosaar, 2016). Related to this, Bernheim *et al.* (2016) show that US consumers increase their liquor consumption in response to increased *on-premise* sales hours, but not in response to extended *off-premise* sales hours. This is inconsistent with the hypothesis that consumers use the "availability strategy" as a commitment device.[4] Our results for Sweden differ; we find that extended off-premise opening hours increase alcohol sales. This may suggest that Swedish consumers are more likely to be time-inconsistent but sophisticated, for whom a relaxation of alcohol availability policy increases their alcohol consumption, potentially because it removes their commitment device.

Changing alcohol availability may also affect outcomes other than alcohol consumption. Previous research exploring the effects of extensions to opening hours of alcohol outlets have found significant increases in assaults (Rossow and Norström, 2011), crime (Hansen and Waddell, 2018; Heaton, 2012) and worker absences (Green and Navarro Paniagua, 2016), but mixed findings for traffic accidents (Stehr, 2010) and alcohol-related hospitalizations (Marcus and Siedler, 2015). Exploiting temporary restrictions to (as opposed to relaxations of) alcohol availability, however, Nakaguma and Restrepo (2018) show large reductions in traffic-related accidents, injuries, and hospitalizations, suggesting that consumers may respond asymmetrically depending on whether alcohol policy is liberalised or restricted. Simulating two distinct changes in the Swedish alcohol setting of (1) allowing privately owned liquor stores and (2) allowing alcohol sales in grocery stores, Stockwell *et al.* (2017) find substantial adverse consequences for public health and safety. Our analysis, however, show no evidence to support that increased opening hours in Sweden led to more alcohol-related adverse events. To explore potential causes for the discrepancy in findings, we set up and empirically test a number of hypotheses.

First, we explore whether there is any evidence of substitution between off-premises (i.e., *Systembolaget*) and on-premises (i.e., bars and restaurants) alcohol sales. Indeed, if individuals substituted away from alcohol consumption in bars and restaurants to consumption off-premises, net sales of alcohol may have remained unchanged, potentially explaining the non-result on health and social outcomes. Studying potential changes in on-premise alcohol sales and in the number of licensed venues, however, we find no evidence of any corresponding offsetting effects that would suggest a decrease in the

[4] A frequent recommendation for exercising self-control in alcohol consumption is to limit the availability of alcohol by not keeping it at home. Bernheim *et al.* (2016) refer to this as the "availability strategy".

demand for on-premise alcohol consumption due to expansion of retail opening hours.

Second, we analyze spatial spillover effects across neighboring municipalities to assess the extent to which our estimates capture cross-border substitution of alcohol purchases. Since municipalities are often geographically close enough to each other to rationalize sacrificing a small increase in travel time for improved flexibility, it is not unlikely that an increase in opening hours in one municipality may have drawn in customers from neighboring municipalities. Indeed, we find that approximately half of our main effect on alcohol sales can be attributed to increases in purchases from residents in neighboring municipalities where opening hours remained unchanged.

Finally, the majority of studies that explore the effects of temporal increases in alcohol availability have investigated changes in accessibility *on-premises* (i.e., where alcohol is purchased for immediate consumption; see e.g. Rossow and Norström (2011); Green and Navarro Paniagua (2016)). We hypothesize that increases in *off-premises* alcohol purchases may not necessary lead to corresponding increases in alcohol consumption, since not all alcohol is necessarily meant for immediate consumption. As such, even if extending alcohol availability increases alcohol *purchases*, it does not necessarily lead to corresponding increases in episodic alcohol *consumption*, such as binge drinking. This is in contrast to *on-premises* consumption where purchased alcohol cannot be legally taken outside the establishment from which it was bought. As we do not have reliable data on alcohol consumption, we indirectly evaluate this by exploring heterogeneity in the effects of increased opening hours. We identify the actual *day(s)* in which retailers extend their hours, and find that municipalities with the largest increases in alcohol sales are those who open an extra hour on weekday evenings. Our analysis shows that these municipalities are driving the effects on alcohol retail purchases. We argue that this is consistent with at least one of two possible explanations: a day-of-the-week or time-of-the-day effect (i.e., increased sales at weekdays/weekends versus evenings/afternoons); or a client selection effect (i.e., increased sales from weekday evenings are driven by a different type of customer). Although we cannot distinguish between these different explanations, our results suggest that the customers who reacted to the changes in opening hours are more likely to be low-risk consumers for whom significant short-run impacts on adverse health or crime outcomes were unlikely to occur.

Hence, the two main conclusions that we draw are (i) that the significant increases in alcohol sales we estimate are substantially diminished when spillover effects from other municipalities are taken into account; and (ii) that remaining net increases in sales did not lead to strong surges in damaging activities, such as harmful episodic (binge) drinking. The latter may be explained by the fact that purchased alcohol was not immediately consumed, or was purchased by clients with a low-risk of experiencing short-run negative impacts from increased consumption. However, more longer-term effects of sustained increases in alcohol consumption cannot be excluded from the results reported in this study. We conclude from our analysis that both the *type of intervention* as well as its *margin of variation* are crucial when predicting aggregate impacts of various policies to reduce alcohol-related harm.

The rest of this paper is structured as follows. The next section provides some background on the Swedish institutional context. Section 3 and and Section 4 describes our data and econometric approach, with Section 5 reporting our findings. Section 6 presents a set of extensions and robustness checks, and Section 7 concludes.

2 Institutional setting

Figure 1 illustrates trends in total and type-specific alcoholic drinks purchases per capita (in equivalents of 100% pure ethanol) in Sweden between 1861 and 2007. Sweden has historically been part of the "vodka belt", with high levels of consumption of distilled drinks and binge drinking being relatively common. Excessive drinking (>16 liters per capita per year) and widespread moonshining led to the introduction of regional alcohol monopolies across the country between 1850 and 1870. Continued social problems and uprisings in the early 1900's prompted the government to implement a rationing system ("motboken") from 1917 and to hold an (unsuccessful) prohibition referendum in 1922. In 1955, the rationing system was abolished and the regional monopolies were merged into one national retail monopoly for alcoholic drinks; *Systembolaget*. Alcohol sales rose significantly in response, reinforced by a more liberal alcohol legislation in the 1960's in conjunction with the post-war economic boom and concurrent expansion of the welfare state. Due to widespread concerns from significant increases in alcohol-related harm, the national government implemented a range of policies to re-regulate the access to alcohol in the early 1980's, most prominently by closing Systembolaget on Saturdays. Since 2001, they are again allowed to open six days per week (excluding Sundays).

[Figure 1 about here]

The Swedish alcohol retail monopoly is a non-for-profit publicly owned enterprise that ensures strict compliance with national regulations, which includes the sale of all alcoholic drinks stronger than 3.5% by volume to individuals over 20 years of age.[5] Its objective, mandated through the Swedish Alcohol Law (2010:1622) and the contractual agreement with the Swedish Government (2019:552), is to regulate alcohol distribution to the Swedish population to limit the negative effects of alcohol consumption. During the time period we study in this paper, all *Systembolaget* outlets were open between 11am and 6pm on weekdays (2pm on Saturdays) at a minimum, and between 10am and 8pm on weekdays (3pm on Saturdays) at a maximum. Hence, the regulations imply that the

[5]The exceptions are export shops at airports (outside Europe) and on boats in international waters.

variation in opening hours of alcohol retailers in Sweden was between 38 and 56 hours per week during our analysis period.

The decisions regarding the exact opening hours within the permitted range are decentralized to the level of the 290 local governments (municipalities). This raises the question of why municipalities change their opening hours. For example, if opening hours are relaxed in response to increased local demand for alcohol, or because the municipality needs additional revenue, the changes cannot be considered exogenous. We explore this indirectly below, where we find no evidence of differences in alcohol purchases between municipalities that change their opening hours compared to those who did not change their opening hours. It is also useful to again note here that the objective of *Systembolaget* is to regulate alcohol distribution to the Swedish population to limit the negative effects of alcohol consumption. For example, the Swedish Alcohol Law states that *Systembolaget* may not advertise in any way that could lead to increased consumption of alcohol. This includes advertising for increased opening hours. Indeed, since *Systembolaget* are strictly operating to *regulate* rather than *influence* the sales of alcohol or its profits, economic incentives are not considered an underlying factor for the changes in opening hours. Indeed, the few announcements on extended opening hours that are published highlight they were motivated by providing better service to customers. Note that while there may exist some variation in opening hours across outlets in the same municipality, the vast majority of municipalities only have one *Systembolaget* outlet, meaning we do not need to consider variation *within* municipalities; we come back to this in the analysis.

Other than the *Systembolaget*, strong alcohol can only be sold at on-premises venues defined as restaurants (meaning that they have to offer warm food on location) with an official permit from the municipality. Licenses are issued after the establishment's owner and relevant staff successfully complete training officiated by the Public Health Agency of Sweden. In general, alcohol may be served only between 11am-1am, but municipalities are allowed to permit a later closure time on a case-by-case basis by request from the owner. Inspections and audits by municipality officials are done on a regular basis by municipality bureaucrats to ensure compliance to regulations.

3 Data

We combine annual panel data records on *Systembolaget* opening hours and alcohol sales, alcohol-related health outcomes, crime rates, and socioeconomic and demographic characteristics from official and publicly accessible government data sources for each of the 290 municipalities in Sweden between 2008 and 2015.[6] We drop two municipalities for

[6]Data on alcohol sales are collected by each *Systembolaget* branch and sent to their statistical office. This information as well as data on opening hours and alcohol-related health outcomes are available from the Public Health Agency of Sweden; crime data are available from the Swedish National Council

which the alcohol retailer did not exist throughout the entire analysis period, leaving us with a total of 2,304 municipality-year observations.[7] In addition, data for a subset of municipalities are missing for some of the outcomes we study.

Figure 2 illustrates changes in the weekly number of opening hours of alcohol retailers in each Swedish municipality between 2008 and 2015 (separated into two panels to enhance visibility). The figure provides two key points: First, there is substantial variation in opening hours across municipalities at any point in time, as well as in the *changes* in opening hours of municipalities across time. Second, we only observe *increases* in weekly opening hours across municipalities over the analysis period, implying a monotonic change over time.

[Figure 2 about here]

Figure 3 plots distributions of weekly opening hours of Systembolaget over Swedish municipalities for the first (2008) and the last (2015) year in our data. On average, municipalities increased their weekly opening hours by just over one hour from 44.9 hours per week in 2008 to 46.1 hours per week in 2015. However, the distribution is very skewed, with 47% of municipalities not changing their opening hours at all, 34% increasing them by one hour a week, and the remaining 19% increasing their opening hours between two and up to eight hours per week.

[Figure 3 about here]

To visualize the empirical variation we use in our analyses, Figure 4 provides two municipality maps of Sweden. The left and right panel describe net changes in our main explanatory and response variables – municipality opening hours and total alcohol retail sales – between 2008 and 2015, respectively. Municipalities that changed their opening hours are geographically dispersed across the country, while retail sales increases are particularly concentrated in the northwestern part of the country. We control for a rich set of predetermined municipality characteristics in our empirical analysis to reduce confounding bias related to any spurious associations between our main regressors and outcomes.

[Figure 4 about here]

for Crime Prevention; and data on general municipality demographics and socioeconomic characteristics are available from the Swedish Association of Local Authorities and Regions database (KOLADA). The time period is chosen due to data restrictions on retail alcohol sales: data on sales are considered unreliable prior to 2008 and *Systembolaget* ceased reporting sales data on the municipality level after 2015. See https://www.folkhalsomyndigheten.se/globalassets/statistik-uppfoljning/folkhalsans-utveckling/definitioner-kommentarer-kallor.pdf (in Swedish) for detailed information.

[7] Öckerö and Knivsta municipalities allowed the establishment of Systembolaget only in 2010 and 2013, respectively.

Table 1 presents summary statistics for the alcohol, health, crime and other municipality characteristics we include in our analysis. Per capita annual alcohol sales in the average municipality between 2008 and 2015 were approximately 3 liters of spirits, 23 liters of wine, 34 liters of strong beer, and 3 liters of cocktails/ciders, together amounting to just over 6 liters of 100% pure ethanol. Alcohol sales in restaurants and bars are substantially lower, amounting to – on average – just 0.6 liters of 100% pure ethanol per capita and year. On average, there are 19 venues with an alcohol license per 100,000 population aged 15 and above.[8]

The second panel of Table 1 reports the descriptive statistics for the health outcomes that we consider. These have been selected based on their relevance to increased alcohol consumption. Indeed, we specifically consider alcohol-related hospital admissions, alcohol-related mortality, alcohol-related traffic accidents, and the share of the population with risky alcohol consumption. In addition, we examine those that the literature suggests are correlated with alcohol consumption, including (i) health-related work absences (see e.g., Green and Navarro Paniagua, 2016), (ii) mental health (Frank and McGuire, 2000), and health more generally (Rehm *et al.*, 2009), proxied here by self-assessed health.[9]

The table shows that the average municipality had 321 alcohol-related hospital admissions, 12 alcohol-related deaths, and 14 alcohol-related traffic accidents, all per 100,000 population. On average, 15 percent of respondents report a risky level of alcohol consumption (defined as exceeding the Public Health Agency's recommended consumption level), and individuals reported approximately one month (33 days) of health-related (i.e., sickness and disability) work absences per year. Furthermore, 15 percent report poor mental health and 69 percent consider themselves to be in good health.

The crime statistics that we consider are shown in the third panel of the table. Similar to those above, these are selected based on their relevance to increased alcohol consumption. As in Carpenter and Dobkin (2010), we focus on crimes for which a causal role for alcohol is possible, such as assault, rape and property crime (including property damage, burglary and theft), as well as crime that by definition is linked to alcohol use (i.e., drink driving). Drink driving, however, may be less likely to be affected by changing access to alcohol due to the relatively strict laws in place in Sweden. Apart from the low blood alcohol limit allowed (BAC limit of 0.02 relative to the global average of 0.05) and tough sanctions (high and variable fines, driving license suspension and even imprisonment for aggravated offenses), there is also a strong and longstanding cultural norm that drinking and driving do not mix. Furthermore, as a more (relative to the above) minor offense,

[8] The high dispersion in alcohol retail sales are related to two factors: cross-border trade from Norway (e.g., Eda and Strömstad municipalities) and holiday resorts (e.g., Åre municipal). A robustness check where the estimation sample is winsorized at the 1st and 99th percentiles of the sales distribution yields quantitatively similar results.

[9] We exclude health outcomes such as the share in the population who is obese or who smokes, as alcohol consumption is less likely to affect this, at least in the short run.

we explore effects on the number of individuals resisting law enforcement.[10] The third panel of Table 1 shows that theft (i.e., pickpocketing and shoplifting) is the most common crime, with a rate of 140 crimes per 10,000 population, followed by burglary, assault, property damage, and drink driving. The least frequently occurring crimes included in our analysis are rape and resisting law enforcement with a rate of 5 and 3 per 10,000 population, respectively.

Finally, the last panel of Table 1 presents summary statistics for a set of control variables we include in our estimations. The average municipality has a support ratio (i.e., the number of residents aged 16-64 per resident aged 65 or older) of 0.8, and a population density of 140 individuals per square kilometer. The average (un)employment rate is (7) 78 percent and 16 percent of residents are low educated (defined as only having compulsory education), respectively. The average net annual income is SEK 194,000 (∼ USD 22,000; ∼ EUR 20,000) and 10 percent of households are considered deprived. Six percent are born in a non-European country. There are also substantial variations in budget balances, local tax rates and vote shares for the Sweden Democrats (national-conservative, populist political party) in the last election across municipalities.

[Table 1 about here]

In Table B.1, Appendix B, we show the descriptive statistics separately for treated and control municipalities, where we focus on the year 2008, before any changes in opening hours. This shows some differences between treated and control municipalities in 2008 in terms of their municipality characteristics (e.g., the proportion of women, level of education, net annual income, though not deprivation). Furthermore, some of the health and crime outcomes in 2008 vary between treated and control municipalities. For example, control municipalities have more risky alcohol consumption, but less health-related work absence, and treated municipalities have fewer cases of assault, property damage and theft. However, we find no significant differences with respect to the amount of alcohol sold, suggesting that municipality-level alcohol purchases do not drive the decision to change *Systembolaget* opening hours.

4 Empirical strategy

We use a set of microeconometric approaches to estimate the impact of changes in Systembolaget retail opening hours on alcohol-related outcomes. We first describe our standard two-way fixed effects specification. We then extend this framework to include an event study using synthetic controls (SCs) from the pool of municipalities for which opening hours remained unchanged. As each method relies on different assumptions, using this

[10] We do not show the analysis for murders, as these are very rare occurrences in our context.

range of empirical approaches provides more robust inference with respect to the validity and interpretation of our findings.

4.1 Two-way fixed effects

We specify the following two-way fixed effects model:

$$f(y_{mt}) = f(OH_{mt}, \beta) + (X_{m,2008} \times t)'\eta_x + \lambda_c \times t + \lambda_m + \lambda_t + \varepsilon_{mt}, \tag{1}$$

where y_{mt} and OH_{mt} denote the outcome of interest and opening hours of Systembolaget outlets in municipality $m = 1, 2, ..., M$ in calendar year $t = 2008, 2009, ..., 2015$, respectively. The function $f(\cdot)$ is specified as linear, $f(z) = z$, or logarithmic, $f(z) = \log(z)$, to allow for different interpretations of the marginal effect of retail opening hours, including linear specifications and elasticities, indicated by β. $\lambda_c \times t$ denotes county-specific trends, λ_m and λ_t represent municipality and calendar year fixed effects, estimated through the inclusion of category-specific dummy variables (i.e., $\sum_v \theta_v \mathbb{1}[\lambda_{v'=v}]$ for $v = t, m$), and ε_{mt} is an assumed random error term. This empirical framework is identical to a difference-in-difference (DD) specification, and we refer to the two-way fixed effects and DD model interchangeably. In other words, conditional on the fixed effects, β can be interpreted as the DD estimand, capturing the marginal effect of a change in Systembolaget opening hours on y_{mt} (see, e.g., Bertrand *et al.*, 2004).[11] We cluster all standard errors at the municipality level and weight each municipality-year cell by the municipality population in 1996.[12] Due to the risk of unobserved dependencies across outcomes, we estimate our models in a seemingly unrelated regression (SUR) framework, allowing for arbitrary correlations between error terms across equations. However, we also estimate the regressions separately to check for robustness, assuming the errors to be independent across equations.

The key identifying assumption in our DD model is (strict) exogeneity of opening hours with respect to the outcome.[13] In other words, we maintain that local Systembo-

[11] This is true in the simple case of two time periods and two groups, but may not necessarily hold in "staggered" settings where treatment start times varies across units (see, e.g., Imai and Kim, 2020; de Chaisemartin and D'Haultfoeuille, 2020). We discuss this in our robustness analysis of Section 6.2 and in Appendix A, where we decompose the two-way fixed effects DD estimator into a weighted average of all possible two-group/two-period settings, as in Goodman-Bacon (in press). We also here explore the robustness of our treatment effect estimate.

[12] If our analysis was to make inferences about the effects of extending opening hours on municipality outcomes, we would not want to weight by the population. However, we are interested in making inferences about *population* outcomes. Weighting allows us to take into account that larger municipalities like Stockholm or Malmö have many more individuals than the more rural municipalities in the north. The estimates, however, are generally of similar magnitude when we do not weight the analysis by population, but they have larger standard errors when unweighted, leading to some of the alcohol-related outcomes to be no longer significantly different from zero.

[13] That is, we assume $\mathbb{E}[\varepsilon_{mt}|OH_{ms}] = 0 \; \forall(t, s)$. We also study spatial spillover effects across municipalities in Section 6.1 below. Another assumption is that there are no other relevant policies occurring

laget retailers did not change their opening hours in response to changes in retail sales of alcohol, health outcomes or crime rates. However, as municipalities decide on the opening hours, one concern is that extensions in opening hours may be spuriously related to other characteristics of the municipality, which in turn may lead to biased estimates of β. To deal with potential violations of the strict exogeneity assumption, we follow the methodology used in Hoynes and Whitmore Schanzenbach (2009) and Acemoglu *et al.* (2004) and include a set of pre-determined municipality-level characteristics from 2008 interacted with a linear time trend, $(X_{m,2008} \times t)$, in the model. The intuition of this modification is that it adjusts for potentially unobserved linear time trends in the dependent variable that may be spuriously correlated with changes in alcohol retailers' opening hours.[14,15]

4.2 Synthetic control event study

The main threat to consistent estimation of β in Equation (1) is the potential endogeneity of OH_{mt}. To explore this in more detail, we specify an event study, allowing us to examine any pre-treatment trends in the outcome variables as well as to study post-treatment effect dynamics. Specifically, we first modify Equation (1) by centering the time index around the year of the *first* change in opening hours, denoted t_0, for each municipality between 2008 and 2015:

$$y_{m't} = \sum_{j=-4}^{4} \beta_j \delta_{m'} \mathbb{1}_{m'}[t - t_0 = j] + X'_{m't}\eta_x + \lambda_{m'} + \lambda_t + \varepsilon_{m't}, \tag{2}$$

where $m' \in M' \subset M$ is the subset of municipalities for which a change in opening hours occurred and β_j is now a vector of leads and lags of the event study estimator, capturing the marginal effect at different points in time relative to the change in opening hours. In addition, $\delta_{m'}$ is a *scaling parameter* indicating the magnitude of the change in opening hours associated with municipality m'.[16] Interacting the indicator variable for the first change in opening hours ($\mathbb{1}_{m'}[t - t_0 = j]$) by $\delta_{m'}$ allows us to interpret the estimated β_j parameters in the same way as in Equation (1); i.e., as the marginal effect of a one hour

in the municipalities at the same time as the change in opening hours. While we cannot entirely write off this possibility, we are not aware of any such changes.

[14]Since Systembolaget is operating strictly in order to regulate rather than increase the sales of alcohol and profits, it is unlikely that economic incentives were an important underlying factor for the changes in opening hours. In the few cases where we could find media and press information on extended opening hours they were mainly motivated by providing better service to customers.

[15]Our findings are robust to excluding these predetermined municipality-level characteristics. An alternative to this approach is to include a full set of municipality-specific time trends. However, this turned out to be infeasible due to problems of sparsity and the resulting imprecision of coefficient estimates. We instead include county-specific trends, but our results are robust to omitting these trends altogether, or to including a separate trend for treated and control municipalities. We show the former in Appendix B; the latter are available from the authors upon request.

[16]That is, $\delta_{m'} = OH_{m't_0} - OH_{m't_0-1}$ where t_0 is the year of the first observed change in opening hours for municipality m'.

extension in weekly opening hours.

One potential issue with the event study in Equation (2) is that any further increases in opening hours (i.e., in the years after the first change) would be captured by the indicator for later observations (i.e., β_j for $j > 0$). However, we believe defining the event of interest as the *first* change in opening hours is cleanest, since it ensures that there should be no effects *prior to* this.

Another potential issue with Equation (2) is that it can only be estimated for the subset of municipalities that changed their retail opening hours. Indeed, municipalities that did not change their opening hours do not have an "event time", since they were never "treated". Identification is therefore driven by the differential timing of the event. However, the absence of a suitable comparison group precludes us from adjusting for potentially confounding time trends that may cause spurious relationships between the outcome and the changes in opening hours.[17] We deal with this issue by applying a synthetic control (SC) method to construct an artificial comparison group for each treated municipality based on a weighted average of their pre-treatment *outcome* values (i.e., the municipality-characteristics shown in Table 1; see, e.g., Abadie and Gardeazabal, 2003; Abadie *et al.*, 2010, 2015).[18]

To implement the SC estimator in our context, we define, for each "treated" municipality m', $\hat{y}_{m't} = \sum_c \hat{w}_c^{m'} y_{ct}$ to be the counterfactual outcome using the pool of potential "control" municipalities, $c = 1, ..., C \notin M'$, for which Systembolaget opening hours remained unchanged throughout the analysis period. The municipality-specific weights $\hat{w}_c^{m'}$ are chosen to minimize functions of pre-change outcome levels ($\Delta y_{m'} = \sum_{j \in t < t_0} y_{m'j} - \hat{y}_{m'j}$) and slopes ($\Delta \partial y_{m'} = \sum_{j \in t < t_0} \partial y_{m'j} / \partial j - \partial \hat{y}_{m'j} / \partial j$) by means of a quadratic optimization routine subject to the restrictions that weights are non-negative and sum to one (see, e.g., Botosaru and Ferman, 2019; Ferman and Pinto, 2019, for details on this approach).[19] This process is performed individually for all treated municipalities and the resulting estimates are used as left-hand side variables in the event study model from Equation (2) where the outcome is replaced with $\tilde{y}_{m't} \in (y_{m't}, \hat{y}_{m't}, (y_{m't} - \hat{y}_{m't}))$. In other words, we estimate three sets of event study regressions: one for the treated municipalities ($y_{m't}$), one for the synthetic controls ($\hat{y}_{m't}$) and one for their difference ($y_{m't} - \hat{y}_{m't}$). As the synthetic

[17] Since changes in alcohol retailers' opening hours occurred at different points in time, we are able to retain calendar year fixed effects in the model described in Equation (2).

[18] Although the synthetic control method was originally developed for a single treated unit, the framework can easily accommodate estimation with multiple treated units by fitting separate synthetic controls for each of the treated units (see, e.g., Abadie, 2020). While there is no important conceptual difference in the contexts of one versus multiple treated units, practice issues relating to the non-uniqueness of the solution to the minimization problem when selecting weights for the synthetic controls are exacerbated in the latter. To address this issue, Abadie and L'Hour (2019) propose a synthetic control estimator that incorporates a penalty for pairwise matching discrepancies between the treated units and each of the units that contribute to their synthetic controls.

[19] The *Stata 16* routine `synth_runner` package is used to obtain synthetic controls for the analysis (see Galiani and Quistorff, 2017, for details).

outcomes are estimated from a prior step, we report bootstrapped standard errors based on 200 replications.

5 Results

5.1 Two-way fixed effects

Panel A of Table 2 presents the estimates from Equation (1) using a linear specification for both the dependent variable and the weekly number of opening hours of alcohol retailers. Columns (1)–(4) of the table report coefficient estimates of β from Equation (1), interpreted as the marginal effect of one additional hour increase in opening times on the annual per capita retail sales of spirit, wine, strong beer and cocktails/cider in the municipality, respectively. For all alcohol types, there is a statistically significant positive impact of extended opening hours, ranging between 0.04 liters for spirits to 0.98 liters for strong beer per additional weekly opening hour. The corresponding point estimate in column (5) implies that each additional opening hour leads to an increase in sales of 100% pure ethanol of 0.15 liters per capita and year.[20]

Panel B and C of Table 2 present estimation results from two alternative model specifications: a log-linear model (where the dependent variables are defined in logs) and a log-log model (where both the dependent and the explanatory variables are defined in logs), respectively. The estimates from the log-linear model in panel B can be interpreted as semi-elasticities (i.e., the percentage increase in alcohol sales for each additional weekly opening hour) and suggest a very similar impact for all alcohol types, ranging from a 1.9 percent increase for spirits to 2.5 percent increase for strong beer. Likewise, the total per capita sales of the equivalent of 100% pure ethanol is estimated to increase by 2.1 percent per additional opening hour.

The log-log specification in panel C provides a full elasticity interpretation of the estimated effect: the percentage increase in per capita annual alcohol sales with respect to a one percent increase in weekly opening hours. Although this specification does not have an obvious interpretation in the context of retail opening hours, it is useful as a benchmark against other policy-relevant estimates of the demand elasticity for alcohol, such as the responsiveness to price changes. The estimates reported in the table range between 0.85–1.11, suggesting relatively high elasticities with respect to changes in retail opening hours.

The estimated effects on alcohol retail sales may not necessarily translate into equiv-

[20] Appendix A shows the estimates for the two-way fixed effects specification with binary treatment, i.e., comparing municipalities that increased opening hours to those that did not. In addition, in Section 6.2 and in Appendix A, we explore the robustness to using the Bacon decomposition (Goodman-Bacon, in press) and synthetic control method (Abadie and Gardeazabal, 2003; Abadie *et al.*, 2010). Both analyses suggest a positive treatment effect, with no evidence to suggest that our findings are obtained by chance.

alent increases in overall alcohol *consumption* if consumers reduce their consumption on-premises (e.g., in restaurants or bars). To empirically assess potential substitution effects across on- and off-premises consumption of alcohol, columns (6) and (7) of Table 2 report estimates from our two-way fixed effects model using information on annual per capita liters of 100% pure ethanol equivalents sold in restaurants and bars and the number of licensed venues per 10,000 population as dependent variables. The reported coefficients are not statistically significant and close to zero, suggesting that increases in opening hours did not impact overall *on-premises* demand for alcohol. Thus, we conclude from this analysis that it is unlikely that substitution effects between consumption on- and off-premises explain our findings above.[21]

[Table 2 about here]

These analyses allow for possible dependencies across the different outcomes of interest using a seemingly unrelated regression (SUR) framework. Because the outcomes within the alcohol, health, and crime categories are likely to be correlated, we prefer this more conservative interpretation, despite this being a less standard approach in the literature. Table B.2–Table B.4 in Appendix B report the estimates that assume independence across equations, showing our results are generally robust to this approach. More specifically, it does not affect our alcohol estimates (including those on the number of licensed venues and restaurant sales), nor does it affect our conclusions for the crime outcomes. For the majority of health outcomes, the conclusions do not change. However, with the reduction in standard errors, there is some evidence of an increase in hospital admissions and a worsening of mental health.

To explore the importance of having multiple *Systembolaget* outlets per municipality, potentially leading to some variation in opening times *within* a municipality, we re-run the analysis dropping the three largest municipalities in our sample (i.e., Stockholm, Göteborg and Malmö). Table B.5–Table B.7 in Appendix B present the estimates, again showing quantitatively and qualitatively similar results, suggesting this does not play a major role in our analysis.

We next turn to study any potential adverse health and social effects of increased retail opening hours. Table 3 and Table 4 present results from estimation of Equation (1) for the set of health and crime outcomes listed in Table 1, respectively. Analogous to Table 2, panels A, B and C of the table refer to the lin-lin, log-lin, and log-log model specifications, respectively. In sharp contrast to the findings on alcohol purchases, the reported estimates imply that none of the included outcomes changed significantly due

[21]The sample size is lower for the specification using restaurant sales in column (6) due to missing values for some municipalities. To study the robustness of our findings with respect to the choice of sample, we re-estimated the models for columns (1)–(5) and (7) with this reduced sample. Our results are qualitatively and quantitatively similar with this additional sample restriction and available upon request.

to increased opening hours. Indeed, the p-values (adjusted for cross-equation residual correlation) do not allow us to interpret these effects as significantly different from zero.[22]

[Table 3 and Table 4 about here]

In summary, our findings suggest that the extended opening hours of Swedish alcohol retailers led to a economically and statistically significant increase in alcohol sales, but without any discernible associated effect on the adverse health- and crime-related outcomes we study.[23] Furthermore, the increase in off-premises alcohol sales did not appear to have had any important impact on on-premises demand from bars and restaurants. We study the robustness of these findings in further detail in Section 6.[24]

One potential concern here is whether the null finding for health and crime is due to a "true" null, or due to insufficient power to detect any effects. We explore this in two ways. First, looking at the 95% confidence intervals of the health and crime estimates, we find that we can rule out large effects for most outcome variables. For example, the estimates in Panel B of Table 3 suggest confidence intervals for alcohol-related hospital admissions, mortality and traffic accidents of [-0.002, 0.01], [-0.024, 0.026] and [-0.121, 0.017] respectively. These are percentage changes; hence, we can rule out increases in alcohol-related hospital admissions, mortality and traffic accidents of more than 1%, 2.6%, and 1.7% respectively for each additional opening hour. Looking at the crime-related outcomes, we can rule out increases larger than 0.6%, 1.5% and 1.6% for assault, drink driving and burglary respectively. Hence, these are relatively well defined zero estimates.

A second way in which we assess the power of our analyses is by doing (ex post) power calculations. We again base these on our log-linear model, where – for each outcome – we estimate the power to reject a false null hypothesis for a range of effect sizes. Figure B.1–Figure B.6 in Appendix B shows these calculations graphically, where the vertical axis shows the power for each of the (absolute) effect sizes displayed on the horizontal axis. Our effect estimates from Table 2–Table 4 are depicted as the black circles (not shown

[22]Furthermore, while some point estimates are rather sizeable in the log-log specification, this may be explained by a poor non-linear approximation when baseline values are close to zero (e.g., for traffic accidents).

[23]In Table B.8 of Appendix B we also report the estimates of the effect of extended opening hours on the use and possession of drugs. Similar to the crime outcomes shown in Table 4, this shows no evidence that an increase in opening hours affected drug-related crime.

[24]We show in Appendix B that our results are robust to the exclusion of county-specific trends (i.e., $\lambda_c \times t$ in Equation 1; see Table B.9–Table B.11) and the omission of the municipality-characteristics measured in 2008, interacted with a time trend (i.e., $X_{m,2008} \times t$ in Equation 1; see Table B.12–Table B.14). To investigate the robustness of results with respect to the health and social consequences of alcohol consumption, we also explore an instrumented difference-in-difference analysis, where we specify the municipality-level change in opening hours as an instrument for the change in alcohol consumption. Although the IV results are consistent with our findings above, we do not report them here, since the assumption that opening hours only affect health and crime through their effect on alcohol consumption is perhaps questionable. Furthermore, the first stage F-statistic was $\sim$16, suggesting we the instruments are relatively weak (Lee *et al.*, 2020). However, the results are available from the authors upon request.

if our estimate exceeds the maximum effect size on the horizontal axis). This confirms that we have sufficient power for the majority of our outcomes. However, there are three outcomes where we have relatively low power: alcohol-related mortality, assaults and rape. It is important to keep this in mind in the interpretation of the estimates.

5.2 Synthetic control estimates

Our identification strategy hinges on the assumption that any trends in our outcomes of interest are (mean-)independent of changes in weekly opening hours. We empirically assess this assumption by estimation of the synthetic control (SC) event study model in Equation (2) described in Section 4.2. Parameter estimates are presented in Figure 5–Figure 9 for the alcohol, health and crime outcomes we study, respectively.[25] Each panel plots the estimated β_j coefficients and 95% confidence intervals from Equation (2) separately for the outcomes of "treated" municipalities that changed their Systembolaget weekly opening hours ($y_{m't}$, black triangles), and for their respective synthetic controls ($\hat{y}_{m't}$, gray squares), respectively. The coefficient labels pertain to the difference between the period-specific treated and SC point estimates. Finally, pooled before-after estimates, using the difference $\tilde{y}_{m't}(= y_{m't} - \hat{y}_{m't})$ as the dependent variable, are reported in each panel title.

The results for alcohol purchases, presented in Figure 5 and Figure 6, suggest that SC's are on average well-matched with the treated municipalities with respect to both pre-treatment outcome trends and levels. The estimated pooled effects are also remarkably similar in magnitude to the point estimates from the DD analysis reported in Table 2 and therefore reassuring in terms of a causal interpretation. Moreover, the event study allows us to study the dynamics of the effect following the extension in Systembolaget opening hours which can best be described as exhibiting a gradually increasing pattern over time. The exceptions are for cocktails/ciders as well as for the on-premises outcomes (restaurant sales and number of licensed venues). With respect to the former, we observe a rather erratic pattern in the trend over time, which can explain the marginally significant point estimate from column (4) in Table 2. Regarding the latter, although there is a slight tendency of a downward trend over the observation period in the number of licensed venues, the pooled effect estimate of -0.2 suggests a small marginal effect of less than one percent relative to the baseline mean in 2008 (see Table 2). All in all, the SC estimates for on-premises alcohol sales mostly confirm our previous findings that there is little empirical evidence of substitution effects across on- and off-premises alcohol outlets due to extensions of retail opening hours.

[25]These figures show the results using the outcome variables in levels, but our results are robust to specifying the dependent variable in logs. The figures using logs are not shown here, but are available from the authors upon request.

[Figure 5 and Figure 6 about here]

Figure 7–Figure 10 present corresponding SC results for our health and crime outcomes. As with the alcohol-related outcomes, the trends of the SC's are generally well-matched to the treated municipalities prior to the change in opening hours. First turning our attention to the health outcomes, we find no systematic evidence that treated municipalities experienced different post-treatment trends compared to their SC's. Moreover, the pooled before-after effect estimates are close to zero (as shown in the figure titles) and far from being statistically significant at any conventional level. With respect to the crime outcomes the story is slightly different, suggesting a significant increase in drink driving and a decrease in property damage of around 0.36 and 3.03 cases per 10,000 population, respectively. With respect to the former, the marginal effect size is around two percent and in parity with the results on alcohol sales. With respect to the latter, the treated municipalities and their SC's do not appear to be well matched in pre-change periods, meaning that the resulting estimate is difficult to interpret.

[Figure 7–Figure 10 about here]

In summary, the results on alcohol purchases from the two-way fixed effects analysis mostly carry over to the SC model. The observed effect dynamics suggest a gradually increasing pattern in off-premises alcohol sales following the change in opening hours. This effect is not driven by a corresponding decrease in demand for on-premises sales, although the SC analysis suggests a small decrease in the number of licensed venues. Furthermore, health and crime outcomes are mostly unaffected by extensions of alcohol retailers' opening hours with the exception of a moderate increase in the rate of drink driving charges.

6 Extensions and robustness analyses

This section presents results from a set of extensions and robustness checks. We first study spatial spillover effects across municipalities by analyzing how opening hours in neighboring areas affect sales in the focal municipality. We then follow Goodman-Bacon (in press) and decompose the two-way fixed effects model into a weighted average of all possible two-group two-period DD estimates. Next, we conduct heterogeneity analyses by variation in the magnitude and timing of opening hours extensions. This may provide insights into the consumption behavior with respect to the type and response of those affected by the policy.

6.1 Spatial spillovers

One potential caveat when interpreting our results on alcohol sales as causally linked to extended opening hours in a given municipality is that residents from surrounding areas were not restricted from travelling to their neighboring municipality. Extensions of opening hours in one municipality may thus have attracted customers from neighboring municipalities where retail opening hours were more rigid. Importantly, such spillover effects would overstate the estimated impact of extended opening hours on retail purchases because changes in alcohol sales from substituting consumers would be counted twice; once as an increase in the municipality consumers substitute to, and once as a decrease in the municipality consumers substitute from.

To explore this potential source of bias, we collect additional geodetic information on the exact location of all alcohol retailers in Sweden and estimate the Euclidean distance from each municipality's geographical midpoint to all alcohol retailer outlets in other municipalities. We then re-estimate our main DD model from Equation (1) by including weekly opening hours of alcohol retailers in *neighboring* municipalities as additional regressors. To keep the model estimable, we restrict the number of neighboring retail outlets to five or to the total number of outlets within a distance of 100 kilometers (a distance that one can travel by car within the additional hour of the retailer being open) from the midpoint of the municipality, whichever restriction binds first. If spatial spillover effects are important, we expect extensions in neighboring municipalities' retail opening hours to negatively impact alcohol purchases in the focal municipality. To estimate the marginal effect of opening hours, taking into account such spatial spillovers, we sum up the opening hour parameter estimates for focal and neighboring municipalities.

Table 5 reports the point estimates of own and neighboring municipalities' opening hours from estimation of the augmented DD model on total retail sales of pure alcohol. The first row shows parameter estimates of the focal municipality's opening hours, corresponding to the point estimate from row one and column (5) in Table 2, while the bottom five rows pertain to the opening hours of neighboring municipalities, ranked by order of distance from the focal municipality. The table is organized in six columns where each column adds the opening hours of the next closest neighbor. As some municipalities have less than five neighboring alcohol retail outlets within 100 kilometers from its geographical midpoint, approximately 50 of the total 288 municipalities are excluded from the sample in the final column. This attrition does not change the estimation results to any important extent, however.

The statistics in the bottom half of Table 5 refer to the total marginal effect, i.e., *the sum* of the coefficient estimates. For example, column (2) shows that the marginal effect of opening hours, taking into account any spatial spillovers with the closest municipality, is 0.089.[26] An F-test shows that this is significantly different from zero at the 5% level ($p = 0.016$).

The table provides four interesting findings. First, the effect on alcohol retail sales from changes in a municipality's own opening hours is largely unaffected by the inclusion of neighboring municipalities opening hours' changes. This suggests that municipalities' timing decisions to extend their alcohol retail opening hours are uncorrelated. Second, only variation in the opening hours of the *closest* neighboring municipality has a significant impact on the sales of alcohol in the focal municipality. This suggests that the benefit of improved access in terms of extended opening hours is, as expected, traded off against the travel time needed to take advantage of it. The insignificance of more distant neighbors, together with the reduction in sample size when including further neighboring municipalities, suggests that the specification in column (2) best captures the relevant marginal effect. Third, the magnitude of this spillover effect is roughly 50 percent of the estimated change in sales from an increase in the focal municipality's own retail opening hours. In other words, almost half of the estimated effect of an increase in weekly opening hours on sales of alcohol can be attributed to spillover effects from neighboring municipalities. Finally, the elasticity of demand with respect to opening hours, shown as ϵ_d in the bottom half of the table, reduces from 0.942 in column (1) to 0.465 in column (2). Hence, taking spatial spillovers into account reduces the estimated elasticity of demand to be very similar to that reported in the literature (see e.g., Pryce *et al.*, 2019).[27]

[Table 5 about here]

6.2 Decomposing the two-way fixed effects model

Section 4.1 discusses the two-way fixed effects difference-in-difference (DD) model, specifying the outcome of interest as a function of municipality opening times, while controlling for municipality and calendar year fixed effects. Conditional on these fixed effects, the parameter estimate on the variable of interest (i.e., β in Equation (1)) can be interpreted as the DD estimand in the simple two group/two time period case (see, e.g., Bertrand *et al.*, 2004).

However, this does not necessarily hold in "staggered" settings where treatment start times vary across units (see, e.g., Imai and Kim, 2020; de Chaisemartin and D'Haultfoeuille, 2020). Indeed, in our setting, opening times are changed in different years across different

[26] This is the "net" coefficient, given by $\beta_{own} + \beta_{neighbor1}$.

[27] In analysis not reported here, we also allow for spatial spillovers in our health and crime outcomes. Indeed, if the extra alcohol purchased in the additional opening hour is consumed in the focal municipality, this may over-estimate any potential effects on health and crime. In contrast, if the alcohol is consumed in neighboring municipalities, potentially causing worse health and increased crime rates, the estimates reported above would be downward biased. The results that allow for spatial spillovers, however, are similar to those above, with no evidence that the increased opening hours affect health and crime in the population.

municipalities. Goodman-Bacon (in press) suggests an intuitive method to decompose the two-way fixed effects DD estimator into a weighted average of all possible two-group/two period DD estimators. Although this method refers specifically to binary treatments, whereas we have a continuous treatment setting, we here study the properties of our DD estimator.

In Appendix A, we show that our findings are robust to the use of a standard two-way fixed effects DD model using a binary indicator for a change in opening hours, instead of a continuous treatment as in Equation 1. Decomposing the two-way fixed effects model, using the Goodman-Bacon decomposition, also shows very similar treatment effects. In particular, we show that the treatment effect is driven by the extensive margin (i.e., treated municipalities versus never-treated municipalities), with somewhat smaller estimates at the intensive margin, specifying control municipalities as those who were treated in a *different* calendar year from the treated municipality. These results are also robust to the a synthetic control specification. Furthermore, using placebo tests and randomization inference, we show that our results are unlikely to be driven by chance.

The findings from the decomposition analysis are graphically presented in Figure 11, where the horizontal axis shows the estimated weight and the vertical axis shows the DD estimate for the different comparison groups. The solid dots denote the extensive margin estimates: the comparison between treated and never-treated municipalities, where the relevant treatment year is indicated next to each marker. The estimates from the intensive margin, specifying control municipalities as those who were treated in a different calendar year to the treated municipality, are shown in the open dots, with the comparison years indicated next to each marker. For example, the largest estimate within this group (indicated with “2011/2009”) is a comparison of municipalities that were treated in 2011 using municipalities that were treated in 2009 as controls. This figure shows two main points. First, the overall estimate is driven by the extensive margin (i.e., the solid dots), for which the DD estimate shown on the vertical axis is larger, on average. Second, the weight (shown on the horizontal axis) given to the comparisons at the extensive margin is substantially larger than those for comparisons at the intensive margin. This has implications for the interpretation of our estimates. Indeed, it may be that municipalities that never change their opening hours are less suitable as control municipalities if they are systematically different from those that do change their opening hours, but at different points in time. Although we are not aware of any *a priori* reason why they may be different and it is unlikely that economic incentives are an underlying factor for the changes in opening hours (see also Section 2), it is important to take this into account when interpreting the estimates.

[Figure 11 about here]

6.3 Heterogeneity in opening hours extensions

We next exploit that our data allow us to derive more detailed information on *which day(s)* the opening hours changed. Table 6 shows specific transitions in the weekly opening hours we observe in our data, with the most commonly occurring changes highlighted in bold font. Note that municipalities may change their retail opening hours multiple times across the observation period.

[Table 6 about here]

The most common change in opening hours is from 43 to 44 hours per week. Such one-hour extensions mainly indicate an increase in opening hours on Saturdays. Moreover, two- and three-hour extensions indicate increased opening hours on Thursday and Friday, and Monday through Wednesday evenings, respectively.[28] To gain more insight into what may be driving the effect on alcohol sales, we decompose our sample into three categories by length of extension, denoting the one-hour (weekend) change by "small", and (weekday) changes of two hours and three or more hours by "medium" and "large", respectively.

Table 7 presents estimates from split-sample regressions for each of the three groups of municipalities in columns (2)–(4) and, for reference, the pooled sample in column (1). The results suggest that only larger extensions in opening hours were significantly associated with increased sales.[29] In other words, it appears as if increases in alcohol sales were concentrated to evening extensions on weekdays only, and in particular on Monday through to Wednesday evenings. Although this is not necessarily interesting in itself, it may shed some light on the potential mechanisms of the effects. Indeed, there are at least two possible explanations for these findings. First, it may be a day-of-the-week or time-of-the-day effect, in that extending opening hours on a Saturday afternoon had a relatively smaller impact than extending opening hours on a weekday evening. Second, different types of extensions might have attracted different types of customers with respect to alcohol consumption (a client selection effect). Although we cannot distinguish between these different explanations, taken together, our results suggest that the type of customers who reacted to the changes in opening hours, i.e., weekday evening shoppers, are more likely to be low-risk consumers for whom significant short-run impacts on adverse health or crime outcomes were unlikely to occur.

[Table 7 about here]

[28] This information has been verified by studying the reported daily opening hours of alcohol retailer outlets available from www.systembolaget.se.

[29] Note that the interpretation of the coefficient is the marginal increase in retail sales per additional opening hour extension and not the cumulative effect from the total change in opening hours.

Finally, we study the distribution of effects, distinguishing between municipalities with small, medium and large changes in opening hours, using the synthetic control approach on the sample of treated municipalities. Figure 12 presents the estimates from an event study for the difference in the change of total retail sales of alcohol between each treated municipality and its synthetic control.[30] The mean effects and its associated labels show the period-specific average relative change in total retail sales across all treated municipalities. The three remaining markers provide corresponding changes for municipalities with small, medium and large changes in opening hours, as defined in the previous paragraph. While there is considerable heterogeneity in the distribution of the individual effects, a similar effect pattern as shown in Table 7 can be discerned. On average, the sales of alcohol increase by between 0.1-0.2 after an extension in opening hours and this effect is mainly driven by municipalities that extended their hours by two or more per week.

[**Figure 12 about here**]

We conclude from our analysis that, although we cannot identify the precise mechanisms that the effect of opening hours on alcohol sales is mediated through, increased sales did not seem have any significant impacts on adverse health or crime outcomes. This suggests that the increased sales were less likely to be driven by individuals prone to heavy episodic drinking. Instead, the extended opening hours may have primarily affected consumer behavior among customers with less time flexibility (e.g., full-time employees) by allowing them to manage their alcohol purchases during weekday evenings. However, without more fine-grained data on the individual level, we are unable to directly assess this hypothesis.

7 Conclusion

Excessive alcohol consumption has been linked to a range of adverse societal outcomes, including premature deaths, worsened labor market outcomes, crime, motor vehicle accidents, and risky sexual behavior. Many communities have implemented policies to regulate and restrict access to alcohol in order to reduce its economic and health burden. This paper investigates the impact of increased access to alcohol in terms of extensions to (regulated) opening hours for alcohol retailers in Sweden on alcohol sales, health outcomes and crime rates. To identify the effects of the extensions, we exploit the fact that mandated opening hours of the Swedish national alcohol retail monopoly are decentralized to each of the 290 municipalities.

Using a difference-in-differences model for years 2008–2015, we find that extended opening hours significantly increased alcohol sales. Each additional weekly opening hour

[30] The data in the figure have been winsorized at the 1st and 99th percentiles to improve visibility.

increased average sales of pure alcohol by 2.1%, or by 0.15 liters per person and year, corresponding to a demand elasticity of one. The increase was similar across the different alcoholic drinks, with the consumption of spirits increasing by 2% (or 0.04 liters per person per year) for each additional opening hour, wine by 2.2% (or 0.63 liters per person per year), strong beer by 2.5% (0.97 liters), and cocktails/cider by 2.1% (0.05 liters). We find no strong evidence that the increases in sales were mediated through substitution between on- and off-premises consumption, measured as restaurant alcohol sales or number of licensed venues.

Despite the substantial increases in alcohol sales, we find no systematic effects on a wide range of alcohol-related adverse health and crime outcomes. Furthermore, we find that the effects are mainly driven by the extensive margin, with smaller – but still positive – effect sizes at the intensive margin. These results are consistent when specifying a synthetic control event study, relying on different identifying assumptions. We provide two explanations for this result: first, actual increases in alcohol sales are substantially diminished once spatial spillover effects from neighboring municipalities are taken into account. This is important especially from a methodological point of view as results from empirical papers evaluating effects of alcohol policies using regional variation in the market for alcohol may be subject to considerable spillover bias. Second, we find that effects on alcohol sales are mainly driven by extensions in evening weekday opening hours. We argue that this result is consistent with at least two possible explanations: a day-of-the-week or time-of-the-day effect (i.e., increased sales at weekdays/weekends versus evenings/afternoons), and/or a client selection effect (i.e., increased sales from weekday evenings are driven by a different type of customer). The fact that we find no externalities on health and crime suggest that the increase in alcohol purchases may have been mainly driven by low-risk consumers of alcohol for whom significant increases in alcohol-related harms in the short run were unlikely. Based on this evidence, we conclude that the increased opening hours were unlikely to generate important adverse effects related to alcohol-related harm in the short-run.

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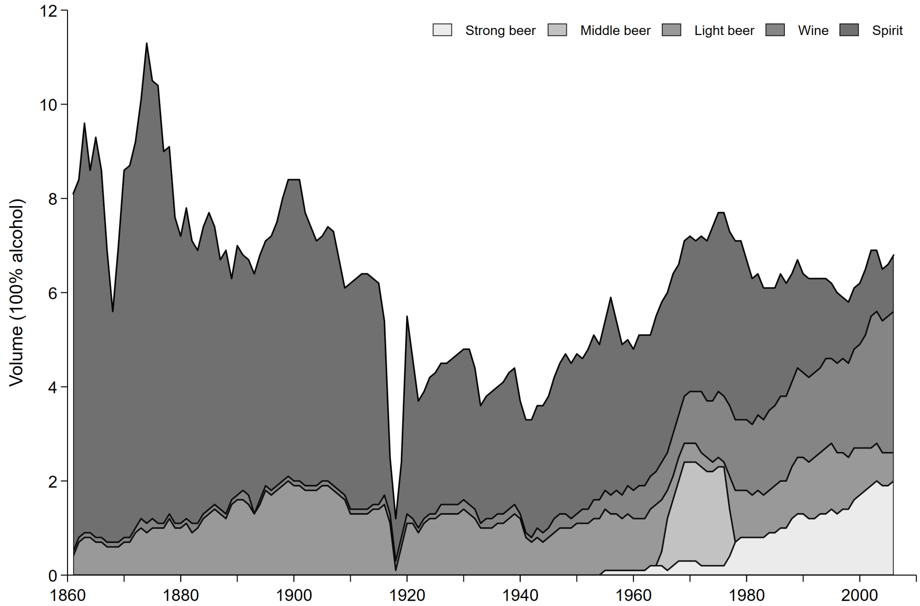
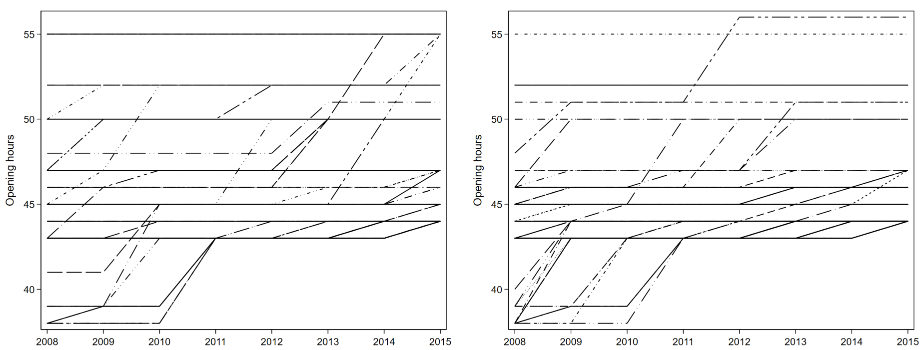
Tables and Figures

FIGURE 1.
Alcohol sales in Sweden, 1861–2007



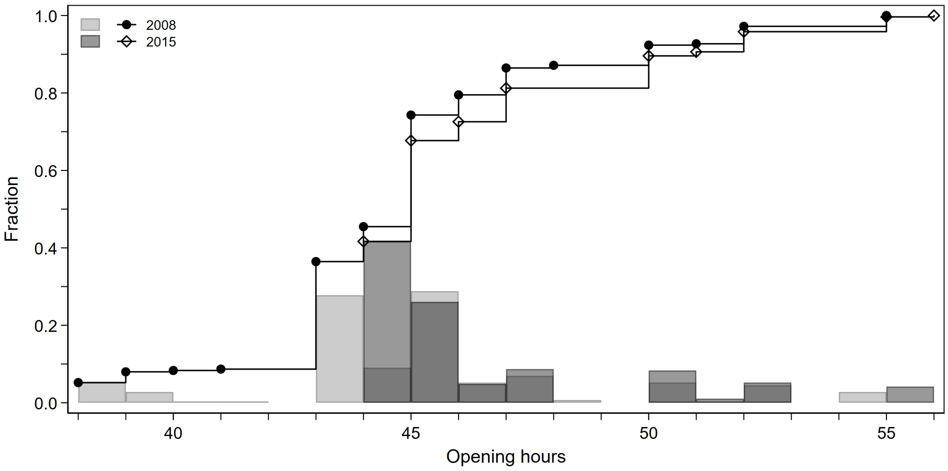
NOTE.— Sales data from CAN (2008). Excludes imports from other countries, home production and consumption abroad. Low-strength (light) beer refers to beer with an alcohol by volume (ABV) of 2.2%-3.5%, mid-strength (middle) beer with ABV of 3.6%-4.5% and full-strength (strong) beer with ABV above 4.5% Mid-strength beer was redefined as full-strength beer in 1977.

FIGURE 2.
Systembolaget weekly opening hours by municipality, 2008–2015



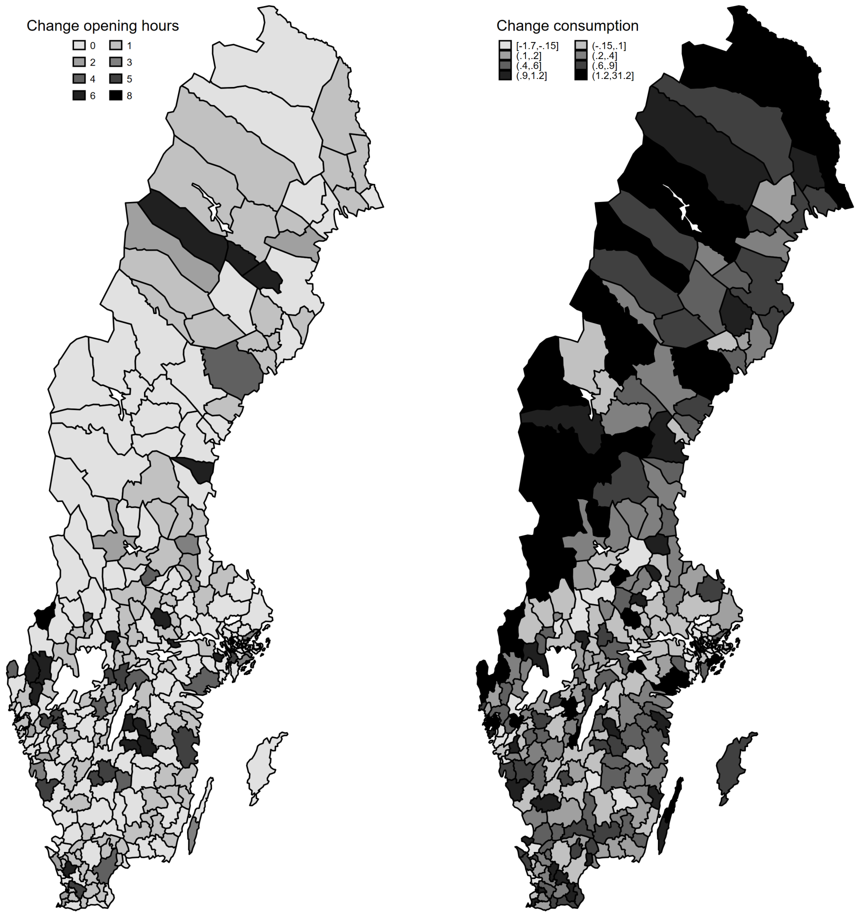
NOTE.— Lines illustrate trends in average weekly opening hours of Systembolaget outlets by Swedish municipalities from 2008 to 2015. Two municipalities without any Systembolaget outlets at the start of the time period are excluded.

Figure 3.
Distributions of Systembolaget weekly opening hours across municipalities in 2008 and 2015



Note.— Lines illustrate cumulative fractions of Systembolaget weekly opening hours across Swedish municipalities in 2008 and 2015. Bars show corresponding densities at specific hourly intervals.

Figure 4.
Net changes in Systembolaget weekly opening hours and alcohol sales between 2008 and 2015



Note.— Left (right) map show changes in weekly opening hours (total retail sales of alcohol) across Swedish municipalities between 2008 and 2015.

Table 1.
Descriptive sample statistics

	2008–2015				
	Count	Mean	SD	Min	Max
Alcohol variables					
Purchases of spirit	2,304	3.0	(3.1)	0.9	40.5
Purchases of wine	2,304	23.3	(28.2)	1.8	441.4
Purchases of strong beer	2,304	33.8	(28.5)	6.7	418.9
Purchases of cocktails & cider	2,304	3.0	(5.5)	0.4	168.3
Total retail sales	2,304	6.2	(6.2)	1.9	91.8
Restaurant sales	1,470	0.6	(0.7)	0.0	6.6
Licensed venues	2,304	18.9	(13.9)	2.9	129.1
Health variables					
Alcohol-related hospital admissions	2,304	321.0	(86.0)	105.4	778.9
Alcohol-related mortality	2,304	12.2	(6.0)	0.0	43.9
Alcohol-related traffic accidents	2,304	14.0	(13)	0.0	114
Risky alcohol consumption	1,648	14.9	(2.6)	8.0	26.0
Health-related work absence	2,304	33.2	(7.2)	12.1	69.0
Poor mental health	1,648	15.1	(2.4)	9.0	28.0
Good self-assessed health	1,648	69.0	(4.5)	55.0	86.0
Crime variables					
Assault	2,304	72.3	(26.2)	8.1	294.2
Rape	2,304	5.0	(6.8)	0.0	252.3
Property damage	2,304	21.7	(43.3)	0.0	474.7
Burglary	2,304	91.1	(36.1)	14.2	276.6
Theft	2,304	140.2	(65.6)	28.9	567.8
Drink driving	2,304	17.3	(8.3)	0.0	70.2
Resist law enforcement	2,304	3.0	(2.6)	0.0	17.4
Municipality characteristics					
Support ratio	2,304	0.8	(0.1)	0.5	1.1
Women	2,304	49.6	(0.8)	46.4	52.1
Population density	2,304	139.6	(491.2)	0.2	5,239.8
Low educated	2,304	15.8	(4.0)	3.2	30.4
Employed	2,304	78.1	(4.0)	60.0	88.2
Unemployed	2,304	6.6	(2.4)	1.1	17.2
Startups	2,304	4.9	(1.7)	1.1	16.2
Net commuters	2,304	-11.6	(19.0)	-62.2	102.9
Net annual income	2,304	194,095	(23,211)	150,304	332,374
Deprived households	2,304	10.1	(3.7)	2.1	31.7
Non-EU born	2,304	6.1	(3.9)	1.0	29.4
Net result	2,304	1,071	(2,658)	-18,242	97,493
Tax rate	2,304	21.5	(1.4)	17.1	33.6
Sweden democrats	2,304	5.5	(4.5)	0.0	23.9

Note.— Municipality sample statistics pooled across years 2008–2015. Alcohol variables measured in liters per capita per year, except for total retail and restaurant sales measured in liters of 100% ethanol, and licensed venues measured in rate per 100,000 population over 15 years of age. Alcohol-related hospital admissions and mortality (admissions and cause for death with a primary or secondary ICD-10 diagnosis code of E24.4, F10, G31.2, G62.1, G72.1, I42.6, K29.2, K70, K85.2, K86.0, O35.4, P04.3, Q86.0, T51, Y90.1-Y90.9, Y91.1-Y91.9, Z50.2, Z71.4, Z72.1) and traffic accidents measured in rates per 100,000 population over 15 years of age. Health-related work absence measured in average days per capita. Remaining health variables measured as percentages. Crime variables measured as rates per 10,000 population. Support ratio is the number of municipality residents aged 16-64 per resident aged 65 or older. Population density is population per square kilometer. Low educated is the population share with only compulsory education. Employed and unemployed are percentages of the working-age population registered as employed and unemployed, respectively. Startups is the number of new businesses per 1,000 population. Net commuting is the difference between number of in- and out-commuters as a share of the total number of employed. Net annual income is the average annual after-tax income of the working-age population in SEK. Deprived households is the share of households receiving social support. Non-EU born is the population share born outside of the European Economic Area. Net result is the municipality's economic result (total revenue minus expenses) per capita in SEK. Tax rate is the percentage municipality tax rate. Sweden Democrats is the percentage vote share for the national-conservative populist political party in the last municipality election.

TABLE 2.
Difference-in-difference estimates, 2008–2015: Alcohol outcomes

	(1) Spirit	(2) Wine	(3) Strong beer	(4) Cocktails & cider	(5) Retail sales	(6) Restaurant sales	(7) Licensed venues
Panel A: Lin-Lin							
Opening Hours	0.043***	0.637***	0.983***	0.053*	0.149***	-0.002	-0.040
	(0.012)	(0.144)	(0.264)	(0.030)	(0.035)	(0.007)	(0.068)
Panel B: Log-Lin							
Opening Hours	0.019***	0.022***	0.025***	0.021***	0.021***	-0.008	-0.003
	(0.005)	(0.005)	(0.006)	(0.006)	(0.005)	(0.012)	(0.004)
Panel C: Log-Log							
Opening Hours	0.842***	0.984***	1.110***	0.955***	0.942***	-0.336	-0.146
	(0.250)	(0.240)	(0.285)	(0.302)	(0.247)	(0.591)	(0.185)
Mean of outcome	2.9	20.1	31.2	2.6	5.6	0.6	17.6
No. of groups	288	288	288	288	288	224	288
No. of observations	2,304	2,304	2,304	2,304	2,304	1,470	2,304

NOTE.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels A, B and C. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE 3.
Difference-in-difference estimates, 2008–2015: Health outcomes

	(1) Hospital Adm.	(2) Mortality	(3) Traffic Acc.	(4) Work Absence	(5) Risk Cons.	(6) Self-Ass. Health	(7) Mental Health
Panel A: Lin-Lin							
Opening Hours	1.103	0.085	-0.027	-0.012	-0.076	-0.085	0.119
	(0.862)	(0.111)	(0.021)	(0.056)	(0.076)	(0.076)	(0.129)
Panel B: Log-Lin							
Opening Hours	0.004	-0.001	-0.052	-0.001	-0.004	-0.001	0.008
	(0.003)	(0.013)	(0.035)	(0.001)	(0.005)	(0.001)	(0.007)
Panel C: Log-Log							
Opening Hours	0.175	-0.109	-2.408	-0.060	-0.177	-0.057	0.384
	(0.135)	(0.662)	(1.655)	(0.058)	(0.226)	(0.051)	(0.329)
Mean of outcome	292.9	12.7	1.6	40.0	15.0	27.1	15.3
No. of groups	288	288	288	288	282	282	282
No. of observations	2,304	2,304	2,304	2,304	1,648	1,648	1,648

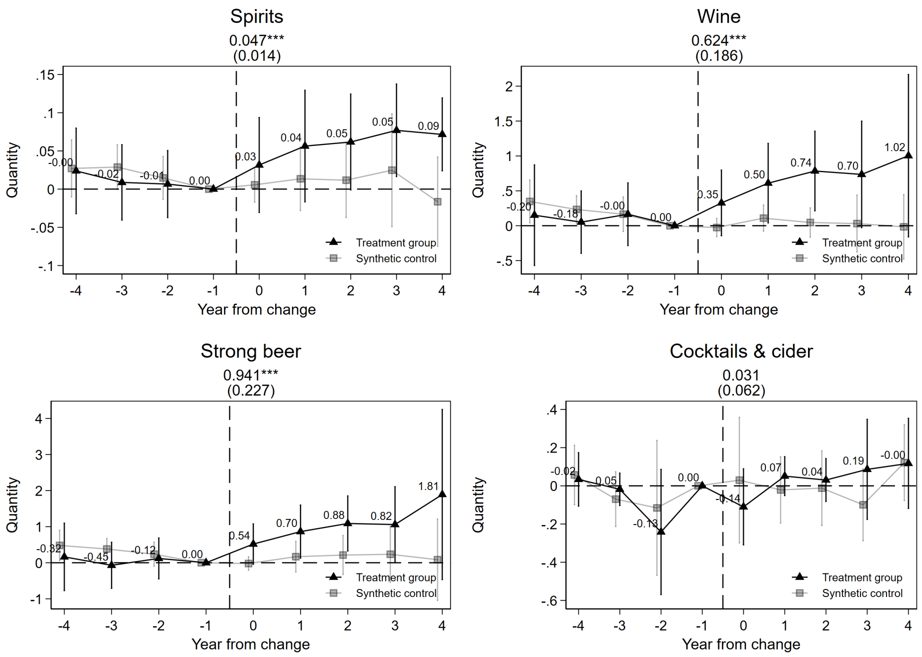
NOTE.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels A, B and C. Dependent variable defined in column headers. All regressions control for for county-specific time trends, municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.
Difference-in-difference estimates, 2008–2015: Crime outcomes

	(1) Assault	(2) Drink Driving	(3) Rape	(4) Resist Law Enf.	(5) Prop. Damage	(6) Burglary	(7) Theft
Panel A: Lin-Lin							
Opening Hours	-0.177	0.175	0.139	0.066	-0.454	0.475	0.758
	(0.340)	(0.122)	(0.110)	(0.054)	(1.112)	(0.537)	(1.065)
Panel B: Log-Lin							
Opening Hours	-0.002	0.005	-0.011	0.025	-0.048**	0.004	0.003
	(0.004)	(0.010)	(0.016)	(0.026)	(0.022)	(0.006)	(0.005)
Panel C: Log-Log							
Opening Hours	-0.108	0.258	-0.601	1.212	-2.340**	0.171	0.146
	(0.191)	(0.472)	(0.783)	(1.269)	(1.053)	(0.268)	(0.221)
Mean of outcome	72.3	20.7	4.4	2.8	28.4	98.8	154.9
No. of groups	288	288	288	288	288	288	288
No. of observations	2,304	2,304	2,304	2,304	2,304	2,304	2,304

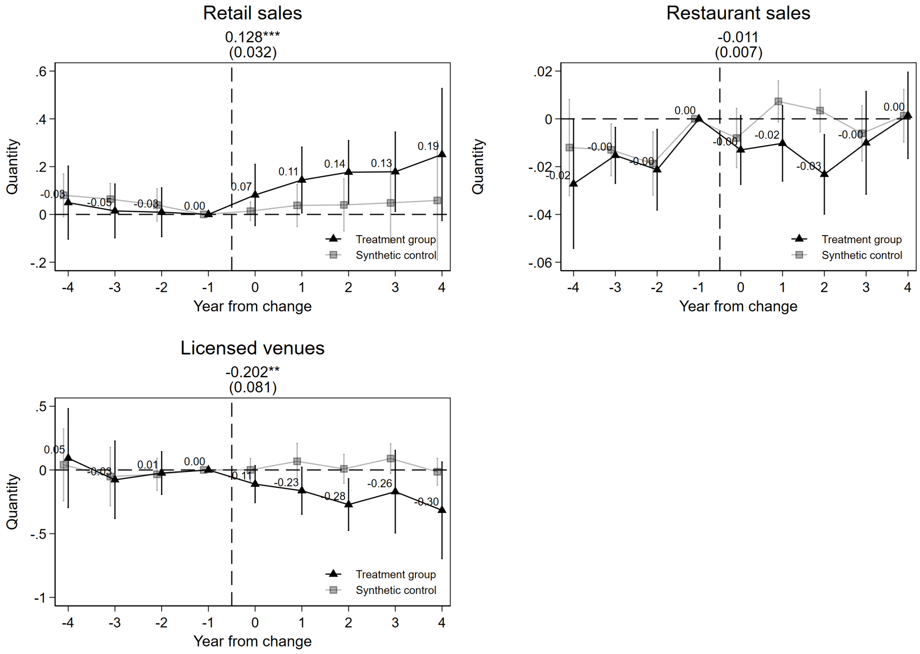
Note.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels *A*, *B* and *C*. Dependent variable defined in column titles. All regressions control for for county-specific time trends, municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 5.
Synthetic control estimates, 2008–2015: Alcohol outcomes I



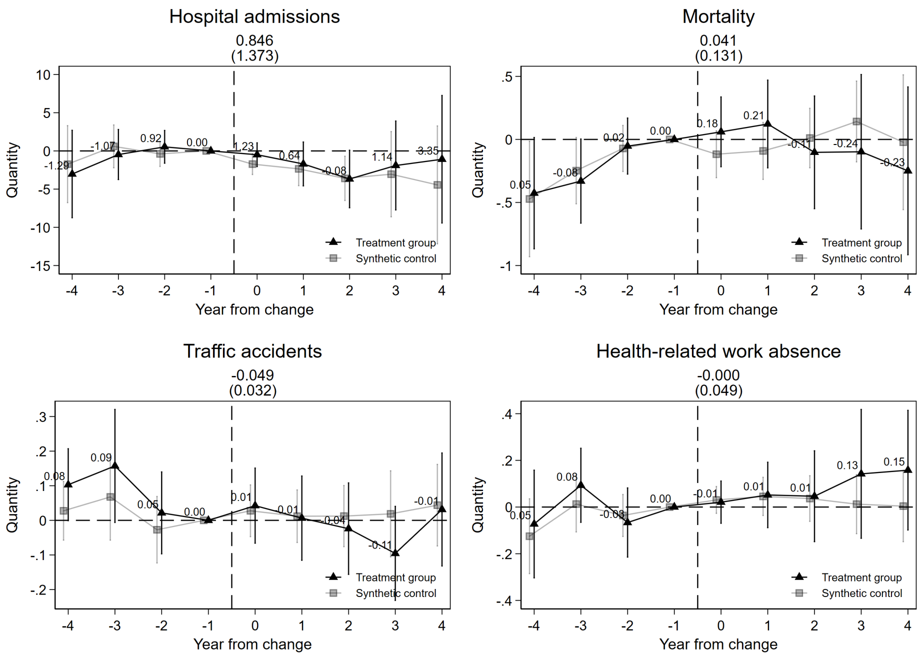
Note.— Each panel refers a specific outcome. Time re-centered around year of first municipality change in Systembolaget weekly opening hours. Point estimates and 95 percent confidence bands obtained from estimation of Equation (2) for municipalities with changes in weekly alcohol retail opening hours (black triangles) and for the corresponding synthetic control matched sample (gray squares). Refer to text for further information. Pooled before-after estimate and standard errors reported in panel title. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 6.
Synthetic control estimates, 2008–2015: Alcohol outcomes II



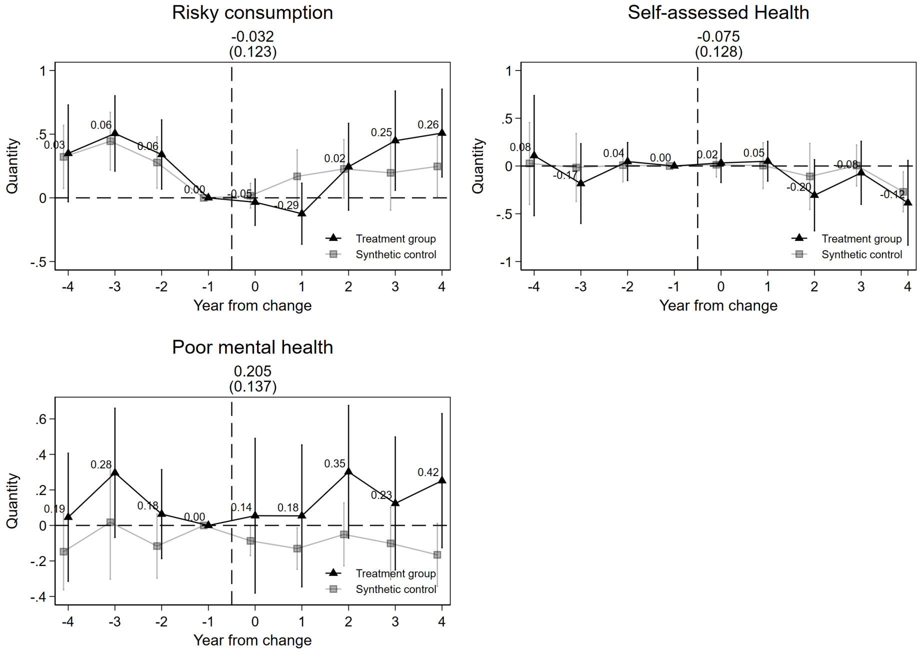
NOTE.— Each panel refers a specific outcome. Time re-centered around year of first municipality change in Systembolaget weekly opening hours. Point estimates and 95 percent confidence bands obtained from estimation of Equation (2) for municipalities with changes in weekly alcohol retail opening hours (black triangles) and for the corresponding synthetic control matched sample (gray squares). Refer to text for further information. Pooled before-after estimate and standard errors reported in panel title. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 7.
Synthetic control estimates, 2008–2015: Health outcomes I



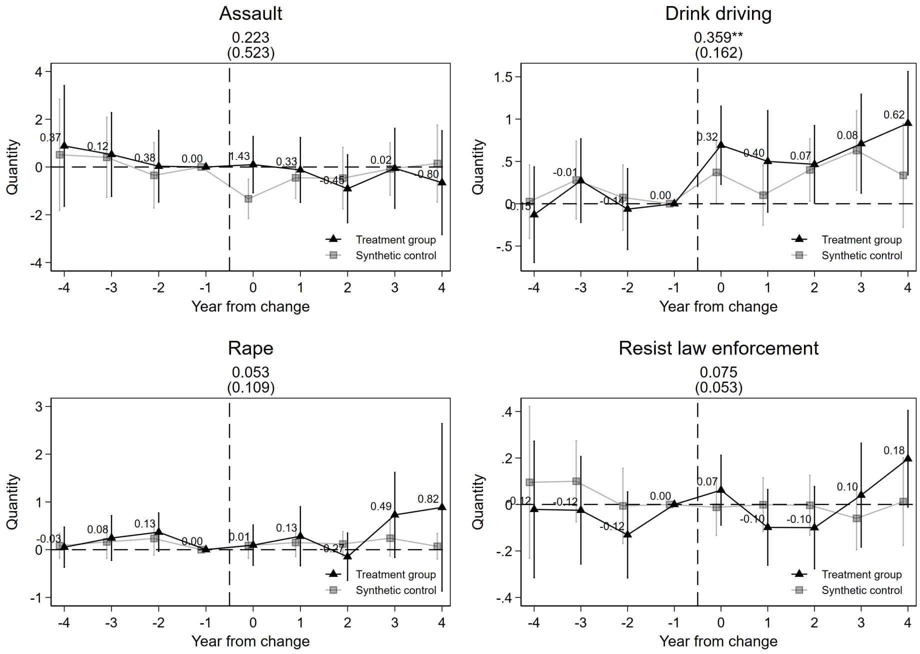
NOTE.— Each panel refers a specific outcome. Time re-centered around year of first municipality change in Systembolaget weekly opening hours. Point estimates and 95 percent confidence bands obtained from estimation of Equation (2) for municipalities with changes in weekly alcohol retail opening hours (black triangles) and for the corresponding synthetic control matched sample (gray squares). Refer to text for further information. Pooled before-after estimate and standard errors reported in panel title. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 8.
Synthetic control estimates, 2008–2015: Health outcomes II



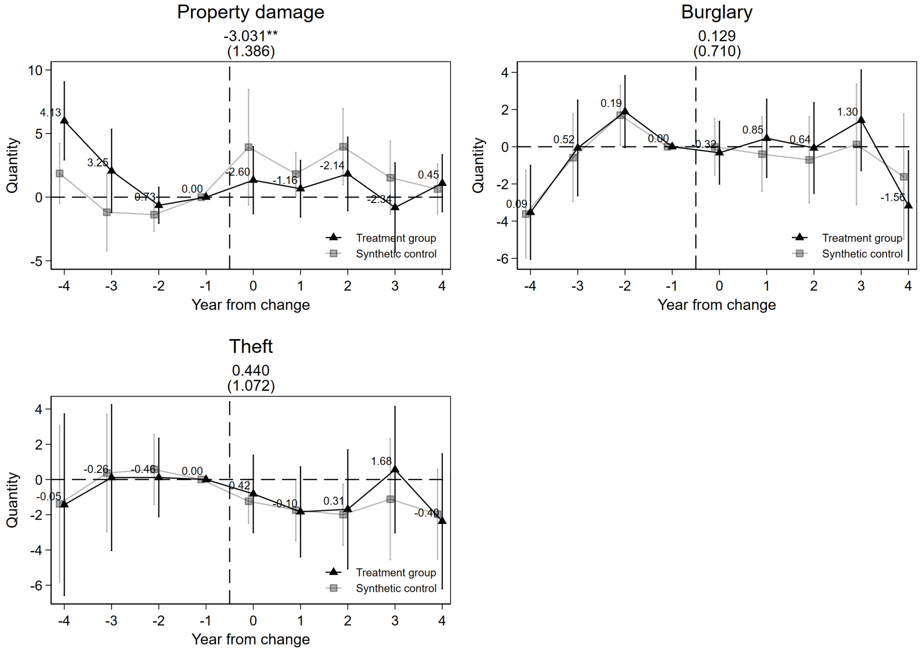
NOTE.— Each panel refers a specific outcome. Time re-centered around year of first municipality change in Systembolaget weekly opening hours. Point estimates and 95 percent confidence bands obtained from estimation of Equation (2) for municipalities with changes in weekly alcohol retail opening hours (black triangles) and for the corresponding synthetic control matched sample (gray squares). Refer to text for further information. Pooled before-after estimate and standard errors reported in panel title. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

FIGURE 9.
Synthetic control estimates, 2008–2015: Crime outcomes I



NOTE.— Each panel refers a specific outcome. Time re-centered around year of first municipality change in Systembolaget weekly opening hours. Point estimates and 95 percent confidence bands obtained from estimation of Equation (2) for municipalities with changes in weekly alcohol retail opening hours (black triangles) and for the corresponding synthetic control matched sample (gray squares). Refer to text for further information. Pooled before-after estimate and standard errors reported in panel title. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 10.
Synthetic control estimates, 2008–2015: Crime outcomes II



Note.— Each panel refers a specific outcome. Time re-centered around year of first municipality change in Systembolaget weekly opening hours. Point estimates and 95 percent confidence bands obtained from estimation of Equation (2) for municipalities with changes in weekly alcohol retail opening hours (black triangles) and for the corresponding synthetic control matched sample (gray squares). Refer to text for further information. Pooled before-after estimate and standard errors reported in panel title. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

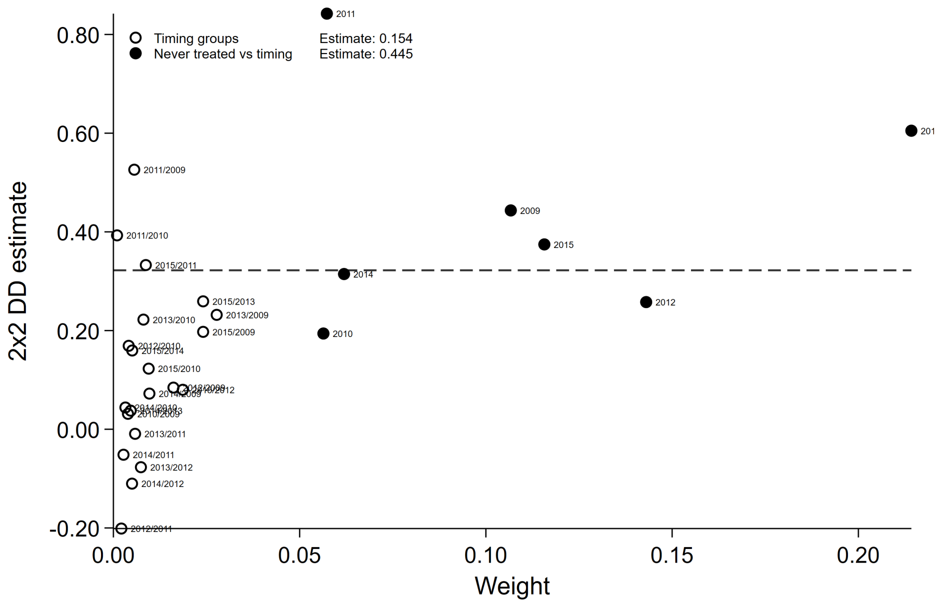
Table 5.
Difference-in-difference estimates, 2008–2015: Spillover effects between neighboring municipalities

	(1)	(2)	(3)	(4)	(5)	(6)
Own	0.149***	0.146***	0.145***	0.145***	0.154***	0.155***
	(0.037)	(0.036)	(0.036)	(0.036)	(0.036)	(0.035)
Neighbor #1		-0.057***	-0.058***	-0.063***	-0.063***	-0.062***
		(0.022)	(0.023)	(0.023)	(0.024)	(0.023)
Neighbor #2			0.030	0.028	0.030	0.033
			(0.051)	(0.050)	(0.050)	(0.051)
Neighbor #3				0.034	0.040	0.041
				(0.039)	(0.040)	(0.041)
Neighbor #4					-0.010	-0.013
					(0.015)	(0.016)
Neighbor #5						0.052
						(0.051)
Net coefficients	0.149	0.089	0.116	0.144	0.150	0.205
Joint F-statistic	15.94	5.853	2.822	2.154	2.407	2.067
p-value	0.000	0.016	0.094	0.143	0.122	0.152
ϵ_d	0.942	0.465	0.294	0.286	0.288	0.211
Municipalities	288	279	270	265	251	239
No. of observations	2,304	2,232	2,160	2,120	2,008	1,912

Note.— Each column reports parameter estimates from a specific regression. Estimates based on estimation of Equation (1). Refer to text in Section 6.1 for details. All regressions control for for county-specific time trends, municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Net coefficients defined as the sum of reported parameter estimates in each column. Joint F-statistics and corresponding p-values for Wald tests of whether sum of reported coefficients is significantly different from zero. ϵ_d defined as the elasticity of demand in alcohol sales from extended Systembolaget opening hours based on log-log model. Robust standard errors clustered by municipality in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 11.

Goodman-Bacon decomposition of difference-in-difference estimate of extended retail opening hours on total sales of alcohol



Note.— Each marker refers to a specific two-group/two-period difference-in-difference estimate (vertical axis) and its corresponding weight (horizontal axis) for the overall estimate (dashed horizontal line) from estimation of Equation (A.1). Estimates are obtained using the Goodman-Bacon decomposition approach from Goodman-Bacon *et al.* (2019) and implemented using the *Stata 16* routine `bacondecomp`. Black dots correspond to comparisons between treated municipalities (labeled by treatment year) and never-treated municipalities. White dots correspond to comparisons between treated municipalities grouped by timing of treatment (labeled by treatment year for treatment and control group).

Table 6.

Systembolaget weekly opening hours transition matrix, 2008–2015

	39	43	44	45	46	47	50	51	52	55	56	Total
38	5	9	1	0	0	0	0	0	0	0	0	15
39	0	**11**	1	1	0	0	0	0	0	0	0	13
40	0	0	1	0	0	0	0	0	0	0	0	1
41	0	0	0	1	0	0	0	0	0	0	0	1
43	0	0	**99**	0	1	0	0	0	0	0	0	100
44	0	0	0	8	0	0	0	0	0	0	0	8
45	0	0	0	0	**12**	3	3	0	0	0	0	18
46	0	0	0	0	0	**10**	4	0	0	0	0	14
47	0	0	0	0	0	0	6	1	1	0	0	8
48	0	0	0	0	0	0	0	2	0	0	0	2
50	0	0	0	0	0	0	0	0	2	2	0	4
51	0	0	0	0	0	0	0	0	0	0	1	1
52	0	0	0	0	0	0	0	0	0	1	0	1
Total	5	20	102	10	13	13	13	3	3	3	1	186

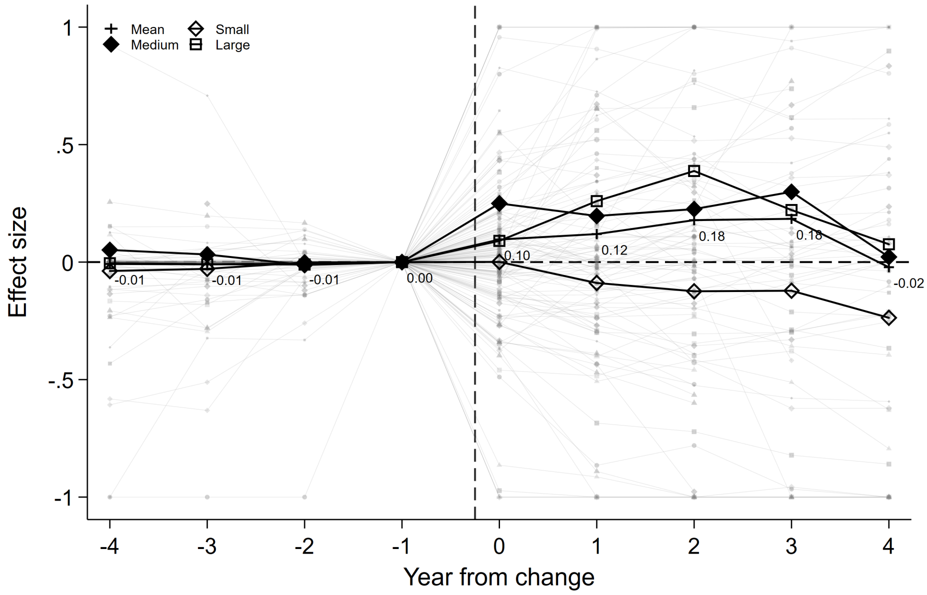
Note.— Origin and destination opening hours reported in table row and column vectors, respectively. Most common transition combinations highlighted in bold font. Municipalities may have multiple changes over the study period.

Table 7.
Difference-in-difference estimates, 2008–2015: Small and large changes in opening hours

	(1) Pooled	(2) Small	(3) Medium	(4) Large
Purchases of spirits	0.043***	-0.012	0.061	0.044***
	(0.012)	(0.027)	(0.045)	(0.013)
No. of observations	2,304	1,864	1,224	1,392
Mean of outcome	2.9	2.7	2.7	3.1
Purchases of wine	0.637***	-0.073	0.657*	0.636***
	(0.144)	(0.270)	(0.391)	(0.160)
No. of observations	2,304	1,864	1,224	1,392
Mean of outcome	20.1	18.6	21.2	22.5
Purchases of strong beer	0.983***	-0.246	0.523	1.021***
	(0.264)	(0.402)	(0.628)	(0.296)
No. of observations	2,304	1,864	1,224	1,392
Mean of outcome	31.2	30.2	30.1	32.5
Purchases of cocktails & cider	0.053*	0.141	-0.015	0.058*
	(0.030)	(0.281)	(0.099)	(0.032)
No. of observations	2,304	1,864	1,224	1,392
Mean of outcome	2.6	2.6	2.7	2.8
Retail sales	0.149***	-0.002	0.137	0.151***
	(0.035)	(0.056)	(0.103)	(0.039)
No. of observations	2,304	1,864	1,224	1,392
Mean of outcome	5.6	5.3	5.6	6.0
Restaurant sales	-0.002	-0.015	0.010	-0.003
	(0.007)	(0.026)	(0.010)	(0.007)
No. of observations	1,470	1,211	1,012	1,021
Mean of outcome	0.6	0.6	0.7	0.7
Licensed venues	-0.043	-0.281	-0.141	-0.036
	(0.068)	(0.361)	(0.138)	(0.076)
No. of observations	2,304	1,864	1,224	1,392
Mean of outcome	17.6	17.7	17.8	18.1

Note.— Each cell reports parameter estimates from a separate regression. Dependent variable defined in leftmost column. Reported coefficients are estimates of β from estimation of Equation (1) for different subsamples of the estimation sample based on municipalities with small (1 hour), medium (2 hours) and large ($>$ 2 hours) extensions in Systembolaget weekly opening hours compared to municipalities without change. All regressions control for for county-specific time trends, municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Mean of outcome defined as the mean of the dependent variable in 2008. Robust standard errors clustered by municipality in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 12.
Distribution of effects of extended retail opening hours on total sales of alcohol



Note.— Time re-centered around year of first municipality change in Systembolaget weekly opening hours. Bold markers correspond to group-specific unadjusted mean differences for municipalities with small (1 hour), medium (2 hours) and large (> 2 hours) extensions in Systembolaget weekly opening hours compared to their matched synthetic controls. Estimates rescaled to the year prior to the opening hours extension and winsorized to the 1st and 99th percentiles. Refer to text in Section 4.2 and Section 6.3 for details. Gray markers lines pertain to *municipality-specific* differences.

Extending alcohol retailers' opening hours: Evidence from Sweden

— Online Appendix —

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Appendix A Decomposition and randomization inference

The parameter of interest in Equation (1), β, can be interpreted as the DD estimand in the simple two group/two time period case (see, e.g., Bertrand *et al.*, 2004). However, this does not necessarily hold in "staggered" settings where treatment start times vary across units (see, e.g., Imai and Kim, 2020; de Chaisemartin and D'Haultfoeuille, 2020). Indeed, in our setting, opening times are changed in different years across different municipalities. We here follow Goodman-Bacon (in press), decomposing the two-way fixed effects DD estimator into a weighted average of all possible two-group/two period DD estimators. Although this method refers specifically to binary treatments, whereas we have a continuous treatment setting, we here study the properties of our DD estimator.

First, we convert our continuous treatment into a binary indicator for a change in opening hours.[a] Second, we estimate the standard two-way fixed effects DD model using the binary treatment (instead of the continuous "opening hours"), as in:

$$y_{mt} = \beta D_m t + (X_{m,2008} \times t)'\eta_x + \lambda_c \times t + \lambda_m + \lambda_t + \varepsilon_{mt}, \tag{A.1}$$

where D_{mt} is a dummy variable that is equal to one if municipality m increases the opening hours of alcohol retailers in (or before) year t, and zero otherwise. The other covariates are identical to those specified in Equation (1), Section 4.1. Third, we use the *Stata 16* routine `bacondecomp` described in Goodman-Bacon *et al.* (2019) to explore whether our findings are robust to using this different approach.

Column (1) of Table A.1 starts by presenting the estimates from the DD model, only controlling for the municipality and calendar year fixed effects and county-specific trends, while column (2) adds the terms $X_{m,2008} \times t$ of Equation (A.1). The parameters are similar, with column (2) showing that being treated increases sales of 100% pure alcohol by 0.32 liters per capita and year.

[a]For municipalities that changed their opening hours more than once, this refers to the *first* change in opening hours.

Table A.1.
Robustness check: Comparison of estimates from different models

	(1) DD No controls	(2) DD Controls	(3) Bacon decomp	(4) Synthetic control
Treatment dummy	0.381***	0.322***	0.322***	0.312***
	(0.122)	(0.102)	(0.095)	(0.097)
Timing groups			0.154	
			[0.197]	
Never vs timing			0.445	
			[0.755]	
Mean of outcome	5.6	5.6	5.6	5.6
No. of groups	288	288	288	152
No. of observations	2,304	2,304	2,304	1,216

Note.— Dependent variable is total per capita retail sales of alcohol in liters of 100% ethanol. Reported coefficients in Columns (1)–(3) are estimates of β from estimation of Equation (A.1) for different specifications. Column (1) controls for municipality and year fixed effects, and Column (2) additionally includes controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Column (3) decomposes the overall difference-in-differences estimates into treatment timing groups according to Goodman-Bacon *et al.* (2019). Group-specific estimation weights are reported in brackets. See also Figure 11 and related text. Column (4) provide estimates from the synthetic control method described in Section 4.2 based on Equation (2) excluding the scaling parameter γ and including a binary treatment variable. See also Figure A.1 and related text. "Mean of outcome" is defined as the mean of the dependent variable in 2008. Robust standard errors clustered by municipality in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

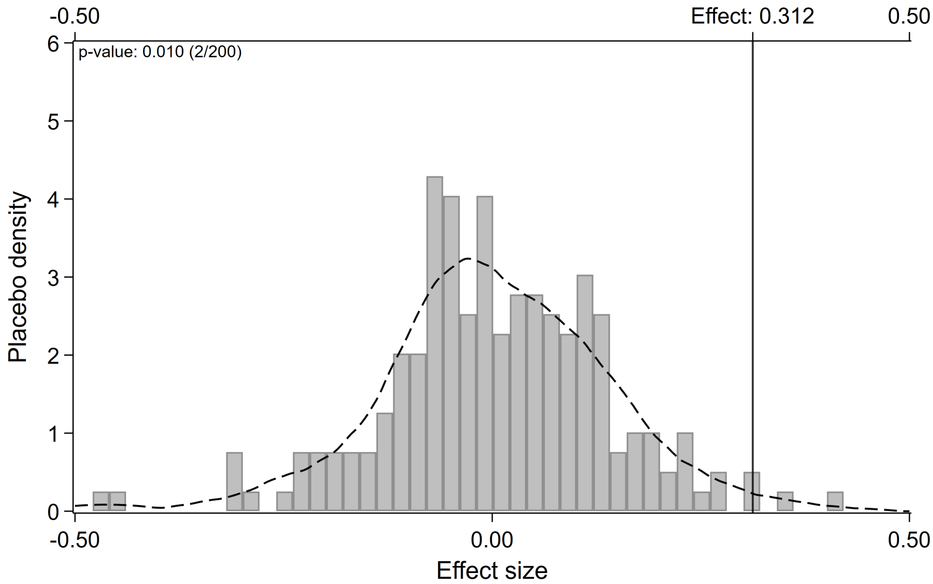
Decomposing the two-way fixed effects model in column (3), using the Goodman-Bacon decomposition, shows a very similar treatment effect overall, with treatment increasing alcohol sales by 0.32 litres of pure alcohol per capita per year. Investigating this in more detail shows that this is driven by the extensive margin (i.e., treated municipalities versus never-treated municipalities), where the treatment effect is estimated to be 0.45. In contrast, the estimates at the intensive margin, specifying control municipalities as those who were treated in a *different* calendar year from the treated municipality, are substantially smaller, with an estimated effect size of 0.15. Note, however, that this lower estimate may reflect the fact that some of the treated municipalities extended their opening hours multiple times during the analysis period, whereas the estimates in Table A.1 are solely based on the first observed change.

To further explore the robustness of our DD results, we next use the synthetic control method, detailed in Section 4.2. We create a synthetic control municipality for each treated municipality that changed their opening times, based on the covariate values prior to the change in opening times. This is presented in Column (4) of Table A.1, showing that our results are robust to this synthetic control specification.

Finally, we ask whether the results in Table A.1 could be driven entirely by chance. As in Abadie and Gardeazabal (2003), Bertrand *et al.* (2004) and Abadie *et al.* (2010), we use placebo tests to explore the likelihood of obtaining a treatment effect estimate of this magnitude. More specifically, we use the synthetic control method to municipalities that did *not* change their opening times during our period of observation and estimate the treatment effect for these "placebo treatments". If we obtain estimates of similar magnitude to those in Table A.1, it suggests that there is insufficient evidence that opening times affect the demand for alcohol, hence suggesting that our estimates could be driven by chance. However, if our placebo effects are substantially different than those in Table A.1, we interpret this as robust evidence of an effect of opening times on alcohol sales.

To do this, we create a synthetic control for each control municipality. We then generate the empirical distribution of placebo treatment effects using bootstrap replications on 10% of all control municipalities, where the (binary) placebo treatment is set to one for a randomly selected year in each control municipality. Figure A.1 shows the distribution of placebo effects for 200 bootstrap replications, with our estimated synthetic control effect from column (4) in Table A.1 denoted by the solid vertical line. This shows that our estimate is substantially larger than the distribution of "placebo treatment effects" for control municipalities. More specifically, the probability of obtaining an estimate of this magnitude if we would randomly label some of the control municipalities as treated, is 0.02, shown in the top left hand corner of Figure A.1. Hence, we conclude that it is very unlikely that our treatment effect is driven by chance.

Figure A.1.
Placebo test of estimate of extended retail opening hours on total sales of alcohol



Note.— Bars reflect effects densities from synthetic control placebo estimates using (control) municipalities for which Systembolaget weekly opening hours remained unchanged. Each estimate is based on a 10% random bootstrap sample with replacement. Placebo municipalities are randomly assigned to treatment in one analysis year and matched with a synthetic control based on pre-intervention outcome levels and trends. Dashed line refers to a non-parametric kernel density plot of the resulting empirical distribution of placebo treatment effects. Vertical line refers to the "true " treatment effect estimate from applying the synthetic control method on the sample of treated municipalities with opening hours extensions. p-value reports the empirical probability that the true treatment effect is less than or equal to the placebo effect.

Appendix B Additional Tables and Figures

Table B.1.
Descriptive sample statistics by treatment status

	2008					
	All	Control	Treated	T-C	*p*	%Diff
Alcohol variables						
Purchases of spirit	3.0	2.8	3.1	0.3	0.41	8.6
Purchases of wine	23.3	21.0	19.3	-1.7	0.50	-7.5
Purchases of strong beer	33.8	30.6	31.9	1.3	0.59	3.9
Purchases of cocktails & cider	3.0	2.7	2.5	-0.2	0.29	-6.6
Total retail sales	6.2	5.6	5.6	-0.1	0.91	-1.1
Restaurant sales	0.6	0.7	0.5	-0.2	0.05	-35.5
Licensed venues	18.9	18.1	17.2	-0.9	0.55	-4.7
Health variables						
Alcohol-related hospital admissions	321.0	302.1	284.9	-17.2	0.08	-5.4
Alcohol-related mortality	12.2	13.3	12.4	-0.9	0.23	-7.4
Alcohol-related traffic accidents	14.0	1.6	1.7	0.1	0.52	0.7
Risky alcohol consumption	14.9	15.6	14.5	-1.1	0.00	-7.4
Health-related work absence	33.2	38.5	41.4	2.9	0.00	8.6
Poor mental health	15.1	15.8	14.8	-1.0	0.00	-6.7
Good self-assessed health	69.0	68.4	66.6	-1.8	0.00	-2.6
Crime variables						
Assault	72.3	78.5	66.9	-11.5	0.00	-16.0
Rape	5.0	4.8	4.2	-0.6	0.15	-12.5
Property damage	21.7	40.1	18.1	-22.0	0.00	-101.3
Burglary	91.1	97.6	100.0	2.4	0.60	2.6
Theft	140.2	175.0	137.1	-37.8	0.00	-27.0
Drink driving	17.3	21.1	20.4	-0.8	0.47	-4.5
Resist law enforcement	3.0	3.7	2.1	-1.6	0.00	-53.9
Municipality characteristics						
Support ratio	0.8	0.8	0.8	0.0	0.00	4.3
Women	49.6	50.0	49.6	-0.4	0.00	-0.9
Population density	139.6	169.7	98.7	-71.0	0.18	-50.9
Low educated	15.8	16.6	18.8	2.2	0.00	13.8
Employed	78.1	79.2	78.8	-0.4	0.38	-0.5
Unemployed	6.6	3.8	3.9	0.2	0.38	2.3
Startups	4.9	5.3	4.7	-0.6	0.00	-12.9
Net commuters	-11.6	-9.0	-13.7	-4.6	0.04	40.0
Net annual income	194.1	182.0	176.8	-5.2	0.01	-2.7
Deprived households	10.1	10.0	10.4	0.4	0.37	3.6
Non-EU born	6.1	5.6	4.3	-1.2	0.00	-20.4
Net result	1,071.0	530.0	678.8	148.8	0.28	13.9
Tax rate	21.5	21.4	21.6	0.2	0.32	0.7
Sweden democrats	5.5	2.7	2.5	-0.1	0.71	-2.2

Note.— Municipality sample statistics in year 2008. Alcohol variables measured in liters per capita per year, except for total retail and restaurant sales measured in liters of 100% ethanol, and licensed venues measured in rate per 100,000 population over 15 years of age. Alcohol-related hospital admissions, mortality and traffic accidents measured in rates per 100,000 population over 15 years of age. Health-related work absence measured in average days per capita. Remaining health variables measured as percentages. Crime variables measured as rates per 10,000 population. Support ratio is the number of municipality residents aged 16-64 per resident aged 65 or older. Population density is population per square kilometer. Low educated is the population share with only compulsory education. Employed and unemployed are percentages of the working-age population registered as employed and unemployed, respectively. Startups is the number of new businesses per 1,000 population. Net commuting is the difference between number of in- and out-commuters as a share of the total number of employed. Net annual income is the average annual after-tax income of the working-age population in SEK 1,000. Deprived households is the share of households receiving social support. Non-EU born is the population share born outside of the European Economic Area. Net result is the municipality's economic result (total revenue minus expenses) per capita in SEK. Tax rate is the percentage municipality tax rate. Sweden Democrats is the percentage vote share for the national-conservative populist political party in the last municipality election.

Table B.2.

Difference-in-difference estimates, 2008–2015: Alcohol outcomes without SUR correction

	(1) Spirit	(2) Wine	(3) Strong beer	(4) Cocktails & cider	(5) Retail sales	(6) Restaurant sales	(7) Licensed venues
Panel A: Lin-Lin							
Opening Hours	0.043***	0.637***	0.983***	0.053	0.149***	-0.002	-0.040
	(0.007)	(0.090)	(0.111)	(0.101)	(0.016)	(0.005)	(0.045)
	[0.012]	[0.144]	[0.264]	[0.030]	[0.035]	[0.007]	[0.068]
Panel B: Log-Lin							
Opening Hours	0.019***	0.022***	0.025***	0.021***	0.021***	-0.008	-0.003
	(0.002)	(0.005)	(0.002)	(0.006)	(0.002)	(0.007)	(0.003)
	[0.005]	[0.005]	[0.006]	[0.006]	[0.005]	[0.012]	[0.004]
Panel C: Log-Log							
Opening Hours	0.842***	0.984***	1.110***	0.955***	0.942***	-0.336	-0.146
	(0.083)	(0.239)	(0.097)	(0.298)	(0.074)	(0.361)	(0.119)
	[0.250]	[0.240]	[0.285]	[0.302]	[0.247]	[0.591]	[0.185]
Mean of outcome	2.9	20.1	31.2	2.6	5.6	0.6	17.6
No. of groups	288	288	288	288	288	224	288
No. of observations	2,304	2,304	2,304	2,304	2,304	1,470	2,304

Note.— Each cell reports parameter estimates from a separate regression, not adjusting for cross-equation residual correlation. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels *A*, *B* and *C*. Numbers in (parenthesis) and [brackets] pertain to standard errors without and with SUR correction, respectively. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3.

Difference-in-difference estimates, 2008–2015: Health outcomes without SUR correction

	(1) Hospital Adm.	(2) Mortality	(3) Traffic Acc.	(4) Work Absence	(5) Risk Cons.	(6) Self-Ass. Health	(7) Mental Health
Panel A: Lin-Lin							
Opening Hours	1.103**	0.085	-0.027	-0.012	-0.076*	-0.085	0.119**
	(0.556)	(0.074)	(0.023)	(0.026)	(0.044)	(0.053)	(0.048)
	[0.862]	[0.111]	[0.021]	[0.056]	[0.076]	[0.076]	[0.129]
Panel B: Log-Lin							
Opening Hours	0.004**	-0.001	-0.052	-0.001	-0.004	-0.001	0.008***
	(0.002)	(0.011)	(0.036)	(0.001)	(0.003)	(0.001)	(0.003)
	[0.003]	[0.013]	[0.035]	[0.001]	[0.005]	[0.001]	[0.007]
Panel C: Log-Log							
Opening Hours	0.175**	-0.109	-2.408	-0.060*	-0.177	-0.057	0.384***
	(0.085)	(0.540)	(1.692)	(0.034)	(0.136)	(0.036)	(0.142)
	[0.135]	[0.662]	[1.655]	[0.058]	[0.226]	[0.051]	[0.329]
Mean of outcome	292.9	12.7	1.6	40.0	15.0	27.1	15.3
No. of groups	288	288	288	288	282	282	282
No. of observations	2,304	2,304	2,304	2,304	1,648	1,648	1,648

Note.— Each cell reports parameter estimates from a separate regression, not adjusting for cross-equation residual correlation. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels *A*, *B* and *C*. Numbers in (parenthesis) and [brackets] pertain to standard errors without and with SUR correction, respectively. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4.
Difference-in-difference estimates, 2008–2015: Crime outcomes without SUR correction

	(1) Assault	(2) Drink Driving	(3) Rape	(4) Resist Law Enf.	(5) Prop. Damage	(6) Burglary	(7) Theft
Panel A: Lin-Lin							
Opening Hours	-0.177	0.175	0.139	0.066	-0.454	0.475	0.758
	(0.294)	(0.108)	(0.145)	(0.042)	(1.106)	(0.485)	(0.632)
	[0.340]	[0.122]	[0.110]	[0.054]	[1.112]	[0.537]	[1.065]
Panel B: Log-Lin							
Opening Hours	-0.002	0.005	-0.011	0.025	-0.048*	0.004	0.003
	(0.004)	(0.007)	(0.021)	(0.031)	(0.027)	(0.005)	(0.003)
	[0.004]	[0.010]	[0.016]	[0.026]	[0.022]	[0.006]	[0.005]
Panel C: Log-Log							
Opening Hours	-0.108	0.258	-0.601	1.212	-2.340*	0.171	0.146
	(0.189)	(0.333)	(1.008)	(1.452)	(1.281)	(0.247)	(0.157)
	[0.191]	[0.472]	[0.783]	[1.269]	[1.053]	[0.268]	[0.221]
Mean of outcome	72.3	20.7	4.4	2.8	28.4	98.8	154.9
No. of groups	288	288	288	288	288	288	288
No. of observations	2,304	2,304	2,304	2,304	2,304	2,304	2,304

Note.— Each cell reports parameter estimates from a separate regression, not adjusting for cross-equation residual correlation. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels *A*, *B* and *C*. Numbers in (parenthesis) and [brackets] pertain to standard errors without and with SUR correction, respectively. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5.
Difference-in-difference estimates, 2008–2015: Alcohol outcomes, excluding large cities (Stockholm, Göteborg and Malmö)

	(1) Spirit	(2) Wine	(3) Strong beer	(4) Cocktails & cider	(5) Retail sales	(6) Restaurant sales	(7) Licensed venues
Panel A: Lin-Lin							
Opening Hours	0.046***	0.660***	1.019***	0.060*	0.155***	-0.003	-0.011
	(0.012)	(0.146)	(0.270)	(0.030)	(0.035)	(0.006)	(0.066)
Panel B: Log-Lin							
Opening Hours	0.020***	0.022***	0.025***	0.023***	0.022***	-0.007	-0.001
	(0.005)	(0.005)	(0.006)	(0.007)	(0.005)	(0.012)	(0.004)
Panel C: Log-Log							
Opening Hours	0.893***	1.004***	1.138***	1.002***	0.970***	-0.291	-0.064
	(0.255)	(0.245)	(0.295)	(0.311)	(0.255)	(0.566)	(0.184)
Mean of outcome	2.9	20.1	31.3	2.6	5.6	0.6	17.6
No. of groups	285	285	285	285	285	221	285
No. of observations	2,280	2,280	2,280	2,280	2,280	1,446	2,280

Note.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels *A*, *B* and *C*. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects. We do not control for the 2008 municipality-level characteristics interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6.
Difference-in-difference estimates, 2008–2015: Health outcomes, excluding large cities (Stockholm, Göteborg and Malmö)

	(1) Assault	(2) Drink Driving	(3) Rape	(4) Resist Law Enf.	(5) Prop. Damage	(6) Burglary	(7) Theft
Panel A: Lin-Lin							
Opening Hours	1.113	0.086	-0.029	-0.020	-0.108	-0.081	0.102
	(0.885)	0.118	(0.022)	0.055	(0.069)	0.068	0.126
Panel B: Log-Lin							
Opening Hours	0.004	0.000	-0.055	-0.001	-0.006	-0.001	0.008
	(0.003)	(0.014)	(0.035)	(0.001)	(0.004)	(0.001)	(0.007)
Panel C: Log-Log							
Opening Hours	0.168	-0.081	-2.552	-0.067	-0.253	-0.055	0.356
	(0.137)	(0.684)	(1.646)	(0.057)	(0.213)	(0.045)	(0.320)
Mean of outcome	291.6	12.7	1.6	40.1	15.0	67.4	15.2
No. of groups	285	285	285	285	279	279	279
No. of observations	2,280	2,280	2,280	2,280	1,624	1,624	1,624

Note.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels *A*, *B* and *C*. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects. We do not control for the 2008 municipality-level characteristics interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7.
Difference-in-difference estimates, 2008–2015: Crime outcomes, excluding large cities (Stockholm, Göteborg and Malmö)

	(1) Assault	(2) Drink Driving	(3) Rape	(4) Resist Law Enf.	(5) Prop. Damage	(6) Burglary	(7) Theft
Panel A: Lin-Lin							
Opening Hours	-0.115	0.201	0.165	0.067	0.547	0.229	0.500
	(0.340)	(0.122)	(0.119)	(0.053)	(1.362)	(0.525)	(0.999)
Panel B: Log-Lin							
Opening Hours	-0.002	0.006	-0.008	0.026	-0.039*	0.002	0.003
	(0.004)	(0.010)	(0.016)	(0.026)	(0.022)	(0.006)	(0.005)
Panel C: Log-Log							
Opening Hours	-0.097	0.312	-0.501	1.252	-1.900*	0.079	0.133
	(0.194)	(0.472)	(0.802)	(1.295)	(1.076)	(0.270)	(0.218)
Mean of outcome	71.9	20.7	4.4	2.8	27.3	98.6	152.3
No. of groups	285	285	285	285	285	285	285
No. of observations	2,280	2,280	2,280	2,280	2,280	2,280	2,280

Note.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels *A*, *B* and *C*. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects. We do not control for the 2008 municipality-level characteristics interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.8.
Difference-in-difference estimates, 2008–2015: Drug-related crime outcomes

	lin-lin		log-lin		log-log	
	(1) Drug use	(2) Drug possession	(3) Drug use	(4) Drug possession	(5) Drug use	(6) Drug possession
Opening Hours	-0.052	-0.031	0.013	0.003	0.609	0.080
	(0.349)	(0.607)	(0.012)	(0.013)	(0.572)	(0.604)
Mean of outcome	14.927	37.138	2.301	3.33	2.301	3.33
No. of groups	288	288	288	288	288	288
No. of observations	2,304	2,304	2,304	2,304	2,304	2,304

NOTE.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours. The dependent variable is defined in the column titles. All regressions control for county-specific time trends, municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008: the number of cases of drug use/possession per 10,000 population. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression (SUR) framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

TABLE B.9.
Difference-in-difference estimates, 2008–2015: Alcohol outcomes, not controlling for county-specific trends

	(1) Spirit	(2) Wine	(3) Strong beer	(4) Cocktails & cider	(5) Retail sales	(6) Restaurant sales	(7) Licensed venues
Panel A: Lin-Lin							
Opening Hours	0.041***	0.644***	0.937***	0.010	0.143***	-0.001	-0.065
	(0.012)	(0.155)	(0.277)	(0.041)	(0.037)	(0.008)	(0.069)
Panel B: Log-Lin							
Opening Hours	0.017***	0.024***	0.024***	0.016**	0.021***	-0.004	-0.004
	(0.006)	(0.005)	(0.006)	(0.008)	(0.005)	(0.013)	(0.004)
Panel C: Log-Log							
Opening Hours	0.764***	1.086***	1.098***	0.735**	0.924***	-0.178	-0.203
	(0.264)	(0.231)	(0.274)	(0.363)	(0.238)	(0.632)	(0.188)
Mean of outcome	2.9	20.1	31.2	2.6	5.6	0.6	17.6
No. of groups	288	288	288	288	288	224	288
No. of observations	2,304	2,304	2,304	2,304	2,304	1,470	2,304

NOTE.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels *A*, *B* and *C*. Dependent variable defined in column headers. All regressions control for municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend (but not county-specific time trends). Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.10.
Difference-in-difference estimates, 2008–2015: Health outcomes, not controlling for county-specific trends

	(1) Hospital Adm.	(2) Mortality	(3) Traffic Acc.	(4) Work Absence	(5) Risk Cons.	(6) Self-Ass. Health	(7) Mental Health
Panel A: Lin-Lin							
Opening Hours	0.597	0.118	-0.017	0.090	-0.056	-0.068	0.133
	(1.247)	(0.102)	(0.022)	(0.093)	(0.077)	(0.075)	(0.115)
Panel B: Log-Lin							
Opening Hours	0.002	0.003	-0.042	0.002	-0.003	-0.001	0.008
	(0.004)	(0.012)	(0.035)	(0.002)	(0.005)	(0.001)	(0.007)
Panel C: Log-Log							
Opening Hours	0.061	0.080	-1.924	0.072	-0.098	-0.047	0.402
	(0.188)	(0.625)	(1.665)	(0.102)	(0.242)	(0.051)	(0.298)
Mean of outcome	292.9	12.7	1.6	40.0	15.0	27.1	15.3
No. of groups	288	288	288	288	282	282	282
No. of observations	2,304	2,304	2,304	2,304	1,648	1,648	1,648

Note.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels A, B and C. Dependent variable defined in column headers. All regressions control for municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend (but not county-specific time trends). Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.11.
Difference-in-difference estimates, 2008–2015: Crime outcomes, not controlling for county-specific trends

	(1) Assault	(2) Drink Driving	(3) Rape	(4) Resist Law Enf.	(5) Prop. Damage	(6) Burglary	(7) Theft
Panel A: Lin-Lin							
Opening Hours	0.179	0.234	0.142	0.075	-0.173	0.492	0.453
	(0.442)	(0.118)	(0.135)	(0.053)	(1.155)	(0.523)	(1.102)
Panel B: Log-Lin							
Opening Hours	0.001	0.007	-0.015	0.03	-0.047**	0.006	0.003
	(0.006)	(0.01)	(0.016)	(0.024)	(0.021)	(0.006)	(0.005)
Panel C: Log-Log							
Opening Hours	0.056	0.357	-0.792	1.455	-2.281**	0.289	0.131
	(0.279)	(0.466)	(0.765)	(1.186)	(1.022)	(0.261)	(0.218)
Mean of outcome	72.3	20.7	4.4	2.8	28.4	98.8	154.9
No. of groups	288	288	288	288	288	288	288
No. of observations	2,304	2,304	2,304	2,304	2,304	2,304	2,304

Note.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels A, B and C. Dependent variable defined in column headers. All regressions control for municipality and year fixed effects, as well as all controls listed in Table 1 defined in 2008 levels and interacted with a linear calendar year trend (but not county-specific time trends). Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.12.
Difference-in-difference estimates, 2008–2015: Alcohol outcomes, not controlling for municipality-specific 2008 characteristics interacted with time trend

	(1) Spirit	(2) Wine	(3) Strong beer	(4) Cocktails & cider	(5) Retail sales	(6) Restaurant sales	(7) Licensed venues
Panel A: Lin-Lin							
Opening Hours	0.049***	0.720***	1.122***	0.075**	0.169***	-0.004	-0.091
	(0.011)	(0.175)	(0.323)	(0.031)	(0.040)	(0.008)	(0.076)
Panel B: Log-Lin							
Opening Hours	0.020***	0.023***	0.025***	0.024***	0.022***	-0.011	-0.006
	(0.005)	(0.005)	(0.005)	(0.006)	(0.005)	(0.012)	(0.004)
Panel C: Log-Log							
Opening Hours	0.898***	1.072***	1.157***	1.085***	0.989***	-0.508	-0.300
	(0.231)	(0.228)	(0.258)	(0.282)	(0.224)	(0.580)	(0.193)
Mean of outcome	2.9	20.1	31.2	2.6	5.6	0.6	17.6
No. of groups	288	288	288	288	288	224	288
No. of observations	2,304	2,304	2,304	2,304	2,304	1,470	2,304

Note.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels A, B and C. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects. We do not control for the 2008 municipality-level characteristics interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13.
Difference-in-difference estimates, 2008–2015: Health outcomes, not controlling for municipality-specific 2008 characteristics interacted with time trend

	(1) Hospital Adm.	(2) Mortality	(3) Traffic Acc.	(4) Work Absence	(5) Risk Cons.	(6) Self-Ass. Health	(7) Mental Health
Panel A: Lin-Lin							
Opening Hours	1.793*	0.037	-0.026	-0.052	-0.023	-0.150**	0.146
	(0.951)	(0.118)	(0.021)	(0.071)	(0.070)	(0.073)	(0.136)
Panel B: Log-Lin							
Opening Hours	0.006*	-0.003	-0.062*	-0.001	-0.002	-0.002*	0.009
	(0.003)	(0.013)	(0.036)	(0.002)	(0.004)	(0.001)	(0.007)
Panel C: Log-Log							
Opening Hours	0.292*	-0.160	-2.765	-0.050	-0.055	-0.091*	0.414
	(0.147)	(0.661)	(1.731)	(0.072)	(0.215)	(0.049)	(0.342)
Mean of outcome	292.9	12.7	1.6	40.0	15.0	27.1	15.3
No. of groups	288	288	288	288	282	282	282
No. of observations	2,304	2,304	2,304	2,304	1,648	1,648	1,648

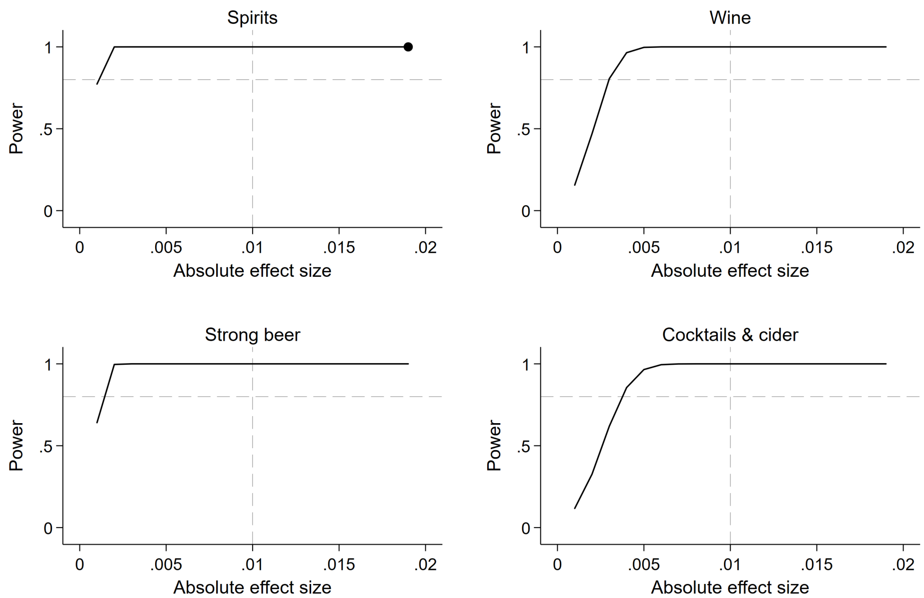
Note.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels A, B and C. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects. We do not control for the 2008 municipality-level characteristics interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.14.
Difference-in-difference estimates, 2008–2015: Crime outcomes, not controlling for municipality-specific 2008 characteristics interacted with time trend

	(1) Assault	(2) Drink Driving	(3) Rape	(4) Resist Law Enf.	(5) Prop. Damage	(6) Burglary	(7) Theft
Panel A: Lin-Lin							
Opening Hours	0.366	0.308***	0.155	0.120*	-2.069	0.317	-0.998
	(0.392)	(0.116)	(0.126)	(0.063)	(1.733)	(0.501)	(1.207)
Panel B: Log-Lin							
Opening Hours	0.003	0.012	-0.013	0.043*	-0.061**	0.003	-0.003
	(0.004)	(0.011)	(0.015)	(0.026)	(0.024)	(0.005)	(0.005)
Panel C: Log-Log							
Opening Hours	0.170	0.613	-0.646	2.132*	-3.034**	0.129	-0.147
	(0.210)	(0.492)	(0.750)	(1.256)	(1.143)	(0.257)	(0.247)
Mean of outcome	72.3	20.7	4.4	2.8	28.4	98.8	154.9
No. of groups	288	288	288	288	288	288	288
No. of observations	2,304	2,304	2,304	2,304	2,304	2,304	2,304

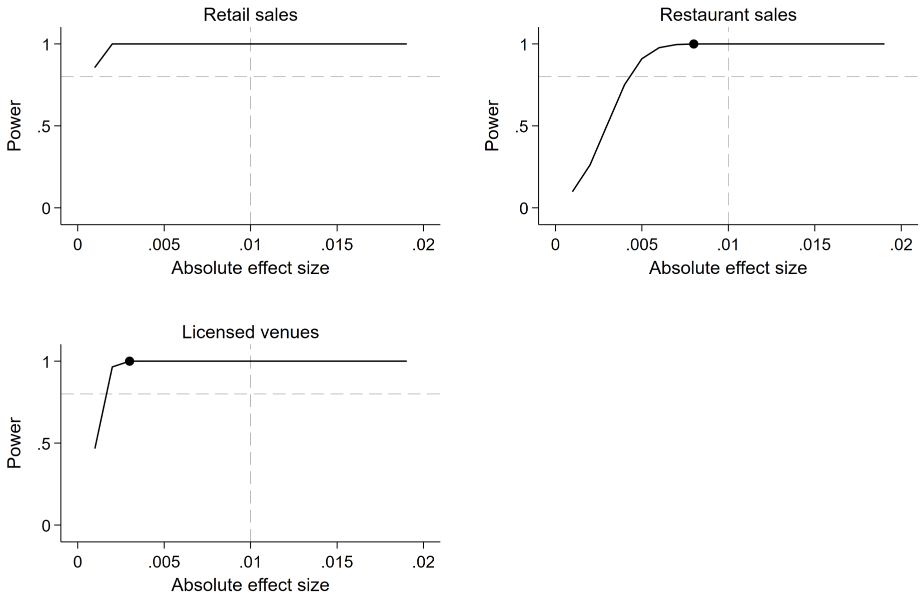
Note.— Each cell reports parameter estimates from a separate regression. Reported coefficients are estimates of β from estimation of Equation (1) under a linear or logarithmic specification of the outcome and Systembolaget weekly opening hours, partitioned into panels A, B and C. Dependent variable defined in column headers. All regressions control for county-specific time trends, municipality and year fixed effects. We do not control for the 2008 municipality-level characteristics interacted with a linear calendar year trend. Robust standard errors clustered by municipality in parentheses. Mean of outcome relates to the mean of the dependent variable in 2008. Standard errors adjusted for cross-equation residual correlations using a seemingly unrelated regression framework. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.1.
Power calculations: Alcohol outcomes I



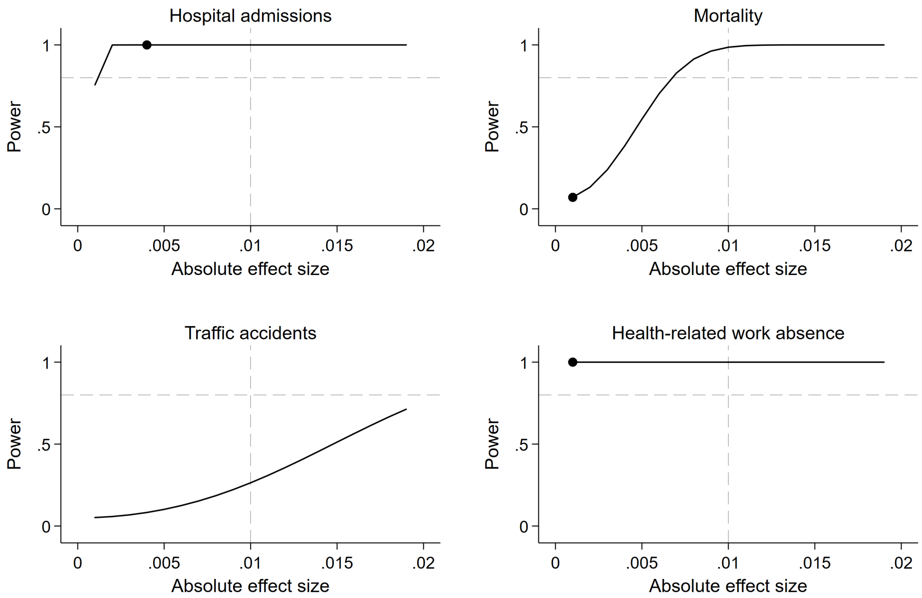
Note.— Each panel refers a specific outcome. Graphs display power calculations relating the probability of rejecting a false null hypothesis to the size of the effect of opening hours on the specific outcome. The x-axis display the effect size in percentages as interpreted from the log-linear model in Table 2–Table 4. The intersection of the dashed lines indicates the point where a one percentage effect can be detected with a power of 0.8. The black circle indicates the estimated effect size reported in Table 2–Table 4 for the respective outcome.

FIGURE B.2.
Power calculations: Alcohol outcomes II



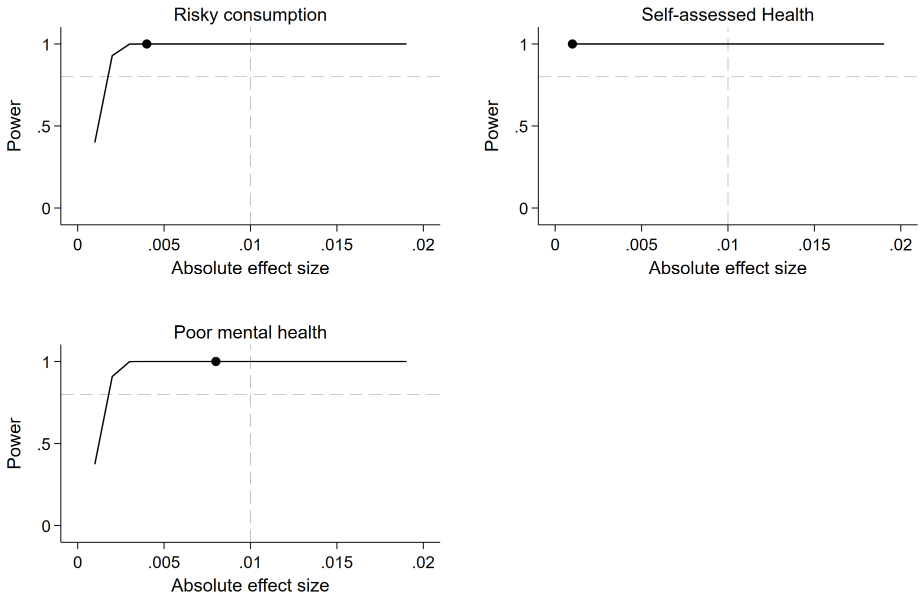
NOTE.— Each panel refers a specific outcome. Graphs display power calculations relating the probability of rejecting a false null hypothesis to the size of the effect of opening hours on the specific outcome. The x-axis display the effect size in percentages as interpreted from the log-linear model in Table 2–Table 4. The intersection of the dashed lines indicates the point where a one percentage effect can be detected with a power of 0.8. The black circle indicates the estimated effect size reported in Table 2–Table 4 for the respective outcome.

FIGURE B.3.
Power calculations: Health outcomes I



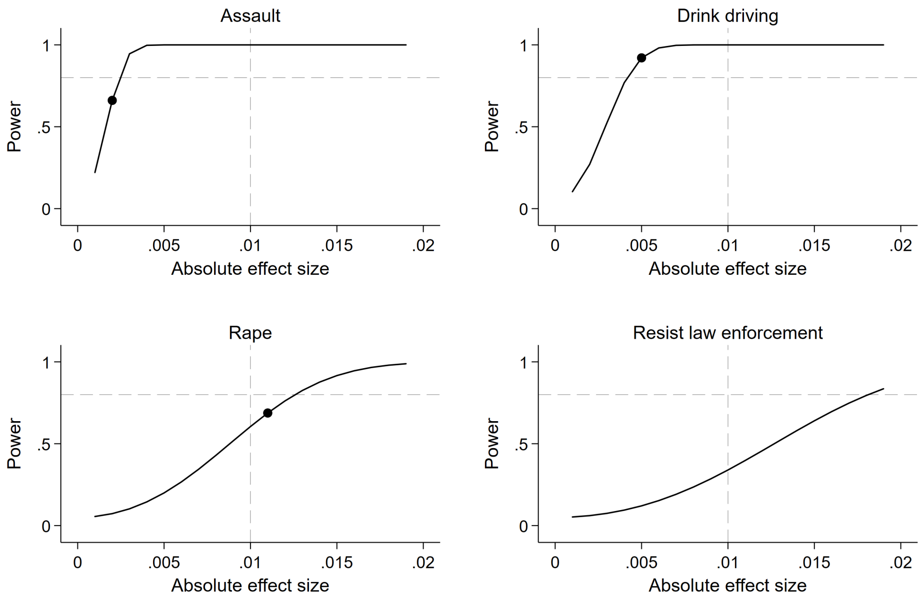
NOTE.— Each panel refers a specific outcome. Graphs display power calculations relating the probability of rejecting a false null hypothesis to the size of the effect of opening hours on the specific outcome. The x-axis display the effect size in percentages as interpreted from the log-linear model in Table 2–Table 4. The intersection of the dashed lines indicates the point where a one percentage effect can be detected with a power of 0.8. The black circle indicates the estimated effect size reported in Table 2–Table 4 for the respective outcome.

Figure B.4.
Power calculations: Health outcomes II



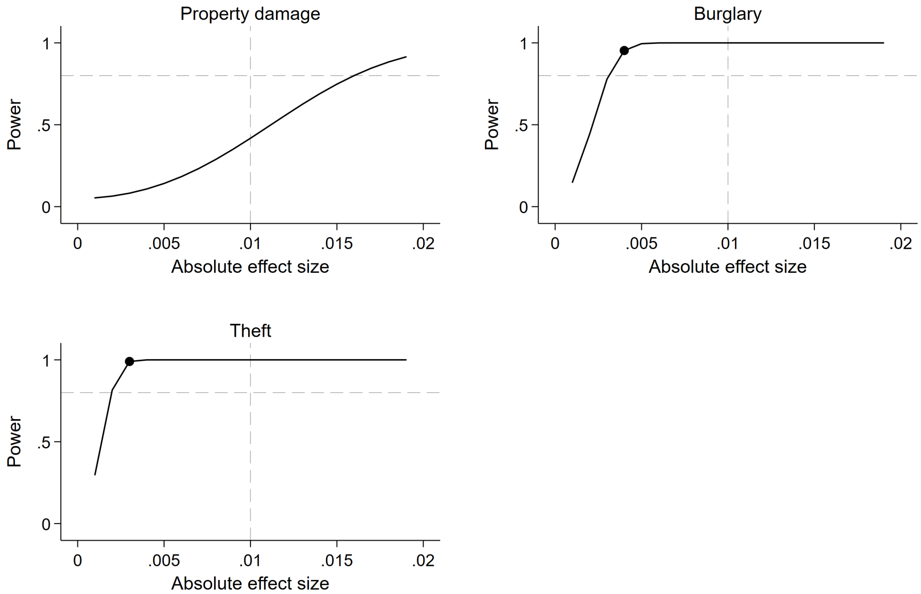
Note.— Each panel refers a specific outcome. Graphs display power calculations relating the probability of rejecting a false null hypothesis to the size of the effect of opening hours on the specific outcome. The x-axis display the effect size in percentages as interpreted from the log-linear model in Table 2–Table 4. The intersection of the dashed lines indicates the point where a one percentage effect can be detected with a power of 0.8. The black circle indicates the estimated effect size reported in Table 2–Table 4 for the respective outcome.

Figure B.5.
Power calculations: Crime outcomes I



Note.— Each panel refers a specific outcome. Graphs display power calculations relating the probability of rejecting a false null hypothesis to the size of the effect of opening hours on the specific outcome. The x-axis display the effect size in percentages as interpreted from the log-linear model in Table 2–Table 4. The intersection of the dashed lines indicates the point where a one percentage effect can be detected with a power of 0.8. The black circle indicates the estimated effect size reported in Table 2–Table 4 for the respective outcome.

Figure B.6.
Power calculations: Crime outcomes II



Note.— Each panel refers a specific outcome. Graphs display power calculations relating the probability of rejecting a false null hypothesis to the size of the effect of opening hours on the specific outcome. The x-axis display the effect size in percentages as interpreted from the log-linear model in Table 2–Table 4. The intersection of the dashed lines indicates the point where a one percentage effect can be detected with a power of 0.8. The black circle indicates the estimated effect size reported in Table 2–Table 4 for the respective outcome.