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Abstract

This paper provides the first empirical analysis of the (short-term) welfare consequences of an alcohol ban. Using subjective well-being data to proxy individual welfare, I apply a regression discontinuity design where the date of the implementation of the ban in the German federal state of Baden-Wuerttemberg functions as discontinuity. I find that the ban reduces life satisfaction of the total population and the subpopulation of drinkers, while life satisfaction of nondrinkers is unaffected. My findings are well in line with the rational addiction model perspective.

JEL: D04, D60, H30, I31

Keywords: Alcohol ban, Well-being, Life satisfaction, Welfare, Addiction, Regression discontinuity design

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Introduction

On March 1, 2010, the German state of Baden-Wuerttemberg banned the sale of alcohol between 10 pm and 5 am at off-premise outlets, such as gas stations, supermarkets, and kiosks. The two main aims of the ban are: (i) to reduce binge drinking and (ii) to reduce alcohol-related violence and damage. In an evaluation of this reform, Marcus and Siedler (2015) indeed find that the ban reduced alcohol-related hospitalizations by 7 percent. Yet even though the reform seems to serve its purpose, the implications for individuals' total welfare that come along are neglected so far.

From a theoretical viewpoint, it is a priori unclear what the consequences of a ban on alcohol sales for individual welfare are. On the one hand, from a rational addiction model perspective (Becker and Murphy, 1988), we would expect that consumers are worse-off, because access to a good they enjoy is restricted. Moreover, since banning a good is more drastic than simply increasing its price, even nondrinkers may be worse-off if they are against banning goods and services in general. Benefits arise only to the extent that the ban successfully internalizes social costs, such as a reduction in alcohol-related hospitalization. On the other hand, from a behavioral economics perspective (O'Donoghue and Rabin, 1999; Gruber and Kőszegi, 2001; Gruber and Kőszegi, 2004), consumers may be better-off by the ban if they have time-inconsistent preferences. In such a case, the ban functions as a valuable self-control device. Hence, two opposing predictions on the welfare consequences of banning alcohol exist and whether individuals benefit from the ban remains an empirical question.

While there are some empirical studies that use subjective well-being data as a proxy for individual welfare to analyze the consequences of tobacco control policies on well-being (Gruber and Mullainathan, 2005; Odermatt and Stutzer, 2015), there is to my knowledge no empirical evidence on the welfare consequences of alcohol control policies so far. The present paper seeks to fill this gap in the literature. For this, I use subjective well-being data to

investigate the (short-term) net well-being consequences of banning late-night off-premise alcohol sales in the German federal state of Baden-Wuerttemberg.

The implementation of the ban took place on March 1, 2010. To assess its impact on individual welfare from a causal angle, I apply a regression discontinuity design (RDD) where individuals interviewed in February 2010 (control group) are compared to those interviewed in March 2010 (treatment group). Although not unexpected, I argue that the implementation of the ban can be seen as a natural experiment where the ban is an exogenous shock to individual welfare for the following two reasons. First, respondents were unable to influence the timing of the ban. Second, although respondents were able to choose the timing of their interview, it seems reasonable to assume that this choice was independent from the implementation of the ban. Hence, this quasi-experimental setting allows me to interpret my findings from a causal angle.

I find that banning late-night off-premise alcohol sales significantly decreases welfare of the total population and the subpopulation of drinkers, while nondrinkers are unaffected. To validate my results, I perform placebo tests and several other robustness checks. In addition, I identify the ban's impact by means of a matching and difference-in-difference strategy. The results are robust to the modifications made.

The paper proceeds as follows. Section 2 reviews the existing literature on the consequences of bans and other restrictions on individuals' welfare. Section 3 briefly describes the institutional background in Germany. Section 4 describes the data and provides descriptive results. Empirical results and robustness checks are presented in Section 5. The results are discussed in Section 6 while Section 7 concludes.

Related Literature

Studies on the individual welfare consequences of control policies using subjective well-being data are scarce. Those that exist mainly focus on tobacco. For example, by analyzing the relationship between individual welfare and within-state variation in excise cigarette taxes in

the US and Canada, Gruber and Mullainathan (2005) find evidence that rising taxes make smokers happier. This finding supports the behavioral economic perspective in which smokers have time-inconsistent preferences and higher taxes serve as a self-control device.

Exploiting the staggered introduction of changes in cigarette prices and smoking bans in 40 European countries by means of a difference-in-difference-type framework, Odermatt and Stutzer (2015) find that higher cigarette prices reduce self-reported life satisfaction of smokers. In contrast, smoking bans are barely related to subjective well-being on average, but make smokers who would like to quit smoking happier.

Considering health outcomes, Kuehnle and Wunder (2016) find a positive average population health effect in response to a smoking ban in Germany. With regard to nonsmokers, they show that this group reports an improvement in self-assessed health status, whereas smokers report no or even adverse health effects. Their identification rests on a difference-in-difference strategy by exploiting regional variation in Germany as the smoking ban was introduced in the 16 German federal states on different dates between 2007 and 2008.

Besides cigarettes, alcohol is another widespread legal drug in most Western countries. Yet the effect of bans or excise taxes on alcohol has not yet been analyzed. This paper tries to make a modest step to fill this gap in the literature. Similar to the above mentioned studies on cigarette taxes and smoking bans, I argue that the introduction of a ban on alcohol sales in the German federal state of Baden-Wuerttemberg can be seen as a quasi-experiment. Since I have information about the distribution of the interviews around the date of the implementation of the ban, I apply a RDD to assess the ban's impact from a causal angle. In a robustness check, I show that the estimates prove robust when the ban's effect on individual welfare is assessed by means of a difference-in-difference strategy as used in Odermatt and Stutzer (2015) as well as in Kuehnle and Wunder (2016).

Institutional Background

In Germany, alcohol-related regulations such as drunk driving laws, alcohol taxes, or youth protection laws are federal laws. The minimum legal drinking age for beer and (sparkling) wine is 16, from the age of 18 any alcoholic beverage can be bought.

The policy intervention I analyze in this paper took place in the German state of Baden-Wuerttemberg.[2] It bans the sale of alcoholic beverages between 10 pm and 5 am at off-premise outlets, such as gas stations, supermarkets, and kiosks.[3] Since it does not prohibit the consumption of alcohol in general and individuals can circumvent the ban by buying alcohol before 10 pm, it is a relatively "light" intervention compared to other alcohol control policies, such as a general prohibition or alcohol taxes. The motivation of the federal state government to ban the sale of alcoholic beverages was due to the following two reasons: (i) to reduce excessive drinking, and (ii) to reduce alcohol-related violence and damage. On July 21, 2009, the draft bill was submitted, and on July 30, 2009, it was discussed for the first time in the state parliament. On November 4, 2009, the draft bill was approved by the state parliament, and eventually went into effect on March 1, 2010. Most importantly, it was the only law that came into force in Baden-Wuerttemberg in March 2010 in general and on March 1, 2010 in particular. Violating the ban entails fines of up to 5,000 Euros. Moreover, it is reasonable to assume that the police immediately began enforcing the ban, because it was its preferred policy to combat binge drinking.[4]

[2] Baden-Wuerttemberg is located in the southwest of Germany. With a population of almost 11 million (2015), it is the third-largest German state after North Rhine-Westphalia and Bavaria.

[3] Before the ban came into effect, it was theoretically possible to buy alcoholic beverages 24h a day at off-premise outlets. On-premise outlets, such as bars or restaurants, were not affected by the ban. Note that on December 8, 2017, the ban was lifted by the federal state government. At the same time, as a compensation, municipalities have been allowed to ban alcohol in limited areas and for a limited period to avoid late-night binge-drinking.

[4] What is more, a review of newspapers that were published (shortly) after March 1, 2010 show little evidence of complaints about a lack of enforcement, according to Marcus and Siedler (2015).

Data and Descriptive Statistics

The data are drawn from the German Socio-Economic Panel (SOEP).[5] Starting in 1984, the SOEP is a representative household panel survey that interviews around 20,000 individuals in each federal state of Germany over the course of a year. Wave 2010 of the SOEP is of particular interest for the purpose of this study as the ban on late-night off-premise alcohol sales in Baden-Wuerttemberg was implemented in this year. Moreover, besides waves 2006 and 2008 it is the only wave in which respondents were asked about their alcohol consumption.[6]

To assess the well-being effect of the ban, I compare respondents interviewed in February 2010 (control group) with those interviewed in March 2010 (treatment group). This is feasible as the SOEP provides information on both month and day the interview was conducted in a given year. Table 1 shows that more than half of all interviews in 2010 in Baden-Wuerttemberg took place in the first three month of the year. Table 2 depicts that the interviews are almost evenly distributed in February and March. The control group consists of 584 respondents, the treatment group numbers 516 respondents. I construct a binary indicator *Treatment*, which equals one if the respondent was interviewed after the ban (i.e., in March) and zero if the respondent was interviewed before the ban (i.e., in February).

[[Table 2 about here]]

Respondents' reported life satisfaction functions as a proxy for individual welfare. It is assessed by the question: "How satisfied are you with your life all things considered?" Responses range from 0 ("completely dissatisfied") to 10 ("completely satisfied").

The SOEP questionnaire asks respondents about their drinking behavior with regard to beer, (sparkling) wine, liquor, and cocktails. In each question respondents can either check "regular",

[5] Socio-Economic Panel (SOEP), data for years 1984-2014, version 31, SOEP, 2016, doi: 10.5684/soep.v31. For more information on the data, see Wagner et al. (2007).

[6] Note that in 2016 respondents were again asked about their drinking behavior. Yet the framing of the question as well as the answer categories differ from those in 2006, 2008, and 2010, suggesting that they are not quite comparable.

"occasionally", "rarely", or "never". I then build a binary indicator *Alcohol* that equals one if the respondent at least rarely consumes either beer, (sparkling) wine, liquor, or cocktails and zero if the respondent is a teetotaler.[7]

Table 3 reports differences in observed variables between treatment and control group. The variables included are similar to those used in Gruber and Mullainathan (2005), Odermatt and Stutzer (2015), or Kuehnle and Wunder (2016). The mean differences with regard to health status, education, gender, marital status, monthly equivalent household income[8], unemployment, drinking status, days before/after the ban are not statistically significant. Yet the differences in age, having a child below age 14, and living in a city are significant, implying that the respondents in treatment and control group differ in these characteristics. Although significant, the differences in having a child below age 14 as well as living in a city are quite small. The largest difference exists in age. Respondents in the control group are on average 4.7 years older than respondents in the treatment group. Hence, controlling for these differences in observable factors is necessary to obtain unbiased estimates.

The variable *Days* gives the average distance to March 1, 2010 for both treatment and control group. Considering the variables in Table 3, *Life satisfaction* acts as the dependent variable in the subsequent analysis. All other variables function as control variables. *Age* additionally enters in its squared form, monthly equivalent household income enters in log-form.

[[Table 3 about here]]

As a starting point for the subsequent analysis, Table 3 shows that respondents in the control group are on average 0.253 points more satisfied with their life than respondents in the treatment

[7] In Table 9, I show that the results are not sensitive to the definition of the alcohol consumption variable.
[8] To calculate the equivalent household income the OECD equivalent scale was used. This scales gives the first adult a weight of 1.0, additional adults (of at least 15 years of age) a weight of 0.5, and children (under 15 years of age) a weight of 0.3. I dropped the top 1% of the equivalent household distribution to avoid biased estimates from outliers, resulting in sample reduction of 19 individuals. In Table 13, I show that the results are not sensitive to using unadjusted monthly household income and retaining the top 1% of the income distribution.

group. Figure 1 gives a graphical representation of this finding. The y-axis reports the daily mean of reported life satisfaction, the x-axis shows the number of days before and after the ban. The regression lines fit the data before and after the ban. The discontinuity at *Days* = 0 visualizes the treatment effect, implying that life satisfaction is lower after the ban.[9]

[[Figure 1 about here]]

Individual Welfare Effects of an Alcohol Ban

Empirical Results

Table 4 gives corroborating evidence that the ban had negative effects on individuals' welfare. Column (1) shows that the descriptive difference in life satisfaction remains when observable differences between individuals are controlled for.[10] Individuals in the treatment group are on average 0.266 points less satisfied with their life than similar individuals in the control group, with the difference being statistically significant at the 1% level. This decline is not only statistically significant but also economically as it is around 35% of the difference in life satisfaction between employed and unemployed individuals.[11]

Nevertheless, treatment and control group may differ with regard to unobserved characteristics. In this case, merely controlling for observable differences is not sufficient to obtain the causal effect of the ban. This problem can be solved using a RDD, which is useful when the mechanism of assignment to treatment and control group is known and assignment varies discontinuously with an observable variable (Angrist and Pischke, 2009). In my case, assignment to the treatment varies discontinuously with the day of the interview. Hence, I implement a RDD by

[9] Note that the following results do not change if I exclude those respondents who have life satisfaction-scores of five or below.

[10] The dependent variable is treated as cardinal in Table 4. The findings do not change if the estimations are repeated with an ordered logit and ordered probit model, as shown in Table 12. In this and the following regressions, covariates show well-known patterns: life satisfaction increases with income and better health status. Men are on average less satisfied than women and unemployed individuals are less satisfied than individuals who have a job.

[11] For comparison, Kuehnle and Wunder (2016) calculate that the negative effect of a smoking ban in Germany is between 40% and 45% of the difference in self-assessed health status between employed and unemployed individuals. Odermatt and Stutzer (2015) calculate that the negative life satisfaction effect of a 50% increase in cigarette taxes corresponds to the negative effect of an increase in the unemployment rate by 2.7 percentage points. Unfortunately, they did not provide such a comparison with regard to individual unemployment and smoking bans.

regressing the dependent variable on treatment status and the number of days before/after the ban the respondent was interviewed (ranging from -28 to +31).

Implementing the RDD in column 2, I again find a negative treatment effect that is significantly different from zero at least at the 5% level, implying a decrease in individual life satisfaction following the ban. The coefficient of the forcing variable *Days* is positive but insignificant, suggesting that the distribution of interviews around the threshold March 1, 2010 has no significant impact on reported life satisfaction.

To allow for different time trends before and after the ban on the dependent variable, I include an interaction term between treatment status and the number of days before/after the ban the respondent was interviewed. Column 3 shows that the coefficient of the treatment variable hardly changes. The coefficient of the interaction term is small and positive but not significant, suggesting that there is no considerable change in the treatment effect over the investigated time span. Thus, even though we see indications of a small positive development in the treatment effect as days go by, I do not find evidence of an increasing treatment effect in the time window I have data for.

Column 4 shows that the negative treatment effect remains when the time span is expanded to the full year. The interaction term becomes even smaller of magnitude and remains insignificant at conventional levels. In each column, the R^2 is around 33%, suggesting that the models explain a large fraction of the variation in reported life satisfaction.

[[Table 4 about here]]

Table 5 shows the distribution of the welfare costs among the subpopulation of respondents who drink alcohol and those who are teetotalers.[12] Column 1 shows that the subpopulation of

[12] For brevity, only the coefficients of the main variables are reported in this and the following tables. A full set of all coefficients is available on request.

respondents who drink alcohol bear the total welfare costs while column 2 shows that the subpopulation of teetotalers is unaffected by the ban, as indicated by the insignificant coefficient.

When analyzing the treatment effect for the subpopulations of drinkers and nondrinkers, we have to keep in mind that the ban could have directly affected respondents' drinking status. As a result, respondents who indicate that they consume alcohol pre- and post-intervention are not comparable, because any difference in individual welfare for drinkers compounds both a possible treatment effect and a selection effect.

I am aware of this problem but argue that it is minor in my case, because alcohol consumption is fairly widespread among individuals in Germany and the ban is a very "light" intervention as it only restricts the purchase of alcohol between 10 pm and 5 am. Hence, drinkers are more likely to change their consumption behavior at the intensive margin rather than at the extensive margin. Nevertheless, to rule out this potential source of bias, I use information from the pre-intervention years 2006 and 2008 to determine respondents' drinking status in 2010. More precisely, I classify respondents as drinkers if they report consuming alcohol at any of the two pre-intervention interviews.[13] Columns 3 and 4 of Table 5 show that potential biases from endogeneity do not alter the results. Taken together, the results of both Table 4 and Table 5 are well in line with the rational addiction model that states that consumers are worse-off in presence of a ban, likely because access to a good they enjoy is restricted.

[[Table 5 about here]]

Robustness Checks

I conduct several robustness checks to validate my main finding that the ban on alcohol sales at off-premise outlets had a negative effect on individuals' welfare. In a first check, I perform

[13] This procedure follows Kuehnle and Wunder (2016) who apply the same strategy when analyzing the health effects of a smoking ban.

placebo tests repeating the RDD estimation of column 3 in Table 4 for the years 2006 to 2014. If my findings are caused by a trend or seasonal pattern, we would find a statistically significant treatment effect when assuming that the ban happened on March 1, in any of the specific years. Table 6 shows that the treatment effects are insignificant in each of the nine years, indicating that my findings are not caused by a trend or seasonal pattern.[14] The sole exception is the treatment effect in wave 2008, which is significant at the 5% level. But this year might not be representative as the ongoing financial crisis may explain the difference in life satisfaction.[15]

[[Table 6 about here]]

As a second modification, I conduct placebo tests repeating the RDD estimation of column 3 in Table 4 for Germany, East and West Germany, as well as each federal state in 2010. If there had been other changes on March 1, 2010 that not only affected Baden-Wuerttemberg, we would find a statistically significant treatment effect in those comparison groups. Table 7 shows that the treatment effect is small and far from significant for Germany, East and West Germany. The same holds true at the federal state level. The treatment effect is statistically insignificant in each federal state, albeit Hamburg and Saxony; yet in these two cases, the effect is positive and not negative. Hence, the results indicate that there had been no overall changes in Germany on March 1, 2010 that could have biased my results.

[[Table 7 about here]]

Except for column 4 of Table 4, so far all individuals interviewed in February are in the control group while all individuals interviewed in March are in the treatment group. Table 8 shows that the previous results are not sensitive to different definitions of treatment and control group,

[14] Note that a full replication of column 3 in Table 4 is only feasible for waves 2006 and 2008 as respondents drinking behavior is assessed in these years only.

[15] Another explanation for the lower life satisfaction could be the implementation of a traffic ban that came into effect on March 1, 2008 and that banned cars which do not fulfill clean environment requirements from certain cities in Germany. In Baden-Wuerttemberg, the following eight cities had been affected by this ban: Stuttgart, Mannheim, Tuebingen, Ludwigsburg, Leonberg, Schwaebisch-Gmuend, Reutlingen, and Ilsfeld.

because the treatment coefficient remains negative and large if the RDD model in Table 4 (column 3) is repeated for different time windows around the threshold March 1, 2010. However, with decreasing sample size, standard errors increase, resulting in an insignificant coefficient if the time window is narrowed to -15/+15 or less days around the threshold. Another explanation for the insignificant difference in life satisfaction is related to the fact that I do not know whether individuals already picked up the treatment at the time of the interview. It might be that those interviewed just after the implementation of the ban were not yet affected by it, implying that there is no treatment effect that can be measured.

[[Table 8 about here]]

To account for nonlinear trends before and after the ban, I include higher order polynomials of *Days* as well as respective interaction terms with the treatment variable. As a result, the estimated treatment effect turns insignificant but both the Akaike and the Bayesian Information Criterion favor the model with a linear trend that I present in Table 4.[16]

In Table 9, I show that the estimates are not sensitive to the definition of alcohol consumption. The different alcohol categories (beer, (sparkling) wine, liquor, and cocktail) are included in column 1 instead of a single binary indicator measuring whether a person consumes alcohol. The estimated treatment effect is very similar to the coefficient presented in Table 4 with a magnitude of -0.345 and statistical significance at the 5% level. Column 2 shows that this pattern is repeated even when dummy variables for all four categories of the four alcohol variables are included. The estimates in Table 4 thus prove robust to modifications of the alcohol consumption variable.

[[Table 9 about here]]

[16] Those results are available on request.

As last checks, I scrutinize whether the results change when identification of the ban's effect on life satisfaction is based on propensity score matching or on a difference-in-difference strategy. With regard to propensity score matching, the average treatment effect on the treated is estimated by performing nearest-neighbor propensity score matching with 5 neighbors and replacements on a probit model. The results in Table 10 show that applying the matching procedure does not alter the findings derived so far.[17]

[[Table 10 about here]]

To assess the ban's effect, I compare life satisfaction of individuals interviewed before and after the implementation of the ban. Such a procedure might result in biased estimates if there are (unobserved) state-specific and time-specific effects. By comparing only individuals with each other who live in Baden-Wuerttemberg, any state-specific effects cancel out. Time-specific effects might, however, still put a thread to unbiased estimates. I dealt with this issue by allowing for (different) time trends in my RDD framework and by running several placebo tests. Moreover, since the observational period comprises only two months (February and March 2010), any time effect should be negligible. To nevertheless rule out such concerns, I identify the ban's effect on life satisfaction by means of a difference-in-difference strategy. In column 1 of Table 11, respondents interviewed in Baden-Wuerttemberg are compared to all individuals interviewed in one of the remaining 15 federal states in February or March 2010 by including a binary indicator which equals one if a respondent is living in Baden-Wuerttemberg and zero otherwise in the underlying regression specification. In columns 2 to 4, I follow Kuehnle and Wunder (2016) and include a set of indicators for each federal state instead. Including more than one federal state as controls is beneficial as it provides a hedge against idiosyncratic shocks in one control state, which might violate the common trend assumption. The estimates in column 1 and 2 support the RDD estimates derived so far that the ban has a negative impact on

[17] Sample statistics after matching are available on request.

individual welfare. Moreover in line with Table 5, columns 3 and 4 display that the subpopulation of drinkers bears the total welfare costs while life satisfaction of nondrinkers is unaffected by the ban.

[[Table 11 about here]]

Discussion

Critically, respondents in Baden-Wuerttemberg in January and February 2010 are aware that they cannot buy alcohol any longer in March between 10 pm and 5 am. This could result in a typical "Ashenfelter's dip" (Ashenfelter, 1978), a pre-treatment reduction in utility in anticipation of the treatment. However, I estimate a negative treatment effect. Therefore, in presence of an "Ashenfelter's dip", my results would even underestimate the true welfare costs. Another potential thread to the identification of a causal effect is that respondents in March did not yet pick up the treatment at the time of the interview. But again, in such a case, the estimated effect would underestimate the true effect of the ban.

An important question is to what extent the ban has any long-term effects on individual welfare, something that is much harder to assess causally than short-term effects. If the decrease in welfare was due to an emotional reaction or a way for people to spontaneously dissociate themselves from the ban, we would expect the effect to be temporary. Since 2010, respondents' alcohol consumption is assessed in the SOEP only again in 2016. Yet as the framing of the question and the answer categories differ, the information can probably not be used for an exact evaluation. Hence, with the data at hand, I am not able to draw any long-term well-being consequences of the ban for individuals.

It is important to remember that what I have done in this paper is to compare the welfare of two different groups in a quasi-experimental setting. Even though I have implemented techniques to overcome potential problems with differences between the two groups, there is still a possibility that they differ in ways that I am not able to account for. The robustness of the results

to different placebo tests, several modifications, the RDD, matching, and difference-in-difference framework is, however, reassuring.

Conclusions

Using subjective well-being data gathered around the time of the implementation of a sales ban on alcohol at off-premise outlets in the German federal state of Baden-Wuerttemberg on March 1, 2010, I investigate its (short-term) net well-being consequences for the total population and the subpopulations of drinkers and nondrinkers. The ban can be seen as a quasi-experiment, because respondents were neither able to influence the timing of the ban, nor is it reasonable to assume that the choice of the interview date was dependent on its introduction.

I find that banning late-night off-premise alcohol sales significantly decreases life satisfaction of the total population and the subpopulation of drinkers, while life satisfaction of nondrinkers is unaffected. My results are well in line with the rational addiction model that states that consumers are worse-off due to a ban, because access to a good they enjoy is restricted.

Future research should try to use similar quasi-experimental settings to assess the welfare consequences of banning other goods or services. With larger data sets, it should also be possible to analyze if various subpopulations are affected differently; for example, whether younger people are more affected, or whether money, health and education play a role. In addition, long-term welfare consequences should be investigated.

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Appendix

Table 1: Distribution of Interviews in 2010 in Baden-Wuerttemberg

Month	Frequency	Cumulative (in percent)
January	6	0.30
February	584	29.68
March	516	55.63
April	311	71.28
May	163	79.48
June	97	84.36
July	33	86.02
August	67	89.39
September	13	90.04
October	128	96.48
November	58	99.40
December	12	100.00
Total	1,988	

Source: SOEP v31.

Table 2: Distribution of Interviews in February and March 2010 in Baden-Wuerttemberg

Day	February	March
1	10	30
2	12	37
3	36	21
4	22	19
5	22	21
6	20	19
7	1	4
8	29	17
9	22	22
10	19	19
11	17	12
12	13	15
13	24	13
14	8	11
15	28	14
16	18	22
17	29	23
18	21	10
19	41	16
20	23	13
21	10	3
22	28	21
23	32	14
24	30	19
25	23	11
26	19	26
27	17	8
28	10	8
29	-	20
30	-	16
31	-	12
Total	584	516

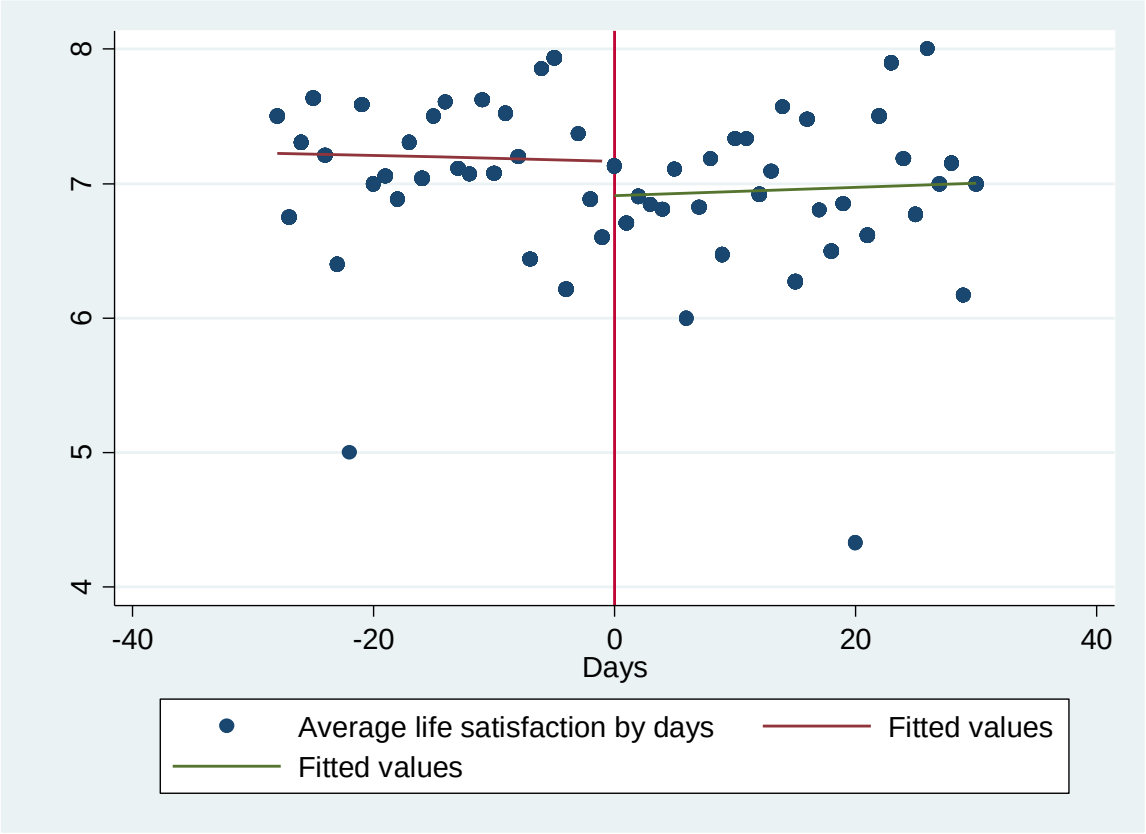
Source: SOEP v31.

Table 3: Descriptive Statistics of Control and Treatment Group

	Control		Treatment		
Variables	Mean	SD	Mean	SD	Difference
Life satisfaction	7.195	1.643	6.942	1.777	0.253**
Days	13.997	7.792	13.481	9.389	0.516
Health					
Very good	0.099	0.299	0.079	0.271	0.020
Good	0.368	0.483	0.388	0.488	-0.019
Satisfactory	0.318	0.466	0.343	0.021	-0.025
Not good	0.173	0.379	0.159	0.366	0.014
Bad	0.041	0.199	0.031	0.174	0.010
Male	0.461	0.499	0.475	0.500	-0.014
Education					
Less than high school	0.680	0.467	0.647	0.472	0.033
High school	0.051	0.221	0.054	0.227	-0.003
More than high school	0.269	0.444	0.298	0.458	-0.030
Age	53.509	18.059	48.643	17.508	4.865***
Married	0.594	0.491	0.618	0.486	-0.024
Equiv. HH net income	1836.728	865.609	1802.135	855.371	34.594
Children < 14	0.192	0.394	0.238	0.427	-0.046*
Unemployed	0.024	0.153	0.041	0.198	-0.016
City	0.916	0.277	0.880	0.325	0.036**
Alcohol	0.865	0.342	0.878	0.328	-0.013
Observations	584		516		

Source: SOEP v31.
Notes: * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level.

Figure 1: Mean Life Satisfaction by Days



Source: SOEP v31.
Notes: Separate regression lines before and after the ban at Days=0. The pattern does not change if individuals with life satisfaction-scores of 5 and below are excluded.

Table 4: OLS Welfare Effects of a Ban on Late-night Off-premise Alcohol Sales

Variables	(1)	(2)	(3)	(4)
Treatment	-0.266***	-0.394**	-0.372**	-0.237*
	(0.086)	(0.163)	(0.164)	(0.132)
Days		0.005	-0.013	-0.003
		(0.005)	(0.017)	(0.015)
Treatment x days			0.012	0.002
			(0.010)	(0.007)
Alcohol	0.080	0.080	0.082	0.084
	(0.153)	(0.153)	(0.153)	(0.109)
Good health	-0.161	-0.157	-0.145	-0.461***
	(0.143)	(0.143)	(0.142)	(0.098)
Satisfactory health	-0.860***	-0.861***	-0.852***	-1.181***
	(0.156)	(0.156)	(0.155)	(0.110)
Not good health	-2.059***	-2.059***	-2.051***	-2.172***
	(0.198)	(0.199)	(0.199)	(0.143)
Bad health	-3.139***	-3.158***	-3.142***	-3.586***
	(0.300)	(0.299)	(0.299)	(0.261)
Male	-0.245***	-0.246***	-0.244***	-0.232***
	(0.087)	(0.088)	(0.087)	(0.065)
Less than high school	-0.309*	-0.307*	-0.307*	-0.138
	(0.178)	(0.177)	(0.177)	(0.141)
More than high school	-0.254	-0.251	-0.245	-0.037
	(0.188)	(0.187)	(0.187)	(0.147)
Age	-0.077***	-0.076***	-0.076***	-0.063***
	(0.016)	(0.016)	(0.016)	(0.012)
Age squared	0.001***	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Married	0.625***	0.623***	0.628***	0.453***
	(0.108)	(0.108)	(0.108)	(0.080)
Log equiv. HH income	0.677***	0.681***	0.683***	0.637***
	(0.123)	(0.123)	(0.123)	(0.084)
Children < 14	0.142	0.139	0.129	0.062
	(0.120)	(0.120)	(0.121)	(0.086)
Unemployed	-0.762***	-0.754***	-0.758***	-0.683***
	(0.278)	(0.277)	(0.278)	(0.189)
City	-0.057	-0.045	-0.043	-0.174
	(0.157)	(0.159)	(0.159)	(0.121)
Observations	1,100	1,100	1,100	1,988
R^2	0.330	0.331	0.332	0.303

Source: SOEP v31.

Notes: Robust standard errors in parentheses. * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. The treatment group is those interviewed after the implementation of the ban on March 1, 2010. Days is the number of days before and after the ban. In columns 1 to 3, I use those interviewed in February and March. Column 4 expands the time span to the full year 2010. Reference groups are no alcohol, very good health, high school, female, not married, no children under age 14, employed, living in an urban area.

Table 5: OLS Welfare Effects of a Ban on Late-night Off-premise Alcohol Sales for the Subpopulations of Drinkers and Teetotalers

	Drinkers	Teetotalers	Drinkers *t-1*	Teetotalers *t-1*
Variables	(1)	(2)	(3)	(4)
Treatment	-0.284*	-0.985	-0.349**	-0.638
	(0.170)	(0.620)	(0.178)	(0.533)
Days	-0.019	0.030	-0.015	-0.064
	(0.017)	(0.063)	(0.018)	(0.050)
Treatment x days	0.013	-0.004	0.012	0.047
	(0.011)	(0.038)	(0.011)	(0.034)
Controls?	Yes	Yes	Yes	Yes
Observations	958	142	897	132
R^2	0.324	0.447	0.321	0.456

Source: SOEP v31.

Notes: Robust standard errors in parentheses. * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. The treatment group is those interviewed in March 2010 after the implementation of the ban on March 1, 2010. Days is the number of days before and after the ban. In column 1 (2), the subpopulation of drinkers (nondrinkers) consists of those respondents who state in the 2010 questionnaire that they at least rarely (never) drink alcohol. In columns 3 and 4, drinking status of respondents in 2010 is derived from their drinking behavior in any of the two pre-intervention interviews in 2006 or 2008. Since some respondents participate in 2010 for the first time, the number of observations in columns 3 and 4 is smaller. Control variables are the same as in Table 4.

Table 6: OLS Placebo Tests for Baden-Wuerttemberg for Years 2006 to 2014

Year	Treatment effect
2006	-0.228
	(0.148)
2007	-0.144
	(0.332)
2008	-0.331**
	(0.035)
2009	0.271
	(0.115)
2010	-0.372**
	(0.023)
2011	-0.0192
	(0.907)
2012	0.121
	(0.429)
2013	0.0800
	(0.640)
2014	0.162
	(0.306)

Source: SOEP v31.

Notes: Robust standard errors in parentheses. * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Replication of the results in column 3 of Table 4 assuming that the ban was implemented in Baden-Wuerttemberg on March 1 of the respective year. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. Control variables are the same as in Table 4.

Table 7: OLS Placebo Tests at the German State Level in 2010

State	Treatment effect	Standard error	Observations
Germany	-0.008	(0.894)	8,764
East	-0.054	(0.466)	2,925
West	0.117	(0.265)	5,839
Federal States			
Baden-Wuerttemberg	-0.372**	(0.164)	1,100
Bavaria	-0.089	(0.569)	1,381
Berlin	0.233	(0.443)	426
Brandenburg	-0.281	(0.333)	463
Bremen	0.762	(0.413)	77
Hamburg	1.176**	(0.036)	117
Hesse	0.071	(0.818)	524
Lower Saxony	-0.163	(0.343)	955
Mecklenburg-West Pomerania	0.356	(0.320)	252
North Rhine-Westphalia	-0.126	(0.318)	1,983
Rhineland-Palatinate	-0.371	(0.142)	480
Saarland	-0.199	(0.712)	138
Saxony	0.512**	(0.012)	881
Saxony-Anhalt	-0.066	(0.788)	439
Schleswig-Holstein	0.101	(0.782)	184
Thuringia	-0.317	(0.256)	464

Source: SOEP v31.

Notes: Robust standard errors in parentheses. * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Replication of the results in column 3 of Table 4 assuming that the ban was implemented on March 1 in Germany, East and West Germany and any of the federal states. In the estimations for Germany and West Germany, the observations for Baden-Wuerttemberg have not been considered. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. Control variables are the same as in Table 4.

Table 8: OLS Welfare Effects of a Ban on Late-night Off-premise Alcohol Sales with Different Time Windows

	(1)	(2)	(3)	(4)	(5)
Variables	-30/+30	-25/+25	-20/+20	-15/+15	-10/+10
Treatment	-0.379**	-0.419**	-0.341*	-0.179	-0.293
	(0.163)	(0.175)	(0.203)	(0.243)	(0.301)
Days	-0.012	-0.007	0.001	0.064	-0.052
	(0.016)	(0.020)	(0.001)	(0.046)	(0.076)
Treatment x days	0.011	0.009	-0.001	0.035	0.035
	(0.011)	(0.013)	(0.019)	(0.029)	(0.045)
Controls?	Yes	Yes	Yes	Yes	Yes
Observations	1,103	978	793	633	454
R^2	0.331	0.326	0.337	0.321	0.366

Source: SOEP v31.

Notes: Robust standard errors in parentheses. * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Replication of the results in column 3 of Table 4 with different time windows. For example, -30/+30 means that those individuals interviewed 30 days before March 1, 2010 are in the control group while those interviewed 30 days after are in the treatment group. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. Control variables are the same as in Table 4.

Table 9: OLS Welfare Effects of a Ban on Late-night Off-premise Alcohol Sales with Alternative Definitions of Alcohol Behavior

Variables	(1)	(2)
Treatment	-0.345**	-0.354**
	(0.037)	(0.032)
Days	-0.015	-0.014
	(0.350)	(0.385)
Treatment x days	0.012	0.012
	(0.231)	(0.248)
Controls?	Yes	Yes
Observations	1,100	1,100
R^2	0.321	0.319

Source: SOEP v31.

Notes: Robust standard errors in parentheses. * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Replication of the results in column 3 of Table 4 with different definitions of alcohol consumption. In column 1, the different variables for alcohol consumption (beer, (sparkling) wine, liquor, cocktail) with its categories "never", "rarely", "occasionally", and "regular" are included. Column 2 includes binary indicators for each category of the four alcohol variables, in addition. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. Control variables are the same as in Table 4.

Table 10: Propensity Score Matching Results of the Welfare Effects of a Ban on Late-night Off-premise Alcohol Sales

Sample	Treated	Controls	Difference	S.E.	T-stat
Unmatched	6.942	7.195	-0.253	0.103	-2.46
ATT	6.938	7.166	-0.228	0.113	-2.02

Source: SOEP v31.

Notes: Propensity score matching estimates of the difference in life satisfaction between treatment and control group by performing nearest-neighbor matching with 5 neighbors and replacements on a probit model. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. Control variables are the same as in Table 4.

Table 11: Difference-in-Difference Results of the Welfare Effects of a Ban on Late-night Off-premise Alcohol Sales

	Germany	Federal States	Drinkers	Teetotalers
Variables	(1)	(2)	(3)	(4)
Treatment	-0.186**	-0.191**	-0.171*	-0.293
	(0.044)	(0.038)	(0.075)	(0.342)
Controls?	Yes	Yes	Yes	Yes
Observations	9864	9864	8458	1406
R^2	0.285	0.291	0.270	0.356

Source: SOEP v31.

Notes: Robust standard errors in parentheses. * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. Difference-in-Difference estimates of the difference in life satisfaction between treatment and control group. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. The treatment group is those interviewed in Baden-Wuerttemberg in March 2010 after the implementation of the ban on March 1, 2010. In column 1, a binary indicator which equals one if a respondent is from Baden-Wuerttemberg and zero otherwise is included in the regression; in column 2, a set of indicators for each federal state is used. In column 3 (4), the subpopulation of drinkers (nondrinkers) consists of those respondents who state in the 2010 questionnaire that they at least rarely (never) drink alcohol. The results do not alter when drinking status of respondents in 2010 is derived from their drinking behavior in any of the two pre-intervention interviews in 2006 or 2008. Control variables are the same as in Table 4.

Table 12: Ordered Logit and Probit Welfare Effects of a Ban on Late-night Off-premise Alcohol Sales

	Ordered Logit		Ordered Probit	
Variables	(1)	(2)	(3)	(4)
Treatment	-0.501***	-0.362**	-0.285**	-0.176*
	(0.210)	(0.166)	(0.118)	(0.096)
Days	-0.017	-0.002	-0.012	-0.003
	(0.021)	(0.019)	(0.012)	(0.011)
Treatment x days	0.015	0.002	0.010	0.002
	(0.013)	(0.009)	(0.007)	(0.005)
Controls?	Yes	Yes	Yes	Yes
Observations	1,100	1,988	1,100	1,988
Pseudo R^2	0.104	0.093	0.100	0.090

Source: SOEP v31.

Notes: Robust standard errors in parentheses. * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. The treatment group is those interviewed after the implementation of the ban on March 1, 2010. Days is the number of days before and after the ban. Replication of the results of columns 3 and 4 of Table 4 using ordered logit and ordered probit models instead of OLS. In columns 1 and 3, I use those interviewed in February and March. Columns 2 and 4 expand the time span to the full year 2010. Control variables are the same as in Table 4.

Table 13: OLS Welfare Effects of a Ban on Late-night Off-premise Alcohol Sales Using Unadjusted Monthly Household Income and Retaining the Top 1% Of The Income Distribution

Variables	(1)	(2)	(3)	(4)
Treatment	-0.339**	-0.225*	-0.330**	-0.205
	(0.165)	(0.086)	(0.164)	(0.132)
Days	-0.017	-0.008	-0.017	-0.007
	(0.017)	(0.015)	(0.016)	(0.015)
Treatment x days	0.012	0.004	0.013	0.004
	(0.010)	(0.007)	(0.010)	(0.007)
Controls?	Yes	Yes	Yes	Yes
Observations	1,111	2,007	1,111	2,007
R^2	0.315	0.288	0.331	0.303

Source: SOEP v31.

Notes: Robust standard errors in parentheses. * Statistically significant at the 10% level; ** at the 5% level; *** at the 1% level. The dependent variable is life satisfaction measured on a 0 to 10 scale, where a higher value means more satisfaction. The treatment group is those interviewed after the implementation of the ban on March 1, 2010. Days is the number of days before and after the ban. In columns 1 and 3, I use those interviewed in February and March. Columns 2 and 4 expand the time span to the full year 2010. In each column the top 1 percent of the income distribution are retained. In the first two columns, (log) unadjusted household income is used. The last two columns report results for (log) adjusted household income like in Table 4. Note that the treatment coefficient in column 4 becomes significant if I exclude the six observations that were interviewed in January. Control variables are the same as in Table 4.

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