

Did the Swiss Smoking Ban affect the Sales of Restaurants?

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Abstract

A majority of the 26 Swiss cantons voted about smoking bans for restaurants, at different points in time and before a national ban was introduced. This variation is used to estimate the effect of this policy on the hospitality industry. The natural experiment is empirically analyzed with a panel of objective micro data for 800 restaurants. The very first results find no robust evidence that smoking bans resulted in lower sales of restaurants.

Keywords: Health care legislation; smoking bans; natural experiment
JEL Classification: I18; L51

1 INTRODUCTION

In line with many other countries Switzerland enacted in 2010 a federal law that prohibits smoking in restaurant. The ban is justified by protecting employees and customers from harmful second hand smoke. It has clearly been proven that smoking bans reduce the exposure to smoke which in turn is good for health. Less clear is the impact on restaurants. Most of the empirical evidence finds no effect of a smoking ban on restaurant. The theoretical predictions are ambiguous. However, many owners of restaurant and bars fear a ban. If bans were in their best interest and under complete markets, restaurants would ban smoking on their own initiative. Thus, it is no surprise that the hospitality industry often units to avoid smoking bans. In the Swiss votes about smoking bans they campaigned against it. No study yet has systematically analyzed the Swiss case.

The Swiss case is a promising natural experiment, because a majority of cantons have enacted own smoking bans between 2007 and 2009; before the national ban was introduced in May 2010. Using these controls this study aims to identify the effect of the smoking ban on the Swiss hospitality industry. The study is based on a five year panel of yearly micro data for around 800 restaurants. These restaurants outsource their bookkeeping to the same accountant, rendering the data objective and comparable.

Chapter 2 reviews the economics of smoking and studies about smoking bans. The empirical estimation model is introduced in chapter 3, data in chapter 4. Results are presented in chapter 5, followed by a discussion.

2 ECONOMICS OF SMOKING

The theoretical prediction about the effect of a smoking ban is derived from the context of economics of smoking (refer for example to the handbook of health economics by Chaloupka & Warner (2000: 1556-1562)). Economists analyze the demand for cigarette and the decision for addictive products in general either by rational addiction models or habit formations. Non time consistent decisions together with the externalities of second hand smoking are used to justify smoking bans in restaurants. The theoretical impact of a ban is ambiguous, because smokers are expected to spend less money in the hospitality industry, but non-smokers are expected to spend more. Empiricism has been analysed using various approaches. Most studies find no

negative impact of smoking bans on the hospitality industry.

2.1 Smoking decision and restriction

Economists explain smoking by two concurring models: time inconsistent habits or rational addiction. The former incorporates past consumption and associates it positively to the current demand for cigarettes. But while the amount of cigarettes consumed hitherto strengthens the taste for it, its effect on future consumption is ignored in the decision to light on a cigarette or not. (Gruber, 2001: 202) This myopic consumer stands in contrast to the rational addiction model of Becker and Murphy (1988), where individuals are fully aware of all future costs and have stable preferences. Laux (2000) questions whether smoking really is a rational decision, given that more people like to quit smoking than actually do so.

The decision to smoke is a function of the price and the demand for cigarette is price inelastic - as it is the case for many other addictive goods. Gruber estimated an elasticity of -0.65 for the US between 1970 and 1990, other studies find -0.45 (Gruber, 2001: 195, 207). The perception of inelasticity is challenged by the inverse correlation between the drop in consumption and the rise in price over time for many countries.

Cigarette prices have gone up mostly due to higher sales taxes. When inelastic goods are taxed, revenue is raised without creating big distortions. However, for inelastic goods price increase are not very effective to reduce consumptions as suggested by proponents of public health. Therefore, smoking is not only taxed but also restricted via bans. First, there is a minimal age of 18 years to buy cigarettes. Moreover, bans to smoke are enacted in public areas such as elevators, government buildings, public meeting rooms, schools and restaurants.

These restrictions reduce the opportunities to smoke (Evans et al. 1999: 789), and have been proven to be effective in reducing smoking intensity and propensity (Tauras & Chaloupka, 1999). For example Chaloupka and Saffer (1992) analyzed a panel of the 50 states of the United States. The single equation pointed to a reduction from private and public bans, the simultaneous equations indicate an effect only from public bans but not from private workplace bans.

2.2 Legitimation of smoking bans

As smoking bans effectively reduce smoking, they prevent lung cancer and other smoking related health problems. For example in Geneva after the smoking ban the number of hospitalisation dropped by 20% due to respiratory disorders and by 7% due to cardiac infarction (Humair et al. 2011). Early results from a more general study in Switzerland indicate that the smoke exposure measured by cigarettes equivalent has been reduced from 2 to 0.2, resulting in a lower risk of cardiac infarction (SR DRS, 2011). So it seems normal to restrict smoking in public places such as schools. And it is a very strong argument for forbidding smoking in private public places such as restaurant. Protecting indoor air quality from second hand smoke is an additional component of ensuring a safe environment. Similarly, businesses are obligated to enforce to food quality inspection, fire codes, and protected drinking water (HPClearinghouse, 2001: 3).

However, health effects are not enough to legitimate smoking bans in restaurant. In liberal constitutions private businesses and consumers should generally be allowed to take their own decisions. As nobody is forced to consume or work in a restaurant one could argue that government should not put a smoking ban for private restaurants. Moreover, it can be problematic to enforce the law.

Many restaurants are against smoking bans. Those who have many smoking costumers still want to offer them the same menu as before. Even non smoking restaurants might oppose the ban in order to keep the market niche (Pakko, 2006: 119). Restaurants fear that they lose revenue, because smokers substitute to venues where smoking is allowed. If it were beneficial for restaurants, many of them would have declared smoke free rooms on their own initiative. After all it is in their interest to be attractive for employees as well as smoking and non smoking customers (Pakko, 2006: 117).

Indeed, increased health awarness has lead to an opening of smoke free restaurants. (Brooks & Mucci, 2001). But as their share still is strikingly low relative to the number of people that desire smoke free restaurants suggests that smoking laws would detoriate revenues. Or it might be that markets fail to be efficient, due to coordination problems, incomplete labour markets, incomplete information or non rational agents:

Restaurants could be in a suboptimal Nash equilibrium. Namely if they do not introduce unilateral bans due to the fear of losing costumers to their competitors. But if these moves were coordinated through a ban, they would

be better off together. (Adams & Cotti (2007), Tiezi (2009: 25)) Voluntary agreements among the smokers and non-smokers in a restaurants are unrealistic, because the transaction costs would be disproportionaly high (Phelps, 1992: 430). However, Craven and Marlos (2008: 58) use a Coase negotiation setting and show that private agreements are possible and therefore smoking bans are not the only solution. In particular voluntary solutions where some restaurants are smoke free and others are not are realistic. But voluntary introduction is limited by a disclaimer of Adams and Cotti (2007), that the only holds under perfect information and no externalities. If addiction is seen as a market failure of myopic consumers as in Laux (2000) then general tobacco regulations are justified more easily. If smoking is rational as suggested by Becker and Murphy (1988), the mandate for government intervention is not the paternalistic protection of smokers, but solely externalities (Gruber, 2001: 202). The externality a smoker puts on customers and employees is used as an argument for bans, which are named as protecting employees. However, externalities are not enough to justify a ban. Rather, burdened employees ask for a compensation for the second hand smoke or otherwise would not do the job. At least under the assumption that labor markets were fully competitive and markets deal with risk efficiently.

Under complete information and rationality individuals take decisions by incorporating risk, not only when taking an airplane but also when eating in a restaurant with second hand smoke. Thus bans on smoking would result in inefficient allocations. With the smoking ban consumers chose a consumption bundle that they could have achieved before but decided not to do so. So it is likely, that they are worse off. This notion of welfare loss is not included in a purely economic welfare analysis. (Pakko, 2006: 116, 117)

Whether smoking should be restricted is debated on many complex grounds such as whether mature citizens need paternalism. The focus of the public debate is on the tradeoff between public health versus economical consequences (Pakko, 2006: 115).

2.3 Impact of smoking bans

Smoking bans have a direct impact on smoking behavior and a second round impact on the hospitality industry (Tiezi, 2010). Smokers are expected to spend less money in the hospitality industry. On the other hand non-smokers are expected to spend more. Corsun (1996) reports for the smoking ban in New York that while smokers dine out less often and spend a shorter time in

a restaurants, non-smokers who are sensitive to smoke dine out more with no net effect on dining out frequency. The relative weights of the groups is unclear, as the smokers are less numerous but tend to have larger bills (Pakko, 2006: 117). Furthermore, if smokers spend less on cigarettes, they might spend more on alcohol, assuming the budget for going out is fixed. The impact on an individual firm can be mixed and depends on its clientele and marketing strategies (Pakko, 2006: 117). Thus, the theoretical prediction is ambiguous and heterogeneous and the topic should be addressed empirically.

From an economic perspective relevant studies for the impact of a smoking ban on hospitality industry are e.g. Ahlfeldt & Männig (2010), Tiezi (2009) and Pakko (2006). The majority of scientific evidence indicates that there is no negative impact of clean indoor air policy on the hospitality industry (Tiezi, 2009). A few studies conclude a positive effects on local businesses (e.g. Alamar & Glantz, 2004; Cowling & Bond, 2005)), however several studies find a negative impact (Kvasnicka & Tauchmann, 2010).

Pakko (2006) runs a linear regression on data in Maryville. The dependent variable is quarterly growth rate of sales, the explanatory variables are a dummy for smoking ban, a time trend and seasonal effects. The point estimated of the coefficient of the smoking ban dummy indicates that sales were more than 7.5% higher during the smoking ban than what trend growth and seasonal variation would predict. The inclusion of the smoking dummy did increase the fit of the regression. In contrast to the original study from Maryville by Mandel, Alamer and Glantz (2005), Pakko accounted for the opening of a very successful restaurant in this small town. This opening was more powerful in explaining sales than the smoking ban.

While the effect of bans on smoking has mostly been discussed in the US, the effect on the hospitality industry is also a big issue in Europe (Tiezi, 2009: 23). Ahlfeldt and Männig (2010) exploit the variation across time and states of German smoking bans and estimate a panel of monthly surveys. They find no significant impact on revenues of bars and restaurants. Kvasnicka and Tauchmann (2011) analyze the German case as well, for a different time period and with a different specification. They found a moderate, but significant negative impact on sales, but not on closure of restaurants. Melberg & Lund (2009) find no impact of the 2004 smoking ban in Norway on restaurants. On bars the effect is less clear (a drop after the ban, followed by a strong increase in 2006). They use a comprehensive panel data set using bi monthly sales data from the value added tax administration and apply

an ARIMA approach. A dummy variable indicates the smoking ban and an additional variable is used with the weather temperature. They first used sales and then also the ratio of restaurant spending as a share of private consumption to see whether people substituted away from restaurants to other consumption goods.

Adda et al. (2007) used survey data to compare sales, number of customers and prices in public houses after the smoking ban in Scotland to public houses in Northern England. Their identification relies on the assumption that in the absence of the smoking intervention there would have been a similar trend in sales of public houses in Scotland and Northern England. They found a negative impact on public houses in the short run. This drop was due to fewer customers and not triggered by a change in price. The price of the best sold beer was not affected (p. 152). The relative effect on restaurant employment is neutral or mildly positive according to Adams & Scott, (2007).

Pakko (2006) offers a summary of related studies and Tiezi (2009: 32, 37) presents a table with results of smoking ban investigations. The mixed results are no clear guidance for policymakers. The variation can be partially explained by:

- The limited quality of data restricts the validity of the results.
- Two strands of literature use different methodologies and not everyone fulfills the requirements for identifying the causal effect. The health literature finds hardly any negative effect on hospitality industry. On the other hand the econometric literature is more concerned with whether the observed data correlation is due to a causal effect of the smoking ban.
- A systematic difference in the results has been found whether the study was sponsored by the tobacco industry or not (Scollo et al. 2003).
- Scollo and Lal (2002) find a four times higher probability of reporting a negative impact when using subjective data instead of objective data. Glantz (2007) argues this is due to a negative placebo effect of tobacco. Scollo and Lal (2002/2008: 19) industry claiming a negative impact. This effect might explain the discrepancy between the voices of owners of restaurant and the empirical findings.

Furthermore, different environments might limit the external validity. So let us look at the Swiss case in more detail, where no empirical study has been undertaken yet, but many voices have been raised in the media. A few headlines were:

- Liberals argue against the smoking ban (NZZ, 2008).

- Casinos blame the smoking ban (NZZ, 2009).
- The regular's table is dead (Tagesanzeiger, 2010).
- Smoking ban reduces sales (Hotelrevue, 2010).
- There are more restaurants than before the ban (Tagesanzeiger, 2010b).
- Much of smoke about nothing (Schweizer Fernsehen, 2011).

2.4 Swiss smoking regulation

The smoking ban in Switzerland is well in line with bans in the whole European Union. A comprehensive ban is in place in Cyprus, Greece, Iceland, Lithuania, Latvia, Norway, Spain and the United Kingdom. Smoker lounges are allowed in Belgium, Estonia, Finland, France, Italy, Poland and Sweden. Some restaurants can still be smoke restaurants in Austria, Bulgaria, Denmark, Germany, Hungary, Macedonia, the Netherlands, Portugal, Serbia, Slovenia, Slovakia, Rumania, and the Tscheque Republic (Federal Office of Public Health, 2011b).

In Switzerland a law protecting employees from secondhand smoke came into effect on 1 May 2010. All closed public places - including restaurants and bars - must be smoke-free. However, there can be smoking rooms, if they are well air conditioned. Restaurants that are smaller than 80 square meters can be accepted as smoking restaurants. The 26 cantons can enact stricter laws than the federal law. These cantonal laws have been introduced over several years and in most cases before the federal law. The Italian speaking Ticino was the first canton to introduce a ban, on 12 April 2007. Next to follow was Grisons on 1 March 2008. 18 cantons have a cantonal smoking law. five sixth out of them¹ still allow smoking rooms, but forbid smoking restaurants, even if they are smaller than 80 square meters. (Federal Office of Public Health, 2011b)

Exemptions are allowed mostly in small and rural cantons. This is no coincidence, because in these cantons small restaurants have political influence, also via their regular tables. In their cities bigger bars and restaurants without exemption complain about their disadvantage over small smoking restaurants across the street. Exemptions to the smoking ban as in the federal law suggest that distributional effects within a market might be even more relevant: Restaurants with exemptions might gain at the expense of smoke free restaurants. A widespread belief is that if there are losers from the smoking ban, than these are smoke free restaurants that are surrounded by small

¹AR, BE, BL, BS, FR, GE, GR, NE, SG, SO, TI, UR, VD, VS, ZH

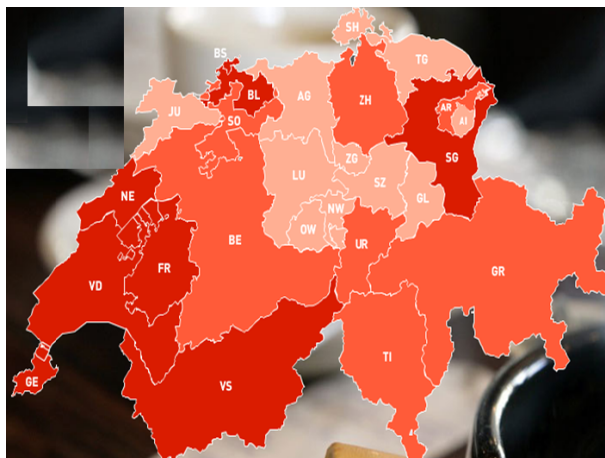


Figure 1: The 26 Swiss cantons and their smoking bans. light: federal law, which allows exemptions; fonce: no exemptions, but served fumoirs; dark: strictest ban, no exemptions and only self-served fumoirs. Source is a figure from 20 Minuten (2010) that was slightly modified.

smoke restaurants. This assertion is in line with the possibility that in absence of a general smoking law there is a suboptimal Nash equilibrium where no restaurant wants to unilaterally introduce a smoking ban.

3 EMPIRICAL ANALYSIS

This study is a natural experiment in the sense that business data from the hospitality industry is compared before and after the introduction of a policy. Controls on a firm and on a cantonal level are achieved from comparable data in cantons with different smoking legislation as required by Tiezi (2009: 18). The data is objective and comparable, because a bookkeeping firm undertakes the bookkeeping for around 1'000 restaurants. The micro data is yearly data on a firm level from 2006 to 2010.

3.1 Identification

The goal of this paper is to analyze the immediate and deferred effect of smoking bans on the hospitality industry.

$$\textit{Hypothesis : } \quad ban_{jt} \Rightarrow sales_{it+k} \quad \textit{for } k = 0, 1, 2 \quad (1)$$

In order to identify and measure this causal effect it is not enough to focus on the sales of restaurants. A pure correlation is no proof of causality or of a structural relation. (Heckmann, 2007: 3) In this study spurious correlation could arise as the introduction of the federal smoking law in 2010 coincided with the recovery of the Swiss economy and fluctuations in the price of commodities. The second issue is controlled by the availability of the wholesale input costs for the restaurant. The identification of the causal relationship relies on:

- Variation in time of the smoking ban introduction.
- A panel of objective microdata from 1'000 firms over five years.
- Several control variables on cantonal and firm level.

3.2 Endogeneity and voting

Identification would be destroyed if the critical assumption of the nature of the natural experiment was violated:

$$\text{Exogeneity assumption} \quad : \quad ban_{jt} \perp \epsilon_{it} \quad (2)$$

The exogeneity assumption requires that (conditional on other covariates) the ban_{jt} should be uncorrelated with other non-observed (time varying factors) that are captured in the error term ϵ_{it} . Concerns arise for the exogeneity assumption, because the introduction of smoking bans were known in advance. As a consequence, costumers could adjust their behavior to circumvent it. But as going to a restaurant is a spontaneous decision, neither hoarding nor pre-drawing is realistic and the characteristic of a natural experiment is not affected. From the firm's perspective, if in anticipation of the ban restaurants would change their investment pattern previous to the ban in a way that affects the growth of sales, then the estimated effect would be biased. With fixed effects it would be biased downwards, if investments are undertaken a year before the ban was introduced and immediately increase the growth of sales. (Papaioannou & Siourounis, 2008: 1526)² Excluding this effect implies assuming that that

$$E[ban_{jt+1}] \not\Rightarrow sales_{it} \quad (3)$$

²Recall that the growth rate is negatively affected if only the base year gets a higher value.

Further concerns arise for the exogeneity assumption, as cantonal smoking bans needed approval from public votes. In the forefront of the votations there were intensive debates. Parts of the hospitality industry did campaign against the smoking ban, because they feared drops in sales. If the sales of hospitality industry were to influence the cantonal smoking ban regulation, then there would be reverse causality (Sunde, 2011: 6).

$$ban_{jt} \Leftarrow sales_{it+k} \quad \text{for } k = -2, -1, 0, 1, 2 \quad (4)$$

A simultaneous impact for $k=0$ can be ruled out, because the political process needs time to be settled and implemented. If bans are less likely to be introduced in cantons with a negative outlook for restaurants relative to restaurants in other cantons, then the estimated coefficient on the ban would be biased upward (Papaioannou & Siourounis, 2008: 1526). We think it is unrealistic that expected future sales growth is both a good proxy for effective growth rate in the future ($k>0$) and also effectively has had impact on the smoking bans. The fact that the Swiss voters accepted in most cases a smoking ban proposition suggests that the campaigning by the hospitality industry could not reject the proposal. But in some cantons smoking bans were rejected (for example in Nidwalden in 2008).

Furthermore, lagged dependent variables are inspected ($k<0$). Among other sales in the hospitality industry might determine the size of the campaign against the ban we test whether lagged dependent variables can explain the policy variable. Our data suggests that the existence and type of cantonal smoking ban by 2009 is unrelated to the growth rate of sales in the precedent years (2007 and 2008).³ As there is no feedback effect from lagged dependent variables on the policy variable, the assumption of predetermined variables is not needed. Instead, we assume strict exogeneity:

$$E[\epsilon_{it}|ban_{it+k}] = 0 \quad \text{for } k = \dots - 1, 0, 1, \dots, \quad (5)$$

which in contrast to the assumption of predetermined variables - still allows serial correlation in the errors. (Arellano & Honore 2001: 3234-2326)

More realistic than reverse causation is endogeneity of the smoking ban,

³A regression with the observations for years 2007 and 2008 on the type of ban by the year 2009 and other controls leads to a negative coefficient of -.33 that is insignificant on a 50% level. Growth rates were used, because level data is more likely to be affected by the sample.

which can lead to biased estimators as well. While the former requires the smoking ban to be influenced directly by the dependent variable net saels, the latter only requires the smoking ban to have been introduced non-randomly, thus in any systematic way. One could imagine that local preferences with respect to smoking have an impact on the cantonal smoking law. It is possible that general political considerations influenced the type of the proposed smoking ban, because in some cantons no smoking ban was proposed, and not every cantonal ban is equally restrictive.

Adams and Cotti (2007: 7) point to the fact that with Utah and California two states with low smoking prevalence introduced the first two smoking laws. Chaloupka and Saffer (1992) wrote a report about the potential endogeneity of smoking restrictions using annual state-level data for 1975-1985. They found that that smoking laws are indeed a function of cigarette consumption. Similarly, Craven and Marlos (2008: 58) claim that the higher the share of non smokers, the higher the probability of a smoking ban.

In Switzerland the prevalence of active smoking is known for 16 cantons in 2002 and for 14 cantons in 2007, for passive smoking for 16 cantons in 2007. The pairwise correlation between cantonal smoking law and smoking prevalence suggests that there is no relationship between active smoking and Swiss bans (considering only cantonal smoking laws and only one observation per canton). Passive smoking has a significant correlation not with bans in general, but with the strictest ban. This correlation occurs as in all French speaking cantons the strictest type of ban was introduced in 2009 and there, smoking rates are above average and the structure of the economy is different. Controlling for being Romand weakens the correlation, but it does not vanish.

If the factors explaining smoking bans also affect the sales of restaurants or the impact of the smoking ban, then the policy variable is prone to endogeneity.

$$ban_{jt} \Leftarrow X_{it-1} \Rightarrow sales_{it} \quad Or \quad (6)$$

$$ban_{jt} \Leftarrow X_{it-1} \Rightarrow \beta_{it} \quad (7)$$

Potential endogeneity can be circumvented by explicitly incorporating observed X_{it-1} into the regression equation and thereby controlling their effect. Thus, as soon as we explicitly incorporate the strictest type of ban separately, we need to incorporate the passive smoking rates as well. Analogously, the density of restaurant is a candidate for fulfilling equation 7 and

should therefore be incorporated. Many candidates for X_{it-1} are not observed. The non observable tolerance towards smokers is suggested by Tauras (2006) or Wassermann (1991) to influence both smoking ban legislation and its effect on sales. The latter because public sentiment has an influence on whether smoking bans are strictly applied enforced.⁴ The leap in time in equation 7 is not relevant, as soon as there is serial correlation in X_{it} . If and only if the serial correlation is unity, i.e. the sentiments are constant over time, the endogeneity problem can be circumvented by using growth rates as the dependent variable or using firm fixed effects. This panel method implies that all time constant factors are controlled.

Expand the paper technically and by a further public choice dimension by adding a first stage to the regression.

If the exogeneity assumption holds, the treatment is not correlated with unobservables that affect the response. As a result, with a panel approach the effect of the smoking ban can be identified (even if we cannot assume ignorability of treatment and even without using instruments). After all our treatment varies over time. "If the treatment is assumed to have the same effect for each unit and if the effect is constant over time, fixed effects can be used". (Wooldridge, 2007: 637)

With serial correlation, e.g. in the residuals, a serial dependence can be transmitted over several time periods.

3.3 Empirical model

In order to analyze the effect of the smoking ban on the sales of restaurants we estimate the following regression equation:

$$g_sales_{it} = \alpha + \beta_1 * \Delta ban_{jt} + \beta_2 * \Delta ban_{jt-k} + \beta_3 * \Delta ban_interact_{ijt} + \gamma_1 * control_firm_{it} + \gamma_2 * control_canton_{jt} + \delta * year_t + \epsilon_{it} // fork = 1, 2, 3$$

- The dependent variable is approximated by the first difference over time in the logarithmic value of real netsales. Net refers to gross sales minus the wholesale cost of input and real implies an adjustment for the

⁴The case of the tribunal in Arbon (TG), who forbid a bar in Romanshorn to be a smoker restaurant as all guests became member of a club stands in contrast to the tolerated club club fümöir in Basel, whose effect ist that most restaurants in Basel ar still smoke-restaurants.

consumer price index. If netsales were used in levels, the effect of the policy variable would be dominated by the composition of restaurant sizes in the sample, unless in a fixed effect panel specification. To a lesser extent this argument also holds for first differences, because bigger restaurants tend to have higher absolute fluctuations in sales. Most robust to the composition of the sample is the growth rate of sales. Using a growth rate as the dependent variable must be congruent with the choice of the treatment variable.

- Control variables are grouped into firm specific and cantonal specific effects as well as year dummies to capture various external effects.
- ϵ_{it} is the unobserved stochastic factor.
- ban_{it} is a dummy variable taking the value 1 if there was a smoking ban and 0 otherwise.

If the ban has an effect, it is unlikely to disappear after one year. The first mover Ticino should be considered as treated in every year from 2008 onwards. This suggests either taking the policy variable in levels or using the difference in policy variable for several years.⁵

Not every canton enacted the smoking ban in the same year. As a consequence, by distinguishing immediate and deferred effects we get a difference in difference interpretation also to the data from year 2010. While in year 2010 all cantons had some sort of smoking ban, some of them had a ban already in the previous year. The difference is further preferred to levels, because it makes it congruent to the dependent variable which is growth rates.

It is unlikely that the ban has the same impact on every growth rate of sales after the ban was introduced relative to sales growth rates before the ban. In contrast a one time shift is more plausible. Thus, we address the policy separately by year after introduction. To test the impact in the first year, we check whether the growth rate drops in the year the ban was introduced. As the policy is introduced at a certain date within the year but sales is a flow variable accumulating over a year an immediate effect can be expected. Unfortunately, few smoking bans were introduced on January 1. The coefficient of primary interest is β . In a panel estimation equation with control variables, β can be interpreted as a difference-in-difference estimator. The effect of a treatment in one canton is used as the first difference and

⁵The difference in the policy variable can only take the nonnegative values 0 or 1, because no smoking ban has ever been abolished

compared to the development in cantons that are not treated as the second difference (Sunde 2011: 5). But because cantonal bans were introduced over several years, the estimate cannot be formally written as the difference over one year between average change in treated versus average change of the non treated. Nevertheless, the interpretation remains similar and corresponds to the change in the average value of sales due to the smoking ban (Wooldridge, 2002: 446). Given that the policy variable is a dummy variable, the interpretation is not a classical elasticity. The interpretation of the estimated coefficient $\hat{\beta}$ in terms of netsales is: (Halvorsen and Palmquist, 1980)

$$\text{Magnitude of the effect} \hat{=} \exp^{\hat{\beta}} - 1 \quad (8)$$

We run ordinary least squares (OLS) as well as a fixed effects panel estimator. The estimates of OLS with the dependent variable in levels would correspond to the traditional cross sectional interpretations: Controlling for other characteristics how much higher growth in netsales have restaurants without ban than those with a ban (Glick & Rose, 2002: 1131). But heterogeneity across firms and cantons might bias these estimations. Thus, the dependent variable is taken in growth rates and fixed effects are used. (Lundberg & Rose 2002: 225) The fixed effect specification exploits the within variation over time to answer the relevant policy: What is the netsales effect of restaurant being forced to adapt to a smoking ban (Glick & Rose, 2002: 1131).

Only if the differenced errors are serially uncorrelated we can use the standard t and F-statistics. (Wooldridge 2002: 453). If the residuals dispose positive serial correlation, then the estimated standard errors are biased downwards, as also the policy variable is serially correlated (Bertrand, Duffo, Mullainathan, 2004). Serial correlation in the dependent variable and the error terms is a serious issue in panel analysis (Wooldridge, 2002:450). Following (Ahlefeld & Maennig, 2010: 510) we will test for autocorrelation in the residuals by a Lagrange Multiplier test (in a fixed effects model).

With the time dimension being five we have a wide panel and time series issues should not be the focus. However, we need to perform tests for serial correlation in the residuals and in the dependent variable. If there is serial correlation in the residuals, this would be a strong indicator that there is also a relevant and persistent process that is not covered by the covariates yet. Use a lagged dependent variable as an additional regressor and check whether

the serial correlation goes down. check for unit roots / trends also in the dependent variable. If there is one in levels, then this is another argument for taking the growth rates as the dependent variable. If there is a trend even in growth rates, then we would have an explosive system or to say more moderately - expanding restaurants.

Robust estimators are used to account for heteroscedasticity in errors.⁶ They correct the standard errors without affecting the values of the coefficients. If serial autocorrelation is found we would cluster the standard errors at the state level (Bertrand, 2004 in Ahlefeld).

4 DATA

The bookkeeping firm undertakes the bookkeeping of 1'952 restaurants. These accounts contain yearly data on many aspects of the restaurants. Unfortunately, the panel is not balanced as for most restaurants some years are not observed. Suitable are the 506 restaurants with data for the year 2009 and 2010. Another 449 firms have data until year 2009. The treatment and control variables are collected from Swiss administration data.

4.1 Policy variable

The policy variable with which restaurants are treated is ban_{jt} and has variation across time t and canton j . A quarter of the observations of this dummy variable is 1, the majority is 0 according to table 1. In Switzerland there

Year	without a ban	wit a ban	ban_{ch}	ban_{kr}	ban_{krfu}
2006	1174	0	0	0	0
2007	1107	34	0	34	0
2008	955	111	0	111	0
2009	372	562	0	301	261
2010	1	537	111	194	233

Table 1: Number of observations per year with and without a smoking ban

are three types of smoking bans for restaurants. The variable $ban_{type_{jt}}$ is

⁶Lundberg & Rose (2002: 225) use Huberized standard errors due to the repeated observations by individual (Huber, 1967).

ordinal, ordered by the strictness of a ban. To account for this nonlinearity and allowing to focus on individual types of policy the type of ban is further reflected in a series of dummy variables:

- *ban_type* = 0: no smoking ban.
- *ban_ch*: The federal smoking ban is the least strictes: Even though restaurants are generally smoke-free, served fumoirs are allowed and exemptions for small restaurants can be granted.
- *ban_kr*: Stricter than *ban_ch*, because no exemptions for smoke restaurants are all. Served fumoirs are still allowed.
- *ban_krfu*: Similar to *ban_kr*, but even stricter as fumoirs must be self-served.

Unfortunately, in only 2% of the total observations a ban was in place according to the federal law. And as this federal law was installed in year 2010 it can contemporarily be compared to only three restaurants without a smoking.⁷ The majority of our observations with a ban are subject to the stricter cantonal law. That makes it hard to compare the federal law to the stricter cantonal laws, where no exemptions for small restaurants are allowed.⁸

4.2 Dependent variable

The dependent variable is an indicator of the success of a restaurant and reflects the relevant impact of the policy change. Candidates are total sales, net sales after subtracting the wholesale input costs, profit I after energy and other costs, gross profit II after wage cost, or earnings before interest and taxes (EBIT). EBIT is the variable with the lowest correlation to the

⁷Basel Stadt is not considered as having a smoking ban, because parts of their restaurants found a way to circumvent the ban.

⁸With the exception of Jura the French speaking cantons introduced in 2009 the strictest type of ban *ban_krfu*. The Romandie constitutes the majority of this strictest type of ban. This correlation is an argument for not taking Romandie as a control variable even if Romandie significantly explains netsales, because otherwise multicollinearity would wash out partially the effect of the strictest type of ban. But then again, we were arguing in equation 6 that covariates that explain both netsales and bans should be included in the regression. As long as we do not distinguish the type of ban, but just use a [0,1] indicator for the ban, then multicollinearity between ban and Romandie is weak, but does not vanish. As soon as we distinguish between *ban* = 2 or 3 then the control dummy has to be dropped. Note that in fixed effects regression the time constant indicator Romandie drops anyways.

other candidates and so it is a non robust measure of success. The correlation between sales and netsales is very high with 0.987, suggesting no major informational difference between them.⁹ Netsales is preferred as it controls for the cost of inputs to account for effects such as that all prices could have increased due to higher prices of wheat. Netsales fluctuate less than sales, partly because they are affected by fewer discretionary influences such as the salary of the owner. Furthermore, the logarithm can only be taken for positive values, which is fine for netsales but not for profits. Thus, we chose netsales as the dependent variable. To control for general price trends we initially deduct the consumer price index to arrive at real numbers. This transformation did not affect the results in previous studies (Kvasnicka & Tauchmann, 2011: 9). We do not divide netsales by the number of days of operation, even though some accounting years were considerably longer or shorter than a calendar year.

The variation in the absolute numbers of netsales data is high. Taking the mean by canton the average netsales is 3.874.000, with a minimum of 1.457.000 and a maximum of 8.052.000. Given that partially it is due to notably differences in the composition of the sample across cantons this dispersion is another justification not to use absolute values as the dependent variable. Instead, in a first step the logarithm of netsales is taken. This recommendation is strengthened by the ease of interpretation and the possibility to construct approximately growth rates. Following an usage in economics, we use the first difference of logarithmic data and multiply it with 100 to arrive approximately at the growth rate in percentage points. Given that logarithm is not a monotonic transformation, the correlation between net sales and the logarithm of net sales is only 0.69

Figure 2 depicts the evolution of the aggregated median of the logarithm in netsales, in levels and in growth rates. Their correlation is only 0.11. The sample in Ahlfeld & Manning (2010: 509) was not found to be non-stationary when they applied standard panel unit root tests as in Levin Lin Chu (2002). We apply the panel unit root¹⁰ only to the subsample of 329 restaurants with data on g_sales_{it} for 2006 to 2010, because a strongly balanced sample is

⁹The high correlation among wholesale costs or sales renders them candidates as instruments for net sales. A dependent variable does not require any instrument, but when testing reverse causality.

¹⁰command in stata: xtunitroot llc growth_netsales

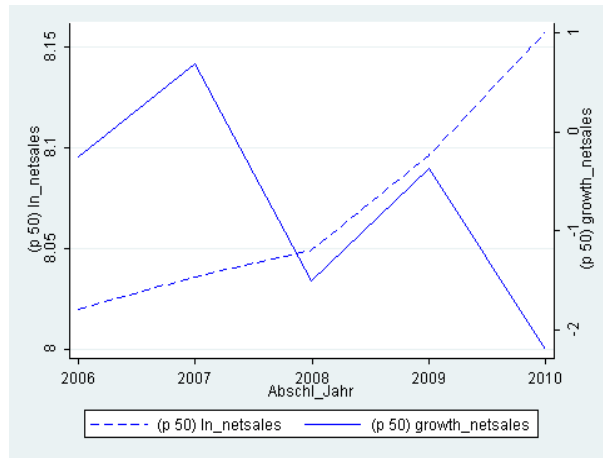


Figure 2: Median evolution of the logarithmus of net sales and its growth rate.

required. The Null hypothesis that the panel contains a unit root is rejected. The panel is stationary not only for growth rates of sales, but also for netsales. The chi-square statistic of a Doornik-Hansen test¹¹ strongly rejects that the dependent variable is normally distributed.

4.3 Outlier/Coding

The sample is restricted to values between the years 2006 and 2010. In 2006 there were no smoking bans yet. We still use the 2006 as a base year to construct growth rates. Furthermore, it is useful to include data from 2006 in general, because it increases the overall size of the sample and by including a dummy for the year 2006 we capture effects specific for this year. This advantage must be weighted against the disadvantage of losing some observations as not all restaurants indicate sales for 2005 respectively the numbers of employment for 2006.¹² The data for year 2010 can be used to compare different types of bans and to distinguish between immediate and lagged impact. If years before 2006 were included as well, then the regression results are not influenced more by observations without a ban if and only if corresponding time dummies are incorporated.

In the dependent variable - but only in the dependent variable - there are several strange values. Observations, where gross sales are negative, are dropped.

¹¹command in stata: mvtest normality growth_netsales, univariate

¹²All other control variables are not jointly specific to a firm and a year.

In most cases these are not restaurants but hotels where sales are generated predominantly by lodgement. Furthermore, we drop observations whose values are extreme, where the growth of net sales is outside an interval of two standard deviations. This cut has only a minor (not even a third of a standard deviation) impact in the mean growth rate per year. One justification for eliminating these outliers is that the identification number identifies the entrepreneur and not the restaurant. So it can be that the entrepreneur closes down its restaurant and opens a new one - of different size resulting in incorrect growth rates. We checked for each firm where the canton changed, whether it was most likely still the same firm (similar number of employees and sales) and adjusted the canton and its law on ban accordingly. Firms, where no evidence for still being the same company was found were dropped, at least some years out of it. Eleven further observations were dropped as we could not identify their canton at all and so neither its smoking ban law.

4.4 Control variables

Control factors are indispensable for smoking bans' studies (Tiezi, 2009: 33), because they absorb spurious correlation. For example many smoking bans were enacted in 2009 and 2010. Increasing prices for raw materials and a recovery of the economy strengthened net sales. As a remedy we incorporate yearly dummy variables to account for time fixed effects. Moreover, we have cantonal controls indexed by jt to absorb demand shifters that are unrelated to the policy variable:

- $nights_{jt}$ The number of persons staying overnight indicates the success of the tourism industry in general.¹³ Even though hotels need to adapt to smoking bans, their sales can be assumed to be unaffected by the ban, rendering this control variable particularly useful for this study.
- ur_{jt} Cantonal unemployment rates are used as in Ahlfeldt & Maennig (2010: 510) to proxy the state of the local economy.
- $population_{jt}$ The more people live in a given canton, the more hospitality products are consumed overall (Tiezi, 2009: 30).

¹³The Swiss Federal Office of Statistics offers a number of data for the hotel industry. Absolute number of firms, rooms or beds represents the supply side, which is altered over a longer time horizon. Relative measures of utilization ratio are more indicative. The correlation with the absolute figure net sales and profits is significant and highest with the number of guests staying overnight.

We also control for firm characteristics influencing the dependent variable independently from the policy variable. Good choices are:

- $labour_{it}$: Number of employees.¹⁴
- $type_i$ We generate dummies for the categorial variable restaurant, bar or hotel that reflect the main types of firms.
- $townsize_i$: An indicator between 1 and 4 for the size of the village, reflecting the structural difference between rural and urban areas.

The summary statistics of the control data is shown in table 2. A control variable should have only few missing values, otherwise the whole datapoint is dropped from the regression. With the controls above, only 77 datapoints vanish. It is only for this reason the we use cantonal unemployment rate, which is lagging the economic environment, instead of cantonal GDP, as the latter is not available for 2010 nor 2006. The share of smokers are ignored due to missing data.¹⁵

variable	obs	mean	s.d.	min	max
<i>nights</i>	4854	2.73	2.00	0.07	6.23
<i>ur</i>	4854	3.11	1.28	0.83	7.03
<i>population</i>	4854	0.48	0.33	0.01	1.28
<i>labour</i>	4779	6.62	7.81	1	308
<i>townsize</i>	4777	1.65	1.032	1	4
<i>type</i>	4116	2.53	2.30	1	8

Table 2: Summary statistics of the control variables.

¹⁴There is also a control variable $size_{it}$ of the firm. $size_{it}$ and $labour_{it}$ are necessarily very highly correlated with $sales_{it}$. The correlation is even stronger for $size_{it}$ than $labour_{it}$, because the former is defined directly from sales. Measures after labour costs such as gross profits II would be a dependent variable that is less correlated with size and labour. Moreover, there would be high multicollinearity between size of restaurant and labour. While this would not affect the coefficient of the policy variable, multicollinearity limits the interpretation of the control variables.

¹⁵The share of smokers is available for 2007 and only for some cantons only for active smoking, passive smoking is missing for 2007 and 2008 and for some cantons. Anyways, There is only limited variability in the passive smoking shares across cantons and it is on a low level. From the 14 cantons where cantonal data is available the min is 13.5, the max is 20.9, and with 2007 there is only one year of observation. So even if data was available, I would not expect too much from it.

4.5 Moderating variables

A smoking ban is expected to have a different impact on different types of firms. The more social interaction there is between smokers and non smokers the more likely losses are to be expected from the ban. Owners with more smoking customers predict losses more often than those with few smoking customers (Marlow, 2010). Some of these differences can be traced:

- Bars are more likely to suffer from smoking ban than restaurants. In a survey by Dunham and Marlow (2000) 39 percent of restaurant owners expected losses from smoking bans but 83 percent of bar owners. The Norwegian study by Melbert & Lund (2009) suggest that the losses from the bars are outweighed by the gains from restaurants.
- Smaller restaurants are more likely to suffer from a ban (Kvasnickea & Tauchmann, 2010: 19), also because they rely more often on customs tables. This finding is in line with the Swiss Federal law which allows exemptions for restaurants that are smaller than 80 square meters.
- Smoking bans have a larger detrimental impact (on bars) in geographic areas with a high prevalence of smokers (Adams & Cotti, 2007). *Generate a moderating variable with smoking rate if there is better data on smoking rates.*
- Adda et al. (2007) suggest a different impact in urban and rural areas.

We generate several terms interacting with Δban_{jt} . One is a geographic indicator of $city_{it}$ ¹⁶, another for small restaurants that can have an exemption ban_exem_{it} or only their peers can ban_noexem_{it} . The third interaction is the type of firm being a bar_{it} . This moderating term can be generalized by constructing the ratio drinks to food ratio. To ensure the interaction term states what the peculiarity is of the ban for this type, we need three variables for each interaction: the policy variable, the interaction term and the control for the moderating variable.

To test whether the impact is different in the romandie, we should generate an interaction term between ban and Romandie. As the smoking prevalence is higher in the Romandie, this approach also takes the suggestion from Adams & Cotti, 2007 about geographic areas with higher smoking prevalence. However following the argument in the section treatment variable we do not include this interaction term.

¹⁶more than 20'000 inhabitants

5 RESULTS

Let us first inspect several figures. Figure 3 shows that restaurants with a ban had a lower level of netsales, but not a lower growth rate than those with a ban. The growth rate is more important, because it is to a lesser extent dominated by fixed effects of the composition of the sample. In contrast the level is predominately affected by the composition of the sample, which also changes when a ban is introduced somewhere. Note that in the beginning there were only few observations with a ban (Ticino and Grisons) and in 2010 there was only Basel City left with no effective ban.

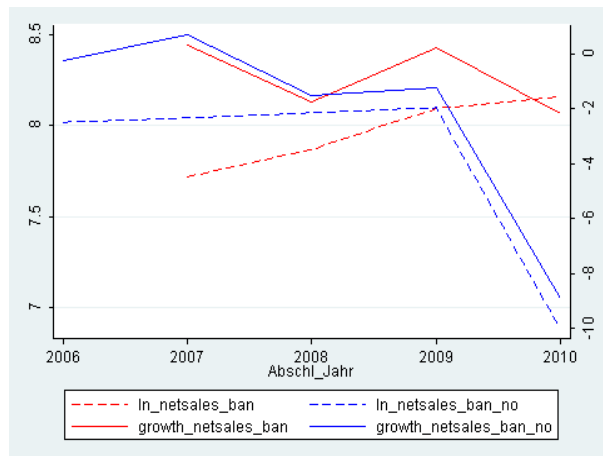


Figure 3: A comparison between the median of those cantons with and without a ban.

Figure 4 shows the evolution of the median nets sales for groups that introduced a ban in 09 and those that did not. As there is a one to one matching between federal law and year of introduction in 2010 the last data point in this figure also compares cantonal to federal law. While the level is similar for both groups, the higher growth rate in 2009 for those with a cantonal ban is striking. It provides a hint that the policy was not negative for restaurants. But then the fact that the mean growth rate of netsales was the lowest in 2010, where we had smoking bans everywhere is indicative for a negative effect of ban. But it lacks the control of further influences - such as the state of the economy - that can be achieved by regressions.

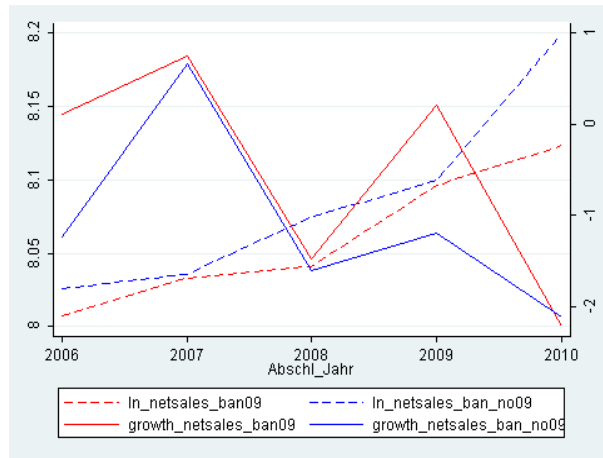


Figure 4: A comparison between the median of restaurant with a cantonal smoking ban to restaurants with a federal ban.

5.1 Regression analysis

We start with an unconditional model where sales is only explained by the policy variable. Table 3 shows the OLS regression output, with the dependent variable growth in netsales. The very first results show no significant and robust effect of the ban on the sales of restaurants.

5.2 Robustness checks and sensitivity analysis

At a later stage we will perform robustness checks: We start by including only the policy variable and then add more and more controls and check whether the coefficient on the policy variable fluctuates a lot (Ahlfeldt & Maennig, 2010). We apply different specifications and panel estimators as Glick and Rose (20002: 1134). Further Robustness checks are undertaken by splitting the sample for every year respectively for every type of restaurant as in Goos & Konings (2007: 17). The calculations are replicated for different types of bans, analogous to Cervellati & Uwe Sunde (2011: 10).

Furthermore, we run the regression where the dependent variable is sales of drinks only, because the consumption of beverages might react most volatile to a smoking ban. Taking the number of employees as an alternative depen-

	OLS						FE					
	restricted		basic		interact		restricted		basic		interact	
cons	-0.53	-2.64	-2.68	-2.19	-2.33	-1.8	-0.14	-1.15	-3.53	-0.69	-3.78	-0.74
Δban	-0.63	-1.34	0.18	0.25	0.18	0.23	-1.79	-3.61	0.13	0.16	0.52	0.59
Δban_{t-1}	-2.27	-3.64	-0.55	-0.53	-0.65	-0.61	-4.16	-5.83	-0.37	-0.28	-0.27	-0.19
Δban_{t-2}	0.37	0.33	1.63	1.17	1.27	0.91	-1.84	-1.18	3.53	1.61	3.49	1.57
Δban_{t-3}	-3.80	-1.31	0.53	0.2	0.27	0.1	-6.05	-2.01	1.61	0.43	1.59	0.42
y06			2.77	2.75	2.68	2.51			4.54	3.13	4.62	3.01
y07			3.77	3.76	3.69	3.46			4.46	2.68	4.60	2.64
y08			1.74	1.82	1.63	1.58			1.82	1.06	1.93	1.08
y09			2.55	3.24	2.56	2.92			2.99	3.17	3.10	2.93
ur			0.05	0.28	0.03	0.15			-0.80	-0.93	-0.76	-0.88
population			-0.49	-0.8	-0.57	-0.94			-5.70	-0.81	-4.91	-0.72
nights			0.17	1.52	0.18	1.68			0.37	0.2	0.15	0.08
rest			-1.13	-1.63	-1.09	-1.58			4.23	1.41	4.40	1.48
bar			-2.31	-1.88	-2.12	-1.6			3.03	0.47	2.81	0.42
city			0.70	1.56	0.93	1.92			0.16	0.07	0.74	0.33
beverages					-2.32	-3.25						
diff_ban_bar					5.92	2.02					5.39	1.66
diff_ban_city					-1.70	-1.46					-3.00	-2.64
ban_exem					-1.15	-0.36					-2.47	-0.66
ban_noexem					0.50	0.31					1.37	0.74
ban_beverages					0.03	0.02					-0.62	-0.37
R-squared	0.00		0.01		0.02		0.00		0.02		0.03	
F test diff_bans	3.96		0.7		0.55		10.2		3.5		1.22	
N	4185		4116		4.116		4185		4116		4116	

Table 3: Regression output with growth of real netsales as the dependent variable; coefficients and t-statistics are reported.

dent variable would test also the implications on the labour market.

6 DISCUSSION

Heckmann (2008: 41) requires for a smoking studies first, that valid measurements of both sales and the policy are available. Second, arguments that other drivers of the dependent variable are taken into account and third replicability of the study. In our sample the measurement of the policy and the dependent variable should not be erroneous, as the bookkeeping firm did the accounting centrally. Following the reasoning of Adams & Scotti (2007) a detrimental impact on restaurants should be bigger in Switzerland than in other countries due to the above average smoking prevalence. The absence of constantly warm temperatures where smoking outside is a small issue further renders the external validity of the Swiss case is promising.

The findings from this study suggest that there was no robust and significant effect from smoking bans on restaurant sales. However, the converse argument does not hold: It is not a statistical proof that restaurants did not suffer from the ban. One could think of statistical not significant effects that might still be relevant for some firms and general distributional distortions. An aggregate effect is not completely informative for individual firms, em-

ployees or customers. It might be that smokers adjusted their behavior and went to restaurants less often, but this drop was compensated mainly by non smokers who dine out more often and longer, and perhaps even by smokers who use the savings on tobacco to spend more on drinking.

This study has a narrow focus on the hospitality industry. Bans have got other implications. A few studies showed positive effects from the Swiss ban for public health (Humair et al. 2011). Related industries might suffer, namely those that were smoke free already before and whose goods are a substitute for the hospitality industry such as cinemas or electronic games. If bars were hurt most, that would be an argument to exempt bars from the smoking ban. This second best outcome achieved through the political process rather than the market might reduce the potential inefficiency and dampens the most extreme negative impacts (Pakko, 2006: 119) unless bars do not gain at the expense of restaurants.

6.1 Limitations of the study

Sales of restaurants fluctuate considerably by nature (Tiezi, 2009). Thus our dependent variable has a big variance even in the absence of any effective regulatory change. And the variation in the policy variable is limited. As a consequence we expect high variation of the estimated effects (Kennedy, p. 369). This variation in turns reduces the likelihood of finding significant effects. Melberg & Lund (2009) discuss that the prevalent nonfinding of a significant effect could be due to the natural high variation of sales revenues. Relative to other studies our sample consists of more observations, which is helpful for judging the significance of an effect. This issue is even more pronounced in a fixed effect setting, as there is more variation in the explanatory variables across firms than over time for a specific firm. In line with Pakko (2006: 122) we included control variables to absorb as much of the non policy related variation in sales as possible.

The fact that the panel is clearly unbalanced violates a requirement for natural experiments, namely that the same individuals should be observed before and after the policy implementation (Blundell and Macurdy, 1999: 1612). To avoid biasedness, those who leave must be in no relevant way systematically different. If the sample of restaurants suffer from survival bias, then it might be that the survivals actually report better figures, but partly because other restaurants closed down leaving a higher market share to the remaining

restaurants. If bankruptcy was a reason for no more being reported, we would have self selection and the remaining data points would be correlated with the idiosyncratic errors ϵ_{it} .

6.2 Further research question

In order to combine the topic with a further topic we might investigate at a later stage the following research topics:

- We might link the result from this study - which is based on objective sales data - to survey data, where restaurants indicate whether they fear that a legislative change will have an impact on their business. A discrepancy between objective and subjective measures would be in line with the placebo discussion that Glantz (2007) suggested for survey data.
- Owners might adjust prices, wages, hours of operation and other business attributes in response to bans. So bans also affect customers and workers. We could analyze the prices of restaurants in response to the smoking ban.
- Explain the outcome of cantonal votes in parliament and population on a community level by cantonal characteristics (income, education etc.) which are relevant for the hospitality industry. This investigation is interesting per se from a public choice perspective and relevant as explained in the section about voting and endogeneity.

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