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The Economic Impact of Clean Indoor Air Laws:
A Review of Alternative Approaches and of Empirical findings

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Abstract - This paper surveys existing approaches and empirical results to estimating the impact of Clean Indoor Air Laws on smoking behaviour, on the one hand, and on the hospitality industry, on the other. The purpose is twofold: first, identifying important gaps, if any, in the literature that could be addressed in future research; second, trying to unfold the reasons of the wide heterogeneity in the results and, as a consequence, to provide an assessment of the reliability of those results.

The discussion begins with a look at the recent regulations that motivate the study of the impact of Clean Indoor Air Laws, with a special emphasis on European smoking bans. This is followed by critical reviews of studies and approaches to estimating the economic impact of Clean Indoor Air Laws. We can distinguish between a direct and an indirect effect of anti-smoking regulations: the direct effect on smoking behaviour and the indirect effect on the economic performance of the hospitality industry. The first review assesses those studies and approaches that have focused on the direct impact on smoking behaviour. The second review analyzes estimation of the economic impact on the hospitality industry. At the end of each of the two broad reviews, we summarize a selection of the empirical findings. The fifth section explores methodological differences and problems that may affect the empirical analysis reviewed in the previous sections with the purpose of shedding light on the wide heterogeneity in the empirical findings. The concluding section asks whether the studies reviewed in this paper place us in a better position to assess the economic impact of Clean Indoor Air Laws.

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1 Introduction

Clean Indoor Air Laws have been widely used to reduce non smokers exposure to second hand smoke as well as overall tobacco consumption. The earliest policies of this type have been implemented in the United States as early as 1971 in order to protect second hand smokers from exposure to air pollution from smoking. Since then, thousands of Clean Air Indoor Air Laws have been introduced in the US at the state and local level so that smoking is forbidden in a majority of public places, including workplaces, bars and restaurants. Canada, Australia and the UK have followed suit, introducing smoking bans in the public places over the eighties.

As Chaloupka and Saffer (1992) emphasize, these Clean Indoor Air Laws generally prohibit smoking in health care facilities, retail stores, public transportation, indoor cultural or recreational facilities, government buildings, public meeting rooms, and schools and require restaurants to provide non-smoking sections. The most restrictive of these laws also prohibit smoking in private work places. These private work place laws are likely to have a much greater impact on cigarette smoking due to the length of time they affect smokers: while public place laws are likely to affect the average smoker for little more than a few hours a week, the private place laws can regulate the smoker's behavior for 40 or more hours each week. Therefore workplace smoking bans are likely to produce a stronger effect in comparison with smoking restrictions in recreational sites.

Smoke free workplaces not only protect non smokers, but they also create an environment that encourages smokers to cut back or quit smoking or to change habits. For this reason restricting smoking in public places may have three different effects: (i) a reduction in smoking participation (otherwise known as smoking prevalence), i.e. on the decision to smoke or not; (ii) a lower tobacco consumption per continuing smoker; (iii) an impact on the hospitality industry. The second effect has been the object of study of a number of papers and there is now a consensus on the belief that smoke-free public places are associated with lower cigarette consumption per continuing smoker. Later studies, as those reviewed by Fichtenberg and Glantz (2002), have assessed that smoke free workplaces reduce prevalence of smoking as well as consumption. Regarding the third effect, restaurants and bar owners, in particular, claim that smoking restrictions would cause revenue losses as their customers could change their habits and seek other venues where smoking was unrestricted. However, the majority of scientific evidence indicates that there is no negative impact of Clean Indoor Air policies on the hospitality industry, with a few studies finding some positive effects on local businesses (Eriksen and Chaloupka, 2007; Scollo and Lal, 2008).

This paper reviews the impact of the implementation of Clean Indoor Air Laws on smoking participation and on per-capita tobacco consumption. The impact on the hospitality industry will also be assessed in a separate section. While a majority of the literature

deals with the United States, a number of European Studies is also included. The focus of this review will be on the methodological reasons that may explain the variability in the results of the empirical works evaluating the economic impact of smoking bans, such as: type of data, econometric techniques, variables included, with the purpose of shedding some light on the main methodological factors, if any, driving the heterogeneity in the empirical studies. Only peer reviewed studies will be considered located through Econ-Lit, REPEC, SSRN, NBER, Social Science Citation Index, Current Contents, MedLine, PsychoInfo.

The remaining of the paper is structured as follows: section 2 offers an overview of Clean Indoor Air Laws in industrialized countries. The impact on per-capita cigarette consumption and on prevalence of smoking is assessed in section 3 distinguishing between studies based on survey data, on time series and on longitudinal data. Section 4 explores the literature on the economic impact on the hospitality industry. Section 5 tries to assess the methodological differences that may explain the variability in the reported results. Section 6 provides a discussion of the main findings of the paper and concludes.

2 Overview of Clean Indoor Air Laws

2.1 The spread of Clean Indoor Air Laws in the United States

A number of reviews has already analyzed the spread of Clean Indoor Air Laws in the US and their economic impact (Eriksen and Chaloupka, 2007; Hopkins *et al.*, 2001; Koh *et al.*, 2007; US Department of Health and Human Services, 2006; Levy and Friend, 2003; among others). Eriksen and Chaloupka (2007) report that the earliest of these policies was the 1973 law in Arizona that limited smoking in a number of public places. This was followed by a Connecticut law restricting smoking in restaurants (1974) and by a Minnesota law that included restrictions on smoking in private workplaces (1975). California was the first to introduce, in the early 1980ies, local clean indoor air ordinances in San Francisco, Los Angeles, Sacramento, San Diego. Following California, the focus on local municipalities spread throughout the nation. In the years following the release of the 1986 Surgeon General's Report new federal regulations were adopted banning smoking on domestic flights of two hours or fewer and on virtually all domestic flights departing from or arriving in the US. State and local governments strengthened existing policies and adopted new policies, including complete bans on smoking in some public venues. At the same time, numerous private companies adopted policies governing smoking in their workplaces. In 1998 California banned smoking in bars without separately ventilated smoking areas. By August 2001, 234 US communities had enacted local ordinances that required all workplaces to be completely smoke-free. In 2002 New York City banned smoking in bars, restaurants and virtually all other workplaces. More recently, the 2006

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Surgeon General's Report led a growing number of states and communities to adopt comprehensive bans on cigarette smoking in public places and private work-sites. In some places, these policies have included some outdoor spaces like public parks and beaches.

2.2 European Clean Indoor Air Laws

With the exception of Finland, European Clean Indoor Air Laws are relatively more recent. This explains why most studies on the economic impact of cigarette bans are US based case studies.

The Finnish Tobacco Control Act (TCA) was first implemented in 1976 and successively amended in 1995, 2000, 2003, 2007. Since June 2007 smoking is prohibited at workplaces in general with the exception of separately ventilated smoking rooms or individual offices and has been totally banned in almost all restaurants and bars, with a deferment until June 2009 for the few restaurants having a smoking section with a separate ventilation system. In March 2004, the Republic of Ireland implemented comprehensive smoke-free legislation in all workplaces, including restaurants and pubs, with no allowance for designated smoking rooms, and few exceptions. By 2006, a number of jurisdictions had implemented or passed legislation to implement similarly strict, 100% smoke-free laws: Norway, New Zealand, Buthan, Uruguay; 14 US States (including the district of Columbia); nine Canadian provinces and territories; seven Australian states and territories as well as Scotland. Norway banned smoking after Ireland, followed shortly by New Zealand on December 10, 2004. Italy introduced a smoking ban in enclosed public places on January 10, 2005. Estonia had smoking banned on June 5, 2007 in all facilities that serve food, including bars and nightclubs. Bar owners were allowed to provide special rooms for smoking without food or beverage service. Each Nation of the United Kingdom implemented a similar ban: Scotland on March 26, 2006; Wales on 02 April 2007; Northern Ireland on 30 April 2007 and England on 1 July 2007. In January 2008 France extended the already existing ban to cover bars and cafs. Denmark banned smoking in clubs and restaurants on 15 August 2007, although the legislation made exemptions for small bars and restaurants with separate smoking rooms. Sweden established a similar ban on July 1, 2005. The Netherlands and Romania banned smoking in bars and clubs on July 1, 2008. Spain has a law, introduced by the Spanish Socialist Workers' Party, which came into force at the start of 2006 and bans smoking in workplaces. It has some restrictions for public spaces, such as airports and train stations, but pubs, restaurants and other public places smaller than 100 m^2 are exempted and it is left to the owner the decision to allow smoking or not. Switzerland introduced the smoking ban in public buildings, bars and restaurants in the Canton of Geneva on July 1, 2008, but the decision was nullified by the country's supreme court on 29 September 2008. In November 2008 the Bulgarian government decided to introduce a ban on smoking in all indoor public places including offices, bars, restaurants and clubs. The ban is due to come into effect on June 1, 2010. On December 9, 2008 the Greek government ratified legislation on a nationwide

ban on smoking in enclosed working and public places, which will come into effect in July 2009. As to Germany, since September 2007, smoking is prohibited in all federal government office buildings. This includes courthouses, the German Parliament and other federal buildings. Smoking is banned on public transportation as well. Smoking in train stations is allowed only in designated smoking areas. Fines for violations range from 5 to 1,000 Euros. From August 2007 to July 2008 all 16 federal states introduced smoking bans[1]. Most of them allowed smoking rooms as an exception. Bavaria has introduced the strictest curbs, enforcing a ban in beer tents as well as pubs and restaurants. Many states won't enforce fines for breaking the rules until a few months have passed. Eight states, Berlin, Bavaria, Brandenburg, Bremen, Hamburg, Mecklenburg Western Pomerania, Saxony-Anhalt and Schleswig-Holstein have banned smoking in public places since January 2008.

2.3   Types of Anti-Smoking Regulations

There are at least two distinctions that should be made in classifying studies analyzing the impact of Clean Indoor Air Laws. The first one is related to the extensiveness of their coverage, i.e. between local versus state laws. In general, local smoking ordinances are more comprehensive than statewide anti-smoking laws and have more enforcement power. Moreover, the adoption of local laws may generate stronger community support than larger scale efforts thereby increasing compliance and, as a consequence, increasing the impact on smoking behavior.[2] Thus, we expect to find stronger negative correlations between local anti-smoking bans and cigarette smoking in comparison with state-wide smoking bans, for given levels of compliance and enforcement. Very few studies actually control for compliance or enforcement levels[3] which could make it more difficult to assess the causal relationship between smoking regulation and the variation in smoking participation and consumption. Gallet (2007) and Carpenter (2007), however,find that enforcement and penalties matter little and Clean Indoor Air Laws appear to be self-enforcing.

A second important distinction is between smoking bans in public venues and smoking bans in workplaces. Clean Indoor Air Laws may restrict smoking in public and private workplaces, or in restaurants, grocery stores, health facilities and other public places. As emphasized earlier, workplaces bans are likely to have a much greater impact on cigarette smoking due to the length of time they affect smokers: while public place laws are likely to affect the average smoker for little more than a few hours a week, the private place

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[1]This led many small bars' owners to appeal against the Law to the Constitutional Court. The Court ruled that most smoking bans were unconstitutional because they disproportionately hurt small bars and gave options to revise the Law.

[2]The interested reader may refer to Fichtenberg and Glantz (2002) for a coverage of studies dealing with local legislation.

[3]Carpenter (2007) is an exception; Gallet (2007) also controls for enforcement.

laws can regulate the smoker's behavior for 40 or more hours each week (Chaloupka and Saffer, 1992). Therefore workplace smoking bans are likely to produce a stronger effect in comparison with smoking restrictions in recreational sites. Fichtenberg and Glantz (2002) suggest that there are no systematic differences between in the results obtained in the workplace and population based studies, but they presumably refer to the sign of the effect of anti-smoking regulation and not to its dimension. Buddelmeyer and Wilkins (2005), on the other hand, stress that work-site studies examine samples drawn from a subset of the community which may be affected by selection bias due to attrition. As a consequence, work-site based studies are likely to overstate the effect of smoking regulation. So the expected impact of workplace smoking bans is not clear cut. A way out of this problem could be to control for attrition in the data and to also control for the level of compliance and enforcement of the laws.

The way anti smoking regulation is measured may change the impact of these laws on cigarette smoking. Studies either individually categorize different Clean Indoor Air policies or combine the laws into a single index. In the latter case, an index of restrictiveness is used based on a classification scheme proposed by the Surgeon General. Typically this index is calibrated on a $0 - 1$ scale, in which 0 indicates no restriction, 0.25 represents basic restrictions (e.g. one to three designated smoking areas locations), 0.5 represent nominal restrictions (e.g. four or more designated areas excluding private worksites and restaurants), 0.75 represents a moderate policy (e.g. restaurants, but no private worksite restrictions) (Levy and Friend, 2003, p.602). The first study to adopt such restrictiveness index was Wasserman *et al.*(1991). Yurekli and Zhang (2000) use a modified restrictiveness index in which worksite laws and laws that have been into effect longer are more heavily weighted.

3 Impact of Clean Indoor Air Laws on Smoking Participation and Consumption

3.1 Studies based on Macro (aggregated) data

Early work on the impact of Clean Indoor Air Laws on Tobacco consumption is mainly based on aggregate annual time series at the state level (on per-capita measures of smoking: e.g. total sales divided by the level of population) and usually considered state level anti-smoking regulations. Such studies have usually found that smoking restrictions are effective in reducing the demand for cigarettes: Chaloupka and Saffer (1992); Keeler, Hu, Barnett and Manning (1993); Wasserman, Manning, Newhouse and Winkler (1991); Lanoie and Leclair (1998); Yurekli and Zhang (2000) and, more recently, Gallet (2007). A summary of these studies is shown in table 1.

While aggregate data have the advantage of being easily available in most countries, they do not allow to take into account the limited nature of the dependent variables and,

therefore, to analyze individual behaviors such as smoking consumption and the effect of smoking bans on the probability of quitting smoking (participation). Estimation of aggregate demand equations is subject to problems of simultaneous-equations bias, because the price may be jointly determined by demand and supply. Thus instrumental variable approaches are typically used to account for the likely endogeneity of cigarette prices[4]. Another potential problem in using aggregate data is that the Clean Indoor Air Laws may be endogenous: the presence of Clean Indoor Air Laws is reasonably expected to be more closely related to average statewide cigarette consumption than to smoking by a given individual in the state. Thus, the laws may be endogenous at the aggregate level creating an upward bias in the coefficient measuring the impact of the law on cigarette smoking. Finally, most time-series cross-sectional studies suffer from serial correlation of the error terms and heteroskedasticity, which should be accounted for by using an appropriate correction.

Chaloupka and Saffer (1992) use a time series of cross sections of the 50 states of the United States and Washington, D.C., from 1975 through 1985, to estimate models of cigarette demand. This is the first econometric study trying to determine the effect of these restrictions on cigarette smoking. Two different types of laws are examined with respect to the increasing degree of restrictiveness: a public place Clean Indoor Air Law and a private place Clean Indoor Air Law restricting smoking at work-sites as well as in public places. To determine the impact of such laws on cigarette demand, single equation models and simultaneous equations models are estimated to address the hypothesis that these laws are exogenous and endogenous. The empirical model is derived from a theoretical one considering an individual maximizing utility, subject to an appropriate budget constraint. Constrained optimization of this utility function yields a demand for cigarettes as a function of the price of cigarette, the price of other goods, income and taste. The empirical demand equation for cigarettes includes among the explanatory variables: Clean Air Laws, the relative price of cigarettes, income and a variety of control variables such as: short distance exports and imports capturing casual smuggling, long distance smuggling, per-capita tobacco production as a determinant of the pressure to pass Clean Air Laws, a proxy for religious fundamentalism, the percentage of voters and two variables measuring stress: the state unemployment and divorce rate.

Previous literature had emphasized that states where smoking is less prevalent are more likely to pass Clean Air Laws. The result would be that OLS estimation of the cigarette demand equation may be biased by the potential endogeneity of the Clean Air Law. Ignoring the problem of endogeneity of the smoking restrictions results in overstating the impact on cigarette sales of both public and private place Clean Indoor Air Laws. A Hausman-Wu test can be used to test this endogeneity assumption. Following this procedure Chaloupka and Saffer (1992) show that the laws could not be treated as exogenous and that cigarette sales have a significant causal effect on the passage of a

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[4]This problem does not apply to countries in which the price of cigarettes is a monopoly price.

private place Clean Indoor Air Law, implying that the passage of such a law is, in part, the result of anti-smoking sentiment. Using a simultaneous equations approach that takes into account endogeneity, it is shown that public place Clean Indoor Laws are found to reduce significantly cigarette demand whereas states that enacted the private place law did not further reduce demand (the coefficient associated to the private place law is not statistically significant). The coefficients associated to the impact of public place and private place laws in the simultaneous equations model are, respectively, -0.028 (-7.38) and 0.003 (0.96). This result does not imply that the level of smoking would not be reduced if a private place Clean Indoor Law were passed nationally. Instead, it suggest that only states with low levels of smoking due to strong anti-smoking sentiment have been able to pass a private work place Clean Indoor Air Law, while states where smoking is prevalent exhibit more pressure not to pass a restrictive law (Chaloupka and Saffer, 1992, p. 83).

Keeler et $al.$(1993) analyze the effect of anti-smoking regulations on consumption of cigarettes in California over the period 1980 through 1990 using aggregate sales of cigarettes at the state level as measure of cigarettes consumption. They specify a single equation model consistent with the theory of Rational Addiction (Becker and Murphy, 1988) and estimate it using full information maximum likelihood. The authors account for seasonal variation in cigarettes wholesales and the likely endogeneity of cigarette prices in the model. Regarding the effects of anti-smoking ordinances, they are quite significant and in the expected direction (-0.0019). However when a time trend is included in the model, capturing long run changes in smoking tastes, the effect of anti-smoking regulation is not significant. The authors claim this result might be due to the time trend correlation with the regulatory variable, i.e. to the fact that the trend captures, in fact, long run trends inherent in regulation, price and addiction variations.

A similar analysis is carried out by Sung et $al.$(1994) on pooled cross sections from 1967 to 1990 on 11 western states of the American federation. A system of two recursive equations for cigarettes demand, consistent with rational addiction, and for oligopoly price is estimated using Generalised Least Squares to control for serial correlation and heteroskedasticity in the error terms and including State and year dummy variables in the price equation to control for omitted variables bias. The variable accounting for anti-smoking regulation is significant and in the expected direction (-0.049).

The recent work of Gallet (2007) uses a new rating system of Clean Indoor Air Laws accounting for the severity of state smoking restrictions as well as for the strength of enforcement and penalties for violation. The paper investigates the impact of Clean Indoor Air Laws on the demand for cigarettes using state-level annual time series for 48 contiguous American states over the 1993-1998 period and estimating a single equation static model using a fixed effects two-stage least squares procedure (FE2SLS) to control for endogeneity of the cigarette price. The author introduces an aggregate measure of the overall smoking restriction for each state i in each year t and he also adds two additional variables accounting for two particular components of the regulation: the severity

of the restriction in each state and the strength of the state enforcement of the laws in each state[5]. The results are consistent with other studies and find that greater restrictions reduce cigarette demand: the coefficient of the smoking restrictions is negative and in the range -0.291 and -0.336. Enforcement and penalties appear to have little impact on cigarette demand lending credence to the idea that the social stigma associated to smoking makes Clean Indoor Air Laws self-enforcing.

Lanoie and Leclair (1998) investigate the relative ability of taxes and regulation in inducing reductions in cigarette consumption using data on ten Canadian provinces for the period 1980-1995 and estimating a single equation model of partial adjustment in cigarette consumption, implying habit formation. When considering the proportion of smokers in the population as the dependent variable, they find that smoking bans in Canada significantly reduce smoking participation (-0.038). However, when per-capita consumption of cigarettes is the dependent variable, anti-smoking regulation is not significant. So it appears that smoking-bans have an effect on the participation decision, i.e. on the decision to smoke or not, but not on quantity of cigarettes smoked. An interpretation of this interesting result is that the negative impact of smoking bans on the smoking participation decision carries the idea that smoking is socially unacceptable.

Yurekly and Zhang (2000) examine the impact of Clean Indoor Air Laws and of smuggling activities on per-capita cigarette consumption, measured as sales of packs of cigarettes, and revenues using a static demand model and data for 50 American states including the District of Columbia over the period 1970-1995. GLS is used in this study to control for serial correlation and heteroskedasticity in the error terms and cigarette excise taxes are used as instrumental variables to control for the potential endogeneity of cigarette prices. The Clean Indoor Air Laws measure has a negative impact on per-capita consumption of cigarettes in all specifications used. The reduction in cigarette consumption from anti-smoking regulation is significant and increases over the period 1985-1995 as the laws have become more restrictive and comprehensive (-1.64).

3.2 Studies based on individual data

3.2.1 Studies based on population-wide surveys

There are many advantages in using individual data rather than aggregated per-capita data. The first one is that they allow the researcher to take into account the limited dependent variable nature of smoking behavior and thus to separate the participation decision from the decision of how many cigarettes to smoke. Given the limited nature of the dependent variables, OLS techniques are inappropriate and different methods based on

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[5]The strength of state enforcement is measured by the presence of an enforcement authority that conducts compliance inspections and the posting of signs forbidding smoking. Strength of penalties is measured by the presence of fines that rise with the severity of the infraction.

discrete choice models are typically used with this type of data. The second advantage is that micro data allow to control for individual heterogeneity affecting smoking behavior. Finally, including Clean Indoor Air Laws indices in cigarette demand equations may present a problem of endogeneity when estimating such equations using aggregate data. This should not be a problem when estimating cigarette demand equations using individual data. The presence of Clean Indoor Air Laws is expected to be more closely related to average statewide cigarette consumption than to smoking by a given individual in the state, because any individual's decision to smoke will negligibly influence the willingness of legislators to enact laws restricting smoking in public places (Wasserman *et al.*, 1991). Thus, the laws may be endogenous at the aggregate level, but are likely to be exogenous at the individual level (Chaloupka, 1992; Chaloupka and Saffer, 1992).

The first group of studies selected in table 2 uses large survey data to conduct their analysis. The advantage of using large nationally representative surveys is that the large sample size usually allows to explore the differential effects that anti-smoking policies have on demographic subgroups of interest. There are also important drawbacks arising from using cross-sections from large surveys. The first one is that there is strong evidence that cigarette smoking is underreported in survey data. In most cases, no information is available on the extent of underreporting or on how underreporting varies with consumption. The usual way of coping with this problem is to consider a given degree of underreporting for each individual in the sample.

A more serious problem is that cross sectional studies may make it difficult to assess whether there is a causal relationship between smoking bans and smoking, because individuals are observed only at one point in time. This may hide omitted variables and/or self-selection bias. This type of problem may be addressed using panel data, when available. Longitudinal studies that analyze smoking prevalence before and after the imposition of bans mitigate some of these problems (Evans *et al.* (1999)).

Wasserman *et al.* (1991) estimate both a generalized linear model and a Double-Hurdle model to examine the impact of excise taxes and of regulations restricting smoking in public places on adult and teenage cigarette demand. They use individual data taken from the National Health Interview Survey from 1970 to 1985. They estimate both a Poisson-like specification of the cigarette demand equation, which they call "the generalized linear model" and a Two-part model (Cragg, 1971). Data on regulations restricting smoking in public places were collapsed into a discrete regulation index going from 1, for the most restrictive regulation, to 0 in case of no regulations in place. This index is shown to have a depressing effect on cigarette consumption across all specifications used of the generalized linear model. The percentage reduction in packs smoked due to an increase in the regulation index is 5.9%. When the two-part model is used to estimate adult participation and consumption, the regulation index has a statistically significant effect on the number of cigarettes smoked, but not on the decision to be a current smoker. An opposite effect is found for teenage smoking: the regulation index exerts a statistically significant

impact on the probability of being a smoker, but not on the conditional equation. Although regulations restricting smoking in public places may not affect adults' decisions to continue to smoke, but merely the number of cigarettes smoked, regulations appear to have an important influence on teenagers' decisions to start smoking. Tauras (2006) updates the work of Wasserman *et al.*(1991), using the same types of models and a large sample of 545,603 observations from the Current Population Survey (CPS) to estimate the impact of Clean Indoor Air Laws on smoking prevalence and intensity (measured as average monthly number of cigarettes smoked) in the United States. In line with what was previously found by Wasserman *et al.*(1991) Clean Indoor Air Laws do not have a significant impact on smoking prevalence, but they are strong predictors of smoking intensity. Simulations predicting average smoking, conditional on the coverage and strength of the regulation, show that an increase in the number and strength of smoking bans decreases the number of cigarettes smoked between 5.18% and 7.96%.

Chaloupka (1992) estimates cigarette demand equations for the United States, derived from the Becker and Murphy model of rational addiction, which include alternative measures of state level Clean Indoor Air Laws. Survey data taken from the Second National Health and Nutrition Examination Survey (NHANES2), conducted from 1976 to 1980 and reporting the average level of individual daily consumption are used, augmented with county level cigarette prices and excise taxes and state level measures of restrictions on smoking. Four levels of restrictiveness on smoking are identified and a set of dichotomous indicators for laws at each of these four levels are included to estimate the anticipated non-linear impact of increased restrictiveness on cigarette smoking. The results suggest that increasing the restrictiveness of these laws, beyond some basic level, does not appear to have a greater impact on cigarette consumption: the value of the coefficient associated to increasing restrictiveness of the regulation, for the full sample (table 2) goes from −0.265, for the basic level of regulation, to −0.159 for the strictest level. This last coefficient, however, is not statistically significant. The author suggests that this counter intuitive result implies that, beyond some point, the degree of restrictiveness of the law is not what is affecting cigarette demand, but there may be unobserved factors, such as publicity on the negative health consequences of cigarette smoking, which are driving the results. Demand equations estimated separately for men and women imply that the impact of the Clean Indoor Air laws is found primarily among men. Cigarette smoking among women is not affected by either the presence or the restrictiveness of the law.

Chaloupka and Wechsler (1997) use a nationally representative sample of 16,570 students at 140 US four-year colleges and universities taken from the individual-level survey data Harvard College Alcohol Study. Based on the location of the college/university, several additional variables were added to capture state and local restrictions on cigarette smoking. The restrictions on smoking are captured by a set of five dichotomous indicators reflecting state and/or local limits on smoking in workplaces, restaurants, retail stores, schools and any other public place. Given the potential multi-collinearity be-

tween the indicators[6], a restrictiveness index is also used which takes a minimum value of zero for sites that do not restrict smoking and a maximum value of four for sites with very comprehensive restrictions against smoking in a number of public places, including restaurants, as well as in private worksites. The authors use two alternative approaches. Since their dependent variable is the ordered level of cigarette consumption constructed from categorical data in the survey, ordered probit models are employed to estimate smoking participation decisions. Then, a two part model of cigarette demand (Cragg, 1971) is estimated. Here, Probit regressions are used to estimate smoking participation in the first step and least squares methods are used to estimate average daily cigarette consumption by smokers, where the dependent variable is the natural logarithm of the continuous average daily consumption measure. Restricting smoking in public places and private workplaces has a mixed impact on cigarette smoking. The restrictiveness indicator is not significant in any of the equations estimated. When disaggregating the index into indicators for different types of restrictions, however, the indicator for restrictions on smoking in restaurants is negative and significant in most participation equations. The same is true for restrictions on smoking in schools. The indicator for other restrictions on smoking in public places is insignificant in the smoking participation equations, but is negative and significant in the quantity of cigarettes smoked.

Evans *et al.*(1999) and Farrelly *et al.*(1999) pool the data from the 1991 and 1993 National Health Interview Survey to obtain a sample of 18,090 workers to be used to examine the impact of workplace smoking restrictions on smoking behavior. They first estimate single equation models, where workplace smoking bans are assumed to be exogenous and then address the issue that these single equation estimates may be subject to an omitted variable bias. To control for problems of omitted variables bias they adopt three different approaches. First, they include a number of variables that may signal the underlying healthiness of the individual or firm and show that the estimated impact does not change. Second, they show that the impact of workplace smoking bans is greatest for workers with the longest work weeks. Third, they use two-stage least squares and instrumental variables to control for the potential endogeneity of the smoking ban, i.e. a smoking ban is more likely to be introduced in firms with a larger proportion of non smokers. Once omitted variable bias is controlled for, they find that a complete smoking ban in all work areas has a slightly larger impact on the prevalence of smoking than in their previous set of estimates, in which bans are treated as exogenous. The result suggest that, in this case, the omitted variable bias does not dramatically alter the conclusion from the single equation estimation and that workplace smoking bans are an effective way to reduce smoking among adults.

Carpenter (2007) provides quasi-experimental evidence of the impact of a number of

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[6]In presence of multi-collinearity the parameter estimates are unbiased, but their statistical significance, i.e. the precision with which they are estimated, is altered.

very strong local smoking bans in Ontario, Canada, over the period 1997-2004. The differential timing of adoption and the possibility to observe smoking behavior before and after the implementation of strong local smoking bans justifies the "quasi-experimental" nature of the work which makes the author more confident of capturing the causal relationship between regulations and smoking behavior. He uses a sample of 5,917 observations from repeated cross-sections over the period 1997-2004 supplied by CAMH Monitor (Ontario Adult Drug Monitor). These data provide information on individual outcomes both before and after the implementation of over 100 local smoking by-laws within Ontario. The author claims his study provides more robust evidence on the impact of local smoking bans in comparison with previous literature in that: 1) he only considers tough 100% smoke-free smoking bans; 2) considers only local laws; 3) explicitly accounts for county-levels fixed effects; 4) explicitly accounts for regulations compliance; 5) provides the first estimates of the effects of local bylaws and smoking bans distinguishing between blue collar and white collar workers. While points 1), 2) and 5) actually strengthen the results, accounting for county fixed effects is a rather standard way of modeling unobserved heterogeneity that is time invariant[7]. As regards as point 4) a previous study by Gallet (2007) also controls for enforcement and penalties finding no significant effect, in line with findings by Carpenter. Estimates for own smoking behavior confirm that workplace smoking bans reduce own smoking and local by-laws reduce the likelihood of reporting daily second hand smoke exposure by about 30%. However these results are entirely driven by blue collar workers. Non-blue collar workers do not exhibit a substantive relationship between a local by-law and reported worksite smoking policies. On the other hand, the probability of blue-collar workers being smokers falls between 4% and 11.3% when the by-law is adopted.

A number of studies, mainly in health research, use logistic models to obtain estimates of the impact of anti-smoking regulation on smoking prevalence, i.e. on the probability to be a smoker or to quit smoking (Woodruff *et al.*(1993), Kinne *et al.*(1993), Stephens *et al.*(1997), Moskowitz *et al.* (2000)) and on both smoking prevalence and smoking intensity (Glasgow *et al.* (1997)). Health Policy studies seem to be less concerned about problems of omitted variable bias or more general problems of endogeneity that may bias the results and that are very common in this area of social policy. Thus, very often, methods that could be used to control for endogenously determined right hand variables, such as instrumental variables methods, are ignored. This strand of literature seems to pay less attention to the fact that complex theoretical and empirical modeling would be required to draw causal inferences regarding the effects of anti-smoking regulation on human behavior. Woodruff *et al.*(1993) examine the relationship between workplace smoking policies and smoking prevalence among 11,704 workers in California between 1990 and 1991. Logistic regressions were used to determine the likelihood of being a smoker under different workplace policies on smoking, taking into account demographic

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[7]Another Canadian study quoted above, Lenoie and Leclair (1998) also uses provincial fixed effects.

variables. Workers in workplaces with only work area restrictions were 15% more likely to be smokers than those who worked in a smoke-free workplace. Workers under lesser restrictions or no restrictions were about 30% more likely to be smokers than those in a smoke-free workplace. Glasgow *et al.* (1997) try to assess if more stringent smoking policies were associated with increased probability to quit smoking. They use a survey of 8,271 employed adult smokers respondents in 1988 and in 1993. Using Multiple Logit models, and after having controlled for a number of variables, they showed that employees who worked in a smoke-free worksite were over 25% more likely to make a serious attempt to quit smoking and over 25% more likely to achieve cessation than those who worked in a worksite that permitted smoking. Among continuing smokers, employees in smoke-free worksites consumed on average 2.8 cigarettes per day less than those who worked in places with a non-restrictive smoking policy. Stephens *et al.*(1997) use data on smoking status of 11,652 individuals, taken from the General Social Survey conducted by Statistics Canada in 1991 to analyze the impact of no-smoking bylaws on smoking prevalence in Canada. This national survey provides data on smoking prevalence for each of the 10 Canadian provinces so that the impact of no-smoking bylaws by province can be studied while controlling for relevant individual differences such as: age, sex, marital status, education and their interaction. A logistic model was used to estimate the variation in the probability of being a smoker. According to the study's results, the presence of no-smoking bylaws decreases the probability of being a smoker by 21%. Moskowitz *et al.*(2000) estimate the impact of local workplace smoking laws in California on prevalence of smoking. Workplace smoking ordinances data from 1990 were merged with 1990 California Tobacco Survey individual data from 4,680 adult indoor workers who were current cigarette smokers or reported smoking in the six months before the survey. The effect of such ordinances were estimated using a multiple logit model controlling for a number of socio-demographic variables. Results show that smokers living in communities with strong smoking ordinances were 38% more likely to quit smoking than those who worked in communities with no ordinances.

3.2.2   Longitudinal data

The crucial advantage of using Panel data to study the impact of anti-smoking regulation is that the same sample of individuals is followed before and after the introduction of the ban, so that the causal relationship between the adoption of a smoking ban and the effect on smoking behavior can be better assessed. Longitudinal studies that analyze smoking prevalence in establishments before and after the imposition of bans may mitigate some of the methodological problems that arise when using cross-sections. For example, longitudinal studies mitigate problems of self-selection bias (e.g. non smokers find work in smoke-free workplaces): if the match of smokers to firms is not random, the estimated impact of bans on smoking prevalence may be biased. Moreover, they are not subject to problems of endogeneity (firms with a lower-than-average prevalence of smoking may be more likely to ban smoking). With longitudinal data the results are driven by compari-

son of within-work site changes over time in the prevalence of smoking rather than by cross-sectional differences and therefore this type of problem is ruled out (Evans *et al.* (1999). Finally this type of data allow to control for time-invariant unobserved heterogeneity across firms or establishments.

A further advantage of Panel or longitudinal data is that they provide more information over individuals and allow to capture the dynamics in their behavior. This is important to study both the effect on consumption and the effect on smoking rates of smoking bans. A dynamic specification may allow to model addiction, i.e. to study the effect of past consumption of cigarettes on their current consumption levels. In addition, longitudinal data allow focusing on transitions in smoking status and, in particular, on the effects of smoking bans on these transitions.

Unfortunately there is scarce international research using nationally representative longitudinal data to study the economic impact of smoking bans. Buddelmeyer and Wilkins (2005) investigate the role of smoking ban regulations in public venues across a number of Australian states on individual level smoking patterns. They examine the determinants of starting and quitting smoking using a sample of Australians interviewed annually over the period 2001 to 2003. This data source allows to model transitions in individuals' smoking status of all individuals in the Australian community. To investigate the effect of smoking bans, the authors exploit a natural experiment provided by the variation across the states and territories of Australia in the timing and nature of regulations introduced over the period spanned by the data. The methodology used is borrowed from Jenkins and Cappellari (2004). The transition from one smoking status to the other is modeled using a trivariate probit structure in which the first equation models the propensity to smoke in the base year; the second equation models the propensity to remain in the sample from one period to the next and the third equation models the latent propensity to smoke in the following period. The results show that restricting smoking in public places does increase quitting probabilities and reduces starting probabilities. The mean marginal effect of introducing tougher smoking legislation is a reduction in the smoking probability of the reference group (a non drinking, non-socialising person aged 60 years or over) by 7%.

Bauer *et al.*(2005) used data from the Community Intervention Trial for Smoking Cessation (COMMIT), a large, population-based prospective cohort of smokers funded by the National Cancer Institute from 1988 to 1993, to examine the effects of worksite smoking policies on 20 US communities and 2 Canadian communities. COMMIT's participants were followed up in 2001 to examine how changes in worksite policies influenced smoking behaviors. The empirical investigation focuses on 1967 participants who indicated that they smoked and were employed in both the 1993 and 2001 survey waves. Smoking status, cigarette smoked per day, serious quit attempts made between 1993 and 2001, and use of smokeless tobacco were used as dependent variables, whereas the primary independent variables of interest was reported changes in worksite smoking policies between

1993 and 2001. Standard logistic and multiple regression models were used. The control variables included in the analysis were gender, race/ethnicity, age in 2001, education in 2001, annual household income in 2001, type of occupation in 2001, amount smoked in 1993, desire to quit in 1988 and number of previous quit attempts in 1993. Results show that the more restrictive is the smoking policy, the greater is the likelihood that individuals were successful in quitting smoking or in lowering their daily cigarette consumption if they continued to smoke. People whose workplaces restricted smoking were 1.9 times more likely to have quit smoking by 2001 than people whose workplaces did not restrict smoking. Those who worked in smoke free environments but continued to smoke reported reducing their average daily consumption by about 2.5 cigarettes per day.

3.3   European studies

To date, there are few European studies trying to investigate the impact of smoking bans on smoking prevalence and intensity. All of them use cross-sectional data (except for Fong *et al.*(2006)), which are exposed to the limitations listed before.

The relationship between prohibition of smoking at the workplace and smoking cessation has been assessed in Germany using a national sample survey conducted in 1987 (Brenner *et al.* (1992)). A sample of 439 individuals was selected and logit regression was carried out. Results show that prohibition of smoking at the workplace was associated with a very large reduction of smoking intensity among women (OR=0.22) and, to a lesser degree, also among men (OR=0.80).

Fong *et al.*(2006) use a national cohort study of 1,679 adult smokers in Ireland and in the UK to conduct a quasi-experimental study on the effect of the Irish smoke-free workplace law in introduced in 2004. Parallel cohort telephone surveys of nationally representative samples of adult smokers in Ireland (n=769) and in the UK (n=416) were conducted before the law and 8-9 months after the implementation of the law. Using generalised linear models the authors find that the Irish law led to a dramatic decline in reported smoking in all venues, including workplaces (from 62% to 14%), restaurants (from 85% to 3%), and bars/pubs (from 98% to 5%).

Helakorpi *et al.* (2007) examined changes in adult daily smoking in 1981-2005 in Finland, in order to evaluate the impact of the 1995 Tobacco Control Act Amendment (TCAA) on the proportion of daily smokers. The study is based on a sample of 73,471 data from a postal survey on adults in 1981-2005. Logistic models were used to test the effect of the 1995 smoking ban on prevalence of smoking. The OR for daily smoking after 1995 was 0.83 for employed men and 0.78 for employed women compared with OR equal to 1.00 in 1994. A study based on the same investigation period, 1994-1996, has been carried out by Heloma *et al.* (2001) on a repeated cross-sectional design involving 967 employees, in the first wave of the survey (1994-1995), and 1035 in the second wave

(1995-1996) in the area of Helsinki to assess the impact of the 1995 TCAA on smoking prevalence. A comparison of the distributions of smoking prevalence in the two waves has shown that the average number of cigarettes smoked, among men and women, has decreased by 16% from 1994 to 1996.

Gallus *et al.* (2006) considered a survey of 3,114 adults carried out in Italy in 2004 and 2005 to investigate the impact of the Clean Indoor Air Law introduced in Italy in 2005. A comparison of the distributions of smoking prevalence and smoking intensity in 2004 and in 2005 revealed a decrease in smoking prevalence by 2.3% and a decrease in the mean number of cigarettes smoked per day by 5.5%.

3.4   Summary of results

A summary of the studies discussed in this section is presented in table 1 and 2. There is wide evidence that Clean Indoor Air Laws reduce smoking intensity, whereas evidence is mixed as regards as smoking prevalence. Among smoking bans, worksite smoking policies are more effective at reducing smoking among workers. A number of methodological points are worth noting. First, in analyzing the impact of smoking bans, local laws are assumed to have more enforcement power than state-wide laws. In order to test this issue, measures of compliance and enforcement are needed. Only a few studies have included such measures (Gallet (2007) and Carpenter (2007)) showing that, in fact, compliance and enforcement measures matter little. This suggests that Clean Indoor Air Laws can be thought of as self-enforcing. Second, worksite smoking bans are likely to have a larger impact than public places smoking bans. However, worksite studies may be affected by selection bias due to attrition. The level of restrictiveness of the law seems to be important in driving the impact of regulation. Third, individual (micro) data provide many advantages over aggregated data: i) they usually allow to include a full set of demographic controls that may affect individual behavior, thus reducing the risk of omitted variable bias; ii) problems of endogeneity of the smoking regulation are ruled out when using individual data; iii) the participation decision may be separated from the consumption decision. The limited nature of the dependent variable requires the use of estimation methods different from ordinary least squares. Fourth, among individual data sets, large Panel data or repeated cross-sections are best suited to assess the impact of anti smoking regulation on smoking behavior. Since longitudinal data analyze smoking prevalence before and after the imposition of the bans, problems of omitted variable bias (differences in the prevalence of smoking across workplaces may be due to factors other than smoking policies; firms may have adopted programs that promote health among workers; firms with a high fraction of non-smokers may have promoted the adoption of workplace smoking bans;) and of self-selection bias (nonsmokers find work in smoke-free workplaces) are mitigated, thus allowing to better assess the causal relationship between regulation and smoking behavior.

4 The Impact of Clean Indoor Air Laws on the Hospitality Industry

A more recent strand of literature investigates the effects of anti-smoking regulation on the hospitality industry. Eriksen and Chaloupka (2007) emphasize that, as the evidence on the negative health consequences of exposure to tobacco smoke increased, arguments against smoke-free air policies concentrated on their economic impact on the hospitality industry. Scollo *et al.* (2003) mention that in California in 1987 a 100% smoke-free restaurant ordinance in Beverly Hills was rolled back, partly in response to claims that the ordinance was responsible for reducing restaurant revenues.

Studies investigating this indirect effect of anti-smoking regulation can be divided into those using objective data on economic outcomes such as: sales, employment, revenues, taxes on revenues, number of licensed establishments; and studies using survey data and subjective measures about the owners' perceptions of the impact of smoke-free air policies on their businesses, self-reported measures of business revenues, individual dining and drinking out patterns and/or expected changes in these behaviors in response to a smoke-free air policy and individual preferences for smoke free dining/drinking. The conclusions of the vast majority of these papers, both based on objective and on subjective measures of outcomes, are consistent in finding no negative economic impact of clean indoor air policies on the hospitality sector. Pakko (2006a), however, calls for caution in evaluating the findings of such studies, because a number of methodological problems may affect the results. Scollo and Lal (2002 and 2008) in their comprehensive review of studies assessing the impact of smoke free regulation on the hospitality industry, estimated that the probability of finding a negative economic impact in works based on subjective data is four times higher than in studies based on objective measures. This may be due to a "negative placebo effect" (Glantz, 2007). This term refers to a well known pitfall in clinical trials according to which patients report feeling better after receiving a treatment even though the treatment had no real benefit. In the context of anti-smoking regulation, the negative placebo effect refers to the impact of claims by the hospitality and the Tobacco industry about the negative economic effect of anti-smoking regulation on the *perception* of people in the hospitality industry. This negative placebo effect is also most likely to occur soon after the law is introduced when contrasting forces work to create the impression that the law is creating economic turmoil.

Many studies assessing the economic impact of anti-smoking regulation on the hospitality industry are natural experiments in which data on outcomes are collected before and after the implementation of the policy, along with comparable data from other jurisdictions where there was no policy change, as a control group. From a methodological standpoint it is crucial to take into account the volatility of the hospitality industry's performance, by including appropriate controls for factors, different from anti-smoking reg-

ulation, that may have an impact on the business activity. Neglecting such factors causes omitted variable bias in the findings which will therefore be misleading (Pakko, 2006a). Among these, seasonality plays an important role in the hospitality industry performance and should appropriately be accounted for. In addition, all data points after the law was implemented and several years before should be included in the analysis and regression methods controlling for secular trends and random fluctuations in the data should be employed.

4.1   Studies based on Natural Experiments

Natural experiments have gained considerable popularity in recent years due to, among other things, the simplicity of the estimation method. This method is known as the difference-in-difference estimator. The statistical apparatus underlying this approach has been extensively used in labor economics and a clear summary of the pros and cons of this approach is contained in Blundell and Macurdy (1999). The basic idea is to compare at least two groups, one of which experienced a specific policy change, and another with similar characteristics whose behavior was unaffected by this policy change. The second group is assumed to mimic a control environment. The control environment is usually created by including exogenous variables in the analysis designed to adjust for relevant differences among sample observations. The impact of the policy intervention is measured as the net difference between the change in economic outcomes for the treatment and the control environment. Blundell and Macurdy (1999) emphasize that, contrary to the common perception, the natural experiment approach is entirely equivalent to the fixed effects model developed in the early 70ies in the panel data literature. They show that the fixed effects and the difference-in-difference estimators do not merely share the same asymptotic distribution, but they are computationally identical. Therefore the structural restrictions required for consistent estimation of a fixed effects model, with both time and individual effects, also hold for natural experiments. Blundell and Macurdy (1999) list two sets of structural restrictions that must be met in order to obtain consistent estimation of these models: i) time effects must be common in both the experimental and the control group, i.e. any difference in trend or cyclical effects among experimentals and controls must be ruled out[8]; ii) the composition of both the experimental and the control group must remain stable before and after the policy change, i.e. each of the two groups must be made up of the same individuals in both periods under investigation. If this is not the case, the coefficient associated to the policy change of interest can be biased, because differencing does not phase out all the unobserved individual effects that are correlated with the explanatory variables. Among the natural experiment studies surveyed the study of Adams and Cotti (2007), Adda *et al.*(2006) and Carpenter (2007) meet both sets of structural restrictions.

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[8]Blundell and Macurdy (1999) p. 1611, stress that trends and cycles differ for married and single people, by gender and for high and low skilled workers and therefore an appropriate specification must be chosen to meet the first set of structural assumptions.

The first of these studies, by Glantz and Smith (1994), focused on the effects of local smoke-free restaurant laws adopted between 1985 and 1992 in 15 California and Colorado communities. Multiple regression methods were used to assess the impact of those ordinances on taxable restaurant sales revenues as a share of total revenues before and after the implementation of smoke-free laws in those communities and in 15 comparable communities that did not implement such ordinances. The authors find no evidence of a negative economic impact on the restaurant business. In their follow up study (Glantz and Smith, 1997), the authors updated their analysis and included the impact of local smoke-free bar ordinances in 7 California sites that also had banned smoking in drinking establishments, using a comparable measure of revenues from businesses licensed to serve alcohol. Even in this case, the authors found no significant economic impact of the local ordinances on either restaurants or bars.

Hyland and Cummings (1999) use measures of employment in New York City's restaurants to assess the economic impact of the smoke-free restaurants ordinances in New York city in April 1995 comparing trends in the city to those in neighboring countries and the rest of the state. They conclude that the policy measure did not result in the job losses opponents had argued would occur. They found that between 1993 and 1997 there was an 18% rise in restaurant employment in New York City compared with a 5% increase in the rest of the state. Similar evidence, using a similar approach, was found in a number of studies referred to nearby counties and published in a special issue of the Journal of Public Health Management and Practice in January 1999.

Hyland *et al.*(2009) investigate how Scotland smoke-free law has impacted on self-reported secondhand smoke exposure in hospitality venues, workplaces and people's homes. They use a quasi-experimental approach using a telephone survey of nationally representative samples of smokers and non-smokers interviewed before the Scottish law (1335 smokers of which 507 in Scotland and 828 in the rest of the UK and 601 non-smokers of which 301 in Scotland and 300 in the rest of the UK) and one year after the law in Scotland (705 smokers and 417 non-smokers) and the rest of the UK (1027 smokers and 447 non-smokers). Second hand smoking exposure was dramatically reduced in Scottish pubs, restaurants and workplaces following their nationwide smokefree regulations and there is no evidence of an economic downturn in Scotland or displacement of smoking from pubs to the home following the smokefree law.

Adda *et al.*(2006) collected a sample of 2724 observations, via telephone interviews, on the sales and number of customers in public houses in Scotland (1590 obs., intervention group) and in Northern England (1134., control group) before and after the introduction of the Scottish smoking ban in March 2006. This is the first European study investigating the economic effect of smoking bans on the hospitality business. The authors use a quasi-experimental approach comparing what happened to pub level outcomes

in a treatment area (Scotland) with an other wise comparable control area (Northern England) before and after the ban was introduced. Linear regression models were used in which three dummies were included, among the explanatory variables, to capture the effect of the location of the pubs before the ban (whether they are located in Scotland or not and observed before the ban was introduced); the effect of the smoking ban (whether the observation refers to a time period before or after the ban) and the effect of the ban on Scottish pubs (whether a pub is located in Scotland and observed after the ban was introduced). The first of these indicators captures permanent differences between Scottish and English pubs. The second dummy captures aggregate changes in sales and number of customers between the two waves that are common to Scotland and England. Finally, the coefficient on the indicator variable for Scottish pubs after the ban captures the causal impact of the smoking ban. Controls were added to account for location (rural versus urban) and size (number of employees at baseline) of the pubs. Finally, they used only the pubs which were surveyed twice and estimated a fixed effects model capturing any time invariant unobserved pub characteristics which may confound the relationship between the outcomes of interest and the smoking ban. The effect of the smoking ban produces a fall in pubs' sales by about 9.7%. This gap arises as the result of a 5.5% growth in sales in Northern England compared with a 4.2% fall in Scotland.

Previous studies, focussed on bans in the United States, had mostly found no negative effect of such measures, whereas this study find a significant and large negative effect. Glantz (2007) observes that these findings might be biased by a negative placebo effect which may occur when using subjective measures of outcomes, such as those obtained when using telephone survey. He also stresses that, once enough time has passed to get hard data, the initial reports of adverse economic effects prove to have been wrong. One way to control for the presence of a placebo effect would be to have data following individuals' behavior for a long time after the introduction of the ban. Adda and his coauthors, however, stress that they have tried to minimize the placebo effect by collecting the data before and after the survey and by relegating any direct question involving the owners' view about the ban towards the end of the survey.

Adams and Cotti (2007) perform the first difference-in-difference study for the US of the impact of anti-smoking regulation on the employment level of bars and restaurants. Their primary finding is that smoking bans reduce employment in bars. On the other hand they have a neutral or mildly positive effect on employment in restaurants. As the prevalence of smokers in a region increases, negative effects of legislation become more pronounced, especially for bars. Positive restaurant employment effects appear to occur where the option of outside seating for potential smokers is most attractive, i.e. in areas with warmer climates all year round. In this study, only 100% smoking bans are considered, and a sample of 16,248 bars and 39,565 restaurants is considered. The data are supplied by the Bureau of Labour Statistics (BLS) Quarterly Census on Employment and Wages (QCEW) on county employment levels, across the US, from 2001 to 2004, across

drinking establishments and full-service restaurants. The availability of large highly dis-aggregated panel data, allows the authors to identify a treatment and control group and to estimate a fixed effects model. Moreover, they identify effects from the passage of laws that came after the first wave of smoking bans. This is an advantage, because it avoids problems of sample-selection bias. They also observe whether effects deviate by climate. Although bar effects do not differ by the climate of a region, the authors find a positive employment effect on restaurants where the option of outdoor seating for potential smokers is most attractive (areas with warmer climates all year round and areas with cooler climates during the warmest months of the year). Restaurants are negatively affected during harsher periods in colder areas, suggesting that the presence of outdoor seating allows an option for smokers while providing non-smokers with an inside option.

4.2 Other types of Studies

Alamar and Glantz (2004), using a database that records the purchase price of restaurants that are sold, estimate the value added to a restaurant by a smoke free policy using weighted least squares (to control for heteroskedasticity) regression of the purchase price of restaurants as a function of the presence of a smoke-free law and a number of control variables. Data on the sale of restaurants and bars in the US were gathered from the BizComps database and included a sample of 726 transactions over the period 1991 to 2002. For each of the transactions observed the authors determined which business was covered by a local 100% smoke-free restaurant ordinance. After controlling for relevant economic variables, the authors observed a median increase of 16% in the sale price of a restaurant which, they claim, is directly attributable to the existence of a smoke-free law. In a similar study, published in 2007, the same authors used an updated version of the database to replicate the analysis on bars' sales alone, using 197 observations from 1993 to 2005, and finding that the smoke-free law was not statistically significant in explaining the variation in the transactions' price. They conclude that the existence of smoke-free bar laws had no detectable effect on the profitability of bars. Despite the authors' claim of evidence of causal link between restaurants and bars sales and smoke-free regulation, richer data could be used and a larger set of control variables included to measure a variety of potential and actual causal factors.

Binkin *et al.*(2007) try to document the effects of the 2005 smoking ban in all enclosed public places in Italy[9]. Four surveys were conducted between December 2004 and February 2006. Different samples were drawn in each of the surveys: 471 bars and restaurants in six local health units (LHU) were included in the pre-implementation survey; 1961 locales in 35 LHU were included in the first post-implementation survey; 1536 locals in 33 LHU were included in the second post-implementation survey and 1616 locales in 34

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[9]Article 4 of the Law n.3 of 16 January 2003, introducing a smoking ban in public places in Italy, makes explicit allowance for designated smoking rooms in those public places which are subject to the Law.

LHU were included in the last post-implementation survey. Descriptive statistics were used to compare differences between the pre and post implementation smoking status, intensity and economic outcomes of the locales. Before the ban, 24% of owners predicted major financial losses. One year later only 7% reported major financial losses. Most owners/managers (88%) reported positive attitudes about the law and 79% reported such attitudes among clients.

Cowling and Bond (2005) present evidence that smoke-free restaurant and bar laws, introduced in California in 1995 and in 1998 respectively, have actually increased revenues from both restaurants and bars. Their central finding is that, relative to trend, restaurant revenues increased following the introduction of the smoke-free restaurant law in 1995; likewise, bar revenues increased, relative to trend, after the smoke-free bar law was introduced in 1998. In both cases, the findings suggest that any substitution by smokers away from the locales affected was more than offset by the substitution by non-smokers towards the sale locales. Data used to develop the study were supplied by the California Board of equalization on quarterly taxable sales, for eating and drinking establishments, in California from 1990 to 2002. Both state level and county level (on 36 out of the 58 counties existing in California) were collected in order to perform both a state-level analysis and a county-level analysis. Linear regression methods were used.

Lund and Lund (2006) analyze descriptive statistics supplied by Statistics Norway to evaluate the impact of the absolute ban on smoking in all hospitality venues in 2004. According to such data, value added taxes (VAT) collected from the hospitality industry increased by 5% in the first sixteen months after the ban compared with the same period prior to the introduction, after controlling for seasonality in the data.

Pakko (2006a) uses a sample of 26 observations, spanning from the first quarter of 1998 through the second quarter of 2004, supplied by the Department of Health and Senior Services (DHSS), on taxable restaurants' sales receipts in Maryville, Missouri, to assess the impact on restaurants' sales of an ordinance, adopted in June 2003, that prohibited smoking in restaurants. He employs standard linear regression techniques (OLS) to test the hypothesis that the smoking ban had no significant effect on Maryville bar and restaurants sales. After controlling for a linear time trend, seasonality, economic factors and new restaurant openings, he finds no statistically significant effect of the smoking ban on restaurants' sales. The increase in sales, registered in the data near the end of the investigation period, is shown to be explained more by the opening of a new restaurant in town than by the implementation of the smoking ban so that any claims about the smoking ban having beneficial effects on bars and restaurants sales in Maryville cannot reasonably be advanced.

An instructive example of how methodological differences may drastically change the results comes from the debate caused by the study of Mandel, Alamar and Glantz (2005)

between the authors of this study and M.R. Pakko (2006b), an economist at the Federal Reserve Bank of St. Louis. Mandel, Alamar and Glantz (2005) show that the implementation of a smoking prohibition in Delaware had no statistically significant effect on revenues in the gaming industry. Pakko (2006b) uses the same dataset and a more general approach to control for heteroscedasticity and seasonality in the data and finds that total gaming revenues declined significantly after the implementation of the Delaware smoke-free law. Pakko was skeptical in accepting the results of Mandel *et al.*(2005), because after correcting for heteroscedasticity in the residuals of their estimates, the coefficient on the dummy variable representing the implementation of the smoke-free law changed significantly in the Mandel *et al.*(2005) study. This led Pakko to replicate the results, because in presence of heteroscedasticity, coefficient estimates are inefficient but unbiased and, therefore, they should not change significantly. First of all, Pakko drops observations from 1996 from the sample, eliminating the main source of heteroscedasticity. Then, he uses the Newey and West procedure to control for heteroscedasticity and serial correlation in the residuals. Finally, seasonal effects are captured in two separate sets of estimates using quarterly dummies and monthly dummy variables. Pakko obtains opposite results: the coefficient on the dummy variable representing the implementation of the law suggests a revenue loss of about 13% in the gaming industry, compared to the year preceding the smoking ban, and it is statistically significant.

5 Methodological Issues

The reviews carried out in the previous sections have touched upon a number of methodological issues that are crucial for obtaining reliable empirical evidence on the impact of Clean Indoor Air Laws. In this section we survey the main methodological issues that arise in shaping empirical applications of the type examined in this paper. The purpose of this section is trying to unfold the reasons of the wide heterogeneity in the results and, as a consequence, to provide an assessment of the reliability of those results.

5.1 Causality

Two points seem worth mentioning at this stage. The first one relates to the attention paid, in the papers reviewed, to the causality nexus. In most tests of economic theory, and certainly for evaluating public policy, the economist's goal is to infer that one variable has a causal effect on another variable. Simply finding an association between two or more variables might be interesting, but unless causality can be established, such association is not compelling. The key question, in most empirical studies, is whether enough other factors have been held fixed to make a case for causality, in other words the notion of *ceteris paribus* (which means "other factors being equal") plays a crucial role in causal

analysis. Carefully applied econometric methods can simulate a ceteris paribus experiment. However, in most serious applications, the number of factors that can affect the variable of interest is enormous, and the isolation of the effect any particular variable may have seems a hard task, especially in the area of public policy, since input parameters of investigated relationships may reflect unobserved individual characteristics and choices (Heckman *et al.* (2008)). Correlations between input parameters and observed outcomes may simply represent individual sorting on latent characteristics, rather than structural relations between input and output variables. Heckman *et al.* (2008) again stress that the acceptance of causal relationships should be founded on estimation of structural relationships based on sound theoretical frameworks. They also notice that the lack of a sound estimation of structural equations seems to be a common weakness in public health literature.

Here comes the second point. A sound economic theory should be, in many cases, the starting point for empirical analysis, because a a robust theoretical framework points out at the causal relationship between variables. So, what economic theory has to say about the economic effect of anti-smoking regulations ? While the relationship between reduced smoking and health benefits is now widely accepted, the positive relationship between the presence or strength of a smoking ban and a reduction in smoking is nowadays more controversial than it was a few decades ago. The rational addiction theory of Becker and Murphy (1988) establishes a negative relationship between factors that raise the cost of smoking (such as anti-smoking regulation) and the level of smoking. This literature provides a strong basis in support of a causal relationship between smoking regulation and level of smoking. However, more recent contributions in behavioral economics (Bernheim and Rangel, 2004 and 2005) point out that, if smoking is driven by compulsive behavior, policy measures such as anti-smoking regulation would not decrease the level of smoking, but they would only have the effect of raising the cost of smoking. In this case there would be no significant effect of smoking bans on smoking levels. So economic theory is not clear cut on even the most basic relationship between the regulatory variable and the level of smoking. The most contentiously debated point is, however, the economic impact on the hospitality industry. Even this controversy is not resolved by appealing to economic theory, as emphasized by Adams and Cotti (2007). Those opposing anti-smoking regulation claim that regulations would stifle the hospitality industry thus reducing the demand for labor in this sector. Policy advocates, on the other hand, claim that smoking regulations do not hurt establishments and may even add to revenue as well as lower costs. Adams and Cotti (2007) suggest that if there was a potential for increased business from going smoke free, restaurants and bars would have done so without regulation. The problem, however, is that they may not have accurate information about the potential changes in revenue that could derive from providing a smoke-free establishment. Second, without the guarantee that all the competitors would also go smoke-free, firms fear losing the business in favor of other establishments that still allow smoking. Craven and Marlow (2008) argue that smoking bans are not necessary since the Coase theorem

predicts that individuals have incentives to solve the smoking disputes privately without appealing to public policies such as smoking regulations. If this was true, not only it would be incorrect to argue that smoking bans are necessary, but it would also be incorrect to argue that such regulation causes no harm on business in the hospitality industry. Instead, the bans may have different effects on different businesses: some experiencing losses, other gains and others no effect at all. This implies that estimating aggregate effects of bans on businesses may be of little help in settling the debate.

Thus the theory leaves us with no firm guidance as to what to expect following smoking restrictions in the hospitality industry. These considerations lead us to the conclusion that developing and accurately estimating a robust structural model is the only way to disentangle the questions. Heckman *et al.* (2008, p. 41) point out that the establishment of reliable empirical evidence on a causal relationship would include the following elements: 1) valid measurements of both smoking outcomes (or turnover outcomes) and alleged causal factors, as well as other probable causes; 2) a clear argument demonstrating that other potential causes of variation in the dependent variable have been accounted for; 3) replicability to allow other researchers to follow and duplicate the analysis. In what follows we consider how these issues have been accounted for in the papers reviewed so far.

5.2   data problems

Rich data sources are the first requirement for developing and accurately estimating a structural model of the determinants of smoking. Estimating the impact of a social policy, such as a smoking ban, significantly increase the number of factors and the interactions of factors that need to be accounted for in the analysis. Individuals should be observed for long periods of time (before and after the introduction of the smoking ban) and detailed local data should be available. The lack of rich and detailed data sources makes it difficult to control for factors that may affect the outcome of interest (omitted variables bias) or it may undermine the ability to adequately test hypothesis (limited degrees of freedom).

5.2.1   Selection bias

Data should be obtained by random sampling from the underlying population. This may not be the case if the data are affected by "selectivity problems". A first type of selectivity problem is known as self-selection bias. This happens when the sample is based on whether the dependent variable is above or below a given value. In analysis of the impact of smoking bans on smoking levels or prevalence it may be that non smokers find work in smoke-free environments. Farrelly *et al.*(1999) suggest this may be generated through a number of different channels. First, nonsmokers (smokers) can be attracted to firms with (without) workplace smoking bans. In this case the impact of the ban would be overstated in single equations estimates. Second, firms with the highest fraction of nonsmokers are

more likely to adopt the ban. Third, firms with a "healthy" attitude may introduce other "healthy" workplace policy measures, therefore smoking may be lower in those firms because of these other measures and not because of the smoking ban.

Large panel (longitudinal) data sets may mitigate some of these problems, but they may be affected by a fourth source of selection bias which is known as endogenous attrition. Attrition occurs when individuals leave the panel before the end of the period of the panel. If these occurrences are random, the only consequence is a loss of information and efficiency. If, on the other hand, those who leave the sample are systematically different from those who remain, this causes a problem of selection which causes the panel to lose its representativeness (non-randomness of the sample).[10] If the ban changes the type of workers that are attracted to a firm or encourages smokers to leave a firm in higher numbers than nonsmokers, those individual units which are dropped from the sample from one period to the other are not a random sample, but are driven by the smoking ban. In this case the sample of observations left to the analysis looses its representativeness.

There is another source of sample selection bias which may affect studies of the impact of smoking bans. Many studies are based on countries or localities that first introduced the ban and are therefore more likely to have a proportionately smaller smoking population and/or fewer businesses that would be adversely affected by a smoking ban. Adams and Cotti (2007, p. 7) notice that Utah and California, the two states in the American federation with the lowest prevalence of smokers, passed the first smoking-bans. Pakko (2006a) also notes that the early studies on smoking bans used data from early bans, which potentially introduces sample selection bias.

5.2.2 Measurement error in the dependent variable

One problem that affect survey data collected through a questionnaire is underreporting. Wasserman *et al.* (1991) suggests that underreporting of individual smoking can arise because of the social undesirability of smoking. Thus, in estimating tobacco demand, one should adjust for underreporting. However, information on underreporting is often not available and the dependent variable might be measured with error. If the measurement error is statistically independent of each explanatory variable, the only consequence of measurement error in the dependent variable is a larger error variance. So OLS would still produce consistent and unbiased estimators of the variables of interest, but their standard errors would not be efficient leading to more imprecise estimates. The "placebo effect" mentioned at the beginning of section 4 also causes measurement error in the dependent variable. In this case, however, the measurement error is correlated with the explanatory variable representing the smoking ban and this causes a bias in the OLS coefficient measuring the impact of the smoking ban on the turnover of the hospitality industry.

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[10]Dealing with non-random attrition may then require selection correction methods beyond the statistical methods required to deal with longitudinal data.

5.2.3 Measurement error in explanatory variables

A more serious problem than measurement error in the dependent variable is measurement error in the explanatory variables. Measurement error in the explanatory variables produces the classical errors-in-variables (CEV) assumption. The implication is an attenuation bias in OLS estimates: on average (or in large samples), the estimated OLS effect will be attenuated, e.g. if β_1 is positive, $\hat{\beta}_1$ will tend to underestimate β_1. Again, measurement errors in the explanatory variables are more likely to occur in survey data collected through a questionnaire, either due to respondents' errors or due to data collection errors.

5.3 Endogeneity

Endogeneity refers to the fact that an independent variable included in the model is potentially a choice variable, correlated with unobservables included in the error term. This implies that the regression coefficient in an OLS regression is biased. Examples of endogeneity include simultaneous equations bias (see Chaloupka and Saffer, 1992), omitted variables bias which will be analysed in the next sub-section, sample selection bias and measurement error bias (Cameron and Trivedi, 2005, p. 92). Endogeneity is likely to occur when cross-sections are used and this is why in economists are very concerned with this problem (Cameron and Trivedi, 2005, p. 92). There are many methods of overcoming this, including instrumental variable regression and Heckman selection correction. In studies using aggregated data to assess the impact of Clean Indoor Air Laws the Clean Indoor Air Law is often endogenous: the presence of the law is likely to be related to the the average statewide level of cigarette consumption. This creates an upward bias in the OLS estimation of the coefficient measuring the impact of the law. One way of handling this problem is to estimate a simultaneous equations model using a two step procedure. In the first step, a probit procedure estimates an equation for the probability of a state passing a Clean Air Law. In the second step, a cigarette consumption equation is estimated, using weighted Least Squares, in which observations on the dichotomous variable representing the smoking ban are replaced with the first step estimates. This is the procedure used by Chaloupka and Saffer in their 1992 study.

Studies based on micro-data can also present problems of potential endogeneity of the smoking ban. In studies of the effect of workplace smoking regulation on the propensity to smoke of workers, based on individual data, the potential endogeneity problem may be due to the non-random match of workers and firms (non-smokers being attracted to firms with workplace smoking bans), for instance.

Heckman *et al.*(2008) observe that in disciplines other than economics, for instance in Health Studies, biases associated with endogeneity do not affect the estimation of structural models because the omitted variables are not affected by individual choice. However, in evaluating the effect of a public policy, problems of endogeneity of right hand variables

are likely to occur when observations on explanatory variables are the result of individual choice.

5.3.1 Omitted Variables bias

In statistics, omitted-variable bias (OVB) is the bias that appears in estimates of parameters in a regression analysis when the assumed specification is incorrect, because it omits an independent variable that should be in the model. Two conditions must hold for omitted-variable bias to exist in linear regression: (i) the omitted variable must be a determinant of the dependent variable (i.e., its true regression coefficient is not zero); and (ii) the omitted variable must be correlated with one or more of the included independent variables. While incorrect omission of variables leads to biased estimates of the parameters that are included, incorrect inclusion only produces inefficient estimates of the parameters that are included. So it's better to include the wrong variables rather than exclude the right ones. In the analysis of the impact of anti-smoking regulation, one source of omitted variable bias can be self-selection (Evans *et al.*(1999)). Suppose that firms adopting workplace smoking bans place a greater emphasis on the health and safety of their employees, so that policies than ban smoking inside the firm simply reflect other programmes adopted by the firm (like onsite exercise facilities or smoking cessation programmes). In those firms, smoking may be lower because of these other programmes, not because of the smoking ban. If these other programmes offered by the firm are an omitted variable in the model, the estimated parameter for the impact of the smoking ban will be biased. More generally, controlling for omitted variable bias by including all the different factors that may have an influence on the outcome of interest and by running appropriate specification tests is crucial in proving a causal relationship between the presence of a ban and the level of smoking intensity or prevalence. Therefore, for informative studies on individual choice, rich data are almost always essential (Heckman *et al.* (2008). Evans *et al.*(1999) adopt three different approaches to effectively control for omitted variable bias. First they control for health-habit indicators and firms' characteristics that may have an impact on the propensity to smoke of workers. Second, they assume that if there is a causal link between bans and smoking, then they would expect the impact of the bans to be a function of the costs they impose on workers. More precisely, the cost of the ban should be greater for workers with long work weeks, because it's more difficult for them to adjust to the ban by shifting the timing of their smoking. Thus, the authors add indicators for the usual hours worked per week to their smoking equations. Third, they allow for the non-randomness of the sample, i.e. for the non-random match of workers and firms. In this case the workplace ban is treated as a potentially endogenous variable and Two Stages Least Squares (2SLS) methods are used to estimate their models.

Another relevant omitted variable is public sentiment against smoking (Wasserman *et al.* (1991) and Tauras (2006)). Wasserman *et al.* (1991) argue that public sentiment against smoking produces higher taxes and more stringent regulations. Since public sentiment is difficult to measure, it has been excluded from virtually all data sets that have

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been used to model cigarette demand. Consequently, all correlated but included variables will suffer from an omitted variable bias, with the omission of a public sentiment variable biasing both the price and regulation index away from zero. Thus omission of this variable would be an indictment of all micro studies of cigarette demand. An exception is Tauras (2006) who controls for the possibility that unobserved state-level sentiment toward smoking is driving both antismoking policy and changes in smoking behavior by including state fixed-effect which are intended to capture all the time-invariant factors that are unobserved at the state level and that may affect cigarette smoking.

More generally, studies of the impact of antismoking regulation on smoking behavior should include appropriate control variables for demographic characteristics, firms characteristics (in workplace studies), time fixed effects and country fixed effects, besides the main explanatory variables such as price, income and the regulation indicator. In studies of the impact of the smoking regulation on the hospitality industry, seasonality, trend effects and the presence of competitors in the same area represent important specific control variables, besides controls for overall economic conditions (including measures of the economic cycle) and besides time and firms' fixed effects. Time trend and seasonal effects are likely to generate a significant fraction of the variation in restaurants and bars' sales, as shown in Pakko (2006a). The same work shows that a large fraction of the variation in restaurants' sales is captured by a dummy variable for the presence of competitors in the same area in which the ban has been introduced.

5.3.2 The importance of demographics

The most recent research on the economic impact of Clean Indoor Air Laws highlights the importance of "demographic controls" (for studies of the impact of regulation on smoking) and of "firms characteristics" (for studies of workplace bans or of the impact on the hospitality industry). Carpenter (2007) for instance, finds significant differences in the impact of local smoking ordinances on blue collar workers smoking behavior and on other types of workers's smoking behavior and on men and women's. Other demographic distinctions might be relevant, such as ethnicity, marital status, education. Analogously, Evans *et al.*(1999) use a rich set of indicators of individual health habits, collected in the National Health Interviews Survey (NHIS), to control for omitted variables bias. This again stresses the point that rich data sets, including precise measures of any factor that may affect choices, are essential to produce rigorous studies on individual behavior. Adequately controlling for factors that may drive differences in the estimated impact of antismoking regulation may be of help in developing more effective policy measures. Not taking into account those factors may result in designed policy measures to have scarce effectiveness or excessive costs.

6 Discussion

A number of *stylized facts* have emerged from the review. First, since the early nineties, a multitude of studies using different methodological approaches has emerged trying to assess the economic impact of Clean Indoor Air Laws. They can be broadly divided into Economics and Health Policy studies. Second, there is a time lag between the spread of Clean Indoor Air Laws in the U.S. and in Europe and, correspondingly, there is a time lag between studies of the impact of Clean Indoor Air Laws in the U.S. and in Europe. Third, most studies find that smoking restrictions, whether imposed by public laws or private firms, reduce the quantity of cigarettes smoked (intensity of smoking) and the probability of being a smoker (prevalence of smoking), although for prevalence rates evidence is less clear (Levy and Friend, 2003). Evidence on the impact of smoking restrictions on the hospitality industry is more controversial and there is no clear-cut consensus on the sign of the effect of smoking restrictions on the hospitality industry. Fourth, the quality of the data used in the analysis of both effects has become, over time, increasingly important, as studies have revealed that demographic characteristics (age, gender, ethnicity, labor status, marital status, religion) and the length of time during which individual behavior is observed play a crucial role as control variables in isolating a causal effect between the smoking restriction and the dependent variable. We now discuss these issues in detail.

The earliest policies restricting smoking in public places in the united States have followed the 1972 Surgeon General's Report on the possible adverse effects of second hand smoke. A second Surgeon General's Report titled "the Health Consequences of Involuntary Smoking" and released in 1986 also concluded that exposure to tobacco smoke caused diseases, including lung cancer. There has been a strong opposition to the introduction of smoking bans right from the start first questioning the scientific evidence proving the adverse effects on health of second hand smoke and, secondly, questioning the efficacy of smoking bans in reducing second hand smoke. This has generated a lively debate in the academic and scientific arena involving economists and health scientists trying to disentangle the debated question of the effects of anti-smoking regulations. When evidence on the health effects of smoking has become incontrovertible concerns about the economic impact of these policies, particularly on the hospitality industry, have been raised, especially by actors in the Tobacco and hospitality industry, claiming that smoke-free air policies will result in declining restaurant, bar and other hospitality industry revenues. This historical and political evolution in the spread of Clean Indoor Air Laws may explain a number of observed patterns: (i) the fact that studies examining the impact of anti-smoking regulation on smoking behavior precede studies of the impact on the hospitality industry; (ii) the fact that we can envisage two separate strand of literature from the point of view of the disciplines: econometric literature on one side and public health literature on the other. While health scientists have been interested in demonstrating that smoking bans are an effective way of reducing smoking, economists have been more concerned of making sure that a causal relationship exists between smoking bans

and smoking intensity or prevalence. The causality nexus seems to be the major concern in applied economics research in this field, while for health scientists this is less crucial because, as emphasized by Heckman *et al.* (2008, p. 38), they are less familiar with the problems involved in evaluating the intervention effects of a public policy. The latter implies modeling human choices which requires more elaborate theoretical and empirical modeling than it would be required to draw causal inferences regarding drug efficacy in a controlled experiment environment, for instance. (iii) Smoking regulation in the U.S. has anticipated smoking regulation in Europe. At the time of the introduction of the first smoking bans in Europe the academic debate had already settled the question of the adverse health effects of smoking and therefore the political opposition to the introduction of the smoking regulation has been weaker. This may explain the paucity of European studies trying to assess the impact of anti-smoking regulations on smoking behavior, whereas the European literature assessing the effect on the hospitality industry is relatively more abundant.

As to the empirical findings, there is a widespread consensus that smoking-bans reduce smoking intensity and a less clear pattern emerges on the effect on smoking prevalence. In both cases, however, there is a considerable variation in the amount of reduction in smoking behaviors observed. The effect of smoking bans on the hospitality industry is, instead, more controversial. In this case, more specifically, there is a large Public Health literature, mainly developed in the U.S., strongly arguing for no negative effect or for a positive effect of smoking bans on the economic outcomes of the hospitality industry (Scollo and Lal, 2003 and 2008; Alamar, Glantz (2004 and 2007); Glantz, 2007; Glantz and Smith 1994 and 1997) and another strand of literature grounded on applied economics analysis finding quite different results (Pakko 2006a and 2006b; Adda *et al.* (2006); Adams and Cotti, 2007). These differences make it difficult for the policy maker to draw economic policy considerations, both on the effectiveness of the smoking bans and on their expected impacts and call for the identification of factors that drive such heterogeneity in order to establish the scientific reliability of the empirical findings. Again, as emphasized by Heckman *et al.* (2008) reliably identifying the causal factors underlying smoking behavior (or the turn-over in the hospitality industry) is an important part of developing scientifically reliable empirical applications.

The first element that should be carefully considered in empirical studies is data. Large samples are better than small samples, because the large number degrees of freedom allow to better assess the causality relationship between the law and the level of smoking by including an appropriate number of controls. Samples should be representative and randomly drawn from the underlying population to exclude sample selection biases. Long longitudinal data set are needed following the behavior of individuals a long time before and long time after the date of the implementation of the smoking regulation. As empirical findings have shown that individual characteristics may drive significant differences in smoking behavior, micro-data, at the firm or individual level, including a rich

set of demographics (such as: age, gender, education, marital status, occupational status (blue collar versus white collar workers), geographical location) seem to be preferable. Gender or occupational differences appear to be important, but many studies do not take them into account. Thus a rich longitudinal micro-data set accurately measuring not only the factor being tested, but also the effect of other causal factors, seems to be desirable in order isolate the effect of the factor being studied. Samples drawn from repeated cross sections of large nationally representative surveys or longitudinal samples seem to be the preferable type of data. Longitudinal data also contain all the above mentioned elements, but may be affected by endogenous attrition causing selection bias. Large national surveys repeated over time are also better than large ad hoc telephone surveys which may be affected by the so called placebo effect. Moreover, since population based studies employ a random selection of individuals or firms they are likely to be more representative of the population impact of smoking bans than studies that select workers within a firm or a small number of firms. In addition, the surveys are not conducted at the workplace, thus reducing the likelihood of workers providing socially desirable responses (Levy and Friend 2003, p. 598).

In order to better identify the economic impact of anti-smoking regulation, additional research is needed taking into account control factors which have been previously neglected. The impact of such laws on youth smoking has rarely been investigated in the literature, because much of the literature investigates the impact of workplace bans on workers who are generally above age 18. The impact of this type of regulation on this age group's smoking is, however, rather important, also to evaluate the impact on the hospitality industry. More generally explicit consideration of age groups in modeling the impact of smoking bans seems to be important as previous research has emphasized that the extent of the impact differs by age groups. Accounting for public sentiment against smoking also seems to be relevant in assessing the impact of smoking bans. Wasserman (1991) stresses that failure to control for the possibility that unobserved sentiment towards smoking may bias the coefficient of the smoking ban away from zero. However, only a few of the later studies take this variable into account. Finally, only very few studies choose a dynamic specification to model smoking consumption. This is surprising given the well known addictive nature of smoking. The influential contribution of Becker and Murphy (1988) on rational addiction has been extensively applied to model the demand of cigarettes and of other addictive consumption goods. The distinction between addictive consumption and other consumption is captured by recognizing that, for addictive goods, current consumption depends on the level of past consumption. If there is habit formation in the consumption of Tobacco, neglecting the contribution of past consumption as a determinant of current consumption may result in an omitted variable bias in the estimation of the coefficient measuring the impact of regulation. Despite the popularity of dynamic models to explain the demand for cigarettes, dynamic specifications have been used only in a minority of the papers analyzed here (Chaloupka, 1992; Keeler *et al.*, 1993; Sung *et al.*, 1994).

Table 1: Summary of studies using aggregate data

Study	Country	Invest. period	Data	Est. Method	Specification	Sample size	Main control var.	Cons. per-capita
International Studies: Using Time series								
Keeler et al.(1993)	California	1980-1990	monthly time series	FIML	Static	-	income, time trend taxes	-0.0019
International Studies: Using Pooled Cross-Sections								
Chaloupka-Saffer (1992)	United States	1975-1985	Pooled Cross Sections	Two Step Weighted Least Squares	Static	-	Tob. price, income, smuggling Tob. production, unemployment divorce, voting	-0.028-0.003*
Gallet (2007)	United States	1993-1998	Pooled Cross Sections	FE2SLS	Static	288	gender, marital status, education, employment presence of children	-0.336
Lanoie-Leclair (1998)	Canada	1980-1995	Pooled Cross Sections	GLS	Dynamic	150	time trend provincial fixed effects per-capita income	-0.037÷ -0.038
Sung et al.(1994)	United States	1967-1990	Pooled Cross Sections	GLS	Dynamic	-	age, gender, race, religion, bootlegging	-0.049
Yurekli-Zhang (2000)	United States	1970-1995	Pooled Cross Sections	2SLS	Static	-	income, education per-capita tourist expend., % pop. employed % of Native Americans and Military pers.	-1.64**

*Reported impacts from the simultaneous equations model. Coefficients refer to the impact of a Public Place Law and a Private Place Law, respectively, on cigarette consumption.
**Dependent variable is per-capita packs of cigarettes per year. Coefficient refers to table 3, model IV.

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Table 2: Summary of studies using individual data

Study	Country	Invest. period	Data	Est. Method	Sample size	Main control var.	Smoking intensity	Smoking prevalence
International studies: Large Survey data								
Carpenter (2007)	Canada	1997-2004	cross-sections	Linear Probit	-	county fixed effects year dummies, age, sex marital status, education	-	-0.056
Chaloupka (1992)	United States	1976-1980	cross-section	2SLS	14,305	price, lagged consumption income, lead consumption sex, education, employment	-0.133 ÷ -0.324	-
Chaloupka-Wechsler (1997)	United States	1993	Cross-section	Double-Hurdle	16,570	sex, age, race, marital status, employment, religion fraternity/sorority parents education	-0.555	-5.0%*
Evans et al.(1999)	United States	1991-1993	NHIS Cross sections	OLS, probit	18,090	age, family size, income location, education, race marital status, employment	-2.3 cig. per day	-5.7%
Farrelly et al.(1999)	United States	1992-1993	Cross sections	OLS, Probit	97,882	-	-2.7 cig. per day	-5.7%
Glasgow (1997)	United States	1993	Cross section	Multiple Logit	2,884	-	-2.8 cig. per day	-
Moskovitz et al.(2000)	California	1990	Cross-section	Multiple Logit	4,680	age,sex, race education, employment ordinance strength	-	1.38[a]
Stephens et al. (1997)	Canada	1990-1991	Cross sections	Logit	11,652	age, sex, marital status, education	-	1.21**
Tauras (2006)	United States	1992-1999	Cross sections	GLM	545,603	age, sex, education family income, race	-0.0059	-0.0020%
Wasserman et al.(1991)	United States	1970-1985	Cross sections	Double Hurdle, GLM	84,301	marital and employment status age, sex, race education, income, family size	-0.048	-0.045***
Woodruff et al.(1993)	California	1990-1991	Cross section	Multiple Logit	11,704	age, education ethnicity, sex	-	1.15÷1.30[b]

[a]Odd Ratio (OR)= 1.38 from a logistic regression.

[b]Odd Ratios (OR) of being a smoker for workers in workplaces with weak or no smoking restrictions compared with workers in smoke-free environments.

* Reported coefficients refer to: "other public place smoking restrictions" in Model 3, Table 3 and to "restaurant smoking restrictions" in Model 3, Table 2, respectively.

** Odd Ratio (OR)= 1.21 from a logistic regression.

*** Reported coefficients refer to the two part model specification for adult consumption, Table 5.

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Table 2: Summary of studies using individual data (Continued)

Study	Country	Invest. period	Data	Est. Method	Sample size	Main control var.	Smoking intensity	Smoking prevalence
International Studies: Panel Data								
Bauer et al.(2005)	United States	1988-1993	Longitudinal sample	Logit and Multivariate regression	1,967	sex, race, age education, income employment, quit attempts	-2.5 cig. per day	1.9*
Buddlemeyer-Wilkins (2005)	Australia	2001-2003	Longitudinal sample	Trivariate probit	22,747	sex, marital status, education, employment presence of children	-	-7.0%
European studies								
Brenner-Mielck (1992)	Germany	1987	Cross-Section	Logit	439	age, marital status, education	-	-8,9%
Fong et al.(2006)	Ireland	2003-2004	national cohort survey	GLM	1,679	education, age		
Gallus et al.(2006)	Italy	2005	national survey	distributions of smoking prevalence and intensity	3,114	sex, income, ethnicity -	-5.5%	-2.3%
Helakorpi et al.(2007)	Finland	1981-2005	national survey	Logit	2002	income, age, education	0.78÷0.83	
Heloma et al.(2001)	Finland	1994-1996	Cross-Sections	distributions of smoking prevalence and intensity	2002	education	-15%	-16%

* Odd Ratio (OR)= 1.9 from a logistic regression.

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Table 3: Summary of studies on the impact of smoke-free air laws on the Hospitality Industry

Study	Country	Invest. period	Subjective/Objective measures of outcome	Method	Sample size	Main control var.	Impact on sales	Impact on customers
Natural experiments								
Adams-Cotti (2007)	United States	2001-2004	objective	panel FE			-4.3%**	-
Adda et al.(2006)	Scotland	2006	subjective	OLS	2724	location, size fixed-effects	-10%	-14%
Glantz-Smith (1997)	United States	1986-1996	objective	OLS	-	location, seasonality	0%	-
Other type of studies								
Alamar-Glantz (2004)	United States	2003	objective	weighted least squares	608	gross state product and its growth rate, unemployment rates,	+16%*	-
Alamar-Glantz (2007)	United States	1993-2005	objective	OLS	197	gross state product and its growth rate, unemployment rates	no sign. effect*	-
Binkin et al.(2007)	Italy	2004-2006	subjective	descriptive statistics	1961/1536	-	small neg. effects	-
Cowling-Bond (2005)	California	1990-2002	objective	OLS	-	time trend, unemployment rate	+0.5%	-
Hyland et al.(2009)	Scotland	2006-2007	subjective	GEE	1936/1618	country, wave, sex, age socioeconomic status heaviness of smoking, ethnicity	-	0%
Lund-Lund (2006)	Norway	2003-2005	objective	descriptive statistics from Statistics Norway	-	-	+5%	-
Mandel et al.(2005)	Delaware	1996-2004	objective	OLS	101	time trend, income seasonality, n. machines	no sign. effect	-
Pakko (2006a)	Missouri	1998-2004	objective	OLS	26	annual time trend, seasonality new openings	no sign. effect	-
Pakko (2006b)	Delaware	1996-2004	objective	OLS	101	time trend, income seasonality, n. machines	-13%	-

Reported impact refers to the sale price of a restaurant.
**Reported impact refers to employment in bars. Impact on employment in restaurants is positive in some areas.*

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