Essay on the Health and Labor Consequences of Unhealthy Habits

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A mi hermano Marcos, siempre has sido mi inspiración

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Abstract

Even though unhealthy habits, drinking, smoking and overeating, are among the most expensive burdens for the health system, much research is still needed to understand how individuals form them, how do they correlate between them and what impacts do they have in labor productivity. The first paper in this thesis fills in the gap of understanding whether individuals substitute among habits by exploring the effect that quitting smoking has on obesity. The second paper analyze the impact that the business cycle, that is, unemployment rate and income per capita have on drinking participation and alcohol consumption. To overcome the lack of a true longitudinal panel which would prevent us from obtaining unbiased estimates in these two first papers, we use cohort analysis methodology to control for unobservables, while instrumenting the habit decision and introducing dynamics into the estimation equation. The third paper focuses on the effects of smoking over labor productivity. Here we exploit many outcomes that are potentially correlated with individual labor productivity using a longitudinal panel and instrumenting the smoking decision. The three papers make use of a dataset on US regulations regarding tobacco use, which was self developed from the compilation of the different laws enacted by the states.

Resum

Tot i que els hàbits no saludables, com poden ser beure, fumar o menjar en excés, són algunes de les càrregues més cares per al sistema de salut, encara és necessari molt més recerca per entendre com els individus formen els hàbits, com aquestes es correlacionen entre si, i quins efectes tenen per a la productivitat. El primer document busca comprendre si els individus substitueixen uns hàbits per altres, en particular, analitza l'impacte que deixar de fumar té sobre l'obesitat. El segon article analitza l'impacte que té el cicle econòmic, és a dir, la taxa d'atur i l'ingrés per càpita, sobre la decisió de beure i sobre el volum d'alcohol consumit. Per superar la manca d'un veritable panell longitudinal que impedeix obtenir estimacions no esbiaixades, en aquests dos primers articles s'ha utilitzat la metodologia de l'anàlisi de cohortes per a poder controlar d'aquesta manera per a les característiques no observables, en particular les preferencies, al mateix temps que s'ha instrumentat la decisió de l'hàbit i s'ha introduït dinàmica en l'equació d'estimació. El tercer document se centra en els efectes del tabaquisme sobre la productivitat laboral. Aquí s'exploren moltes variables que potencialment estan correlacionades amb la productivitat del treball, utilitzant un panell longitudinal i instrumentant la decisió de fumar. Els tres documents fan servir un conjunt de dades sobre reglaments pel que fa a l'ús del tabac als Estats Units.

JEL classification codes: C23, E23, I12, I18, I19, J31, J38, J70

Foreword

"*Although of Course You End Up Becoming Yourself*" (Title of David Lipsky's book about his 1996 interview with David Foster Wallace)

This thesis explores the links between habits, health status, labor market conditions and labor productivity. The first part estimates the impact that quitting smoking has on gaining weight and on the probability of becoming obese. The second part studies the effect that the business cycle has on drinking participation and on the number of drinks consumed by drinkers. The third part estimates the effect that habits have on labor productivity, focused on the case of smoking. There are several contributions: First, we show that not controlling for unobserved heterogeneity can lead to biased estimates of the effect of habits, be it on drinking or on other habits. Second, we develop a data set that collects all the legislation on tobacco use in the US from 1980 to 2008. This allows us to obtain a set of instruments for smoking, one in particular which has not been exploited in the past, that is, the protection of smokers in their workplace. Third, we show that for policy purposes it is relevant to take into consideration the trade offs involved when it comes to remove an habit from the society, as it is the case of smoking. Fourth, we show that there are important gender differences in the trade off between smoking and weight gain. Fifth, we show that on average alcohol consumption is independent of the business cycle and therefore universal actions to prevent alcoholic abuse during hard times will be fruitless. Finally, we show that by means of just the wage rate we can not fully capture the impact of smoking on labor productivity.

The first chapter [1] studies a hot topic in health economics, that is, what happens with a person's weight after he quits smoking and helps resolve contradicting evidence in the literature regarding the contribution that (quitting) smoking has on the average increase in weight. Smoking is not randomly assigned and the decision to start or stop smoking depends on the preferences of the individual among others. Also, weight does not change overnight. Therefore to estimate the impact of quitting smoking, special attention should be paid to preferences, adjustment costs and the fact that excess weight may cause smoking. To do so we apply the pseudo-panel methodology developed by Deaton (1986) to cross sectional data. The advantage of this endeavor is that it helps overcome not having the same individual more than once therefore preventing the standard fixed effects analysis. We then instrument the decision to quit smoking using taxes on tobacco, the regulations on tobacco use and the number of adults in the house. Finally we also incorporate weight dynamics to account for adjustment costs. We find that after quitting smoking individuals gain weight. We also find that the initial effect overshoots even though part of the effect stays in the long run. However, only males are affected by this phenomena. We also find that the probability of becoming obese after quitting smoking increases significantly.

[1] A joint work with Sergi Jiménez and José María Labeaga

The second chapter [2] estimates the impact that the business cycle has on alcohol intake. From a policy perspective it is very important to know whether economic hard times also bring up a worsening of the population's health. Using the pseudo panel methodology and incorporating habit formation in the estimation equation, we show that the unemployment rate increases drinking participation but the conditional intake of alcohol is not affected. Therefore, policy makers should be aware that universal policies to prevent alcoholic abuse are not going to be very effective in preventing a worsening of the health and consequently, it is necessary to identify different groups of individuals to carry out specific policies.

The third chapter represents an update of my thesis proposal. The basic idea is to exploit all the relevant labor market information of an individual in order to understand what are the consequences of smoking on labor productivity. Following recent literature that criticizes the use of the wage rate as a substitute for labor productivity we present a set of outcomes that are also correlated with how much an individual produces while working. Here we make use of a long panel that allow us to measure better the long term consequences of smoking. After instrumenting the decision to smoke, with an instrument that has not been used so far in the literature, we find evidence that smoking decreases labor productivity. However, we also find evidence that smokers are discriminated in their workplace, particularly when it comes to firing them.

[2] A joint work with Sergi Jiménez and José María Labeaga and Cristina Villaplana

Contents

1 CHAPTER ONE

1.1 Introduction

In the last 40 years the percentage of US adults who smoke regularly dropped from above 42% in 1965 to below 20% in 2007, according to the Center for Disease Control and Prevention. This drop has been regarded as one of the most important "health victories", Gruber and Frakes (2006). The logic behind these words is that even today cigarette smoking is calculated to kill 438,000 people per year. On top of that, smokers are up to 40% more expensive for the health care system than non-smokers. In between 2000-2004 cigarette smoking was estimated to be responsible for \$193 billions in annual health-related losses (Armour, Wollery, Malarcher, Pechacek and Husten (2005), Barendregt, Bonneuxand and van der Maas (1997), Miller and Rise (1998) and Adhikari, Kahende, Malarcher, Pechacek and Tong (2008)). These are only some of the direct effects of smoking. Indirect effects range from lower labor productivity to 49,000 deaths per year due to secondhand smoking. The social cost of smoking, calculated in \$11 per pack, almost doubles its private cost (CDC (2006)).

As the battle against smoking started to show very positive outcomes, health practitioners began to notice a new problem: the negative correlation between smoking rates and the prevalence of obesity. As we can see in Table 1.1, in 1985 the average American man was 1.78 mt. tall and weighed 80 Kg. 22 years later he weighs almost 10 Kg. more, representing an increase of 12%, even though he is as tall as before. The picture is even worse for women. During the same period they faced a similar average absolute weight gain and consequently a larger relative growth rate[1]. This trend is in contrast with historical evidence from the past 150 years where weight increases were not as abrupt and pronounced and were accompanied by increases in height, (Costa

[1] According to the NHS data, in the UK for the period that goes from 1993 to 2007 men increased their height by 1 cm and their weight by 4.6 Kg. while women stayed the same height and gained 3kg.

and Steckel (1997)).

Table 1.1: United States Average Weight and Height: 1985 versus 2007

	Men		Women	
	1985	2007	1985	2007
Height (in mt.)	1.78	1.78	1.63	1.63
	(0.076)	(0.075)	(0.067)	(0.070)
Weight (in Kg.)	80.06	89.52	63.30	73.31
	(13.09)	(18.11)	(12.35)	(17.69)
BMI*	25.25	28.25	23.82	27.58

Source: Behavioral Risk Surveillance System. Standard Deviation between parenthesis.
*Body Mass Index equals weight in kilograms divided by height in squared meters

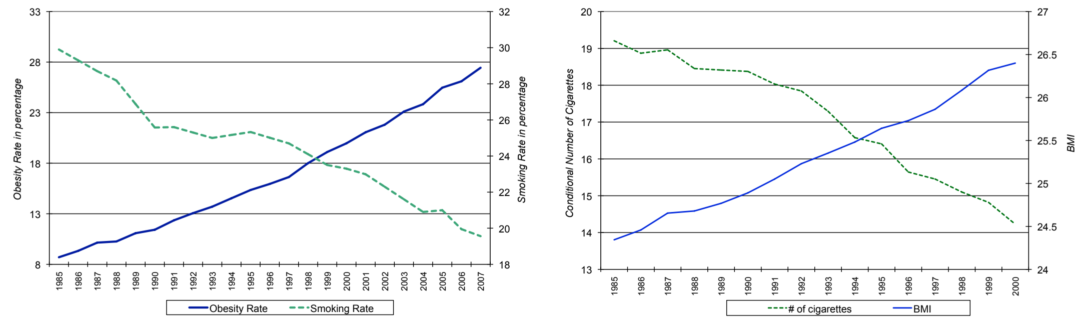
The increase of the intensive margin went hand in hand with an increase of the extensive margin. Indeed, during the past two decades obesity rates have jumped dramatically among the US population, becoming one of the biggest health concern for policy makers. Before 1980 only 14% of its population was obese, yet nowadays 38% of the men and 34% of the women classify as obese (that is approximately 65 million people). In fact, nowadays American are more likely to be overweight than to pay federal income tax[2]. But not only the number of obese have increased. An increasing proportion of the obese population now belongs to the classes of obesity deemed more troublesome[3].

The negative correlation between the conditional number of cigarettes smoked and average body mass index (1.2a and 1.2b) is a temptation to conclude that net calorie intake has substituted smoking as a habit and consequently that the decrease in the incidence of smoking is responsible for the increase in weight. In addition, because smoking affected a large share of the adult population, it is a natural suspect to analyze. The growing concern about obesity on the one hand and the impulse the anti-smoking campaign has all over the world on the other, make it critical to examine whether the two processes are causally connected. If quitting smoking has a positive effect on weight, that is, smoking is a substitute for eating, some costs related to the anti-smoking

[2]The Economist, Jan 21 2010.

[3]Childhood obesity has also been rising. Its prevalence has nearly tripled. Almost 19% of children aged 6 to 11 years and 17.4% of adolescents aged 12 to 19 are now obese, according to the National Center for Health Statistics. America is not only getting fatter, it is doing it at a younger age.

Figure 1.1: Correlation between Smoking and Weight - US 1985-2007



(a) Smoking Rate and Obesity Prevalence (b) Number of Cigarettes Smoked and BMI

Source: National Health Interview Survey and Behavioral Risk Factor Surveillance System. 1985 to 2007

campaign were not fully internalized in the law making process. On the contrary, if the effect happens to be negative, then there would be benefits that were probably not taken into consideration. This would put additional pressure over the states that have not passed a tough legislation on smoking. Unfortunately, economic theory remains vague in providing testable predictions about how individuals react to the elimination of a habit and this problem is further compounded by the fact that there is hardly any conclusive empirical evidence on this issue.

In addition, there are many economic and biological reasons why quitting smoking and increasing weight might be correlated in an idiosyncratic and non-causal way. First, common omitted factors such as, risk aversion, preferences or variation across individuals in the Basal Metabolic rate provides one motivation for suspecting the presence of individual-specific effects. For instance, if people that quit the habit of smoking are potentially more concerned about health (McCaul, Hockemeyer, Johnson, Zetocha, Quinlan and Glasgow (2006) and Clark and Etile (2006)) then they should be less prone to weight gains than continuers. In contrast, if it is true that quitting smoking leads to weight increase, then quitters are less concerned by the risks derived from the increase, than non smokers. In such a scenario, lack of a priori knowledge about the individual specific directional bias can easily generate non-causal correlations. Second, reverse causality posses a similar problem to the analysis. Overweight individuals may use smoking as a weight control method. As a consequence even after controlling for unobserved heterogeneity the error term will still be correlated to the decision of quitting smoking. Similarly, both processes, that is smoking and weight changes, might just be the consequence of a third common factor, for instance the stress due to a harsher labor market. Finally, weight adjustment does not happen instantaneously. On the contrary, weight adjustment costs create an autoregressive process where present weight depends on past weight realizations. Thus, failing to incorporate lagged BMI in the estimation might cause a bias in the estimation.

Summing up, the observed correlation among the two process could be completely spurious. In order to measure the causal effect of quitting smoking on weight then, it is necessary to control for unobservables and the sources of exogenous changes in the individuals decision for quitting smoking.

The economic literature that analyzes the effect of smoking on BMI is relatively recent and so far the results remain inconclusive. Chou, Grossman and Saffer (2004), Baum (2008) and Rashad (2006) find that individuals that stop smoking increase their weight. In contrast, Gruber and Frakes (2006) and Courtemanche (2009) arrive to the opposite conclusion. All of these studies concentrate on the effect of increases in the price of cigarettes, whether it be the final price or the excise tax on tobacco. Instead of concentrating on the reduced form regression, we use the exogenous changes in cigarette prices to focus on the impact of quitting smoking on weight. Our decision to to evaluate the final product of the anti-smoking campaign, that is, the decision to quit smoking instead of focusing on just one dimension, for instance the pecuniary cost of smoking, is substantiated by the following observation. The anti-smoking campaign has many highly correlated dimensions, from information on the consequences of smoking and limitations on the advertisement of cigarettes in TV to smoking prohibitions in public places[4].

[4]For instance, nowadays it is much harder to see a person smoking in a Hollywood movie.

We contribute further to the literature as well as to the debate by applying cohort data techniques to the cross sectional data in order to construct a synthetic panel. This allows us to control for unobservables, and at the same time take into account the dynamic nature of the problem by incorporating the lags of BMI. We instrument the decision to give up smoking using lags of the excise taxes on tobacco, regulations regarding tobacco use in closed spaces and family characteristics. We find these instruments compelling since, conditional on a set of controls, it is difficult to argue that policy makers decided tobacco taxes and regulated its use with the purpose of controlling voters' weight and consequently conditional on certain characteristics of the population we have to control for, they are exogenous. Finally, in order to analyze how quitting smoking affects the probability of becoming obese, we propose and estimate a logistic model for obesity prevalence. The logistic model applied to cell data can be log-linearized, so standard panel and IV methods can be directly applied to the data without loosing the properties of the logistic formulation.

According to our results a 10% decrease in the incidence of smoking leads to an average weight increase of 1Kg. to 1.5Kg. for the average cohort, that is, a 2% weight increase assuming constant height. We also find that the effect overshoots in the short run. However, a significant part of it remains even after two years. We also find that quitting smoking affects the extensive margin as well, with an implied elasticity of quitting smoking to obesity of 0.58. According to the CDC, an obese individual costs $1,400 more to the health system than a healthy person and a smoker costs 3,200$ more than a non-smoker. Taking this into consideration implies that, on average, a 1% decrease in the incidence of smoking has a a net gain of $1.4 billions: the cost of $0.6 billions is offset by the gross benefit of $2 billions.

The paper is organized as follows. In Section 1.2 we provide a list of alternative explanations for the increase in obesity rates and a review of the economics literature linking smoking to weight increase. In Section 1.3 we discuss the proposed methodology and analyze the data that we will use in the empirical analysis. In Section 1.4 we estimate the static and dynamic models of the effect of quitting smoking on Body Mass Index using the constructed pseudo-panel. In Section 1.5 we present an alternative to study the impact of quitting smoking on the probability of becoming obese. Section 1.6 concludes the paper.

1.2 Key Facts, Alternative Explanations and Literature Review

While smoking is the leading cause of death in the U.S., with up to 435,000 adult deaths each year, excess body weight is the third most important risk factor contributing to the burden of disease, most notably type II diabetes, hypertension, cardiovascular disease and disability (WHO, 2006). Flegal, Graubard, Williamson and Gail (2005) calculate that in 2000, obesity caused 112,000 excess deaths in the US, while Mokdad, Marks, Stroup and Gerberding (2004) estimate 365,000 deaths due to obesity in that same year. Life expectancy of a 40 year old obese male is 6 years shorter than his non obese counterpart and for females the figure jumps to 7 years while for younger adults the effect is even higher (General Surgeon's 2001 report). Moreover, American life expectancy is projected to decrease due to obesity, for the first time since Civil War (Olshansky, Passaro, Hershow, Layden, Carnes, Brody, Hayflick, Butler, Allison and Ludwig (2005)).

As it happens with smoking, obesity carries with it several negative externalities and therefore the social cost of being obese is higher than the individual's. One of those externalities is the increase in health care utilization. Finkelstein, Fiebelkorn and Wang (2004) found that in 2003 weight problems represented a medical expenditure of $75 billions in the U.S.. The Urban Institute updated this figure to 200 billions for 2008, half of which comes from Medicare and Medicaid. Andreyeva, Sturm and Ringe (2004) find that an obese person generates an average of $700 more in health expenditures than a comparable non obese, a figure that is even larger than the increase in health costs due to smoking. Nowadays obesity accounts for 9.1% of all medical spending in the United States, up from 6.5 % in 1998, an average of $1,400 more a year, although these costs are not distributed uniformly among the obese: as the degree of obesity worsens, the associated burden increases almost exponentially. Unfortunately the categories that account for the larger part of the burden are the ones rising at the highest rate (Andreyeva et al. (2004)). Labor productivity is another cost that is shared with the non obese. The U.S. Health and Human Services secretary estimated that obesity related problems costs $13 billions to U.S. businesses[5] and another study finds that on average, of every 100 workers, obese ones had lost 190 days per year, while normal weighted's 14 (Stevens (2004)).

a Alternative explanations

As we have seen above, the policy maker's concern about obesity is not unjustified[6]. But while most of obesity costs have been documented, we still lack a broad consensus about what caused the contemporary increase in weight and obesity rates. People put on weight when they consume more calories than they are burn off. Therefore, there are only three channels that can explain the mentioned increase. The first channel is that society, on average, started to consume more calories per day than before, keeping the same physical activities as in the past. The second one is that agents decreased the rate of calorie burning, while consuming the same amount of calories. Finally it could be due to a change in the equation relating the ins and outs of calories.

Genetics is one possible explanation of the third channel. Those that were born weighing above a certain threshold are more prone to develop obesity problems (Baird, Fisher, Lucas, Kleijnen, Roberts and Law (2005), Serdula, Ivery, Coates, Freedman, Williamson and Byers (1993) and Whitaker, Wright, Pepe, Seidel and Dietz (1997)). Moreover, if

[5]The total cost is the result of health insurance costs related to obesity ($8 billion), paid sick leave ($2.4 billion), life insurance ($1.8 billion), and disability insurance ($1 billion).

[6]The World Health Organization has qualified obesity as a disease.

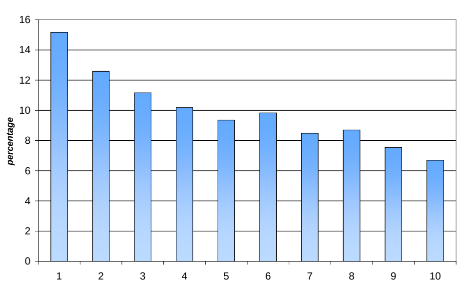
both of the parents are obese, it is more likely that their child would reach the obesity threshold (Wrotniak, Epstein, Paluch and Roemmich (2004), Whitaker et al. (1997)). So, as the proportion of obese increased in the adult population, more and more children were born at a higher risk of becoming obese during adulthood. This process might have induced a change in people's metabolism, making the burning of calories harder than before. But were this to be the case, a simple fixed effect regression would take this channel into account, unless this effect exhibited time variance. Metabolism does change through time, as adults find it harder to burn calories than youngsters. Consequently it is necessary to control for factors correlated with these changes.

The other two channels are trickier to measure, since they have several explanations that are definitely time varying. One possible justification for the decrease in the rate of calorie burning is related to technological change. Technology at work has changed dramatically in the last 30 years in favor of less physically intensive jobs (Lakdawalla and Philipson (2002)). Nowadays, the calories that used to be burnt during the labor intensive working hours have to be burnt during spare time. Therefore, the people that worked in physically intensive jobs drastically changed their pattern of physical activity without an equal change in consumption habits leading to an increase in permanent weight.

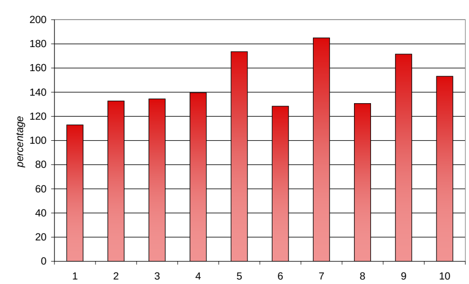
The other part of the equation has some possible explanations as well. Since 1976 food price has fallen by more than 12% compared to other goods (Lakdawalla and Philipson (2002)). Although this could be a viable explanation for the increase in average BMIfrom 1972 to 1976 and from 1984 to 1991, food prices increased sharply. Indeed, today's price of food relative to price of all items less food is only 5% lower than in 1972. However, what did change is the cost of the lowest quintile of energy density food compared to the highest quintile. Today, the cost of the former is around \$18.61/1000 kcal as compared to only \$1.76/1000 kcal for foods in the top quintile (Drewnowski, Monsivais, Maillot and Darmon (2007) and Monsivais and Jacobsenski (2007)), revealing a disproportionately unequal increase in prices. On top of this, the increase in the relative price of cooking at home, coupled with a reduction in household time, has made it harder for people to eat healthier at home (Lakdawalla and Philipson (2002)). The increasing female participation in the labor market also made eating outside unavoidable for some households (Fokuda (2006) and Jacobsen (2006)). This problem has been confounded by the growth of the fast food industry[7], decreasing the cost in time of eating outside.

Two features are clear from figure 1.3a the prevalence of obesity across income deciles is such that the lowest decile of income has the largest ratio of obese, and, the decrease in the prevalence of obesity is almost monotonic with increases in income. In fact, the lowest income decile of the population has a rate of obesity that almost doubles that of the highest decile. Both the story of food prices and of technology at work are suitable for explaining the distribution of obesity across income.

Figure 1.2: Obesity Ratio by Income decile



(a) Obesity Prevalence



(b) Obesity Prevalence Growth Rate

Source: Behavioral Risk Factor Surveillance System

However, figure 1.3bshows that the increase in obesity rates between the 80's and the 00's was similar for all deciles and even slightly larger for the richest ones. This suggests two issues. First, obesity increase shares some common characteristics among the different income deciles. Second and most importantly, the preceding explanations

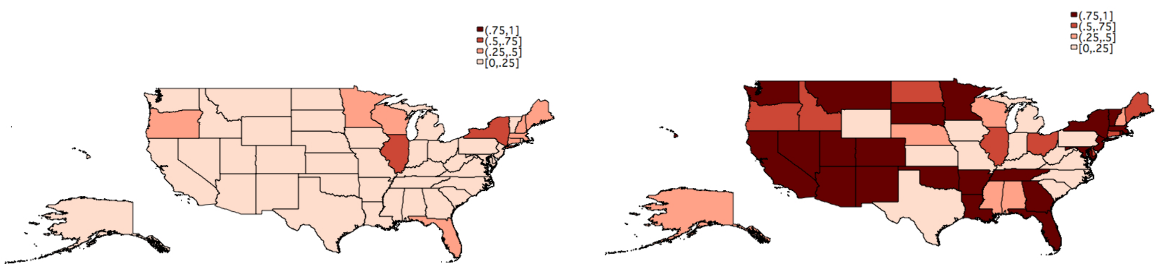
[7]13% in a 10 year period according to the National Retail Census.

for weight increase are at odds with this stylized fact. To begin with, if technology at work changed the physical intensity of labor, it did so for the poorest deciles and not for the richest ones. The highest income earners, be it professionals or white collars, were already making little or no physical effort in their work. On the other hand, the poorest income deciles are more prone to budget constraints and therefore more affected by changes in the price of unhealthy food. But this is rarely the case with the higher incomes, as they tend to be more sophisticated in their eating habits and incorporate better food into their diet. Thus, it is hard to explain changes in obesity rates in the first deciles of income using arguments that are best suited for the lowest income deciles.

A good story for modern obesity rates has to explain not only the raise in BMI but also the fact that it affected all income deciles similarly, although it had more incidence on the highest income earners. The decline in smoking rates is a potential candidate for two reasons. First, it affected a significant part of the population. Indeed at the beginning of the eighties, almost 30% of the American population was an active smoker. Moreover, while by 2008 that ratio decreased to less than 20%, it is still remains a significant part of the American population (Figure **??**). Second, that decline was due, among other reasons, to a very aggressive campaign to ban smoking for most public places. Society's demands regarding a healthier environment forced the introduction of a number of changes in the regulation regarding tobacco use (Figure 3a and 3b) reshaping the average American smoking habit. Government and private offices, restaurants, recreational facilities, retail stores and educational institutions, all of them suffered some sort of restriction which in some cases manifested in smoking bans within private buildings and their immediate surroundings. These restrictions, however, had an unequal impact. They affected the most those that worked in offices and ate frequently in restaurants. As a matter of fact, already by 1993 nearly 82% of indoor workers faced some restriction on workplace smoking and 47% worked in a 100% smoke-free environment (Farrelly, Evans and Sfekas (1999))

Information policy regarding smoking was yet another reason that helps explain the decline in smoking rates. Smoking advertisements were banned from TV and other mass media and supported by an increase in published information focusing on the causal links between smoking and adverse health[8]. High income and educated individuals were at least as likely to be affected by the anti-smoking campaign as they were in a better position to accumulate, process and understand this information and correctly update their costs of smoking.

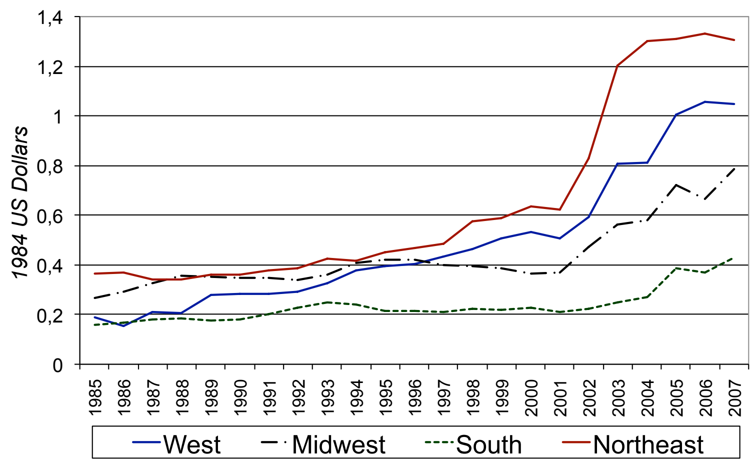
Figure 1.3: Clean Indoor Air Regulations and Excise Taxes: 1990 versus 2007



(a) CIA Regulations 1990 (b) CIA Regulations 2007

Source: Own Recopilation of State Laws Regarding Tobacco Use

Figure 1.4: Real Excise Taxes on Tobacco by Zone: 1985 - 2007



Source: Own Recopilation of State Laws Regarding Tobacco Use

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[8]A policy that took full strength during the last two decades, beginning with the 1980 General Surgeon's report on the subject.

b Literature Review

Several authors have studied the weight impact of smoking in recent years. However, most of them have investigated the question in a reduced-form, that is, assuming that increases in tobacco prices reduce smoking rates and through this channel impact weight. While it is frequently assumed in the literature that this is actually the case (see for instance Chaloupka (1999)), price increase is not the only mechanism to induce people to quit nor is it the most relevant one. Chou et al. (2004) is the first paper we know in the economic literature to link the increase in BMI to smoking. The authors adapt a behavioral model of the determinants of obesity to pooled individual-level data from the Behavioral Risk Factor Surveillance System, matched with the prices of food cooked at home and fast food, tobacco and alcohol prices, and the number of per capita restaurants and fast foods chains, as well as indicators for the regulation regarding tobacco use in private offices and restaurants. They analyze the determinants of BMI in a reduced-form OLS regression with state level fixed effects and a quadratic time trend. Other regressors include the demographic characteristics of individuals, the prices of several commodities and the number of fast food restaurants. The authors account for the fact that certain regressors are likely to be related to BMI in a non-linear fashion. Among the main conclusions of the study, they find that increases in cigarette prices significantly increases BMI as well as obesity rates[9] and also that it helps explaining a significant proportion of the increase in BMI (up to 20%). Therefore, the authors find that tobacco consumption substituted net calorie ingestion as a habit. This result is very important since it says that part of the increase in BMI is due to policy decisions.

However, the study has some potential flaws. Firstly, the channel of identification is that increases in cigarette prices and the tightening of the regulation regarding tobacco use induced people to quit smoking, reduced the frequency of smoking or deterred the

[9]A unitary increase in cigarette price leads to an increase of 0.486 in BMI and a 10% increase in the cigarette price would raise by 0.445% the probability of becoming obese for an individual.

starting of the habit. As mentioned above, there are other reasons, such as health problems, that could explain why some individuals quit the habit. Moreover, tobacco companies could be raising prices in response to a diminishing pool of smokers or authorities could be responding to tobacco derived health problems by raising taxes on its use. In addition, if smoking and eating are substitutes, then the significant effect should be found among perennial smokers or former smokers, but not among those who have never smoked. Unfortunately, the framework the authors use is unable to discriminate among the different subgroups.

Secondly, the authors do not attempt to control for unobserved heterogeneity among individuals. Because eating and smoking are closely connected to preferences then raises concerns about the consistency of the estimation. In particular, risk attitudes might will operate through the exclusion restriction. Thirdly, the study does not account for dynamics in the dependent variable. In the case of BMI this is certainly a problem since adjustment costs are non-negligible. Moreover, the estimated equation does not separate the short run effect from the long run one. A fourth issue is nonlinearities. The average effect might be significantly different from the effect in the obese and overweight sample. This is relevant because the health consequences of the tobacco policy would be higher if the effect is larger for the obese. Moreover, and perhaps more importantly, the reported elasticity of smoking to BMI and obesity is too large to truly believe in the results.

Finally, the regression is not satisfactorily robust. Using the same data set and a similar specification, Gruber and Frakes (2006) finds the opposite effect, that is, increases in tobacco prices significantly decreases BMI ($1.00 rise in tobacco taxes lowers BMI by 0.151 and the probability of becoming obese by 1.5%). The main differences between the two papers arise due to the use of state excise tax on tobacco instead of tobacco prices and through differences in time effects estimation (the latter authors introduce year dummies rather than a quadratic time trend) . Gruber and Frakes (2006) also instruments the smoking decision by means of a 2SLS regression, where in the first stage they regress the smoking odds against the tobacco excise tax. However, even after correcting for the potential endogeneity, the resulting coefficients again are too large to be plausible. As in the Chou et al. (2004) their estimation considers neither unobserved heterogeneity and error clustering nor the dynamic problem.

Rashad (2006) performs a similar analysis of Chou et al. (2004) extending the dataset to food and caloric intakes. He separates the analysis to see how the increases in prices and regulation affected caloric intake and tobacco use. Contrary to what should be expected, increases in tobacco prices and regulation did not affect smoking but it did increase caloric intake while changes in food prices did not change caloric intake but changed smoking decision. Nevertheless, he does nothing to correct the mentioned problems in the previous specifications.

Baum (2008) addresses some of the issues of the previous papers through a difference in difference approach, using changes in cigarette prices as the treatment. People that smoked at least 100 cigarettes before the age of sixteen are assigned as the treated group and people that didn't smoke before this age as the control group. He finds a similar result to Chou et al. (2004), that is, a rise in either prices or taxes increases BMI and the likelihood of becoming obese, regardless of the time controls. However, two caveats should be mentioned. First, the study does not use the same dataset as the previous authors and therefore, comparison is limited. Second, he relies on the same assumption as the two previous papers, that raising cigarette costs will lead to a decrease in smoking.

In a very interesting and recent exercise, Courtemanche (2009) revisits Chou et al. (2004), Gruber and Frakes (2006) and Baum (2008) and puts them together using not only the contemporaneous cigarette price/tax but also their lags. He finds that while in the short run increases in cigarette prices might lead to opposite results, in the long run each and every one of the three specifications leads to a decrease in BMI. Moreover, he finds that the decrease in weight is due to both better eating and more exercise. However, because the data he uses is cross-sectional, matching the individual with previous period taxes might lead to an error, in particular if it is done at the state level. Also, the survey used has self reported answers and the error in reported food consumption and exercise should not be overlooked. In addition, a very small fraction of the observations was given the food complementary survey and sample size drops substantially. Last but not least, he uses only increases in the price/tax of cigarettes and does not look at the other dimensions of the anti-smoking campaign.

Eisemberg and Quinn (2006) is the only paper we found that does not rely on the assumption that changes in cigarette prices affects cigarette consumption. In the study the authors use the Lung Health Study, a randomized smoking cessation trial with 5,887 smokers. Unconventionally, however, the authors use weight instead of BMI as the dependent measure and find that the effect of quitting smoking is a weight increase of 10 kg. This paper solves some of the issues mentioned before, however, it is not clear if the entire smoking cessation sample do indeed quit permanently.

1.3 Data and Methodology

a Data Set Description

The main source of data that we use is the Behavioral Risk Factor Surveillance System, for the period spanning 1984-2007. This is the same data set as in Chou et al. (2004) Gruber and Frakes (2006), with additional waves. The BRFSS is a phone survey designed as a series of independent cross sections with the intention of obtaining information regarding the prevalence of unhealthy habits and behavioral risks among the US population above 18 years and living in family households[10]. The BRFSS survey started in 1984 and since 1995 all states have been participating continuously. The number of yearly interviews has been constantly increasing and by 2007 it was more

[10]More information is available at www.cdc.goc/nccdphp/brfss.

than 270,000. The survey is a rich source for demographic and economic status variables including state of residence, number of children, race, family income, education, marital status and age. The survey asks the subjects weight in pounds and height in foot and inches. We transform these measures into the metric correspondence and from this we calculate the Body Mass Index, calculated as height over weight squared. From this survey we also obtain information on tobacco and alcohol consumption, including whether the person has smoked more than 100 cigarettes during his/her life, whether he or she currently smokes, the number of cigarettes smoked, whether the individual has ever tried to quit and if the individual drinks regularly. Because the data in the survey is self-reported, in order to avoid extreme self reporting bias, we only include observations for people that reported a BMI above 13 and below 100, the complete valid sample yields us 3,286,800 observations[11].

Other sources of data are the Bureau of Labor for the state unemployment rate, consumer price index, food price and number of fast food restaurants, and the Bureau of Economic Analysis for the quarterly per capita income of the state which are used to control for the business cycle. Finally, we complete the data set with an index of regulations regarding tobacco use and the effective real tax on tobacco that we develop using data from the National Cancer Institute State Legislative Database Program. Following Chriqui, Frosh, Fues, el Arculi and Stillman (2002) we accountfor all the effective changes in state regulations regarding tobacco use from 1970 to 2007 that affected the ability of a smoker to smoke in his daily activities. However, we only concentrated in those laws that had an effective enforcement.

In order to construct the index we identified seven different categories: Government offices, Private offices, Restaurants, Recreational public places, Hospitals, Educational facilities and Public Transport. The index goes from 0 to 5 for each category, except

[11]In the first survey, information was only available for 15 states and the number of useful observations was around 23,882. Although the survey has been growing in scope and coverage, unfortunately the number and quality of questions changes through time. For instance the question on the number of cigarettes smoked is not available after 2000 and the drinking variable is not asked every year.

for transport that goes from 0 to 3. The higher the number, the tighter the regulation. The categories are 0 for no regulation, 1 whenever there is some restriction to smoke but does not impose a high cost on the smoker in terms of his time budget, two if smokers and non smokers have to be in a separated room, three if smoking is banned in certain areas, four if smoking is prohibited within the building and five if it is also prohibited in the surrounding areas of the building. For instance, a category 5 in Private office means not only that smoking is not allowed in private places of work, but also within a certain distance from the entrance to the building. Whenever the law creates an important exception, we subtractone point from the index. Because small and medium firms employ a large proportion of US workers, the deduction was considerably higher in case the law exempted this type of business. Using this regulatory data, we constructa new variable that tries to capture tightness of the regulations regarding tobacco consumption in the state. We addthe punctuation the state received in each category and normalize the new variable by its maximum possible score to make it continuous between 0 and 1.

Table 1.2: Summary Statistics: Individual variables. 1985 - 2007

	Full Sample		**Never-Smokers**		**Current**		**Past**	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Smoke Currently	0.23	0.42	0	0	1	0	0	0
Smoke Ever	0.49	0.5	0	0	1	0	1	0
BMI	26.49	5.59	26.47	5.63	25.78	5.46	27.14	5.54
Obese	0.20	0.403	0.21	0.41	0.17	0.38	0.23	0.42
Drink	0.53	0.50	0.48	0.50	0.59	0.49	0.57	0.50
Women	0.58	0.49	0.64	0.48	0.56	0.50	0.5	0.50
Age	47.29	16.22	45.71	16.56	43.8	14.54	53.41	15.26
White	0.81	0.39	0.78	0.41	0.81	0.39	0.86	0.35
Black	0.08	0.27	0.09	0.29	0.08	0.27	0.05	0.22
Hispano	0.06	0.23	0.07	0.25	0.05	0.22	0.04	0.20
Married	0.59	0.49	0.61	0.49	0.50	0.50	0.64	0.48
Divorced	0.16	0.37	0.12	0.33	0.24	0.43	0.16	0.37
Widowed	0.09	0.28	0.09	0.28	0.07	0.26	0.11	0.31
Kids	0.46	0.50	0.48	0.50	0.50	0.50	0.38	0.48
School Dropout	0.11	0.31	0.09	0.28	0.16	0.37	0.11	0.31
High School	0.32	0.46	0.28	0.45	0.39	0.49	0.31	0.46
Some College	0.27	0.44	0.26	0.44	0.28	0.45	0.27	0.44
College	0.31	0.46	0.37	0.48	0.17	0.37	0.31	0.46
Real Income	40.8	22.65	42.77	22.91	34.6	20.8	42.17	22.72
Unemployed	0.04	0.20	0.03	0.18	0.07	0.26	0.03	0.17
Exercise	0.66	0.47	0.69	0.46	0.58	0.49	0.68	0.47
General Health	0.85	0.36	0.88	0.32	0.79	0.4	0.82	0.39

Note: All variables have 3,286,800 useful observations except for the Drink variable which has 2,892,973
The sample contains 1,685,770 never-smokers, 743,216 current smokers and 856,418 former smokers
Source: Behavioral Risk Factor Surveillance System 1985-2007

Table 1.3: Summary Statistics: Enviromental Variables. 1985 - 2007

	Mean	Std. Dev.
Price Food Away	162.94	3.67
Price Food Home	160.41	4.69
Number Fast Food Restaurant per capita	1.83	0.32
Tobacco Price	2.8	0.8
Excise Tax on Tobacco	0.54	0.43
Alcohol Price	165.49	5.67
Tax on Beer	1.72	0.49
Clean Indoor Air Regulation Index	0.31	0.31
Restaurants	1.54	1.74
Private Offices	1.17	1.61
Government Offices	1.94	1.77
Recreational Places	1.81	1.78

Table 1.2 contains summary statistics of all the variables we use in the study. Several things are worth mentioning of this first exploration of the data. As we can see, the average sample individual is overweight. Almost 25% of the sample smokes although with a large variance. This is due to the fact that throughout the years, smoking rates have diminished considerably. The unconditional average amount of cigarettes smoked in the sample is four. In order to control for other habits we include whether the individual drinks regularly and whether they exercise regularly. In the sample, 53% of the individuals reports to drink regularly[12], 66% reports doing exercise regularly, 46% of the sample has kids, 31% has a college degree and almost 60% is married.

Table 1.2 also reports the different summary statistics for the never-smoker, current smoker and past smoker groups. The first group is the one that shouldn't be affected by changes in taxes on tobacco or regulation regarding tobacco use. The second group is the one we would be interested in using in a randomized experiment of quitting smoking. Because that is not available, we are going to compare it to the third group, that is, quitters. As we can see, the last group is the one with the largest BMI, while smokers are the group with the lowest, this difference being statistically significant. The average profile of a quitter is usually is someone who is in their fifties, married, white and enjoys a higher level of education and real income than the average smoker. These statistics confirms our initial beliefs. It is hard to argue that technological change

[12]Although only 13% are binge drinkers, results available on request.

or food price changes are the main forces behind the increase in weight. Moreover, it shows that the group with the largest BMI is also the group that has stopped smoking. Table 1.3 reports the summary of the environmental variables.

b Econometric Methods

Individual weight is a stock variable and it is the result of the combination of genetic, metabolic, behavioral, environmental, cultural, and socioeconomic influences. Accordingly, weight increment is the result of consuming more calories than what is burnt. A natural question then is which of the factors is more relevant to explain the increase in U.S. obesity rate. Weight at birth, weight of the parents, gender and ethnicity are among the most relevant genetic variables that influenceit. This group of variables are invariant trough time and as a result, more related to the steady state weight and not to changes per se, although they might be deeply related to how calories are processed. In the group of cultural, behavioral and environmental variables the main determinants are civil status, family composition, education, place of residence, veteran of war, employment situation, industry, tenure, hours of work, household income, wife work status, health status, previous period weight and relevant habits. This second group of variables contains variables both constant in time and some that exhibit time variation. The third group of variables, socioeconomic, consists primarily of food prices, sin goods prices and regulations, all of them time variant. So, in order to investigate the effect of quitting smoking on the individual weight we have to control for this thee groups of variables. Finally, in order to make weight comparable across individuals it is necessary to normalize it. This is usually done by dividing it by height squared, which is called Body Mass Index. This will be the outcome variable in our study.

So for individual i, who resides in state j at year t, the effect of quitting smoking on weight ideally would be estimated through the following equation:

$$BMI_{ijt} = \alpha + \beta_1 X_{ij} + \beta_2 Z_{ijt} + \gamma \text{Quit Smoking}_{ijt} + \delta_j + \delta_t + \eta_i + u_{ijt} \tag{1.1}$$

Some of the variables mentioned above are not present in the BRFSS dataset but unfortunately correlated with both weight and quitting smoking. A second problem with the dataset is its cross-sectional structure which does not allow us to control for unobserved heterogeneity with fixed effects. Because, preferences for health can explain both BMI and smoking, not being able to include fixed effects for the individuals might bias the estimation. An additional problem, not related to the dataset, is the fact that quitting smoking, as a decision, might be influenced by BMI, as was previously explained in the introduction. As a result, the coefficient of interest in equation (1.1) , that is γ, will suffer from conditional bias and there is no a priori idea of the direction.

Fortunately, the fact that most of the non available variables are fixed through time allows us to control for them by means of fixed effects. That is, if panel data would be available, this problem could be solved by treating η_i as a fixed effect, using a transformation of the model or parameterizing the conditional expectation of the individual effects as a function of the explanatory variables. Therefore, solving the unobserved heterogeneity problem means also solving the missing variables one. In order to do that we use cohort analysis. This technique, developed by Deaton (1985) and further improved by Browning, Deaton and Irish (1985), Moffit (1993) and Collado (1997) among others, allows us to control for fixed effects, use lags of variables as instruments. The basic idea of this procedure is to construct population means of the cohorts, in order to form a panel structure for the data. To do that, Deaton (1985) recommends to divide the population in cells with homogenous individuals and to form cohorts according to one or several characteristics which remains constant in time. For that purpose, in our dataset we could consider date of birth, sex, race and residential location.

Equation (1.1) then will be transformed into equation (1.2), where now BMI_{cqt} stands for the BMI of cohort c at quarter q and year t.

$$BMI_{cqt} = \alpha + \beta_1 X_c + \beta_2 Z_{cqt} + \gamma \text{Quit Smoking}_{cqt} + \delta_t + \eta_c + \epsilon_{cqt} \tag{1.2}$$

When analyzing cohort data we must bear in mind that all cohort variables[13] are error ridden measurements of the true cohort population means. The advantage with respect to standard errors-in-variables models is that we can estimate the variances of the measurement errors using individual data. Moreover, if the size of the cohort is large enough, sample means approximate well enough their population counterparts.

We define n_{cqt} as the size of cohort c in quarter q of year t. Every element of $\bar{X}_{cqt}$, for example a dummy for education, is the average (proportion) of individuals in that category of eduction observed for individuals belonging to cohort c in quarter q of year t, and analogously for other variables in the model. The main estimation problem is that $\bar{\eta}_c$ is unobservable and likely correlated with some variables in $\bar{X}_{cqt}$. Therefore, equation (1.2) does not constitute an appropriate base for obtaining consistent estimates, unless the size of the cohorts is large enough. In this case, $\bar{\eta}_c$ is a good approximation to η_c, and we can replace $\bar{\eta}_c$ by a set of binary variables (fixed effects) one for each cohort. A natural estimator then, is the covariance or within groups estimator based on the weighted means of the cohorts, where the weights take into account potential heteroskedasticity between cohorts.

Let $\bar{X}_c = (\sum_{q=1}^{Q} \sum_{t=1}^{T} n_{cqt})^{-1} X_{cqt}$ be the average of the observed means for cohort c, and define $\bar{Y}_c$ analogously. Then $\hat{\beta}_{WG}$ will be biased in small samples but it will be consistent as n_{cqt} tends to infinity if standard assumptions about second order moments are met. There exists a trade-off between variance and bias of the estimator. That is, the bigger is the number of cohorts (C), the smaller is their size (n_{cqt}).The trade-off has to be solved

[13] This includes the cohort specific effect.

in such a way that the variation within cohorts is small, i.e. homogenous individuals, while the variation between cohorts is large, i.e. heterogenous cohorts. Identification of the true parameter requires that the expectation of each element conditional on the cohort identifying variables varies with time. On the other hand, as we have pointed before, enough people in each group or cohort is necessary for the average within a group to be an unbiased estimator of the population mean. Browning et al. (1985) mention that 150 individuals per group is a relatively good number to avoid sampling bias. In this study, we are going to use only those cells with more than 100 individuals within.

b.1 Dynamic Specification

Adjusting ones weight is a costly procedure that takes time. As a consequence, past period weight can be a determinant of today's. The BRFSS does not ask about weight in earlier time periods. Thus, previous authors were unable to incorporate dynamics into their estimations, with the resulting potential omitted variable bias in their estimations. Using cohort analysis also gives us the possibility of estimating dynamic models from individuals observations at a single point in time. In this case, the equation to estimate is:

$$BMI_{cqt} = \alpha + \rho BMI_{c(q-1)t} + \beta_1 X_c + \beta_2 Z_{cqt} + \gamma \text{Quit Smoking}_{cqt} + \delta_t + \eta_c + \epsilon_{cqt} \quad (1.3)$$

Unfortunately, including lagged BMI might also lead to a bias if the panel is too short (Arellano and Bond (1991)). Therefore, dynamics posses yet another methodological issue to solve. The methodology of Blundell, Duncan and Meghir (1998) can be used to address that problem by means of a system GMM when individual data is used. Furthermore, Collado (1997) proposes an instrumental variables estimator based on first differencing the model, which corrects the error-in-variables problem for dynamic models in the context of cohort data.

The estimation procedure in those cases relies on the idea that internal lagged instruments can be found, if they are not correlated with future error terms. While the lagged dependent variable is correlated with past error terms and uncorrelated with the current and future error terms, some of the other variables are potentially endogenous given that they are correlated with the current error. Though, if we assume that they are uncorrelated with future error terms, the system GMM includes a restriction which assumes that although lagged BMI might be correlated with the unobservable, the first differences are uncorrelated with $\eta_c + \epsilon_{c,q,t}$, which implies that deviation from long term trends in BMI are not correlated with individual effects.

Fortunately, when the number of available periods is large enough, the error-in-variables problem tend to disappear as shownin Nickel (1981), Browning et al. (1985) and Jiménez, Labeaga and López (1998). Since we have data on almost 100 quarters, we can estimate the dynamic specification without instrumenting BMI's lag. Therefore, we have two potential methods to estimate consistently the effect of quitting smoking on weight in a dynamic setup.

c A first exploration of the data

As a first attempt to understand the issues at hand, we replicatethe results of both Chou et al. (2004) and Gruber and Frakes (2006), with some minor differences. The only correction we make for self-reporting bias is to restrict BMI to lie within the range of 13 to 100. In addition we replace tobacco prices as used in Chou et al. (2004) with data from the Tax Burden on Tobacco[14].

[14]Chou et al. (2004) source their data from the ACCRA cost of living index, which is not publicly available, unlike our measure current measure.

The first two columns of Table ?? show our replication of the original formulation of Chou et al. (2004) with the original sample years and with the full sample years. The third and fourth column refer to the Gruber and Frakes (2006) specification with the years used in the published paper and with the complete waves respectively. The sixth column is the specification we will test.

Table 1.4: Replication Table: Chou and Gruber with individual data

	Chou's		Gruber's		Ours
	1985-1999	1985-2007	1985-2002	1985-2007	1985-2007
	b/se	b/se	b/se	b/se	b/se
Quitting Smoking					0.699***
					(0.02)
Cigarette Price	0.490***	0.431***			
	(0.13)	(0.12)			
Cigarette Price Squared	-0.09***	-0.063***			
	(0.026)	(0.019)			
Tax Tobacco			-0.092***	-0.046	-0.031
			(0.03)	(0.05)	(0.06)
Clean Indoor Air Regulation					0.072
					(0.05)
Private Office	-0.144**	-0.022	0.004	-0.038	
	(0.07)	(0.05)	(0.05)	(0.05)	
Restaurants	0.043	0.020	0.030	0.066	
	(0.05)	(0.05)	(0.04)	(0.05)	
Price Food Away	-0.389***	-0.412***	0.016***	0.017***	-0.217*
	(0.06)	(0.09)	(0.00)	(0.00)	(0.12)
Food Away Squared	0.001***	0.001***			0.001*
	(0.00)	(0.00)			(0.00)
Price Food Home	-0.090	0.110	-0.011***	-0.006	-0.059
	(0.08)	(0.07)	(0.00)	(0.00)	(0.08)
Price Food Home Squared	0.000	-0.000*			0.000
	(0.00)	(0.00)			(0.00)
Fast Food Establishment	1.238***	1.600***	-0.317***	-0.406**	-0.091
	(0.27)	(0.54)	(0.05)	(0.16)	(0.20)
Fast Food Establishments Squared	-0.353***	-0.507***			
	(0.07)	(0.14)			
Price Alcohol	-0.013***	-0.012***			
	(0.00)	(0.00)			
Tax Beer			-0.017	-0.003	0.021
			(0.04)	(0.11)	(0.11)
Drink					-0.898***
					(0.02)
Exercise					-1.060***
					(0.03)
Linear Trend	Yes	Yes	No	No	No
Year Dummies	No	No	Yes	Yes	Yes
Quarterly Dummies	No	No	No	No	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes
Observations	938,746	2,956,132	1,499,474	2,956,132	2,636,461

The variables included but not reported are: Race (White, Afroamerican, Hispanic, other); Education (Drop out, High School, Some College, College), Marital status (Married, Separated, Divorced, Widowed, Single), Demographic (Gender, Age, Age squared, Children) and Income (Real Income, Real Income squared, Unemployed). No correction for miss reporting of the BMI. Observations are not weighted. White-Huber Robust Standard Errors clustered by State reported.

A first thing to check is whether the estimated coefficients are sensitive or not to the number of waves included. As we can see, the Gruber and Frakes (2006) finding that BMI decreased with increases in tobacco prices is no longer significant once we use the 1985-2007 waves. Accordingly, the only result that does not depend on the sample is Chou et al. (2004), that is, raising tobacco prices leads to an increase in BMIWhen we include quitting smoking as one of the determinants of BMI, the effect of the tax on tobacco and regulations regarding tobacco use is not significant. However, quitting smoking it is.

This means that Chou et al. (2004) and our specification have similar conclusions, although our specification does not rely on a reduced form assumption such that increases in tobacco prices leads people to stop smoking. Our next priority then, is to replicate the exercises of Chou et al. (2004) and Gruber and Frakes (2006) with cohort data and see the impact that unobserved heterogeneity has on the estimated coefficients.

1.4 Cohort Analysis

a Cohort Definition

Cohorts are defined using the following characteristics: Year of birth, Gender and Geographical region of residence and data is aggregated by quarter and year. Since each cell is the average of individual observations within the cohort, dummy variables will be transformed into the proportion of people within a cell that have a certain characteristic. For instance, currently smoking is defined as either 0 or 1, therefore the transformed cohort variable will tell us the proportion of people among the cohort that smoke.

The structure of the sample in terms of the aggregation variables is the following:

- Year of birth: This grouping has 5 possible categories corresponding to different decades of birth. The first category is for those born before 1940 while the last one is for those born after 1970. The largest proportion of the male population was born during the 50's while the largest proportion of females were born before the 40's.

- Sex: The data set over represents females as they are 58% of the sample.

- Geographical Area: geographical location has been divided into the four categories that the Bureau of Labor uses to produce the CPI. The Southern region is the one more represented, while the Northern East region is the one with the fewestobservations, both for males and females

When we include quitting smoking as one of the determinants of BMI, the effect of the tax on tobacco and regulations regarding tobacco use is not significant. However, quitting smoking it is.

Table 1.5: Observations per cell

Male					
			Region		
Decade of Birth	West	Midwest	South	Northeast	*Total*
Before 1940	69,419	68,961	98,963	51,591	288,934
1940 - 1949	62,746	54,615	82,166	45,493	245,020
1950 - 1959	83,589	76,180	99,764	61,055	320,588
1960 - 1969	70,820	67,530	91,551	55,167	285,068
After 1970	61,532	52,875	76,052	41,122	231,581
Total	348,106	320,161	448,496	254,428	1,371,191
Female					
			Region		
Decade of Birth	West	Midwest	South	Northeast	*Total*
Before 1940	101,199	109,418	164,657	80,580	455,854
1940 - 1949	79,172	70,200	118,296	60,696	328,364
1950 - 1959	104,164	94,798	143,852	79,240	422,054
1960 - 1969	91,505	86,442	133,692	75,258	386,897
After 1970	80,195	70,150	114,740	57,355	322,440
Total	456,235	431,008	675,237	353,129	1,915,609

Using this cohort definition and taking into consideration that our dataset goes from the first quarter of 1984 to the fourth semester of 2007 we have a total of 3,680 potential observations. Unfortunately some data needed to adjust household income[15] is not available in the 1984 survey, and as a result we dropped that year, leaving a total of 3,520 potential observations. Following Blundell et al. (1998), we dropped from the analysis those cells with less than 100 observations in order to avoid sampling bias [16], resulting in 3,439 useful observations.

b Static Specifications

b.1 Accounting for Unobserved Heterogeneity in Chou and Gruber Specifications

Since the pseudo panel allows us to apply regular fixed effects analysis, and with that to control for confounders like unobserved heterogeneity, we first explore how sensitivethe results of the Chou et al. (2004) and Gruber and Frakes (2006) specifications are to a fixed effect regression. Recall that Chou et al. (2004) find that cigarette prices significantly increase BMI under a quadratic time trend and a quadratic effect of prices while Gruber and Frakes (2006) use a specification linear in the cost of cigarettes and yearly dummies.

As we can see in table 1.6 the linear effect of tobacco prices on BMI[17] in the Chou et al. (2004) specification is now higher than in the OLS regression using individual data. Once fixed effects are included the value drops to almost half[18]. The Gruber and Frakes (2006) price effect is also reduced significantly after including fixed effects[19].

[15]Household income is coded as an interval variable so we adjusted the values it by means of a interval regression. In order to do this, several variables were used as predictors, including the number of individuals that live in the house, a question that is asked from 1985 onwards.

[16]Many states were only incorporated after 1995 and the number of interviews has also increased through time and thus some cells have very few observations within. Therefore this is does not represent an endogenous problem between BMI and the number of observations within a cell.

[17]the total effect at the average is 1.35+2*(-0.217)*2.55=0.24.

[18]the total effect also drops almost half to 0.16.

[19]However, it is true that the coefficients are higher in absolute value than when using individual

Table 1.6: Gruber and Chou using cohort data: OLS versus Fixed Effects

	Chou's		Gruber's	
	OLS	FE	OLS	FE
Tax Tobacco			-0.322***	-0.274***
			(0.09)	(0.10)
Cigarette Price	1.35***	0.72**		
	(0.35)	(0.30)		
Cigarette Price Squared	-0.217***	-0.109*		
	(0.07)	(0.059)		
Private Office	0.000	0.281	0.073	-0.104
	(0.29)	(0.31)	(0.24)	(0.27)
Restaurants	-0.275	-0.112	-0.097	0.020
	(0.17)	(0.17)	(0.16)	(0.15)
Tax Beer			-0.558***	-0.174**
			(0.09)	(0.08)
Price Alcohol	-0.016***	-0.006**		
	(0.00)	(0.00)		
Price Food Away	-0.497***	-0.351***	-0.003	0.007*
	(0.09)	(0.08)	(0.00)	(0.00)
Food Away Squared	0.001***	0.001***		
	(0.00)	(0.00)		
Price Food Home	0.028	0.023	-0.007**	-0.002
	(0.11)	(0.10)	(0.00)	(0.00)
Price Food Home Squared	-0.000	-0.000		
	(0.00)	(0.00)		
Fast food establishment	3.837***	4.072***	0.249	-0.244
	(0.89)	(1.04)	(0.16)	(0.19)
Fast food establishment Squared	-1.011***	-1.135***		
	(0.26)	(0.27)		
Time Trend	Yes	Yes	No	No
Year Dummies	No	No	Yes	Yes
Observations	2,149	2,149	2,378	2,629

$* p < 0.10; ** \ p < 0.05, \ *** p < 0.01$

The variables included but not reported are: Race (White, Afroamerican, Hispanic, other); Education (Drop out, High School, Some College, College), Marital status (Married, Separated, Divorced, Widowed, Single), Demographic (Gender, Age, Age squared, Children) and Income (Real Income, Real Income squared, Unemployed). Observation weighted by the number of individual in the cell. Only cells with more than 100 observations within included White-Huber Robust Standard Error reported. Original sample years used

This points out that unobserved heterogeneity is an important force behind the results obtained by both papers The immediate question is whether the effect in a structural model is significantor not.

data.

b.2 Analysis of Quitting Smoking with Fixed Effects and Instrumental Variables

In this subsection, we conduct regression analyses of the effect of quitting smoking on BMI in a structural model. Using the constructed cohort data, we are now are able to estimate equation 2 and correct for the potential bias that the simple OLS estimation has. For that, we introduce fixed effects in order to control for unobserved heterogeneity and we instrument the decision to quit smoking in order to estimate the causal effect on BMI. The specification is the same as Chou et al. (2004), except for the inclusion of the decision to quit, the non-parametric time controls, and the use of excise tax on tobacco instead of tobacco prices.

The decision to quit the habit of smoking is instrumented using a one year lag[20] of the tax on tobacco and the numbers of adults within a house. These instruments, from an ex ante point of view, satisfy the exclusion restriction of not being a predictor of contemporaneous BMI, as it is very hard to argue that local governments introduced changes in tobacco taxes in order to modify the weight of the voters. On the other hand, they are relevant for quitting smoking. A 10% increase in tobacco taxes leads to a 4% decrease in smoking prevalence (U.S. Department of Health and Human Services (2000)) and smoke free workplaces reduce smoking incidence by 6%. Nevertheless, we report both Hansen's test for excluded restriction and Cragg-Donald's test for instruments weakness (Stock, Wright and Yogo (2002)), in order to know if they are good from an ex post analysis. The findings for *Quitting Smoking* are reported in Table 1.7.

Column (A) contains the estimates for the decision to quit smoking using an OLS regression[21]. The effect of quitting smoking on BMI is negative and significant, something at odds with the same regression using the individual data, yet the effect is small in terms of BMI's variability. The specification in column (B) contains cohort fixed ef-

[20]In the present context, that is a four period lag, since our data is aggregated by quarters.

[21]As explained in Table 7, several controls were included. Except for the price of food at home, the availability of fast food restaurants and having children, all the other controls have the expected sign.

Table 1.7: Quitting Smoking Effect on BMI

	A: OLS	**B**: FE	**C**: IV FE	**D**: Log	**E**: Num Cig	**F**:Men	**G**: Women
Quitting Smoking	-0.333**	0.261	4.990*	0.174*		5.320	2.723*
	(0.17)	(0.26)	(2.57)	(0.10)		(2.72)	(2.51)
Smoke Intensity					-0.257		
					(0.37)		
Observations	3,389	3,389	3,299	3,299	2,179	1,523	1,528
Hansen J test			0.08	0.09	5.62	1.31	0.46
Hansen J p-value			0.78	0.76	0.02	0.52	0.80
Excluded Instruments			1.00	1.00	1.00	2.00	2.00
Weak Stat Test			22.30	22.30	1.43	6.32	11.73

$*p < 0.10; **\ p < 0.05,\ ***p < 0.01$

(A) is the OLS estimation. (B) is the Fixed Effect regression, (C) is the Fixed Effect regression instrumenting the decision to quit. (D) is similar to (C) but uses the log of BMI. (E) uses the change in the number of cigarette as the endogenous variable. The variables included but not reported are: Race (White, Afroamerican, Hispanic, other); Education (Drop out, High School, Some College, College), Marital status (Married, Separated, Divorced, Widowed, Single), Demographic (Gender, Age, Age squared, Children) and Income (Real Income, Real Income squared, Unemployed), Prices and Regulations(Price Food Away, Price Food Home, Tax Tobacco, Tax Beer, Clean Indoor Regulations, Number of Fast Food Establishments) and Drink. Yearly and quarterly dummies included. Quitting smoking is instrumented using Tax on Tobacco (-4) and Number of Adults in the House (-4). Observation weighted by the number of individual in the cell. Only cells with more than 100 observations within are included. White-Huber Robust Standard Error reported. BRFSS from 1985 to 2007

fects. As we can see, once unobserved heterogeneity is taken care of, the sign on the coefficient changes and the effect becomes insignificant. This means that the omission of unobserved confounders introduces a negative bias on the estimated coefficient.

Once we add instrumental variables for the decision to quit smoking, column (C), the effect increases and turns significant once again, which is further evidence of the direction of the bias in the OLS regression. Quitting smoking has a positive effect on weight once unobserved heterogeneity has been taken care of and the decision to quit instrumented. The implied elasticity of quitting smoking to BMI is 0.048[22]. That is, a 10% decrease in the incidence of smoking leads to an increase of 1.5 Kg. in the weight of the average cohort, that is, a 2% increase, assuming a constant height. A Hansen J test on the validity of the exclusion restriction fails to reject the null hypothesis, which means that the instruments are not rejected as such. This test is similar to Sargan's test but allows for heteroskedasticity and therefore more suitable for our specification. On the other hand, the Cragg-Donald's test on weak instruments is above 20, which means that the estimated effect is within the 5% bias interval, so we need not be worried that

[22]Full tables are available upon request.

the results are driven by the wrong set of instruments.

Specification (D) reestimates equation (1.2) using the log of BMI instead of BMI. As we mentioned in the introduction, BMI is the result of dividing weight by height squared. Since the information in the survey is self reported, the measurement error regarding weight and height would not be linear and as a result the standard conclusions of measurement error in the endogenous variable do not apply here. In that sense, the log of BMI will log linearize the error. The estimated effect in this case, 17%, is the growth rate of BMI after quitting smoking and it has a similar value to the one implied in the linear specification. Finally, column (E) studies the impact of decreasing the intensity of smoking but marginally. As we can see, small changes in rate of smoking does not seemto have a significant effect, although the effect is positive [23]. This means that only the complete abandonment of the addiction has a significant impact on weight but minor therapies do not.

Several robustness checks have been performed to see how sensible the results are. We have repeated the experiment including in the cohort only those individuals for whom the habit of smoking is already developed, that is, with individuals 26 years or older. Also, we have tried with more lags of the instruments and with other instruments as well. Finally we have tried a different definition for quitting smoking. Instead of using the proportion of former smokers in the cell we used the change in the number of active smokers. In all the cases the result remains relatively unchanged, although the power of the instruments do change and sometimes the Hansen test is not rejected in the margin. [24]

Gender Differences Column (F) and (G) repeats the experiment of (C) splitting the sample between men and women. As we can see, the effect of quitting smoking

[23] the regressor here is changes in the number of cigarette smoked. As a result, the effect is positive for reductions. It should be noted that the question on the number of cigarettes smoked was discontinued after 2000 and therefore sample size is smaller.

[24] Results available upon request.

is significant for men but not for women[25]. This is at odds with the medical literature (Basterra-Gortari, Forga, Bes-Rastrollo, Toledo, Martínez and González (2010), which finds that both women and men gain weight. The potential explanation for this difference is that women are penalized more than men when they deviate from their "optimal" weight. As a matter of fact, the likelihood of an obese or overweighted women getting married or being hired is significantly lower than that of a men of similar demographics (Hamermesh and Biddle (1994), V. Atella and Vuri (2007), Cawley (2000), Cawley and Danziger (2004) and Brunello and D'Hombres (2007)). As a result, women will act in consequence and will probably eat healthier than their men counterparts or do more exercise in order to avoid the negative consequences of gaining too much weight[26].

Persistence in Time Longitudinal data allows us to test in the context of the static model the time persistence of the effect. The evidence so far says that quitting smoking leads to an increase in weight, but there is no evidence of whether such an effect remains in time or if it vanishes after a few quarters. As a matter of fact, it could well be that the weight which is gained after leaving the addiction is lost in the middle run, like a Christmas or Thanksgiving day effect of eating too much. On the contrary, it could be that the effect remains there, changing permanently the weight of the person. To answer that question we have regressed BMI on the lags of quitting smoking, in order to see whether the effect remains significant after several periods.

Table 1.8 shows the effect of the different lags of quitting smoking on contemporaneous BMI. That is, specification (C) using the contemporaneous variable, the first lag, fourth (one year) and eighth (two years). The first thing to notice is that even after two years the effect remains significant and positive, although it diminishes moderately after one year, leaving the increase to an approximately 14% weight growth. This means that

[25] As far as we are aware, this is the first study that finds a difference between men and women.

[26] However, there is no evidence whether the health consequences of obesity differs between the two groups.

Table 1.8: Persistence in Time

	Fixed Effects with Instrumental Variables			
	Contemporary	**Lag 1)**	**Lag 4**	**Lag 8**
Quitting Smoking	4.990*			
	(2.57)			
1st Lag Quitting Smoking		4.386**		
		(2.22)		
4th Lag Quitting Smoking			3.602*	
			(2.17)	
8th Lag Quitting Smoking				3.45*
				(2.04)
Observations	3,299	3,299	3,299	3,188
Hansen J test	0.08	0.03	1.17	0.227
Hansen p-value	0.78	0.86	0.28	0.6337
Excluded instruments	1	1	1	1
Weak Instrument Statistic	22.30	24.92	18.96	23.37

$*p < 0.10; ** \ p < 0.05, \ ***p < 0.01$

Same controls as in the previous regression. Observation weighted by the number of individual in the cell. Only cells with more than 100 observations within included White-Huber Robust Standard Error reported. BRFSS from 1985 to 2007

the steady state weight of the quitter increases after leaving the habit but the dynamics are such that the effect overshoots initially.

c Results for the Dynamic Model

As commented in the introduction, adjustment costs make last period weight an important determinant of today's. Cohort data allows us the possibility to include this variable and instrument it using internal instruments. However, because the panel is large enough, in principle the usual Arellano-Bond problem should not be present here. Nevertheless, we have estimated equation (3) instrumenting and without instrumenting BMI's lag.

Table 1.9 presents the results of the effect of quitting smoking in equation (3). Specification (F) includes cohort fixed effects and instruments the decision to quit smoking[27]. Specification (G) instruments lagged BMI using the difference in the lag of BMI, as in Collado (1997). Specification (H) uses the log(BMI) as the independent variable and its lag as one of the regressors.

[27]Non reported controls are the same as in specification (C) while the instruments for decision to quit smoking are Tax on Tobacco (-4, -8 and -12).

Table 1.9: Dynamic setup

	H	I	J	K: Women	L:Men
Quitting Smoking	4.426**	5.121**	0.155*	3.059	5.290*
	(2.20)	(2.42)	(0.08)	(2.44)	(2.74)
Lagged BMI	0.433***	0.069*		0.165***	0.023
	(0.02)	(0.04)		(0.02)	(0.03)
Lagged log(BMI)			0.470***		
			(0.02)		
Observations	3,299	3,299	3,299	1,528	1,523
Hansen J test	1.50	0.52	1.56	0.37	1.44
Hansen p-value	0.47	0.77	0.46	0.83	0.49
Exc Ins	2.00	2.00	2.00	2.00	2.00
Weak Stat Ins	16.33	12.29	16.30	11.71	6.23

$*p < 0.10; **\ p < 0.05,\ ***p < 0.01$

The variables includes but not reported are: Race (White, Afroamerican, Hispanic, other); Education (Drop out, High School, Some College, College), Marital status (Married, Separated, Divorced, Widowed, Single), Demographic (Gender, Age, Age squared, Children) and Income (Real Income, Real Income squared, Unemployed), Prices and Regulations(Price Food Away, Price Food Home, Tax Tobacco, Tax Beer, Clean Indoor Regulations, Number of Fast Food Establishments) and Drink. Quitting smoking is instrumented using Tax on Tobacco (-4, -8, -12) and the Lag of BMI using the difference of it. Observation weighted by the number of individual in the cell. Only cells with more than 1000 observations within included. White-Huber Robust Standard Error reported. BRFSS from 1985 to 2007

In the first three specifications, H, I and J, Quitting Smoking is positive and significantand of a similar magnitude as in the static model. In this context, a 10% decrease in the incidence of smoking leads to a weight increase of about 1.5 Kg. Lagged BMI is positive and significant in all three specifications, although the magnitude substantially changes when it is instrumented. As a result, the effect of quitting smoking is similar even after taking into consideration the initial situation of the stock variable. On the other hand, the static model conclusions about the differential effect between women and men are also present in the dynamic set up. As we can see, quitting smoking has a significant effect only for men.

d Robustness Check: Different Cohort Definition

To conclude this section we redefinethe structure of the cohort. Cohort definition plays an important role in controlling for unobserved heterogeneity and a valid question is whether our results are driven by a particular definition. To see how sensitive the results are, we introduced race as one of the variables that define the cohort[28].

As a result of this new definition, more cohorts are added which allow us to get more variation and as before we only utilize those cohorts with more than 100 individuals[29]. Table 1.10 re estimates specification A, B, C and D using the new cohort definition.

Table 1.10: Quitting Smoking Effect on BMI: Cohort definition including Race

	A*:OLS	**B***:FE	**C***:IV FE	**H***:Dynamic
Quit Smoking	0.253	0.635***	3.071*	3.402**
	(0.21)	(0.20)	(1.73)	(1.69)
Obs	4,971	4,971	4,783	4,783
Hansen			2.70	1.87
Hansen p-value			0.26	0.39
Excluded			2.00	2.00
Weak Instrument Statistic			27.12	26.00

$* p < 0.10; ** \ p < 0.05, \ *** p < 0.01$

The variables includes but not reported are: Education (Drop out, High School, Some College, College), Marital status (Married, Separated, Divorced, Widowed, Single), Demographic (Gender, Age, Age squared, Children) and Income (Real Income, Real Income squared, Unemployed), Prices and Regulations(Price Food Away, Price Food Home, Tax Tobacco, Tax Beer, Clean Indoor Regulations, Number of Fast Food Establishments) and Drink. Yearly and Quarterly dummies included.
Quitting smoking is instrumented using Tax on Tobacco (-4 and -8) and Number of Adults in the House (-4).
Observation weighted by the number of individual in the cell. Only cells with more than 100 observations within included. White-Huber Robust Standard Error reported. BRFSS from 1985 to 2007

With this new definition we reduce the scope for bias at the expense of increased variance of the estimator. Nevertheless, similar results obtain[30].As we can see, the effect of Quitting Smoking in the static specification (C*) is positive and significant as in the previous cohort definition, although the implied weight growth rate is 14% instead of 19%. In the dynamic specification (F*) the effect is again positive and significant

[28]We tried also to aggregate using month instead of quarter and using States instead of Regions. Similar results were obtained, although the number of cohorts with more than 100 observations was considerably lower.

[29]In this case only 67% of the cohorts remains after removing those with less than 100 observations.

[30]The coefficients are slightly smaller than before and the OLS estimation (A*) is now positive, although not significant as before.

and of the same magnitude as in the previous cohort definition. As a consequence, the redefinition of the cohort does not bring any substantial modification to the conclusions. We safely conclude that quitting smoking increases permanently the weight of a person.

1.5 The Extensive Margin: An Investigation on the Probability of becoming Obese

To conclude the analysis of cohort data in the context of weight and smoking, we should have a better understanding of the impact that quitting smoking has on the increase in the probability of becoming obese.

A natural specification to investigate the effect of a set of variables on the probability of being obese, given a set of covariates X would be $E[p(Obese)|F(X'\vartheta)]$ where the standard choice of F is the logistic function of the form $\Lambda(z) = \frac{e^x}{1+e^x}$[31], that evaluates the expectation by nonlinear least squares. Unfortunately, as discussed throughout the paper, the explanatory variable of interest, quitting smoking, is potentially correlated with the error term. In addition unobserved heterogeneity can bias the estimation. Each of these problems could be dealt with separately. But the two at the same time are much harder to solve.

A more appealing approach to dealwith these concerns simultaneously, is an equation of the form $p(Obese) = \Lambda(X^{'}\beta+u)$, where u is correlated to X but not to set of instruments Z. Using the logistic transformation of the obesity variable into $y = F^{-1}(p(Obese)) = log(\frac{Obese}{1-Obese})$ the model now allows one to linearly instrument the variables and even use fixed effects through the generalized method of moments as in Arellano and Bover (1995).

Obesity has been traditionally defined as BMI above 30, while overweight is a BMI between 25 and 30. Although thesethresholds are widely used, critics point out that

[31] Alternatively the probit function can also be used.

thesetwo measures do not take into consideration different bone structures or different lifestyles. For instance, American football players will weighmore than a person of their same height, yet in general one would not consider them as overweightor obese. Therefore, we implementdifferent thresholds to determine the participation rate, that is, whether an individual is obese or not, going from a BMI of 25 to a BMI of 40. Consequently, for each threshold the aggregation of individuals in each cohort that are obese according to the threshold, gives us the proportion of obese in each category. This set of variables is the one we used to estimate whether quitting smoking affects the probability of becoming obese. Equation 1.4 represents the dynamic specification in which we assume that the probability of being obese is affected by its own past

$$log(\frac{Obese}{1-Obese})_{c,j,t} = \alpha + \rho BMI_{c,j,t-1} + \beta X_{c,j,t} + \gamma \text{Quit Smoking}_{c,j,t} + \delta_j + \delta_t + \eta_c + u_{c,j,t} \tag{1.4}$$

In our exercise we repeatedly estimate a static ($\rho = 0$) and a dynamic version of equation 1.4 by varying the threshold for obesity. Figure 1.5 and figure 1.6 presents the implied elasticities of obesity to quitting smoking for the static and dynamic versions of the model.

As we can see in figure 1.5 and figure **??**, the elasticity of the probability of becoming obese after quitting smoking is positive and significantboth for the static and the dynamic specification[32]. However, due to a diminishing sample size, standard errors increase exponentially above the 30 threshold. Consequently, a 1% decrease in the incidence of smoking leads to an increase in the probability of becoming moderately obese by 0.58%. However, there is no significant evidence that the probability of becoming severely obese is affected.

[32] In a model with the regressors in logs is calculated as $\hat{\xi} = \hat{\beta}(1-\hat{p})$.

Figure 1.5: Elasticity of the Effect of Quitting Smoking on different thresholds of Obesity: Static Model

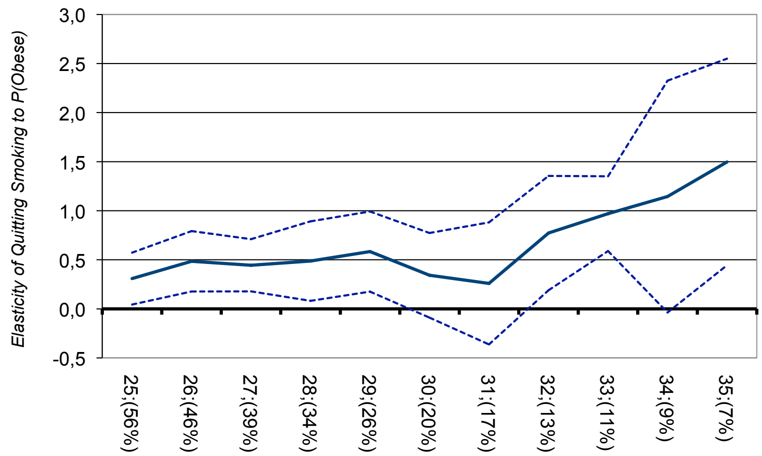
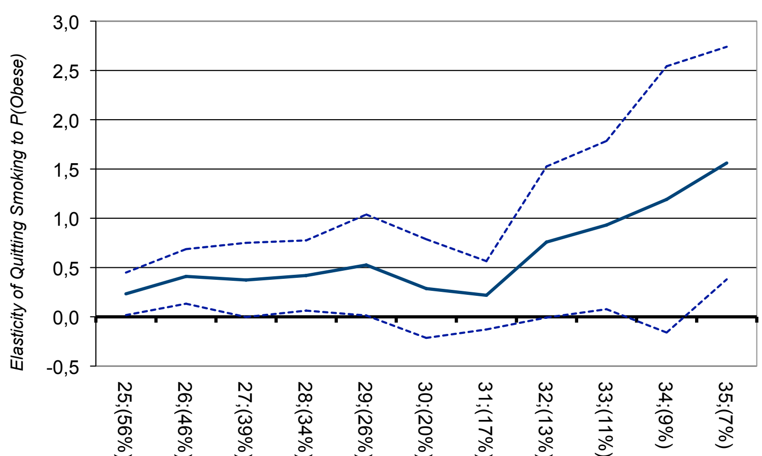


Figure 1.6: Elasticity of the Effect of Quitting Smoking on different thresholds of Obesity: Dynamic Model



1.6 Conclusions

Policy makers are enacting a great number of new policies that aim at reducing health risks such as tobacco consumption and obesity. This study contributes to a very relevant policy question. Do we have to worry about the implementationof policies that aim to reduce one health risk in the society without internalizing the externalities? This is a question similar to that of the theory of the second best: in a context of multiple market imperfections, reducing one imperfection does not guarantee an improvement in welfare.

This paper studiesthe consequences that reducing smoking rates on obesity. We use a structural modelthat has the advantage that it is not necessaryto assume that people stop smoking through higher cigarette prices. Instead the treatment, in this case quitting smoking, is incorporated in the estimation to evaluate its impact on BMI. In addition we instrument the decision to quit smoking and use cohort analysis to control for unobserved heterogeneity.

We find that reducing the incidence of smoking in society leads to another disease which is as bad as the one that is removed. We should remember that while smoking kills prematurelyup to 435,000 adults each year, it is estimated (Mokdad et al. (2004)) that obesity kills 365,000 and the gap between thesestatistics is closing down quickly.

We show that smokers increase their weight significantly after they leave the habit of smoking. This conclusion is robust to the introduction of dynamics into the estimation and to changes in the definition of the cohort.According to our results a 10% decrease in the incidence of smoking leads to an increase of 1Kg. to 1.5Kg. for the average cohort, that is, a 2% weight increase for a constant height. We also find that the effect overshoots. However, a significant part of it remains even after two years.

The results indicate that both unobserved heterogeneity and the endogeneity of the decision to give up smoking downward biases the effect of quitting. We also show that marginally reducing the number of cigarettes that a person smokes does not have a significant effect on weight. This means that the effect on weight comes through the extensive margin of the substitution effect, and not from the intensive margin. Another relevant finding is that quitting smoking only significantly affects the weight of the male population and not the female population.

Using logistic analysis we have also shown that quitting smoking leads to an increase in the odds of becoming overweightand obese. According to our findings, the elasticity of quitting smoking to obesity is 0.58. The Department of Health and Human Services has estimated that the average cost of healthcare for a person is $8,000, while for a smoker it is $11,200, that is, an additional $3,200, and for an obese it is $9,400. Given that 20% of the adult population currently smokes, 1% decrease in the incidence of smoking would then have an average benefit of $2 billions. However, once we take into consideration the elasticity of quitting smoking to obesity, the net average benefit decreases to $1.4 billions.

Our results suggest that for successful implementation, anti-smoking campaigns should be coordinated with campaigns for obesity reductions, be it in the form of a better diet or more exercise. If society is demanding more tight policies regarding habits with negative externalities on health, people should be prepared to pay a larger cost than what was previously thought. Although the net benefit is estimated to be positive, we risk that individuals quit a habit only to substitute it with another without reducing the cost on public health.

2 CHAPTER TWO

2.1 Introduction

Concerns about the economic implications of the relationship between alcohol consumption and the labor market are very well grounded. So far, most of the empirical literature has maintained the commonly held view that alcohol drinking is associated with lower earnings, greater unemployment and lower productivity. Nevertheless, some authors (Forcier (1988), Catalano, Cooley, Wilson and Hough (1993)) emphasize that the direction of the causality between unemployment and alcohol consumption is not conclusive. While some studies have shown that unemployment is positively correlated with alcohol consumption (Kessler, Turner and House (1987)), with alcohol abuse (Crawford, Plant, Kreitman and Latcham (1987)) and with diseases and psychological problems derived from alcohol abuse (Catalano et al. (1993)), other analyses suggest that the correlation is either nonexistent or even runs in the opposite direction (Ettner (1997), Ruhm and Black (2002)).

A common argument within the first set of studies is that unemployment originates a situation of financial strain, which induces the individual to canalize stress through the consumption of alcohol (Peirce, Frone, Russell and Cooper (1994)). While some authors support the existence of a positive relation between financial strain and depression, understanding chronic financial strain as a situation in which it is difficult to satisfy basic needs (Kessler et al. (1987), Hamilton, Hoffman and Renner (1990)), others have found a positive relation between depression and alcohol consumption (Hartka, Johnstone, Leino, Motoyoshi, Temple and Middleton (1991)). Analyses within the second set argue that unemployment usually implies lower consumption through an income effect. This reduction should not happen when unemployment is transitory and the unemployed receive benefits or family support.

An important part of the previous literature uses aggregate data and thus, is based on the assumption of a representative consumer. This could be a non-realistic approach when economic and sociological factors (intrinsic to individuals) coexist and they do not allow to generalize results about participation and alcohol consumption. Recent papers use individual data and therefore relax the representative consumer assumption (Dee (2001) and Ruhm and Black (2002) are two good examples) but fail to take into consideration unobserved heterogeneity. In this paper we propose a further step to explicitly consider unobserved heterogeneity among individuals. Since panel data with enough frequency for the data is not easily available, in particular one with enough frequency and geographical discrimination, we will rely on cohorts built from independent cross-sections taken from the *Behavioral Risk Factor Surveillance System* (BRFSS from now on) to control for unobserved heterogeneity.

Another important issue in estimating of alcohol consumption is the recognition of the habit formation inherent in the consumption of alcoholic drinks. In this view, the accumulated past use of alcohol has an effect on current consumption[1]. For the same reasons that results could be misleading when unobserved effects are not taken int consideration, the omission of dynamics in the consumption (participation) equation could affect very much the results. So, we also try to reconcile our results with those in the previous literature in the context of a rational addiction framework. There are nowadays a lot of papers analyzing the existence of rationality in the consumption of several goods from the seminal paper of Becker and Murphy (1988). Becker, Murphy and Grossman (1994), Moore and Cook (1995), Grossman, Chaloupka and Sirtalan (1998), Bentzen, Ericksson and Smith (1999) and Baltagi and Griffin (2002) constitute some interesting examples.

Finally, it might not be a good idea to pool data for drinkers (positive consumption) with data for non-drinkers (zero consumption). It could be done when we are sure that zeros correspond to non-purchasing the good at the reference period of the survey. But

[1]something that implies that long-run elasticities will be larger than short-run ones

the questions asked at the BRFSS do not allow for this possibility. So, zeros could correspond to non-participants or potential participants that have quitted drinking at that time. Under this possibility, potential drinkers could have and effect on the elasticities in the future an since they can transit either from employment to unemployment or the reverse, their non explicit consideration can bias the estimates of the effect of the unemployment rate.

The objectives of the paper are therefore threefold. The first and fundamental one is to show the effects of misspecification caused by missing unobserved heterogeneity. Omission of unobserved effects could producer bias in the parameter estimates or their standard errors. As a previous step we also want to replicate results obtained by previous authors, especially by Dee (2001) and Ruhm and Black (2002) in samples of different time dimension, in order to avoid the critique of obtaining different results because of using different sample periods. This first step will also help us in interpreting the results produced by more complex models. The second objective is to introduce habit formation in the form of dynamics into the estimation model in order to take into account the nature of the drinking habit, that is either myopic or rational. The third aim consists in emphasizing the effects of pooling zero and positive observations over the specification that we need to propose.

Our results confirm that, once the unrestricted specifications are estimated, unemployment is not a significant determinant of the decisions of becoming drinker and consuming alcohol. These results are robust to several specification exercises as well as several time periods.

The structure of the paper is the following one: in section 2 we review the literature and describe the methodological aspects of the models studying the relation between alcohol consumption and the cycle and we propose alternative specifications. Section 3 describes the dataset. Section 4 is devoted to comment on the results using individual and cohort data. In section 5, we propose econometric and economic interpretation of the results. Finally, section 6 summarizes the main conclusions.

2.2 Model and relationship to previous literature

a The model

Let's assume that Y is an indicator of whether an individual is or not a drinker, binge drinker or chronic drinker or that Y constitutes the number of drinks consumed by him, whose latent variable Y^* is a linear function of some explanatory variables. We observe Y as result of comparing the utility of consuming a number of drinks including zero consumption. So, the observability rule is $Y = \mathbb{1}[Y^* > 0]$ for the binary choice being a drinker or not or $Y = \max(Y^*, 0)$ for the number of drinks consumed, where $\mathbb{1}[A]$ is the indicator function of event A.

Consider a general linear model for the latent variable:

$$Y^*_{ismt} = \alpha + \beta X_{ismt} + \gamma UR_{smt} + \delta_m + \lambda_t + \eta_i + \varepsilon_{ismt} \tag{2.1}$$

where the observed counterpart of Y^*_{ismt} denotes alcohol consumption (number of drinks) or the decision to drink of individual i interviewed in state s in month m of year t. X is a vector of explanatory or control variables for the individual i, UR refers to the state unemployment rate in month m and year t, α, δ, λ and η are state, month, year and individual unobserved factors, and ε is the error term.

Let suppose that X gathers all the determinants of the probability of being drinker or of the number of drinks consumed. Then, this model is equivalent to the one proposed by Dee (2001), in which we allow the possibility that the dependent variable be limited or qualitative (binary or a count).[2]

[2]From the specification above we can also generate simultaneous models if we established a double

b Previous literature

The nature of the data used in the literature that examines how alcohol consumption is affected by environmental factors is usually aggregate data -like for instance state alcohol sales - or cross sectional data, as the one from the National Health Interview Survey. Unfortunately, both types of data render problems identifying the impact of unemployment rate, or income, on the drinking outcome.

In the case of aggregate data (Freeman (2000), Ruhm (2000)) there are several things that might potentially curtail proper identification. Firstly, it is necessary to claim for the existence of a representative consumer in order to identify the effect of unemployment and the rest of the variables in the model. Also, the set of covariates is usually very limited and therefore or omission of relevant variables is a potential problem (i.e., personal attitudes towards alcohol, legislation, advertising or dynamics in consumption). Third, some times the connection between the events lacks a proper model for choosing the lag structure. For instance, Brenner (1975, 1979) argued that during recessions, mortality rates increased, although there was a lag between the growth of unemployment rate and the increase in mortality rates. Nevertheless, Brenner's work has been very criticized by other authors as Gravelle, Hutchinson and Stern (1981), Stern (1983) or Wagstaff (1995) arguing absence of rationality in the election of the unemployment rate lag. Finally, Freedom (1999), for instance, has pointed out econometric problems such as unit roots or omission of relevant variables (i.e., personal attitudes towards alcohol, legislation, advertising or dynamics in consumption).

Single cross sectional data as in Ettner (1997) and Ruhm (1995), where the 1988 National Health Interview Survey is used, shares a similar problem to aggregate data: we can't identify the effect of unemployment on alcohol since we may be confounding

hurdle decision for consuming alcoholic drinks (Tobit type II, for example if Y were continuous for a part of the sample or Hurdle-Poisson or negative binomial for the decision and the counts). In case that variables affecting participation and consumption were the same and had identical effects over both decisions, we would be in the case of a standard Tobit (Poisson or negative binomial) model.

the impact of economic conditions with unobserved determinants of drinking that vary across states. In a photography of individual alcohol consumption, the advantage is that X will contain a wide range of demand determinants (income and socioeconomic characteristics) but the disadvantage is that we are not going to be able to establish causal effects relating alcohol consumption and the economic cycle. An additional problem is the potential endogeneity of the unemployment rate since poor health may be the cause rather than the consequence of unemployment (Janlert, Asplund and Weineball (1991)). Some authors (for instance Hammarström, Janlert and Theorell (1998), have tried to test health status of employed and unemployed workers but only have managed to capture part of the impact of changes in economic conditions, since recessions do not affect only the unemployed workers.

Luoto, Poikolainen and Uutela (1998), Bobak, McKee, Rose and Marmot (1999), Dee (2001) and Ruhm and Black (2002) have used pooled cross sectional data to evaluate the impact of unemployment on drinking participation and alcohol consumption. Luoto et al. (1998) uses finish data and a very simple regression analysis and finds that unemployment is weakly yet significantly related to alcohol consumption only during recessions. Bobak et al. (1999) employs another quite simple set up and finds that in Russia, unemployment is positively correlated with alcohol intake, but only for men. Dee (2001) estimates the effect of the state unemployment rate on alcohol participation, alcohol consumption and binge drinking, in a model like equation 2.2, where X includes age, gender, race/ethnicity, education, month, year and state dummy effects. The study uses the Behavioral Risk Surveillance System from 1984 to 1995.

$$Y^*_{ismt} = X_{ismt}\beta + UR_{smt}\gamma + \alpha_s + \delta_m + \lambda_t + u_{ismt} \tag{2.2}$$

When including state fixed effects, as well as marital status and education, the author finds that unemployment rate and alcohol consumption are negatively related, although it has not a significant effect on participation. There are, however, three potential problems with the study. First of all, the period of choice is problematic since it was not until 1994 that all the states where included in the survey. Secondly, there is no attempt to control for some observables like beer price, health status and/or retirement status. Given the fact that beer price and health status might be correlated with business cycle, this casts some doubts about the findings. Not only that, because the author includes retirees in the sample, the finding that participation is not significant might be due to the fact that the sample includes people that should not be affected by the business cycle. Last but not least, the nature of cross sectional data precludes the possibility of controlling for unobserved heterogeneity and to model habits either in the form of myopic habits or as in the rational addiction model.

Ruhm and Black (2002) re estimates equation 2.2 using the same source of information yet includes more waves of the survey and corrects for some of the potential problems in the previous study. For instance, the authors include the tax on beer as one of the regressors and they also control for whether the individual has been unemployed for a long term unemployed or if it is recent and state trends instead of yearly dummies. They find that drinking participation is insensitive to unemployment, while conditional drinking is sharply pro-cyclical, meaning that the decrease in bad economic times is due to changes in existing drinkers. However, the study does nothing to correct for unobserved heterogeneity.

Failing to control for unobserved heterogeneity requires some extra assumptions in order to identify the parameters of interest. We need to assume that $\eta_i = \eta$ for all i, such that Ordinary Least Squares (in case Y were to be a continuous variable) would provide consistent estimates of the parameters or else we only require absence of correlation among the η's and the regressors for the consistency of the parameters with individual random effects. In any case, from an economic point of view a model that does not allow correlation between individual effects and explanatory variables does not seem very interesting. For example, if individual tastes were correlated with professional occupation, then the coefficients corresponding to occupation would be biased when unobserved effects are not controlled for. If unemployment rates were different across occupations, then correlation with unobserved heterogeneity moves to the variables that proxy the economic situation. When panel data is available, this problem can be solved by treating η_i as fixed effects, using a transformation of the model or parameterizing the conditional expectation of the individual effects as a function of the explanatory variables. Obviously, it is not possible to apply these strategies if we do not have repeated observations for the same individuals, as it happens in Dee (2001) and Ruhm and Black (2002).

c The pseudo-panel approach

Since the data used in Dee (2001), Ruhm and Black (2002) is a combination of independent cross-sections, we cannot control for unobservable characteristics affecting consumption decisions (i.e., preferences for working, different tastes, religious beliefs, genetics, etc.). Moreover, unobserved variables could be correlated with regressors in equation **??** and so, the effect of unemployment on consumption would not be properly identified. We can deal with this problem by constructing pseudo-panels. Deaton (1985) suggests to divide the population in homogeneous groups (cohorts) according to one or several characteristics. At the population level, groups have to contain the same individuals along time. The basic idea of this procedure is to construct population means of the cohorts, in order to form a panel structure for the data. Since cohort population means are not observable, we can use their sample analogs to proxy them, being aware that we end up with an errors-in-variables model. The advantage with respect to standard errors-in-variables models is that we can estimate the variances of the measurement errors using individual data. Moreover, if the size of the cohort is large enough (Deaton (1985), establishes 150 observations per cell), we can forget measurement errors because sample means approximate well enough their population counterparts.

From equation 2.1, we derive the cohort specification by adding up in $i \in c$ (that is, for all individuals who satisfy the aggregation criterion defined) and dividing by the sample size of the group. Thereby we have:

$$\bar{Y}_{cqt} = \bar{X}_{cqt}\beta + \overline{UR}_{cqt}\gamma + \bar{\alpha}_s + \bar{\delta}_m + \bar{\lambda}_t + \bar{\eta}_c + \bar{\varepsilon}_{cqt} \qquad c = 1, ..., C \tag{2.3}$$

We define n_{cqt} as the size of cohort c in quarter q of year t. Every element of $\bar{X}_{cqt}$, for example a dummy for education, is the average (proportion) of individuals in that category of education observed for individuals belonging to cohort c in quarter q of year t, and analogously for other variables in the model. The main estimation problem is that $\bar{\eta}_c$ is unobservable and probably still correlated with some variables in $\bar{X}_{cqt}$. Therefore, 2.3 does not constitute an appropriate base for obtaining consistent estimates unless the size of the cohorts is large enough. In this case, $\bar{\eta}_c$ is a good approximation to η_c, and we can replace $\bar{\eta}_c$ by a set of binary variables (fixed effects) one for each cohort.

Then a natural estimator is the covariance or within groups estimator based on the weighted means of the cohorts, introducing weights to take into account potential heteroskedasticity between cohorts. Let $\bar{X}_c = (\sum_{q=1}^{Q} \sum_{t=1}^{T} n_{cqt})^{-1} X_{cqt}$ be the average of the observed means for cohort c, and define $\bar{Y}_c$ analogously. Then:

$$\hat{\beta}_{WG} = [n_{cqt}(\bar{X}_{cqt} - \bar{X}_c)'(\bar{X}_{cqt} - \bar{X}_c)]^{-1}[n_{cqt}(\bar{X}_{cqt} - \bar{X}_c)'(\bar{Y}_{cqt} - \bar{Y}_c)]$$

$\hat{\beta}_{WG}$ will be biased in small samples but it will be consistent as n_{cqt} tends to infinity if standard assumptions about second order moments are met. There exists a trade-off between accuracy and number of pseudo-panel observations. The bigger is the number of cohorts (C), the smaller is their size (n_{cqt}), which implies a trade-off between bias and variance of the estimator.

d Consumption and habits: The myopic and rational addiction models

The longitudinal dimension of our cohort data not only allows us to control for unobserved effects, it also gives us the opportunity of including dynamics in the specification of the model of the volume of alcohol consumption. In particular we can model the estimation taking into consideration habits, either the myopic or the rational version of them. The myopic version of the model in equation 2.3 introduces the lagged outcome as a regressor (Frank (2002) and Luo, Abdel-Ghany and Ogawa (2003)).

$$\bar{Y}_{cqt} = \theta\bar{Y}_{cqt-1} + \bar{X}_{cqt}\beta + \overline{UR}_{cqt}\gamma + \bar{\alpha}_s + \bar{\delta}_q + \bar{\lambda}_t + \bar{\eta}_c + \bar{\varepsilon}_{cqt} \qquad c = 1, ..., C \tag{2.4}$$

However, nowadays there are a lot of papers analyzing the existence of rationality in the consumption of several goods from the seminal paper of Becker and Murphy (1988). Becker et al. (1994), Moore and Cook (1995), Grossman et al. (1998), Bentzen et al. (1999) or Baltagi and Griffin (2002) constitute some compelling examples. Assuming quadratic utility (see Becker et al. (1994)), the consumption dynamics can be expressed as:

$$\bar{Y}_{cqt} = \theta\bar{Y}_{cqt-1} + \theta\mu\bar{Y}_{cqt+1} + \bar{X}_{cqt}\beta + \overline{UR}_{cqt}\gamma + \bar{\alpha}_s + \bar{\delta}_q + \bar{\lambda}_t + \bar{\eta}_c + \bar{\varepsilon}_{cqt} \qquad c = 1, ..., C \tag{2.5}$$

where $\mu = 1/(1+\kappa)$, and κ is the rate of time preference assumed to be equal to the interest rate in the rational addiction model. An important implication of the 2.5 is that the error terms is autocorrelated. In this case, neither lagged or forward values of the outcome variable are valid instruments. Identification relies then in the availability of instruments correlated with consumption but uncorrelated with the error term as we explain in the empirical section below.

Therefore, as instruments for consumption we are going to use something standard in the literature, that is, lags and forwards of taxes on beer, taxes on tobacco, regulations on tobacco use, as well as the number of adults in the house. Justification for this instruments is straightforward. On the one hand, according to the National Institute on Alcohol Abuse and Alcoholism, beer accounts for half of the total alcohol consumption in 44 states and over 60% in 23 states and it is a relatively homogeneous product with sales dominated by three firms, changes in the tax on beer will affect alcoholic consumption. On the other hand, since tobacco is considered a complement of alcohol, an increase in the cost of tobacco, be it pecuniary or non pecuniary, will affect the consumption of alcohol.

2.3 Data

The main dataset is the BRFSS for the 1985-2008 period in which each wave constitutes an independent cross-section. This survey is a joint project of the Center for Disease and Control Prevention (CDC) and the US states and territories. The survey is a program designed by the CDC's Behavioral Surveillance Branch (BSB) to measure the behavioral risks of the population 18+ living in family households.

The BRFSS is a phone survey designed to give state uniform and specific information of the prevalence of health habits, including alcohol consumption[3]. Uniform data collection procedures ensures the comparability of the data from one point in time to another, as well as over a given period of time, across selected populations and geographic areas. The results are used by public head officials to determine the problematic areas in their states, to develop prevention policies and intervention strategies, and to evaluate

[3]Researchers who have approached the issue about the validity of self-reports of alcohol consumption, have concentrated their efforts in the direction of under-reporting, and have tended to discount the possibility of over-reporting behaviors by attributing false positives to measurement errors (Midanik, 1989)

success in reducing the prevalence of behaviors that affect public health[4].

For the first survey (1984) information is available on just 15 states. However, since 1995 all the states plus the District of Columbia have been participating continuously. The questions referring to alcohol consumption are located in the main module and are made to all individuals in the sample, except for the 1994, 1996, 1998 and 2000 that they were located in the optional module. The valid, in the sense they respond the alcohol consumption variable, sample size for the period 1985-2008 is 3,644,491 observations[5].

a Description of the variables

The survey reports several questions on alcohol consumption. First, respondents are asked whether they have consumed at least one drink of any alcohol beverage (a can/bottle of beer, a glass of wine, one cocktail, a shot of liquor) in the last month[6]. Those answering affirmatively are questioned about the number of drinks, the number of days of the week with positive consumption, the number of times they have consumed more than five drinks and whether they drove under the effects of alcohol.

We use in this study four different proxies of alcohol consumption, in addition to the indicator:

- Drinker: binary variable which takes the value one for respondents with some consumption during the last 30 days.
- Conditional consumption: number of drinks for drinkers in 30 days (in logs).

[4] More information about the survey can be found at http://www.cdc.gov/nccdphp/brfss.

[5] For the period 1987-1999, we have 1,032,970 observations. We have excluded observations for Guam, Puerto Rico and Virgin Islands.

[6] The survey does not distinguish among types of drinks (except for 1985-1988 and 2003), so it is not possible to introduce any weighting that refers to their different ethylic content.

- Chronic consumption: binary indicator which takes one for male (female) having more than 60 (30) drinks during the last month[7].

- Binge drinking: binary indicator that takes one if the respondent has imbibed five or more beverages on a single occasion.

All these measures have been frequently used in the literature. For example, Manning and Moulton (1995) uses the first two; Dee (1999) tries to capture the implications of alcohol abuse and uses two measures very similar to the fourth and fifth. Finally, Ruhm and Black (2002) and Dee (2001) uses all indicators.

We also control for socioeconomic characteristics of the respondent: race (white, black, hispanic), marital status (married, divorced, separated, widowed, single) and level of schooling (high school dropouts, some college, college). In addition to these variables we use the state-month-year unemployment rate (*Bureau of Labor Statistics*), the state-year real per capita income (*Bureau of Economic Analysis*) and, as a price variable, beer state-specific taxes[8].

Some individuals do not provide information about age, race, level of studies or marital status. We define *missing-value* dummies in order to keep the observations. This concerns 0.75% of the sample[9]. To avoid the influence of outliers we have established a maximum of 450 drinks consumed in the last month (an average of 15 per day). This upper limit affects 0.018% of the sample and information about drinking participation

[7]The literature suggests that a moderate consumption of alcohol may have beneficial effects on health. Nevertheless, differences exist in the consumption depending on the sex (Baum-Barker (1985). Women have lower probability of being alcoholic, it is more probable that they are abstemious and, on average, they consume less alcoholic drinks than men (Mullahy and Sindelar (1991), Wilsnack and Wilsnack (1992), Caetano (1994), Wilsnack, Wilsnack and Hiller-Sturmhofel (1994)). There is also evidence that women answer in a different way to alcohol consumption. With the same consumption, women experience more serious hepatic damage than men. Moreover, Federal recommendations advise women not to consume more than one alcoholic drink a day, and for men not to consume more than two (US Department of Health and Human Services, 2000).

[8]There are three types of taxes: beer, wine and spirits. As Ruhm and Black (2002) we use the state-specific taxes on the beer. http://www.taxfoundation.org

[9]We do not present both sets of results, though they do not substantially differ.

is unavailable for 0.21% of the sample.

Table 2.1 contains descriptive statistics. For the period 1985-2008, 51% of the sample reports having consumed at least one alcoholic drink in the last month. The average number of drinks consumed by the drinkers is 20.02. Nevertheless, 51.3 % has consumed less than 10 drinks, 76.5% less than 25 and 4.9% more than 80. Besides that, 16% declares at least 5 drinks in the same occasion and 5% has consumed more than 60 (30) drinks if he is a man (woman) in the last month. Finally, weights indicate that men, hispanics or other ethnic minorities and young people are underrepresented in the survey.

b Alcoholic drinks and unemployment: a first look

In table tab:summ2 we compare our descriptive statistics with those of Dee (2001) and Ruhm and Black (2002). We can observe that unemployment, age, sex, composition of the population by race, percentage of drinkers and consumption are very alike for the three samples.

Figure 1 presents the standard deviation with respect to the mean for the unemployment rate and all alcohol consumption indicators. The pattern of the relationship between unemployment and alcohol consumption indicators follow a pro-cyclical pattern for most of the figures.

Table 2.1: Descriptive Statistics 1985-2008[a]

Variable	Description	Without Weights		With Weights	
		Mean	Std. Dev	Mean	Std. Dev
Drinker	1 if he/she has consumed one alcoholic beverage in the last month	0.50	0.50	0.48	0.50
Mean Consumption	Average # of drinks per individual by month and state	13.19	5.82	12.01	4.99
Consumption	# number of drinks consumed by drinkers in the last month	20.22	33.98	20.56	35.55
Binge Drinking	1 if he/she has consumed 5 or more drinks in the same occasion	0.12	0.33	0.11	0.31
Chronic Drinking	1 if he/she has consumed more than 60 drinks in the last month (30 drinks for women)	0.07	0.25	0.07	0.26
Retired	1 if he/she is retired from the labor market	0.27	0.44	0.35	0.48
Unemployed	1 if he/she is unemployed	0.04	0.20	0.04	0.18
Short Term Unemployed	1 if he is unemployed for less than a year	0.02	0.15	0.02	0.14
Long Term Unemployed	1 if he is unemployed for more than a year	0.02	0.13	0.02	0.13
Per capita real Income	Per capita real income in 1999 $	39.31	22.30	34.50	21.27
State per capita Income	State Real Income in 1999 $	27.45	4.91	28.74	4.86
State Unemployement Rate	State Unemployment Rate	5.09	1.35	4.63	1.24
Good Health	1 if individual reports good health	0.82	0.38	0.79	0.41
Exercise	1 if individual reports to do exercise	0.65	0.48	0.68	0.47
Tax on Beer	State beer tax rate per gallon in 1999 $	1.66	0.47	1.62	0.47
Tax on Tobacco	State tobacco tax rate in 1999 $	0.56	0.45	0.68	0.48
Female	1 if the individual is female	0.60	0.49	0.68	0.47
Age	Age in years	49.65	17.83	55.22	17.06
White	1 if he/she is white	0.81	0.40	0.83	0.38
Black	1 if he/she is black	0.08	0.27	0.05	0.22
Other Race	1 if he/she belong to another ethnicity	0.05	0.22	0.07	0.25
Race not reported	1 if he/she does not report race	0.01	0.08	0.01	0.09
Hispanic	1 if he/she is hispanic	0.06	0.23	0.04	0.20
Hispanic not reported	if he/she does not report hispanic condition	0.00	0.06	0.00	0.07
High School Dropout	1 if High School not completed	0.12	0.32	0.12	0.32
High School	1 if he/she reports High School finished	0.31	0.46	0.33	0.47
Some College	1 if he/she has some college education	0.26	0.44	0.26	0.44
College	1 if he/she finished college	0.30	0.46	0.29	0.45
Education not reported	1 if educational level no reported	0.00	0.05	0.00	0.06
Married	1 if he/she is married	0.58	0.49	0.00	0.05
Separated	1 if he/she is separated	0.16	0.36	0.46	0.50
Widowed	1 if he/she is widowed	0.12	0.32	0.22	0.41
Marital Status not reported	1 if educational level no reported	0.00	0.05	0.19	0.40
Children	1 if he/she has children	0.43	0.50	0.30	0.46
# Children	Number of children reported	0.66	1.09	0.49	0.99
Observations		**3,644,491**			

[a] Data are from 1985 to 2008 period of the BRFSS. Information of all-items Consumer Price Index used to deflate income comes from Bureau of Economic Analysis. The first column of the table shows unweighted means; the third weights the observations using BRFSS final sampling weights.

Table 2.2: Comparison of descriptive statistics [b]

Variable	Individual Data	Cohort Data	Ruhm and Black	Dee
	1985-2008	YOB-Gender-Zone	1987-1999	1984-1994
General				
Female	0.60	0.50	0.59	0.58
Age	49.65	48.54	46.10	45.50
State Unemployment Rate	5.09	5.23	5.40	6.00
State per capita Income	27.45	25.95	24.90	140.00
Drink Related				
Drinker	0.50	0.54	0.50	0.50
Consumption	20.22	20.58	19.70	20.90
Binge Drinking	0.12	0.14	-	0.19
Chronic Drinking	0.07	0.07	0.04	0.04
Beer Tax rate	1.66	1.72	1.92	-
Race/Ethnicity				
Black	0.08	0.06	0.09	0.09
Other	0.05	0.04	0.05	0.05
Race not reported	0.00	0.00	-	-
Hispanic origin	0.06	0.05	0.05	0.03
Hispanic not reported	0.00	0.00	0.00	-
Education				
High School Dropout	0.31	0.12	0.15	0.34
Some College	0.26	0.25	0.26	0.24
College	0.30	0.29	0.26	0.26
Education not reported	0.00	0.00	0.00	0.00
Marital Status				
Married	0.58	0.60	0.57	0.56
Separated	0.16	0.14	0.15	0.14
Widowed	0.12	0.09	0.11	0.11
Marital status not reported	0.00	0.00	0.00	0.00

[a]Dee (2001) does not indicate if descriptive statistics are weighted or not. To build this table we have used Ruhm and Black (2002) descriptive statistics and ours without using final weights. [b]For Ruhm and Black and us (2002) per capita real income is measured in 1999$. Dee (2001) doesn't indicate which is the base year, but there is a great disparity among his figures, ours and Ruhm and Black (2002).

Figure 1. Mean consumption and unemployment by sex. Source: BRFSS

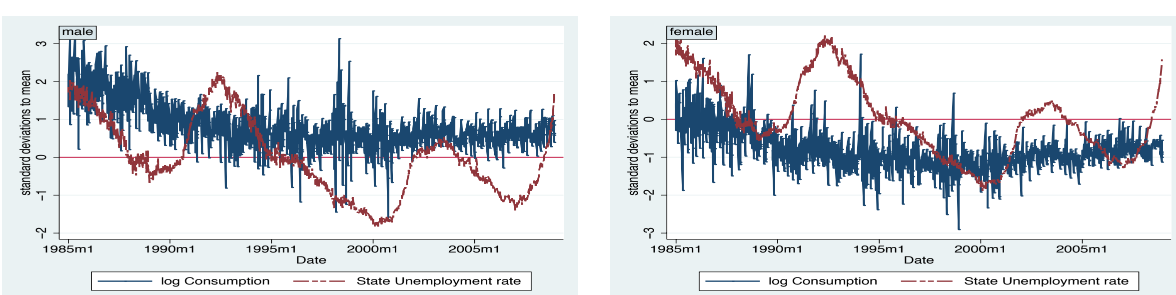


Figure 2. Mean consumption and unemployment by age cohorts. Source: BRFSS

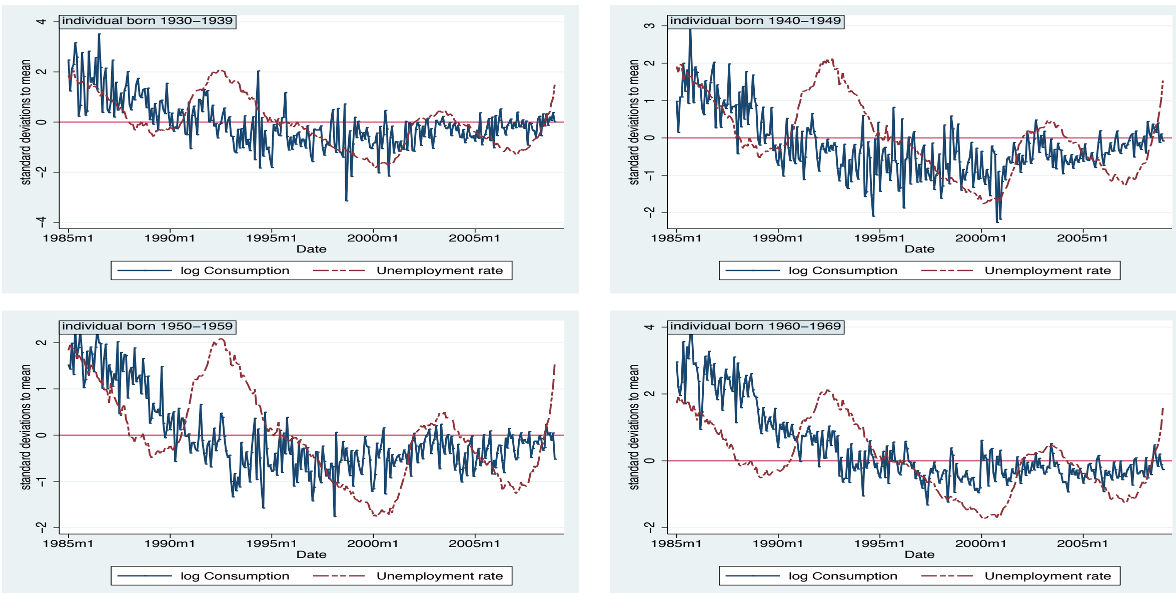


Figure 3. Mean consumption and unemployment by age/gender cohorts. Men. Source: BRFSS

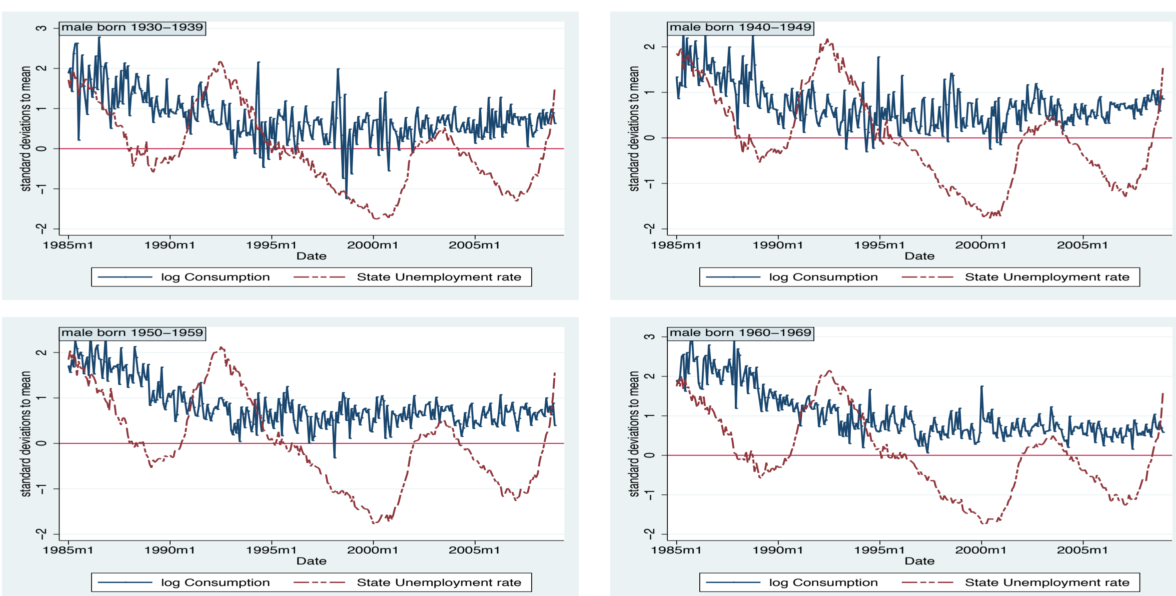
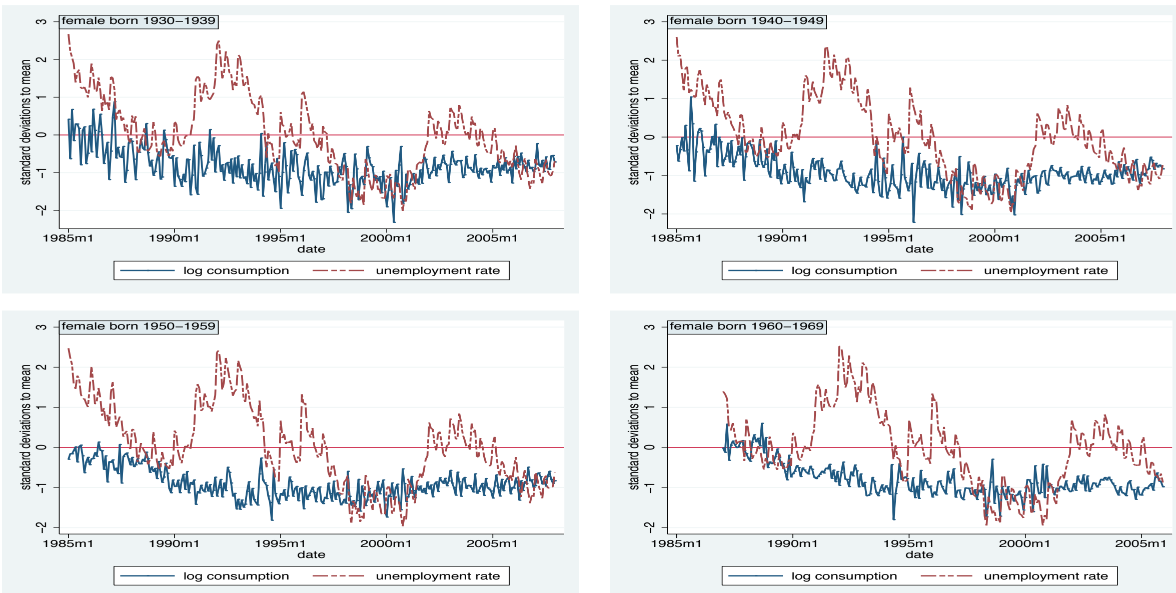


Figure 4. Mean consumption and unemployment by age gender cohorts. Women. source: BRFSS



When considering that the only source of heterogeneity in the decisions of becoming drinker and the number of drinks consumed is sex, we get a clear pro-cyclical profile. However, the situation changes drastically when there is another source of heterogeneity. When we re do the figures for men and women grouped by age cohorts in ten year intervals from 21 to 50 years, and a last one for those aged 50 to 65, the relationship between mean consumption and the rate of unemployment is not as clear as before[10]. According to Figures 2 and 3, it seems that average consumption is pro-cyclical only for men and women from 21 to 30. Although we cannot establish any causal relationship based on correlations, it seems possible that economic conditions could have some effect on consumption at the intensive margin for some group of the population.

[10] It also happens for the rest of alcohol consumption indicators. We omit these graphs for reason of space, but they, as well as additional graphs for men and women at different age brackets, are available upon request.

2.4 Empirical results

a Estimates using individual data

As a first step to cover one of the objectives of this research, we present in table 2.3 comparison of our estimates with those obtained by Dee (2001) and Ruhm and Black (2002). For each of the four alcohol consumption indicators we estimate Equation 2.1 by weighted least squares, using BRFSS final weights. As Ruhm and Black (2002) we have included in all the regressions controls for state, month, age and its square, gender, race/ethnicity, level of schooling, marital status and real per capita income, but we also add year dummies and we cluster at the state level. For the period 1987-1999 we obtain similar results than Ruhm and Black (2002). Minor discrepancies can be explained by small differences in the unemployment variables and in the sample. However, Dee (2001) finds that real per capita income is not significant for consumption, binge drinking and chronic drinking and the unemployment rate is positive and significant for binge drinking. The reasons for this disparity among results may be that Dee (2001) neither introduces final weights in his estimations nor includes any measure for alcohol prices and considers first waves of the survey, which are less reliable due to the small number of states interviewed[11].

When we compare the full sample results of the BRFSS of both Ruhm and Black (2002) and Dee (2001) agains the original sample for the drinking participation decision we surprisingly find that the results for the period 1985-2008 are very different from the original ones, specially regarding the unemployment rate, offering support for a countercyclical effect of unemployment for the participation and consumption equations.[12] Once we use the full sample, the positive coefficient of the unemployment rate variables

[11] Dee (2001) does not clarify the source of the State Real Income per Capita, which is substantially different from the one both Ruhm and Black (2002) and us use

[12] This confirms one of the explanations offered by Ruhm and Black (2002) in the sense that the results could be sensitive to the period of analysis as our results for different periods confirm.

Table 2.3: Estimates using individual data

	Dee (2001)		Ruhm and Black (2002)		**Ours**	
	1984-1995	1985-2008	1987-1999	1985-2008	1987-1999	1987-2008
Drink Participation						
State Unemployment rate	0.0008	0.0047**	-0.0039	0.0026	-0.0008	0.0010
	0.0045	0.0020	0.0069	0.0018	0.0054	0.0030
State Income per capita	-0.0061	0.0045	-0.0067	0.0003	-0.0080*	0.0047
	0.0070	0.0031	0.0050	0.0022	0.0044	0.0037
R^2	0.15	0.13	0.15	0.11	0.17	0.14
Consumption						
State Unemployment rate	-0.0355	0.1529	-0.6049	-0.0901	-0.4519*	-0.1732
	0.1435	0.1162	0.4382	0.1293	0.2593	0.1399
State Income per capita	-0.7424	0.2642***	-1.9065**	0.4736**	-0.4490*	0.1521**
	0.5884	0.0853	0.8562	0.2275	0.2524	0.0732
R^2	0.08	0.07	0.10	0.07	0.11	0.06
Cond Log of Consumption						
State Unemployment rate	-0.0030	-0.0005	-0.0114	0.0176***	-0.0127*	-0.0094*
	0.0048	0.0044	0.0111	0.0059	0.0067	0.0056
State Income per capita	-0.0118	0.0032	-0.0253	0.0204***	0.0002	0.0008
	0.0153	0.0027	0.0210	0.0055	0.0113	0.0022
R^2	0.12	0.09	0.15	0.09	0.15	0.10
Binge Drinking						
State Unemployment rate	0.0026	0.0024**	-0.0042	0.0002	0.0003	0.0001
	0.0026	0.0011	0.0057	0.0009	0.0041	0.0015
State Income per capita	-0.0014	0.0017	-0.0108**	0.0034*	-0.0016	0.0011
	0.0024	0.0015	0.0045	0.0018	0.0031	0.0013
R^2	0.12	0.09	0.16	0.09	0.16	0.10
Chronic Drinking						
State Unemployment rate	-0.0012	0.0007	-0.0009	0.0002	-0.0032**	-0.0012
	0.0014	0.0009	0.0025	0.0006	0.0014	0.0011
State Income per capita	-0.0051	0.0019**	-0.0094**	0.0025**	-0.0033	0.0018**
	0.0048	0.0008	0.0042	0.0012	0.0030	0.0007
R^2	0.02	0.02	0.07	0.04	0.04	0.02
State Trend	No	No	Yes	Yes	No	No
State	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	No	No	Yes	Yes
Month	Yes	Yes	Yes	Yes	Yes	Yes
Observations	742,553	3,611,416	1,032,288	3,620,251	1,096,647	3,644,491

Heteroscedastic-consistent errors clustered by state. Standard Errors reported. All regressions include state dummies, month dummies, age, race/ethnicity, education, gender, marital status. Ruhm and Black (2002) includes state-specific linear trends and beer tax rate. Dee (2001) includes year dummies and age squared. Ours include age squared, year dummies and beer tax rate and the health status.

is very robust to changes in the specification (for instance, to the inclusion of year dummies, and a quadratic trend), however, its statistical significance it is not[13]. In fact, we only find that the conditional number of drinks decreases with unemployment but not the participation rate.

[13] As the unemployment rate should affect those that are actives in the labor market we have also replicated the same equation without those individuals that reports themselves as retired. Similar results were obtained although the coefficients were always larger without retirees than with them.

b Results using cohort data

b.1 Definition of the groups

Once we have covered our first aim of comparing the results using individual data with previous ones in the literature, we move on to estimates using cohort data. We define cohort cells by year of birth (12 groups) and region (4 regions: East, South, West, Central); and, by year of birth, region and gender (studies worth mention using the same methodology are Attanasio and Weber (1993) or Blundell, Browning and Meghir (1994)).[14] For each synthetic individual we have pooled observations within a given quarter, something that insures us consecutive observations by year and quarter.[15] The resulting sample has 4,432 and 8,863 observations in the first and second cases, respectively. The average sample size for each cohort in the year of birth-region aggregation is 822 and in the year of birth-region-gender one is 411. Thus, given this sample size, we can neglect the errors in variables problem according to the results in Deaton (1985).

b.2 Results with state variables

One of the worries we have before presenting the results of these models is related to the exogeneity of the variables entering the drinking indicators and consumption equations. We test for the exogeneity of the state unemployment rate and real state per capita income. The unemployment rate could be endogenous because of reverse causality (Ettner (1997), Mullahy and Sindelar (1991) and Terza (2002)). Income is potentially endogenous under absence of separability conditions or due to the good influence over efficiency at work that moderate consumption of alcohol may produce. It consequently

[14]Results with other definitions of cohort cells (year of birth and gender) and observations (month x year) are very similar and are not reported for the sake of simplicity but are available on request.

[15]As we mentioned before, the errors in variables problem could be serious whenever the number of observations per cell is significantly smaller than 150, something that occurs if we group observations monthly. Thus, we choose quarter instead of month to built cohorts and we will include quarterly dummies to control seasonality in consumption.

could affect earnings (Hamilton and Hamilton (1997), French and Zarkin (1995)). As instruments for the unemployment rate and real per capita income we propose their respective values in the same quarter of the previous year. Since consumption exhibits seasonality, the correlation between the regressors and the instruments is high. On the other hand we have no reason to suspect that the they exhibit correlation with the error term. We then compare the LS and IV estimates by means of a Hausman test, and, for all cohorts and specifications we are not able to reject the null of absence of systematic differences in the coefficients. Consequently, it seems both variables are exogenous under the identifying assumption of exogeneity of the other variables in the regression.

In 2.4 we present least squares estimates for year of birth-region and yob-region-gender cohorts. All regressions include quarterly and yearly dummies, state fixed effects, age and its square, gender, race/ethnicity, the level of schooling, marital status and real per capita income[16]. We present two different sets of results - with and without cohort effects - and we do it for two time periods - 1987-1999, which is the one used in Ruhm and Black (2002), and 1985-2008. These results show some common traits for all beverage consumption indicators. First of all, the 1987-1999 time period, seems to have a higher unemployment effect in almost all the outcomes and specifications. Secondly, when cohort effects are omitted the unemployment rate appears to be significantly higher, being the case of binge drinking in the 1987-1999 period an exception. Third, the magnitude of the coefficient is, in all cases, very similar to that found in the pooled cross-section samples (see again Table 3). Also, the magnitude of the income coefficient is similar. When cohort effects are introduced, the unemployment rate remains significant in the drinking decision and binge drinking however it does not in the conditional quantity of drinks.

Real per capita income is as a rule significant (even for the participation decision),

[16]Because the number of states participating in the survey changed, we have included the proportion of individuals in each state as one of the regressors to account for that variation.

Table 2.4: Ruhm and Black (2002) specification with additional cell controls and with and without cohort fixed effects. Cohorts defined by year of birth, gender and region (96 cells) and year of birth and region (48 cells). Cells aggregated quarterly.

	YOB-Gender-Region Cohort				YOB-Region Cohort			
	1985-2008		1987-1999		1985-2008		1987-1999	
	NO FE	FE	NO FE	FE	NO FE	FE	NO FE	FE
Drink Participation								
State Unemp Rate	0.0104***	0.0038***	0.0139***	0.0074***	0.0089***	0.0031***	0.0109***	0.0072***
	0.0013	0.0009	0.0020	0.0014	0.0009	0.0008	0.0014	0.0015
State Income	0.0113***	0.0031**	0.0145***	-0.0038	0.0111***	0.0027***	0.0133***	-0.0003
	0.0009	0.0010	0.0011	0.0023	0.0004	0.0007	0.0007	0.0016
R^2	0.95	0.97	0.94	0.96	0.96	0.97	0.94	0.96
F	747.58	173.75	613.61	47.41	1745.75	114.84	1086.00	47.45
Conditional log # drinks								
State Unemp Rate	0.0121***	0.0008	0.0132**	-0.0006	0.0049	-0.0035	-0.0005	-0.0065
	0.0027	0.0031	0.0045	0.0048	0.0030	0.0030	0.0046	0.0051
State Income	0.0185***	0.0062**	0.0254***	-0.0105*	0.0180***	0.0068**	0.0140***	0.0071
R^2	0.97	0.97	0.95	0.96	0.70	0.76	0.71	0.76
F	2834.21	213.55	1854.10	115.52	182.27	186.99	136.81	137.90
Binge drinking								
State Unemp Rate	0.0024	0.0021*	0.0010	0.0027**	0.0027***	0.0021***	0.0010	0.0028**
	0.0013	0.0011	0.0014	0.0010	0.0006	0.0006	0.0010	0.0010
State Income	-0.0011*	-0.0007	0.0004	-0.0036***	-0.0011***	-0.0011*	-0.0006	-0.0041***
	0.0004	0.0007	0.0008	0.0009	0.0002	0.0005	0.0004	0.0009
R^2	0.96	0.97	0.95	0.96	0.96	0.97	0.94	0.95
F	1450.21	117.65	988.04	50.26	1837.11	165.57	954.15	49.77
Chronic drinking								
State Unemp Rate	0.0036***	0.0019*	0.0017	0.0015	0.0026***	0.0008	-0.0013	-0.0014
	0.0008	0.0007	0.0013	0.0017	0.0006	0.0006	0.0010	0.0013
State Income	0.0025***	0.0042***	-0.0007	0.0014	0.0031***	0.0037***	0.0003	0.0017
	0.0005	0.0005	0.0006	0.0015	0.0002	0.0005	0.0005	0.0011
R^2	0.61	0.67	0.41	0.46	0.71	0.74	0.54	0.58
F	215.41	175.68	154.18	49.19	192.33	165.79	60.80	46.83
Observations	7,078	7,078	3,619	3,619	4,098	4,098	2,182	2,182
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Heteroscedastic-consistent standard errors, clustered by cohort reported. All regressions include quarterly dummies and yearly dummies, age, age squared, race/ethnicity, marital status, education, health status and beer tax.

regardless the inclusion of cohort effects, but the reduction in the parameter show misspecification when cohort dummies are not taken into account since these effects are really important in all specifications with important implications for the income elasticity. The magnitude of the coefficient of income increases significantly when estimating the conditional (on drinking) number of drinks equation instead of the unconditional one. Since the elasticity for non-drinkers is zero and the conditional quantity is greater than the unconditional one, this is an expected result. The income effect also decreases significantly in models with fixed effects. Income seems to be positively correlated with the preference for drinking. Income is positively correlated with unobserved heterogeneity in the consumption equation. This can be rationalized if the unobserved effects contain the preference for drinking and in the evolution of consumption dominates the income effect. In our opinion, the most important implication from these set of results is the need for different specifications at the level of participation and consumption since some variables affect in a different way both decisions while some other variables have effects on the same direction but with different magnitudes. We have proved to estimate the model including a selection term built using the results of a probit index and we always get that selection correction matters.

Table 2.5 presents the same regressions than in Table 3 but we include cell unemployment rate and cell household income amongst the regressors. The state unemployment rate tries to capture differences in consumption arising because of the state of the economy which are common to individuals while the cell unemployment rate is intended to capture how the business cycle affects in a different way to different individuals. In this sense, the In this case we only present results for the 1985-2008 period. Since cell variables can be endogenous we present both LS and IV estimates. We include in the instrument set for household income and cell unemployment rate, lags of cell unemployment rate, lags of cell income, lag of the alcohol price index and of the number of adults in the household. In general, the results are maintained in all specifications either including or not cohort effects, except that now the state unemployment rate seems to affect the conditional number of drinks positive and significantly. It is interesting to note that the cell unemployment rate seems to have the opposite sign to the state unemployment rate, something that does not happen with the income.

Table 2.5: Ruhm and Black (2002) specification with additional cell controls. Cohorts by YOB-Gender-Region (96 cells) and YOB-Region (48 cells). Cells aggregated quarterly.

	YOB-Gender-Region				YOB-Region			
	NO FE		FE		NO FE		FE	
	LS	IV	LS	IV	LS	IV	LS	IV
Drink Participation								
State Unemp Rate	0.0115***	0.0129***	0.0041***	0.0034*	0.0080***	0.0054***	0.0025**	-0.0003
	0.0014	0.0019	0.0009	0.0014	0.0010	0.0013	0.0009	0.0013
State Income	0.0111***	0.0108***	0.0025**	0.0030**	0.0115***	0.0113***	0.0023**	0.0027***
	0.0009	0.0009	0.0009	0.0009	0.0004	0.0004	0.0007	0.0008
Cell Unemp Rate	-0.0011*	-0.0038*	-0.0001	0.0003	0.0006	0.0027*	0.0011**	0.0042**
	0.0005	0.0015	0.0004	0.0016	0.0005	0.0012	0.0004	0.0013
Cell Real Income	0.0251	0.0612*	0.0261*	0.0288	-0.0357***	-0.0613***	0.0107	-0.0090
	0.0140	0.0246	0.0129	0.0239	0.0085	0.0162	0.0082	0.0171
R^2	0.95	0.95	0.97	0.42	0.96	0.96	0.97	0.56
F	788.60	772.72	171.21	203.51	1656.17	1638.85	112.49	106.52
Conditional Log(# drinks)								
State Unemp Rate	0.0173***	0.0189***	-0.0004	0.0009	0.0025	-0.0004	-0.0056	-0.0068
	0.0039	0.0044	0.0038	0.0047	0.0033	0.0045	0.0032	0.0046
State Income	0.0305***	0.0306***	0.0070**	0.0086***	0.0196***	0.0210***	0.0093***	0.0101***
	0.0025	0.0025	0.0024	0.0024	0.0013	0.0014	0.0025	0.0029
Cell Unemp Rate	-0.0030*	-0.0077*	-0.0013	-0.0054	0.0010	0.0035	0.0019	0.0004
	0.0012	0.0038	0.0011	0.0045	0.0014	0.0041	0.0014	0.0048
Cell Real Income	-0.0164	-0.0105	0.0365	0.0287	-0.1316***	-0.2440***	-0.0991***	-0.1465*
	0.0417	0.0646	0.0420	0.0727	0.0282	0.0619	0.0282	0.0709
R^2	0.96	0.96	0.97	0.56	0.71	0.70	0.77	0.65
F	1310.82	1373.51	125.22	133.96	177.35	168.96	181.94	168.75
Binge drinking								
State Unemp Rate	0.0029*	0.0049**	0.0021*	0.0022	0.0019**	0.0017	0.0011	-0.0004
	0.0013	0.0016	0.0011	0.0013	0.0007	0.0009	0.0006	0.0009
State Income	-0.0008	-0.0007	-0.0002	0.0001	-0.0007**	-0.0006*	-0.0001	0.0002
	0.0004	0.0004	0.0006	0.0007	0.0003	0.0003	0.0005	0.0005
Cell Unemp Rate	-0.0010*	-0.0039***	-0.0002	-0.0006	0.0005	0.0001	0.0009***	0.0023*
	0.0004	0.0011	0.0002	0.0011	0.0003	0.0008	0.0003	0.0009
Cell Real Income	-0.0132	0.0099	-0.0181	-0.0161	-0.0352***	-0.0390***	-0.0415***	-0.0566***
	0.0102	0.0167	0.0102	0.0199	0.0054	0.0112	0.0055	0.0121
R^2	0.96	0.96	0.97	0.60	0.96	0.96	0.97	0.73
F	1468.71	1511.68	132.25	123.92	1758.56	1769.10	162.01	153.03
Chronic drinking								
State Unemp Rate	0.0029*	0.0038**	0.0018*	0.0013	0.0026***	0.0029***	0.0007	0.0029***
	0.0013	0.0014	0.0008	0.0012	0.0006	0.0009	0.0007	0.0009
State Income	-0.0008	0.0027***	0.0039***	0.0037***	0.0030***	0.0032***	0.0035***	0.0032***
	0.0004	0.0005	0.0005	0.0006	0.0002	0.0003	0.0005	0.0003
Cell Unemp Rate	-0.0010*	-0.0000	0.0002	0.0008	0.0001	-0.0001	0.0002	-0.0001
	0.0004	0.0012	0.0003	0.0013	0.0003	0.0008	0.0003	0.0008
Cell Real Income	-0.0132	-0.0055	0.0094	0.0069	0.0070	0.0046	0.0065	0.0046
	0.0102	0.0160	0.0064	0.0159	0.0055	0.0119	0.0059	0.0119
R^2	0.96	0.62	0.67	0.57	0.71	0.71	0.74	0.71
F	1468.71	229.84	179.99	217.72	185.90	179.38	160.13	179.38
Obs	7,078	6,916	7,078	6,916	4,098	3,945	4,098	3,945

Heteroscedastic-consistent standard errors, clustered by cohort reported. All regressions include quarterly dummies and yearly dummies, age, age squared, race/ethnicity, marital status, education, health status and beer tax. Instruments for the cell unemployment rate and cell income are (-1 -4) cell unemployment rate, (-1 -4) cell income, (-1 +1) tax on beer and (-1 +1) number of adults in the house.

c Dynamic models: habit and rational addition models

The second advantage of using longitudinal data is the possibility to introduce habit formation in the specification of the model in the form of dynamics. There is an extensive literature covering the extent of habits in consumption and, specifically, in consumption of certain goods as alcoholic drinks or tobacco. This literature moves from myopic models in which the consumer does not take into account the future consequences of smoking or drinking to rational models where these potential consequences are considered. The rational addiction model is derived by Becker and Murphy (1988) and Becker et al. (1994), Moore and Cook (1995), Grossman et al. (1998), Bentzen et al. (1999) or Baltagi and Griffin (2002) constitute some interesting applications. Here we present the rational addition model estimates for yob-region-gender cohorts for the period 1985-2008. All regressions include yearly and quarterly dummies, age and its square, gender, race/ethnicity, the level of schooling, marital status and health status as well as they control for the proportion of individuals within a state and includes cohort fixed effects. We report results for the participation equation, consumption in the uncondicional sample (both the least square and instrumental variable regression) and in the conditional sample (least square, instrumental variables and the selection model equation). As instruments we use lags and forwards of the tax on beer, the tax on tobacco and the number of adults in the house. We propose three different specifications. The first one matches the business cycle to the state unemployment and the state income as in Dee (2001). The second adds the cell unemployment and the cell income, instrumenting these two, while the third one replaces cell unemployment with information on the proportion of people within the cell that is unemployed in the short run and in the long run. These last two are more in the spirit of Ruhm and Black (2002).

c.1 Controlling for Retirees

Tables 2.6, 2.7 and 2.8 show the results of the three aforementioned models for the sample that includes and controls for the individuals that have retired from the labor market. The last two columns (fixed effects and instrumental variable without and with selection in the conditional sample) are the ones we are most interested in. As we can see there, the business cycle effect of unemployment loses both size and significance once we have controlled for fixed effects, include habit formation and Heckman's lambda selection parameter. That is, the results here seems to indicate that there is no effect of unemployment on consumption of alcohol. However, it is worth mentioning that in the third specification, where we split the cell unemployment into short run (transitory effect) and long run unemployment (permanent), the effect is significant and negative, which means that those individuals who happen to be unemployed for a period long enough reduce their alcoholic intake. It should be noted however, that the results for this sample are not entirely satisfactory since the implied beta is too low and the Hansen J statistic is relatively high, plus in the second specification the myopic model is not entirely rejected.

c.2 Excluding Retirees

When we construct the pseudo-panel data excluding those individuals that are retired from the labor market, the results improve significantly, as 2.9, 2.10 and 2.11 show. To begin with, the implied beta in the three specifications is much closer to one. Second, it seems that incorporating habits into the equation it is very important in terms of the results, particularly for unemployment to be significant or not. Although it is never significant in statistical terms, state unemployment has a positive effect on consumption. However, unemployment at the cell level has a negative effect, which can be interpreted as if the effect would cancel out. Income, on the other hand, does not seems to have any significant effect either. Finally, selection does not seems to be an issue here. Even

Table 2.6: Habit Formation Specification 1 - Year of Birth-Gender-Region

	Participation	Unconditional Sample			Conditional Sample		
		OLS	FE	IV FE	FE	IV FE	Selection
Lag # Drinks		0.14	0.14	0.06	0.05	0.50	0.50
		7.51	7.51	0.29	2.78	3.37	3.36
Forw # Drinks		0.13	0.13	0.83	0.04	0.24	0.24
		6.45	6.45	4.14	2.43	1.57	1.56
State Unemp Rate	0.01	0.01	0.01	-0.00	0.01	0.00	0.00
	2.85	1.95	1.95	-0.24	1.85	0.24	0.23
State Income	-0.00	-0.01	-0.01	0.00	-0.00	0.00	0.00
	-1.36	-1.68	-1.68	0.13	-0.12	0.53	0.53
λ							0.01
							0.13
Stability			0.07	0.19	0.01	0.47	0.48
Implied β			0.95	14.34	0.88	0.48	0.47
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,181	4,066	4,066	3,394	3,009	2,476	2,476
R^2	0.96	0.97	0.97	0.16	0.97	0.20	0.19
Hansen j stat				18.85		11.17	11.08
Hansen j df				6		6	6
Hansen j p-val				0.00		0.08	0.09
Weak Ins Stat				1.84		3.67	3.62

Heteroscedastic-consistent standard errors, clustered by cohort. t-statistic reported. All regressions include quarterly dummies and yearly dummies, age, age squared, race/ethnicity, marital status, education, health status, retired status, state dummies and beer tax. Only those cells with more than 100 observations within were included.

Table 2.7: Habit Formation Specification 2 - Year of Birth-Gender-Region

	Participation	Unconditional Sample			Conditional Sample		
		OLS	FE	IV FE	FE	IV FE	Selection
Lag # Drinks		0.13	0.13	0.15	0.04	0.26	0.27
		9.07	9.07	2.57	2.16	2.87	2.82
Forw # Drinks		0.13	0.13	0.52	0.05	0.13	0.12
		8.33	8.33	6.39	2.39	1.17	1.11
State Unemp Rate	0.01	0.02	0.02	0.01	0.03	-0.00	-0.01
	2.32	1.49	1.49	0.53	1.77	-0.09	-0.40
State Income	0.00	0.00	0.00	0.00	0.01	0.01	0.01
	1.74	0.43	0.43	0.38	0.65	0.91	0.82
Cell Unemployment	-0.01	-0.01	-0.01	-0.01	-0.03	0.01	0.01
	-1.56	-0.94	-0.94	-0.89	-1.32	0.38	0.74
Cell Income	-0.00	-0.01	-0.01	-0.01	-0.00	-0.00	-0.00
	-4.32	-3.96	-3.96	-1.60	-0.92	-0.70	-0.34
λ							0.07
							0.34
Stability			0.001	0.31	0.00	0.13	0.13
Implied β			1.00	3.50	0.15	0.50	0.46
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,119	4,019	4,019	3,393	2,973	2,476	2,476
R^2	0.32	0.49	0.49	0.39	0.31	0.36	0.34
Hansen J stat	6.62	0.42	0.42	43.28	4.11	32.26	31.46
Hansen J df	2	2	2	12	2	12	12
Hansen J p-val	0.04	0.81	0.81	0.00	0.13	0.00	0.00
Weak Ins Stat	7.93	7.64	7.64	1.77	1.75	1.04	1.07

Heteroscedastic-consistent standard errors, clustered by cohort. t-statistic reported. All regressions include quarterly dummies and yearly dummies, age, age squared, race/ethnicity, marital status, education, health status, retired status, state dummies and beer tax. Only those cells with more than 100 observations within were included.

Table 2.8: Habit Formation Specification 3 - Year of Birth-Gender-Region

	Participation	Unconditional Sample			Conditional Sample		
		OLS	FE	IV FE	FE	IV FE	Selection
Lag # Drinks		0.14	0.14	0.19	0.03	0.40	0.40
		6.94	6.94	1.06	0.95	2.95	2.63
Forw # Drinks		0.13	0.13	0.80	0.02	0.26	0.25
		7.60	7.60	4.25	0.69	1.73	1.57
State Unemp Rate	0.01	0.02	0.02	0.02	0.03	0.01	-0.01
	2.09	1.88	1.88	1.24	1.50	0.94	-0.47
State Income	0.00	0.00	0.00	0.01	0.00	0.01	-0.00
	1.77	0.69	0.69	1.00	0.09	0.82	-0.03
Cell Long Run Unemp	1.31	0.41	0.41		-13.21	-2.70	-7.75
	0.93	0.12	0.12		-1.57	-0.65	-1.66
Cell Short Run Unemp	-1.93	-2.98	-2.98		-0.45	-1.72	3.41
	-1.87	-1.12	-1.12		-0.13	-0.87	1.09
Cell Income	-0.00	-0.01	-0.01	-0.01	-0.01	-0.00	-0.00
	-2.39	-3.28	-3.28	-1.12	-1.22	-0.64	-0.29
λ							0.34
							1.47
Stability			-4.87	0.59	23.88	0.41	0.411
Implied β			-7.28	4.28	0.03	0.63	0.63
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,119	4,019	4,019	3,393	2,973	2,476	2,476
R^2	-0.03	0.44	0.44	0.11	-0.35	0.18	-0.09
Hansen J stat	2.63	0.72	0.72	17.57	0.59	20.01	15.22
Hansen J df	3	3	3	8	3	9	9
Hansen J p-val	0.45	0.87	0.87	0.02	0.90	0.02	0.09
Weak Ins stat	1.12	1.09	1.09	1.39	0.69	0.72	0.81

Heteroscedastic-consistent standard errors, clustered by cohort. t-statistic reported. All regressions include quarterly dummies and yearly dummies, age, age squared, race/ethnicity, marital status, education, health status, retired status, state dummies and beer tax. Only those cells with more than 100 observations within were included.

though some of the specifications have a significant selection parameter, the estimates almost do not change.

Table 2.9: Habit Formation Specification 1 - Year of Birth-Gender-Region

	Participation	Unconditional Sample			Conditional Sample		
		OLS	FE	IV FE	FE	IV FE	Selection
Lag # Drinks		0.13	0.13	0.17	0.05	0.46	0.46
		7.57	7.57	1.36	3.35	3.13	3.16
Forw # Drinks		0.13	0.13	0.62	0.05	0.27	0.28
		6.25	6.25	4.93	3.54	2.19	2.21
State Unemp Rate	0.01	0.01	0.01	0.00	0.01	0.00	0.00
	4.14	2.03	2.03	0.30	1.13	0.09	0.04
State Income	-0.00	-0.01	-0.01	-0.00	-0.00	0.00	0.00
	-0.64	-1.62	-1.62	-0.31	-0.64	0.24	0.23
λ							0.01
							0.46
Stability			0.07	0.43	0.01	0.50	0.51
Implied β		0.99	0.95	3.53	0.997	0.59	0.60
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	5,107	4,971	4,971	4,174	4,061	3,329	3,329
R^2	0.97	0.98	0.98	0.32	0.96	0.14	0.13
Hansen J stat				31.05		11.72	11.48
Hansen J df				6		6	6
Hansen J p-value				0.00		0.07	0.07
Weak Ins Stat				3.92		4.50	4.50

Heteroscedastic-consistent standard errors, clustered by cohort. t-statistic reported. All regressions include quarterly dummies and yearly dummies, age, age squared, race/ethnicity, marital status, education, health status, state dummies and beer tax. Only those cells with more than 100 observations within were included.

Several messages emerge from these two sets of results. First, the general rational addiction model reject the restricted myopic one. The effect of the consumption lead is very important for every specification. Second, misspecification of the dynamic components of the models has an important effect on the significance of the state and cell unemployment variables in the conditional log number of drinks. Once the general model is estimated unemployment has no effects on alcohol consumption for drinkers. However, we find business cycle effects in the long run unemployed. Our opinion is that young (or poor) people can make decisions on starting drinking and/or quitting and these decisions are correlated with the state of the economy. On the other hand, the behavior of the average drinker is not sensitive to the business cycle.The rest of results are the expected ones, i.e. the price elasticity is very small both for participation and unconditional consumption and it is close to be zero for drinkers[17]. Income elasticity

[17]Not reported, but available upon request

Table 2.10: Habit Formation Specification 2 - Year of Birth-Gender-Region

	Participation	Unconditional Sample			Conditional Sample		
		OLS	FE	IV FE	FE	IV FE	Selection
Lag # Drinks		0.12	0.12	0.18	0.05	0.40	0.39
		9.53	9.53	1.25	3.09	2.40	2.45
Forw # Drinks		0.12	0.12	0.36	0.05	0.31	0.30
		8.80	8.80	2.68	3.45	1.80	1.80
State Unemp Rate	0.01	0.01	0.01	0.01	0.02	0.02	0.02
	3.11	1.57	1.57	1.05	1.45	1.69	1.42
State Income	0.00	0.00	0.00	0.00	0.00	0.01	0.01
	2.39	0.77	0.77	0.75	0.38	0.88	0.91
Cell Unemployment	-0.00	-0.01	-0.01	-0.01	-0.03	-0.04	-0.03
	-1.51	-1.04	-1.04	-1.09	-1.20	-1.90	-1.47
Cell Income	-0.00	-0.01	-0.01	-0.01	-0.01	-0.00	-0.00
	-5.03	-5.46	-5.46	-4.09	-1.61	-0.17	-0.25
λ							-0.07
							-1.46
Stabability			0.001	0.26	0.001	0.50	0.47
Implied β		0.99	1.45	1.95	0.20	0.80	0.78
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,023	4,906	4,906	4,558	4,022	3,708	3,708
R^2	0.41	0.48	0.48	0.44	0.33	0.05	0.12
Hansen J test	4.93	0.09	0.09	15.88	2.62	7.33	10.13
Hansen J df	2	2	2	8	2	8	8
Hansen J p-val	0.08	0.96	0.96	0.04	0.27	0.50	0.26
Weak Ins Stat	20.06	19.71	19.71	2.21	2.83	1.45	1.44

Heteroscedastic-consistent standard errors, clustered by cohort. t-statistic reported. All regressions include quarterly dummies and yearly dummies, age, age squared, race/ethnicity, marital status, education, health status, state dummies and beer tax. Only those cells with more than 100 observations within were included.

is positive for participation, i. e. the greater the income the higher the participation rate while both potential consumers and drinkers do not react to income changes. The implications for the effect of the business cycle on the number of drinks by drinkers is that once a complete specification is used, the relationship disappear.

It would be possible to argue that cohort dummies and the rate of unemployment show a high level of collinearity. To see if these is the reason behind the drop in significance we test this by means of running regression of the unemployment rate on cohort effects in the sample of cohorts by age and we obtain an R^2 of 0.33, which is rather low. On the other hand, we might also think that including quarter, year, state and cohort fixed effects the variation of the unemployment rate is not sufficient large to properly identify its effects separately from other micro and macroeconomic determinants. In order to check if this is the case we have re-estimated all the models excluding individually

Table 2.11: Habit Formation Specification 3 - Year of Birth-Gender-Region

	Participation	Unconditional Sample			Conditional Sample		
		OLS	FE	IV FE	FE	IV FE	Selection
Lag # Num		0.11	0.11	0.18	0.04	0.40	0.40
		5.60	5.60	1.25	2.20	2.26	2.31
Forw # Num		0.11	0.11	0.36	0.02	0.35	0.34
		6.52	6.52	2.68	0.90	2.07	2.08
State Unemp Rate	0.01	0.01	0.01	0.01	0.01	0.02	0.02
	3.09	1.34	1.34	1.05	0.91	1.49	1.33
State Income	0.00	0.00	0.00	0.00	0.00	0.01	0.01
	2.30	0.38	0.38	0.75	0.05	0.70	0.81
Cell Long Run Unemp	-0.38	-3.95	-3.95		-10.84	-2.49	-1.47
	-0.35	-1.11	-1.11		-1.62	-0.62	-0.38
Cell Short Run Unemp	-0.39	0.85	0.85		1.68	-3.19	-3.07
	-0.57	0.39	0.39		0.57	-1.36	-1.19
Cell Income	-0.00	-0.01	-0.01	-0.01	-0.01	0.00	0.00
	-3.77	-4.58	-4.58	-4.09	-1.84	0.08	0.06
λ							-0.06
							-1.40
Stability			-13.51	0.26	-72.79	0.56	0.55
Implied β		0.99	-0.22	1.95	-0.15	0.87	0.85
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	5,023	4,906	4,906	4,558	4,022	3,708	3,708
R^2	0.41	0.45	0.45	0.44	-0.03	0.09	0.12
F	40.93	43.30	43.30	40.97	15.86	15.63	16.32
Hansen J test	6.51	2.33	2.33	15.88	2.11	10.89	12.82
Hansen J df	3	3	3	8	3	9	9
Hansen J p-val	0.09	0.51	0.51	0.04	0.55	0.28	0.17
Weak Ins Stat	1.70	1.51	1.51	2.21	1.07	0.77	0.72

Heteroscedastic-consistent standard errors, clustered by cohort. t-statistic reported. All regressions include quarterly dummies and yearly dummies, age, age squared, race/ethnicity, marital status, education, health status, state dummies and beer tax. Only those cells with more than 100 observations within were included.

each of the subsets of quarter, annual and geographical dummy variables. The result are conclusive: we get negative and significant effects of unemployment on the demand for alcoholic drinks only when cohort effects are excluded from the specifications, independently of other set of dummies being excluded or not. These results confirm our hypothesis that unobserved effects are important determinants of alcohol consumption. We have re-estimated the models based on cohort data excluding income. We observe that unemployment rate is significant without cohort fixed effects, but is not when we include them[18].Although the magnitude of the coefficient experiments small variations (ranging from 1 to 10 per cent), it seems to be sufficient to loose its significance. These changes could be related to negative correlation among unobserved effects capturing preference for working, for instance, and the unemployment rate.

[18]All these results are available upon request.

2.5 Conclusions

In this paper we have analyzed the influence of macroeconomic conditions captured by unemployment on the decisions of participation and consumption of alcoholic drinks. We have used cross-section data for the period 1985-2008 from the BRFSS. Opposite to previous studies (Dee (2001) and Ruhm and Black (2002)) that did not controlled for unobservable heterogeneity, we have considered it explicitly. Since genuine panel data is not available to us, we have constructed age, age-gender, and age-gender-education cohorts combining the cross-sections through time. Provided with this data, we have estimated cohort models with fixed effects by LS and IV.

With practically no exception, all the results for the 1987-1999 confirm that unemployment is not a significant determinant of the decisions of becoming drinker and consuming alcohol. It is particularly important to confirm the robustness of most of the results to alternative specifications of cross-section, homogeneous and heterogeneous, static and dynamic cohort models. Results for the extended sample period 1985-2008 are mixing with some positive coefficients and some negative ones.

There are some important implications for health policies from these results. If alcohol consumption is independent from the business cycle as estimated in this paper, the health expenditure associated to alcohol abuse is not going to be affected by the phase of the cycle. Whether the authorities are interested on preserving the efficiency of public expenditure, it is necessary to identify different groups of individuals to carry out specific policies, since any attempt to perform universal and homogeneous actions is going to be fruitless.

3 CHAPTER THREE

3.1 Introduction

The growing concerns regarding the consequences of smoking are very well documented. As it is the case with many of other addictions, smoking has been reported for having negative consequences on both health and the labor market. Among the first group, smoking is causally linked to several severe illness (Surgeon General's Report, 2004), among which lung cancer is probably the most known one, yet it does not stand alone. Indeed, the list includes:

- Cancer: Bladder, Cervical, Esophageal, Kidney, Laryngeal, Leukemia, Lung, Oral, Pancreatic and Stomach.
- Cardiovascular diseases: Abdominal aortic aneurysm, Arthereosclerosis, Cerebrovascular disease, Coronary heart disease.
- Respiratory diseases: Chronic obstructive pulmonary disease, Pneumonia, Respiratory effects in utero, Respiratory effects in childhood and adolescence and Adults, Wheezing, Phlegm and Asthma.
- Reproductive effects.
- Other effects: Increased absenteeism and increased use of medical care services, Hip fractures, Peptic ulcer disease and low bone density.

According to the Center for Disease Control and Prevention, in the last 40 years the percentage of US adults who smoke regularly has dropped from above 42% in 1965 to below 20% in 2008. However, according to the same report smoking is still the number one cause of preventable death in the US, being responsible for the death of up to 438,000 smokers every year and another 49,000 individuals due to secondary smoking.

On top of that, smokers are up to 40% more expensive for the health care system than non-smokers, being responsible for up to $193 billions in annual health-related losses during the 2000-2004 period (Armour et al. (2005), Barendregt et al. (1997), Miller and Rise (1998) and Adhikari et al. (2008)). The social cost of smoking is estimated at $11 per pack, something that almost doubles its average private cost (CDC (2006)).

In this paper we will concentrate on the economic implications of smoking, in particular its potential consequences on labor productivity. Technically, there are two channels by which smoking can affect labor productivity. One way is by means of a worsening in the health status of the worker that potentially might cause more absenteeism and a worse on-the-job performance during his lifetime. The second channel is through the time budget. Addictions have to be fueled, which means that during working hours smokers will have the necessity of allocating part of their time to fulfill their need, decreasing the effective hours worked and, therefore, producing less than what they could have. If each smoke takes up to five minutes, the time effectively devoted to produce for a person that smokes up to 20 cigarettes per day can be reduced by one hour, that is, by more than 10%. It also might affect the concentration the smoker has just before he stops his duties in order to smoke (Smith (2001), Gilbert, Hannan and Lowe (1998) and UnityCommunicator (3, 2009))[1]. Moreover, many employers probably face higher costs when they hire a smoker, due to health insurance. However, it could be the case that smokers are just as productive as non smokers, but they suffer discrimination in their workplace reflected in lower earnings, less promotions, more firing or more time unemployed. Discrimination might arise because of the effects of secondary smoking on other employees or costumers, or just because of trends in society's preferences[2]. Similarly, the preferences of smokers regarding present and future consumption may lead them to invest less in productivity-enhancing human capital and therefore to get lower earnings.

[1] This is also be possible for many other habits, such as drinking coffee

[2] In this case, smoking would make other people less productive so it is not clearly whether it is discrimination or not

So far, the literature has focused mainly on measuring the wage penalization due to smoking. As long as the individual cannot effectively hide his habit, firms might be tempted to pay less to smokers or directly not hire them at all due to a lower productivity or because of discrimination reasons. Therefore, in a perfect competitive labor market the wage difference of two ex-ante identically individuals,where one of them smokes and the other does not would be explained by the bad habit. Unfortunately, the wage differential may also be the reflection of other factors, for instance an anti tobacco sentiment in the society. In any case, it is not entirely clear if the wage rate actually reflects labor productivity(Haefke, Sonntag and van Rens (2008)).Consequently, measuring whether smoking leads to a lower labor productivity might not be that easy using wage differentials solely. Instead of doing that, we will incorporate other outcomes in order to test whether smoking does affects labor productivity and see whether it has a discrimination component or not. Among the variables that we are going to use for this task are absenteeism from work due to health reasons, whether the individual was fired from his job, job tenure and on-the-job promotions.

A second thing that we have to be cautious about is that a person might have quitted smoking due to a very bad health shock. That is, people may have stopped smoking after the habit did enough damage. In that case he would probably have a lower labor productivity but he will be coded as non smoker. Therefore, not controlling for past negative health shocks would lead us to conclude that a current addict that has not suffered a severe health shock from his addiction is more productive than a person that does not smokes any more. Third, we have to control for whether the individual is a former smoker. According to the rational addiction theory (Becker and Murphy (1988)), smoking has stock effects. Therefore, the potential negative consequences on productivity would be carried through time. This would allow us to understand whether discrimination is present or not. Fourth an individual might choose the industry or the job taking into consideration how smokers are penalized. It is quite possible that highly addicted smokers are concentrated on those jobs that do not involve a great deal of routine or concentration. If this is the case, it is necessary to control for the type of employment and/or the type of industry. Fifth, unhealthy habits might be correlated among themselves. If smokers are more prone to be obese, chronic drinkers or binge drinkers, then their productivity is affected from two different channels and we might be incorrectly assigning an effect to smoking when the true problem could be alcohol. It is important, therefore, to include those addictions that affect labor productivity and are correlated with smoking, like for instance, weight or drinking problems.

Finally, people's addiction might be correlated with some unobservables that also affects labor productivity. If those unobservables are constant in time, then introducing fixed effects would control for this problem. However, it could be that the same thing that causes the individual to smoke, could make him less productive. In this case, we would have to instrument the decision to smoke. One possibility would be to use the tax on beer and tobacco as instruments for smoking and whether the individual started smoking before he was 16. We will also use a the enactment of a legislation to protect smokers in their job or in the hiring process. Indeed, 16 states passed a bill protecting smokers against discrimination between 1990 and 1992. Smokers in those states with a legislation that protect them would enjoy a higher wage than those in the states that do not protect them. Plus, the introduction of regulations to control tobacco use affected the time constraint of smokers in the regulated industries. As a result, we can identify if smoking has an impact on productivity through the legislation to control tobacco use.

This paper contributes to the existing literature on the effect of unhealthy habits on labor productivity on several dimensions. First of all, we propose a new set of instrument for the smoking decision. The Clean Indoor Regulations plus the bill to protect smokers right's in the job, can predict the decision of a person to smoke and on the other hand they should not be related to labor productivity. Indeed, the bill protecting smokers right's has never been used in any paper up to our knowledge. Secondly, we use a long panel. This is a great advantage because the negative effects of smoking accumulates through life, as in the rational addiction model(Becker and Murphy (1988) and Becker et al. (1994)). Consequently, we should be able to see it's consequences on labor productivity after some time. Third, we propose a set of measures to distinguish between the productivity and the discrimination effect of smoking. Finally, this is one of the first papers in the literature that corrects for standard error using clusters as the literature has recently shifted towards(Bertrand, Duflo and Mullainathan (2004), Thompson (2009) and Barrios, Diamond, Imbens and Kolesar (2010)). The remainder of the paper is organized as follow: In Section 2 we present a review of the literature. Section 3 describes the data that it is going to be used in the analysis and the methodology. In section 4 we show the econometric results and in section 5 we present the conclusions.

3.2 Literature Review

The literature on labor consequences of addictions and health problems is not that recent, although very few research has been devoted to measure the impact of smoking on labor productivity. One of the first attempts to relate income and alcohol consumption is Harwood, Napolitano, Kristiansen and Collins (1984). Using data from the 1979 National Alcohol Survey, they estimate the household income impact of alcohol problems. They found that a 21% reduction in household income is due to alcohol problems but that low levels of alcohol consumption increase income. However, Heien and Pittman (1995) use the same dataset and find that the results are not robust to different definitions of alcohol consumption.

Using data on drug abuse from the British Crime Survey, Macdonald and Pudney (2001) tries to assess the effect of addictions on labor market outcome through a Probit regression. They find that hard class drugs history is associated with current unemployment but there is not such a finding on soft drugs. Nonetheless their data was not enough to identify whether a consumer today was also a consumer yesterday. To overcome this problem, they restrict today's consumer to also having consumed in the previous period.

Therefore, the dynamics of their estimation is probably not robust. But more importantly they do not consider the amount of consumption, therefore equalizing frequent consumers to those that just tried drugs.

Another study analyzing the effect of alcohol consumption on labor productivity is Sato and Ohkusa (2003). Their research attempts to find a relationship between labor productivity and drinking while controlling for the diffusion of knowledge about the consequences of alcohol on health using a system of equations. The first equation treats current alcohol consumption as the endogenous variable, with current income as the exogenous variable and the second the other way around. There is a subset of covariates common to both regressions and a subset that affects only to one of the equations. They use duration of drinking, knowledge of the potential harms from drinking and the cause of the death of the individual parents as instruments for alcohol consumption and tenure and age as the instruments for income to deal with the potential endogeneity problem using a 3SLS approach.

Although they find that alcohol consumption raises labor productivity, measured by labor income, with an elasticity of 13% their identification is not clean enough. Among the different kind of problems it has, two are the most relevant: first, the sample size is particularly small; second, they do not correct for reverse causality problems. For instance, if an alcoholic person had cirrhosis in the past due to heavy alcohol consumption (which probably implies large duration of drinking as well as age) they will have a very low consumption today and probably a low labor productivity as opposed to a person that did not suffer a severe health shock. Therefore, the set of instruments is not proper. Also, they do not control for the self selection problem between drinking and the industry or the type of jobs they were employed in.

Levine, Gustafson and Velenchik (1997) is one of the few studies that analyzes whether smokers receive a wage lower than non-smokers, conditional on a set of characteristics, and they also try to identify the reason for the lower payment. Using the NLSY dataset, they test if the difference in the growth rate of wages between siblings is due to smoking. They find that smoking significantly reduces wages by between 4 to 8%. However they are unable to match that difference to any of the potential causes. Also, neither the health status of the individual nor alcohol consumption are included among the covariates.

Heineck and Schwarze (2003) uses the German Socioeconomic Panel to measure whether smoking leads to differential in earnings. To do so, the study uses a fixed effect regression trying different specifications for the way of measuring tobacco consumption. Accordingly, while smoking has no effect on women's wages, it reduces the wage of men by between 2 to 8%. Although their findings are quite similar to previous ones and the robustness check from different ways to measure smoking are a step towards correctly identifying the problem, the data set does not include important variables, like the health condition or other unhealthy habits like drinking. Consequently, the estimation shares some of the problems of Levine et al. (1997).

Studies that investigate the simultaneous effects of smoking and drinking on wages are Auld (2005) and Lye and Hirschberg (2004). Lye and Hirschberg (2004) study the effect of alcohol and smoking on wages using the 1995 Australian National Health Survey. They modify the standard capital health model in order to include the possibility of affecting earnings through education, health and the occupational choice and test it empirically using a simultaneous equation approach, such that they control for possible reverse causality. To control for the potential smoking endogeneity, the authors used the two-step Heckman's correction method. They attempt to control for previous health status by including the amount of medical experiences. They find that moderate drinking benefits workers but when including smoking the benefit disappeared, as in Heineck and Schwarze (2003). Yet, the use of cross section data undermines some of the potential identification possibilities.

Auld (2005) considers the impact on earnings of two different addictions in his paper:

that is, alcohol and smoking. By means of estimating a set of equations -one relating wages to smoking and alcohol, other for the drinking decision and another for the smoking decision- using Full Information Maximum Likelihood estimation on a repetition of cross sectional studies on Canadian prime-age male workers, they find that daily smoking causes a loss in earnings of about 24% while drinking does not have any significant effect. Even though this study is certainly an improvement, in the sense that it includes more than just one unhealthy habit, it is not free of questionings. First, the use of cross sectional data can lead to severe identification problems, since it is not possible to see how was the health in previous periods for instance. It is also difficult to assess changes in the behavior of the person or connect it to preferences. In that sense, it is possible that all the results are driven by unobserved variables that affects one cohort and not the other. van Ours (2004) also uses both addictions to check productivity. He uses a similar simultaneous equation approach, but with a different specification. Since all the usual potential instruments are not free of objections, he controls for unobserved heterogeneity by using as an instrument whether or not an individual started drinking or smoking before the age of 16. His finding confirms the usual result that moderate drinking increases in 10 percent labor productivity, measured as wages, but smoking reduces the wage in about the same amount. His study unfortunately uses only one cross section from a Dutch survey of 2001. Thus, it is also very unlikely that the starting age will control for all the unobserved heterogeneity. Plus, the usual self selection problem is not dealt with at all.

Keng and Huffmanm (2007) try to measure the causal link between binge drinking and labor market success. In their study, they use the NLSY to see what are the consequences of drinking in the wage an individual would receive in the market. Using a instrumental variables simultaneous equation approach, as in Nelson and Olson (1978), they measure the interaction between drinking and earnings. In the drinking specification, they include fixed effects per year and per state, the health demand of the individual and the lags and forwards of the drinking decision, proxy variables for the cost of health care, and current earnings. In the current earnings equation, besides fixed effects and the current drinking decision, they include current health status. They also include the inverse Mills ratio to control for potential self selection in the labor market. To instrument for potential endogeneity, they choose some of the usual instruments but include the MLDA as one of the them. Their findings are quite opposite to the usual cross sectional results: the decision to binge drink reduces earnings significantly. Although they include current health condition in the earnings equations, which as the authors claim would otherwise bias the results significantly, they do not attempt to include the health condition of previous periods. Since early periods health condition may determine current job status and productivity and also current health condition, it is a possible source of bias. Also, they do not include other variables correlated with drinking and earnings, as smoking.

Falba, Teng, Sindelar and Gallo (2005) try to answer the same question from a different perspective. Instead of measuring the impact of smoking, they test whether quitting smoking produces any significant increase in labor productivity. However, their findings clash with common sense, since the conclusion from their results is that labor productivity decreases after quitting smoking. However, the reason behind such a finding is that they do not control for alcohol consumption, health habits and they do not have longitudinal data in order to control for individual heterogeneity. Also, the information on the health condition was rather bad, since it does not include any chronic conditions.

Lokshin and Beegle (2006) estimate the wage losses due to smoking in a developing country using cross sectional data from Albania. They run a 2SLS, instrumenting smoking with parental smoking history and find that smokers have a 20% penalization on their wages. However, they cannot say whether this is due to discrimination or to lower productivity. Moreover, their dataset does not allow them to control for unobserved heterogeneity and they do not control for health shocks or whether the individual is a former smoker. Anger and Kvasnicka (2008) control for whether the individual is a former smoker and finds that when you do not control for that, the penalization is three time higher than otherwise. Braakmann (2008) uses panel data information from the UK in order to control for unobserved heterogeneity and he finds no evidence of a wage penalty.

3.3 Econometric Methodology and Data Analysis

a Data description

The main data source for the present research comes is the Panel Study on Income Dynamics (PSID hereafter). The PSID begun in 1968 and it was conducted annually until 1997, when it was changed to biannual at the same time that the number of families was reduced and they incorporated different ethnicity into the sample. The PSID is a longitudinal study of a representative sample of the US individuals. Prior to 1997 the sample size was 8,500 families but it was later reduced to 6,168. Overall, from 1968 to 2007 a number of more than 65,000 individuals were asked through 36 years of their life. In this study, we will use only the 1986, 1999 and 2001 waves. There are two reasons for doing this. Some of the outcomes of interest, most notably on-the-job promotion, are not available after 2001. On top of that, attrition is a potential problem and expanding the dataset until 2007 can made the situation worse-off[3].

The PSID provides a wide variety of info about the individual and his or her family on demographics, employment, residential location, health status, health costs, health behavior and other relevant variables. The main variables we are going to use from the PSID are demographics (age, sex, education, marital status and children), habits (whether the individual smokes or not, whether he has smoked in the past, cigarettes per day, whether he drinks, drinks per day), health status (health shocks, weight and height), industry/employment, whether he was promoted, number of days absent from work, whether the individual was fired, the spell of unemployment, job tenure and

[3] About 10% of the individuals are not repeated in the data.

earnings per hour. Using the SIC and NAICS classification of the industry and job we constructed the categories of government, primary, construction, manufacturing and services, as well as private office worker, recreational, restaurant, education and hospital. Employment was used to define the individual as blue collar, white collar or professional. In table 3.1 we can see a description of the main variables.

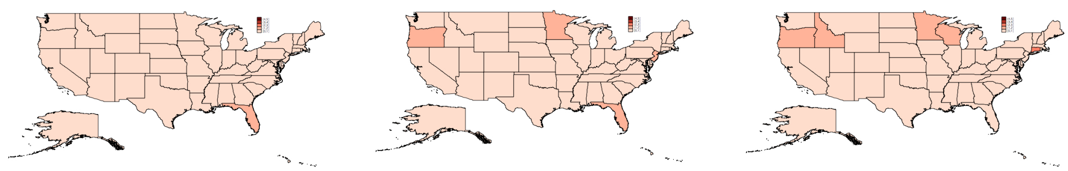
Table 3.1: Summary Statistics

Variable	**Mean**	**Std. Dev.**	**Variable**	**Mean**	**Std. Dev.**
Outcomes			*Religion*		
Log (Hourly Wage)	0.84	2.20	Catholic	0.19	0.39
Absent	0.30	0.46	Jewish	0.02	0.14
Frequency of Absenteeism	1.46	7.72	Protestant	0.54	0.50
Job Tenure	5.07	7.52	Muslim	0.01	0.08
Looking Job	0.09	0.77	Orthodox	0.00	0.04
Job Promotion	0.03	0.17	Other Religion	0.06	0.24
Demographics			*Job*		
Gender	0.52	0.50	Blue Collar	0.33	0.47
Age	43.54	15.64	White Collar	0.14	0.35
Education			Professional	0.26	0.44
Drop out	0.07	0.26	*Industry*		
High School	0.71	0.45	Primary	0.02	0.15
College	0.14	0.34	Construction	0.05	0.22
Graduate	0.08	0.27	Manufacturing	0.19	0.39
Marital Status			Services	0.46	0.50
Married	0.66	0.48	*Habits*		
Single	0.14	0.35	Binge Drinker	0.13	0.33
Widowed	0.06	0.24	Drink	0.58	0.49
Divorced	0.10	0.31	Exercise	0.84	0.37
Separated	0.04	0.19	Underweighted	0.05	0.21
Children	0.98	1.20	Overweighted	0.72	0.45
Race/Ethnicity			Obese	0.56	0.50
White	0.32	0.47	Smoke Currently	0.18	0.38
Hispanic	0.03	0.18	Smoke Ever	0.50	0.5 0
Black	0.12	0.33	Cond. No Smokes	18.54	11.44
Asian	0.01	0.12			
Other Race	0.03	0.17			
Observations	**26,589**				

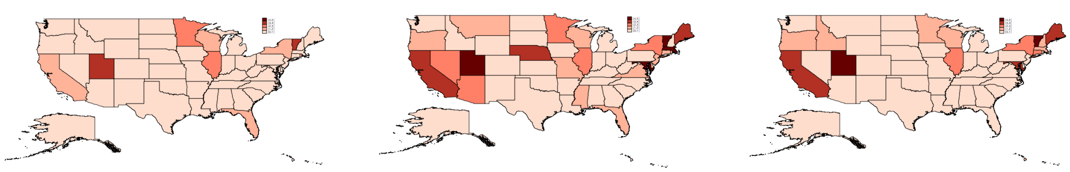
Source: *Panel Study of Income Dynamics -1986, 1999 and 2001.*

The other relevant source of data are the set of bills enacted by each state legislatures from 1985 to 2001. This is a compendium of all the regulations that were enacted by state legislatures regarding tobacco use obtained from the National Cancer Institute. Using the raw data, we classified it and developed an index of laws regarding tobacco use that we are going to use as exogenous changes. The scaling of the index is based on Chiriqui, Frosch, Shelton, Sciandra, Hobart, Fischer and Alciati (2002) and the indications from the American Lung Association. Some changes were introduced in order to enrich the index. For instance, while Chiriqui et al. (2002) use a separate variable to account for the enforceability of the law, we included only those laws that were enforceable and had a penalization to avoid identification problems. The second relevant change is that we have included a dummy variable those states that have enacted a law that protects smokers on-the -job. Usually this laws forbids any type of discrimination against smokers, be it in the hiring process or in the compensation. Finally, in the regressions we have grouped the legislations into three categories: no regulation, mild regulation and strong regulation. Figure 3.1 shows an account of the distribution of the regulations in 1986 and 2000 for Private Offices, Public Offices and Restaurants. The darker the color, the tighter the regulation. Figure 3.2 shows the distribution of smokers right's on the job market as of 2000. States that have no bill to protect smokers are in dark red.

Figure 3.1: Regulations Regarding Tobacco Use - USA 1985-2000



(a) Private Offices, 1986 (b) Public Offices, 1986 (c) Restaurants, 1986



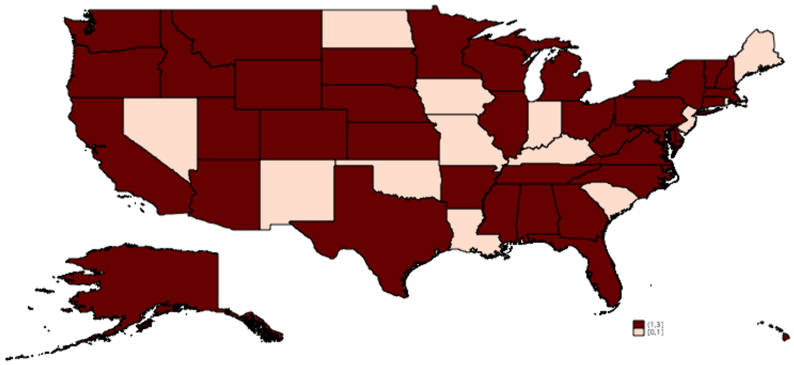
(d) Private Offices, 2000 (e) Public Offices, 2000 (f) Restaurants, 2000

Source: Own Compilation of Regulations Regarding Tobacco Use from the State Cancer Legislative Database, National Cancer Institute

b Labor Productivity

In a perfect competitive labor market, producers set marginal costs equal to marginal income. Therefore, in this context, the wage a person earns would automatically tell us his labor productivity. Unfortunately, if the labor market is subject to frictions this is no longer the case. Whether there are cost associated with the search of a worker

Figure 3.2: Smokers Rights



Source: Own Compilation of Regulations Regarding Tobacco Use from the State Cancer Legislative Database, National Cancer Institute

in the hiring process or if a group of workers has certain bargaining power, then their wage might be different than their labor productivity.

As a consequence, using the wage rate as a substitute for productivity can lead to two problems. If the error in measuring productivity is uncorrelated with the variable of interest, then standard errors will be inflated and thus it is possible that we conclude that smoking has no effect on labor productivity when it does have it[4]. However, if the measurement error is correlated with smoking or with other variables themselves correlated with smoking but not included in our study, the estimates will be biased, with no prior knowledge of its direction. In order to avoid reaching a wrong conclusion, we decided to include several outcomes correlated with labor productivity, beyond the wage rate. We are going to include as outcome variables whether the individual was promoted in his job, whether he was fired from his job, whether he was absent from work due to illness and the tenure in his job All these variables are correlated with labor productivity and they might be able to capture it with even more precision than the wage rate. For instance, in some industries all the workers have a collective clause and earn the same wage rate regardless of their productivity. However, only the best of them will be offered a promotion and the worst will be fired. While observing how much they earn might not tell us how good or not they are, observing the history in

[4]That is, assuming this is the only identifying issue

the job may do the job.

c Methodology and Identification Strategy

Besides coming up with a good proxy for labor productivity, there are a three more obstacles to overcome in order to properly identify the effect of smoking on labor productivity. The first one is preferences. Preferences are a big determinant of the decisions a person makes. The type of job one has and therefore his wage rate and the decision to smoke are correlated with his preferences. Alternatively, smoking and labor decisions may be jointly determined by a common attitude towards risk. Therefore, controlling for them is very important in order to avoid having biased estimates. Unfortunately. preferences are not observable, yet they are usually consider stable through time. This means that it is possible to control for them by means of introducing fixed effects for each individual. The downside of this is that we are going to loose the possibility of knowing the impact of things that are constant in time, like sex, race, gender or regulations that were introduced before our data starts.

The second problem is that people that quitted smoking might have done so for reasons that are correlated with labor productivity. For instance, in the event of lung cancer a person will probably quit smoking Clark and Etile (2002). However, his productivity will also be lower than otherwise. As a result, we have to be careful of including individuals who suffered a major health shock before our data starts and also we need to control for current health.

The third problem in identifying the true parameter is related to reverse causality. Labor productivity may determine tobacco use. For instance, a positive correlation between unemployment and smoking does not necessarily means that smoking causes a higher likelihood of being unemployed, as it can turn out to be that loosing a job leads to smoking. Failing to address endogeneity will lead to biased coefficients estimates.

In order to sort out this problem, we need to find a set of instruments for the smoking decision such that two conditions are satisfied. First, after controlling for all the other exogenous variables it must be significantly statistically with the decision to smoke. Second, it must be exogenous to the error. In other words, the instrument should not be able to predict the outcome.

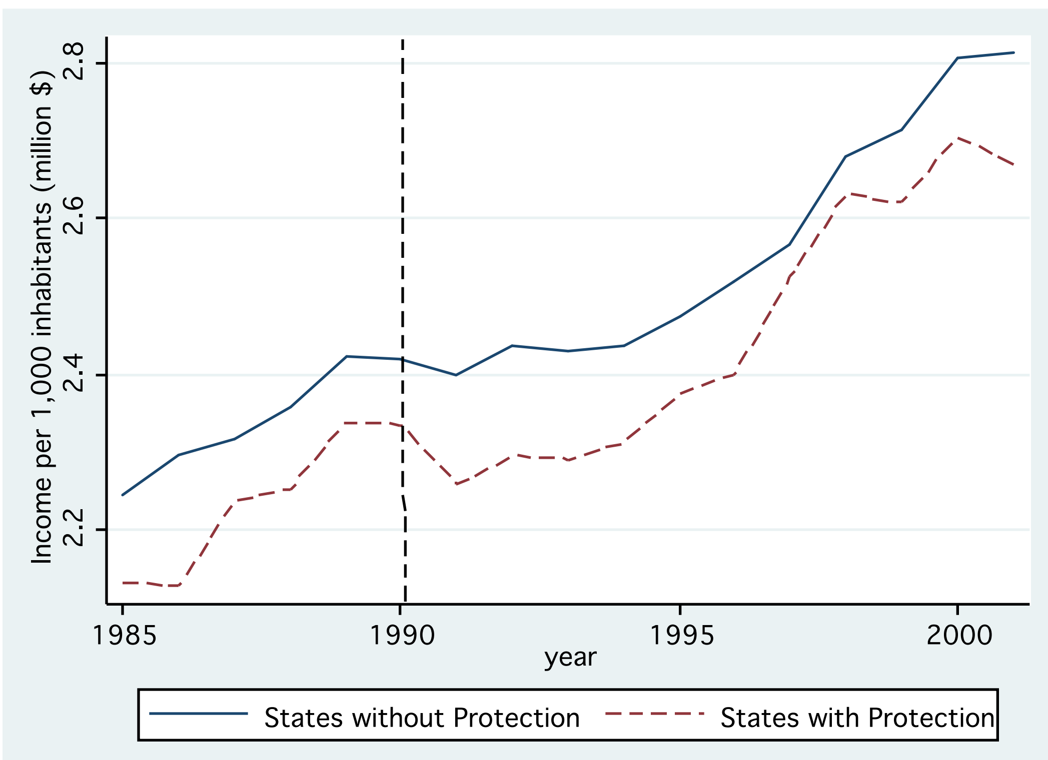
The literature on smoking has used policies on tobacco use, taxes and prices of tobacco (Chaloupka and Wechsler (1997) and Ross, Powell, Tauras and Chaloupka (2005)) based on the assumption that higher cigarette taxes or a tighter regulation will discourage smoking personal beliefs. Although they proved to be weak, it has also used personal characteristics (Clark and Etile (2002), Auld (2005) and Lundborg (2007)). Some papers have used if the individual smoked before the age of 16 (van Ours (2004)) as an instrument. In this paper, we are going to use as instruments the tax on tobacco, the tax on beer, regulations regarding tobacco use and whether the individual smoked before the age of 16 as instruments.

The equation we are interested into estimate is therefore:

$$\text{Productivity}_{ijt} = \alpha + \beta \ \text{X}_{ijt} + \gamma \ \text{Smoking}_{ijt} + \theta \ \text{Industry}_{ijt} + \delta_j + \delta_t + \mu_i + \epsilon_{ijt} \quad (3.1)$$

Labor productivity of individual i who lives in state j in period t is a function of individual characteristics (including those habits correlated with smoking, like drinking or eating), the industry where he works, the regulations and taxes of state j in period t, and whether he smokes or not. The decision to smoke is modeled as a dichotomic variable, whether he/she is a current smoker, whether the individual has smoked at some point in his life, whether he is a heavy smoker (number of cigarettes smoked greater than 15) or whether he is a light smoker (strictly less than 15 cigarettes a day). We are going to combine the different outcomes and see whether there is a different effect for smokers and for former smokers. Because the negative effects of smoking accumulates through time, if it is productivity related we should see an effect not only on current smokers but also on former smokers. The coefficients of interest are therefore γ. The decision to smoke will be instrumented with taxes on tobacco and beer, regulations on tobacco use, the bill protecting smokers right's and whether the individual started smoking before his 16th birthday, all of them previously used in the literature, except for the bill protecting smokers. Graphic **??** shows the trend in the per capita income for the states that enacted a bill protecting smokers before and after the changes. As we can see, there is no obvious difference in the trend before the introduction of the bills, although there seems to be a larger drop in income per capita in the states that introduced the protection, and a catching up after 1996.

Figure 3.3: Income differences between states that protect smokers' on-the job versus those that do not



Source: Bureau of Labor

3.4 Results

In Table 3.2 we present the pooled least squares regression of the impact of smoking on the different outcomes we are going to use as proxies for labor productivity. While from this table we can not infer causality, it shows a small flavor of what actually expect a priori. Indeed, smoking is negative and significantly correlated with wages and on-the-job tenure and positively correlated with absenteeism and the probability of being unemployed. We can also see from the table that all the other coefficients have the expected sign. Since we are only interested in the effect of smoking on labor productivity, from now on we are only going to present those results regarding smoking coefficients, whether it is current smoking, ever smoker, heavy smoker or light smoker[5].

As we mentioned in the introduction, the regressions showed in table 3.2 have two problems at the least. First of all, unobservables that are correlated with both labor productivity and the decision to smoke are not controlled for. Time preferences, ability, etc., will have an impact on both variables. Therefore, it is important to control for unobserved heterogeneity by means of a standard fixed effects regression. Second, as smoking is a decision, it can be endogenous to labor productivity. Unfortunately the direction of the bias is not clear ex-ante. For instance, people with a lower ability might use smoking in the short run in order to work the extra mile and thus catch up with more productive individuals. It can also be the case that people with more ability can afford the 'pleasure' of smoking. Or, it can be that smoking and low productivity are correlated with a third variable. In any of the cases, it is necessary to correct for both problems in order to have a clean identification of the effect.

[5]Full results available upon request

Table 3.2: Pooled OLS Regression

	Log(Wage)	Absent	Promotion	Fired	Unemployed	Tenure
Smoke Currently	-0.087***	0.004	-0.002	0.011***	0.015***	-0.550***
	0.01	0.01	0.00	0.00	0.01	0.13
Man	0.273***	-0.055***	0.001	0.010**	0.028***	0.004
	0.02	0.01	0.00	0.00	0.01	0.12
Age	0.049***	0.001	-0.002**	0.002**	0.008***	0.583***
	0.00	0.00	0.00	0.00	0.00	0.04
Age^2	-0.001***	-0.000**	0.000	-0.000***	-0.000***	-0.005***
	0.00	0.00	0.00	0.00	0.00	0.00
Educ 2	0.324***	0.024	0.005	0.001	0.010	0.292
	0.04	0.02	0.00	0.01	0.01	0.24
Educ 3	0.574***	0.029	0.002	-0.003	-0.001	-0.016
	0.04	0.02	0.01	0.01	0.01	0.30
Educ 4	0.721***	0.046*	-0.006	-0.001	0.001	0.207
	0.04	0.02	0.01	0.01	0.01	0.49
Union	0.189***	-0.04*	0.01	-0.003	-0.005*	4.895***
	0.01	0.02	0.01	0.00	0.00	0.29
Blue	-0.013	0.362***	0.027	-0.048**	-0.212***	5.399***
	0.06	0.07	0.02	0.02	0.02	0.71
White	0.181***	0.393***	0.050**	-0.050***	-0.202***	5.372***
	0.06	0.07	0.02	0.02	0.02	0.75
Professional	0.341***	0.404***	0.069***	-0.052***	-0.204***	5.989***
	0.07	0.07	0.02	0.02	0.02	0.66
Primary sector	-0.214***	-0.091	-0.017	-0.016	-0.004	-0.616
	0.08	0.07	0.03	0.02	0.02	1.08
Constr sector	0.077	-0.024	-0.025	-0.009	-0.021	-0.692
	0.06	0.06	0.02	0.02	0.02	0.77
Manuf sector	0.183***	0.008	0.002	-0.017	-0.022	2.026**
	0.06	0.07	0.02	0.02	0.02	0.77
Services sector	-0.031	-0.005	-0.014	-0.011	-0.010	0.702
	0.06	0.06	0.02	0.02	0.02	0.67
Obese	0.003	0.034***	0.000	0.001	0.016***	0.019
	0.02	0.01	0.01	0.00	0.00	0.23
Over weighted	-0.027*	0.018	0.007	0.001	0.004	0.045
	0.02	0.01	0.00	0.00	0.00	0.14
Under weighted	-0.003	-0.009	0.004	0.001	-0.000	0.119
	0.03	0.02	0.01	0.00	0.01	0.31
Constant	-4.512***	0.080*	0.072***	0.045***	0.062***	-14.902***
	0.09	0.05	0.02	0.01	0.02	0.90
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	Yes	Yes
Obs	15,758	17,988	17,988	17,988	17,988	17,968
R^2	0.90	0.13	0.03	0.04	0.20	0.30

$*p < 0.10; **\ p < 0.05, ***p < 0.01$

Heteroscedastic-consistent standard errors clustered at the state level reported. All regressions include but not report for race, religion, marital status, children, State dummies and Yearly dummies. Panel Study of Income Dynamics 1986, 1999 and 2001.

a Earnings

To begin with the analysis, as in most studies, we proxy productivity with the wage rate. Table 3.3 shows the effect of current smoking and ever smoker on the log of the wage rate in the context of a pooled OLS regression and a fixed effect regression, without instrumenting the decision to smoke. As we can see from table 3.3, smoking is penalized in terms of the wage rate, both in the decision to smoke, if he is a heavy and light smoker and also if the person happens to be a former smoker. Once we control for unobservables by means individual fixed effect's, statistical significance is lost. However, the sign remains negative but the coefficient is small enough to be neglected. The fact that former smokers are also penalized provides some evidence that at least part of the effect does not come from discrimination[6], since there is no reason why former smokers also bear a penalization if it is not due to a lower productivity.

Table 3.3: OLS and Fixed Effect Regressions of Wage rate

	OLS		**FE**	
	Model 1	Model 2	Model 1	Model 2
Smoke currently	-0.077***		-0.001	
	0.02		0.02	
Smoke ever	-0.015	-0.015	-0.025	-0.008
	0.01	0.01	0.02	0.03
Heavy smoker		-0.079		-0.001
		0.05		0.03
Light smoker		-0.094***		0.011
		0.03		0.03
Year	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes
Obs	15,758	15,758	15,758	15,267
R^2	0.90	0.90	0.95	0.95

$*p < 0.10$;$**\ p < 0.05$, $***p < 0.01$

Heteroscedastic-consistent standard errors clustered at the state level reported. All regressions include but not report for race, religion, marital status, children, employment, industry, union, BMI, Age, Age squared, Gender, State dummies and Yearly dummies. Panel Study of Income Dynamics 1986, 1999 and 2001.

The specifications introducing fixed effects correct for one part of the bias, the one related to things that are constant in time yet correlated with both the decision to smoke and the data generating process of the wage rate, like for instance a higher discount rate or a different risk preference. However, those specifications still do not correct for the bias related to things that might change in time, like for instance, the decision to start smoking because of reasons correlated with workplace choice or with ability. In order to correct for this problem we are going to instrument the decision to smoke. In table 3.4 we show three specifications. Model 1 uses and instrument smoke currently, Model 2 only includes heavy smoking and the third columns is the same specification as the first one, but the set of instruments is different. The first two

[6]It is important to remember that we have removed from the sample those individuals that have suffered a health shock causally related to smoking

columns are the preferred ones in terms of the instrumental variables. As we can see, the sign of smoking in both models is negative plus the penalization seems to be higher for heavy smokers (Model 2), as we would have expected. However, we can not reject the null hypothesis that smokers have a different wage than non smokers, as the standard errors are quite large. In the third specification we find a negative and significant effect for smoking, although we use a non conventional instrument, that is, health shocks. While the Hansen J test does not reject this instrument, the effect on wages is too large to be credible. So, even though the evidence goes in the direction of smokers earning a lower wage, we can not say much because it is not significant. Nevertheless there may be other channels that can show that smokers are less productive.

Table 3.4: Fixed Effects of Wage rate instrumenting the smoking decision

	Model 1	Model 2	Model 1
Smoke currently	-0.027		-0.554**
	0.28		0.27
Heavy smoker		-0.187	
		0.33	
Year	Yes	Yes	Yes
State	Yes	Yes	Yes
Obs	13,044	13,044	12,196
R^2	0.95	0.95	0.94
Hansen j stat	2.31	2.23	2.30
Hansen j df	3	3	
Hansen j p-val	0.51	0.53	0.51
Weak Ins Stat	10.80	12.33	14.85

$*p < 0.10; ** \ p < 0.05, ***p < 0.01$

Heteroscedastic-consistent standard errors clustered at the individual and at the year level reported. All regressions include but not report for race, religion, marital status, children, employment, industry, union, BMI, Age, Age squared, Gender, State dummies and Yearly dummies. Smoke currently and heavy smoker instrumented using Clean Indoor Regulations, Smokers right's bill, tax on tobacco except in column three where we use health shocks. Panel Study of Income Dynamics 1986, 1999 and 2001.

b Absenteeism

The next outcome we are going to explore is the effect of smoking on labor absenteeism due to health reasons. Since tobacco use is causally related with several diseases, then we should expect that smokers have a larger probability of becoming sick. But since former smokers might have done enough damage to their body, we should also see some evidence in them as well. Table 3.5 shows the least squares specification and the fixed effects regression with and without instrumenting the decision to smoke. Model 1 refers to the one where all current smokers are pooled and in Model 2 we split them into heavy and light smokers, but both of them also includes if the individual used to smoke[7]. As we can see from the least squares regression, smokings has long run consequences, since people that used to smoke also have a larger probability of absenteeism. Controlling for unobservables does not change the sign nor the non significance on current smokers, although it makes the heavy smoker coefficient significant[8] However, once we instrument the decision to smoke we see that the probability of being absent from work because of health reasons is positively and significantly affected both for current and heavy smokers and, as expected a priori, heavy smokers have a larger effect.

That is to say that the health dimension seems to be relevant for labor productivity. Smoking does seems to increase on average the probability of being absent from work and therefore this should be reflected in the smoker's labor productivity. Another thing that it is interesting is that quitting smoking does not seems to improve the chances of not being absent from work. That is, the stock of 'damage' that tobacco does to the organism stays long enough such that people that used to smoke also have a higher chance of missing work due to health reasons. It is important to see that this variable has no discrimination imbedded and therefore it is evidence that smoking decreases

[7]except in the IV specification since we do not have instruments that are orthogonal for the two variables

[8]Still, the effect for current smokers and heavy smokers is non significant as the excluded group here is people that used to smoke but does not smoke any more. Current smokers coefficient is then the sum of both Smoke Currently and Smoke ever.

Table 3.5: Smoking effect on Absenteeism

	OLS		FE		IV FE	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Smoke currently	-0.012		-0.024		0.106*	
	0.01		0.02		0.06	
Smoke ever	0.029***	0.027***	0.036**	0.036**		
	0.01	0.01	0.02	0.02		
Heavy smoker		-0.007		-0.030*		0.213*
		0.01		0.02		0.13
Light smoker		-0.022		-0.016		
		0.02		0.03		
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	No	No
Zone	No	No	No	No	Yes	Yes
Obs	17,988	17,988	17,988	17,988	15,484	15,484
R^2	0.13	0.13	0.09	0.09	0.07	0.06
Hansen j stat					1.19	1.26
Hansen j df					2	2
Hansen j p-val					0.55	0.53
Weak Inst Stat					48.90	7.66

$* p < 0.10; ** \ p < 0.05, *** p < 0.01$

Heteroscedastic-consistent standard errors clustered at the individual and at the year level reported. All regressions include but not report for race, religion, marital status, children, employment, industry, union, BMI, Age, Age squared, Gender, State dummies and Yearly dummies. Smoke currently and heavy smoker instrumented using Smokers right's bill, tax on beer and if the individual started smoking before his 16th birthday. Panel Study of Income Dynamics 1986, 1999 and 2001.

labor productivity.

c Promotion

Another potential way to see whether smoking has an effect on labor productivity is through on-the-job promotions. If indeed smoking decreases labor productivity, it should be the case that smokers are less promoted than non smokers, that is, the probability of being promoted if you are a smoker should be lower than if you are not. Again, because smoking has long run consequences, both current and former smokers should face this penalization, otherwise it would be evidence of discrimination. The results in table 3.6 points in the former direction. Indeed, the sign of smoking is negative, that is, smoking decreases the probability of a promotion in the job, both for current and former smokers. However, we only find a significant coefficient for smokers in general, that is, both former and current smokers, in the fixed effect regression. In the instrumental variables regression, the sign is still negative, although not significant.[9] Smokers do seem to have a lower probability of being promoted. Nevertheless this is not due to the fact that their bosses dislike the habit, as not only current smokers are penalized but also former smokers.

[9] We should bear in mind that we are clustering the standard errors in two dimensions and using a system GMM estimation, which is to say that we are taking a very conservative strategy.

Table 3.6: Smoking effect on Promotion in the Job

	OLS		FE		IV FE	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Smoke currently	-0.004		-0.002		-0.015	
	0.00		0.01		0.01	
Smoke ever	0.002	0.002	-0.022**	-0.022**		
	0.00	0.00	0.01	0.01		
Heavy smoker		-0.006		-0.005		-0.027
		0.00		0.01		0.03
Light smoker		-0.000		0.001		
		0.01		0.01		
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	No	No
Zone	No	No	No	No	Yes	Yes
Obs	17,988	17,988	17,988	17,988	15,484	15,484
R^2	0.03	0.03	0.02	0.02	0.01	0.01
Hansen j stat					1.73	1.78
Hansen j jdf					3	3
Hansen j p-val					0.63	0.62
Weak Inst Stat					66.18	32.52

$*p < 0.10; **\ p < 0.05, ***p < 0.01$

Heteroscedastic-consistent standard errors clustered at the individual and at the year level reported. All regressions include but not report for race, religion, marital status, children, employment, industry, union, BMI, Age, Age squared, Gender, State dummies and Yearly dummies. Smoke currently and heavy smoker instrumented using Smokers right's bill, tax on beer and on tobacco and if the individual started smoking before his 16th birthday. Panel Study of Income Dynamics 1986, 1999 and 2001.

d Firing

Being fired from the job is yet another proxy variable for labor productivity. The argument is similar to the probability of on-the-job promotion: if an employer has to make a choice that involves firing an individual among a group, he will probably choose the least productive one. So, if it is true that by means of smoking labor productivity decreases, we should then see that smoking increases the probability of being fired. The regression analyses do corroborate the story, although in this case, evidence does not rule out discrimination. In table 3.7 all three specification, that is least squares, fixed effects and instrumental variables with fixed effects, indicate that smoking significantly raises the probability of being fired. It also indicate that heavy smoking involves an even larger probability. However, there is no evidence that having smoked in the past has any effect on the probability of being fired. In this case, the regression shows that smokers seem to suffer some sort of discrimination.

Table 3.7: Smoking effect on the probability of being fired from the Job

	OLS		FE		IV FE	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Smoke currently	0.010**		0.012**		0.022*	
	0.00		0.00		0.01	
Smoke ever	0.002	0.003	-0.007	-0.007		
	0.00	0.00	0.01	0.01		
Heavy smoker		0.014***		0.017***		0.041*
		0.01		0.01		0.02
Light smoker		0.003		0.004		
		0.01		0.01		
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	No	No
Zone	No	No	No	No	Yes	Yes
Obs	17,988	17,988	17,988	17,988	15,484	15,484
R^2	0.04	0.04	0.05	0.05	0.04	0.04
Hansen j stat					2.02	2.07
Hansen j df					5	5
Hansen j p-val					0.85	0.84
Weak Inst Stat					44.63	23.22

$* p < 0.10; ** \ p < 0.05, *** p < 0.01$

Heteroscedastic-consistent standard errors clustered at the individual and at the year level reported. All regressions include but not report for race, religion, marital status, children, employment, industry, union, BMI, Age, Age squared, Gender, State dummies and Yearly dummies. Smoke currently and heavy smoker instrumented using Smokers right's bill, tax on beer and tobacco, clean indoor regulations and if the individual started smoking before his 16th birthday. Panel Study of Income Dynamics 1986, 1999 and 2001.

e Job Tenure

The last outcome we analyze as a proxy for labor productivity is on-the-job tenure. A long tenure is a signal that the worker is happy in his workplace but also, that his employer is happy with his performance. As a consequence, more productive individuals should have a larger tenure than less productive one. In this case, the least squares specification shows significative evidence that smokers, both current and past ones, have a lower on-the-job tenure. However, Table 3.8 neither the fixed effects nor the instrumental variable regressions have a significative result. While the sign is the expected one, standard errors are large enough such that we can not reject the null hypothesis. An extra problem in this specification is that the coefficient on heavy smoker is larger than one in absolute value, something that unfeasible in terms of probability. While this is a standard problem in linear probability analysis, solving both unobserved heterogeneity and reverse causality is non trivial in non linear analysis.

Table 3.8: Smoking effect on the Tenure

	OLS		FE		IV FE	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Smoke currently	-0.272*		0.005		-0.632	
	0.15		0.18		0.57	
Smoke ever	-0.443**	-0.326*	-0.077	-0.073		
	0.17	0.17	0.26	0.26		
Heavy smoker		-0.201		0.049		-1.108
		0.16		0.24		1.14
Light smoker		-0.451**		-0.056		
		0.18		0.23		
Year	Yes	Yes	Yes	Yes	Yes	Yes
State	Yes	Yes	Yes	Yes	No	No
Zone	No	No	No	No	Yes	Yes
Obs	17,968	16,893	17,968	17,968	15,462	13,825
R^2	0.30	0.30	0.23	0.23	0.21	0.20
Hansen j stat					2.11	2.18
Hansen j df					5	5
Hansen j p-val					0.83	0.82
Weak Ins Stat					44.38	11.13

$*p < 0.10; **\ p < 0.05,\ ***p < 0.01$

Heteroscedastic-consistent standard errors clustered at the individual and at the year level reported. All regressions include but not report for race, religion, marital status, children, employment, industry, union, BMI, Age, Age squared, Gender, State dummies and Yearly dummies. Smoke currently and heavy smoker instrumented using Smokers right's bill, tax on beer and tobacco, clean indoor regulations and if the individual started smoking before his 16th birthday. Panel Study of Income Dynamics 1986, 1999 and 2001.

3.5 Conclusions

In recent years, smoking has been associated with several diseases, some of them very harmful like for instance lung cancer. It is also suspected of reducing the effective time dedicated to work, in particular now that it is becoming harder and harder to smoke inside the working space. The connection with labor productivity therefore should be quite straightforward. However, finding such a connection is not such an easy task since there are many potential sources of bias. On top of that, there is not an unique way of measuring labor productivity. Moreover, since labor productivity is usually associate with earnings, discrimination is an extra source of noise.

In this paper we propose a set of instruments and a set of outcomes variables, such that we can eliminate three types of bias, that is, unobserved heterogeneity, reverse causality and attrition due to health reasons and have a better understanding of how smoking affects labor productivity at the same time. After correcting for them, we find that smoking is indeed linked to a lower labor productivity. Even though we find that the wage penalization of smoking is not significant yet negative, when we take into consideration all the other potential candidates to proxy for labor productivity we have significative evidence that smoking increases the probability of becoming sick, reduces the probability of being promoted and job tenure and increases the probability of being fired. We are also able to say something on discrimination against smokers. Firing is only significative for current smokers when it should also affect those that are past smokers.

A couple of important implications for policy making can be extracted from the results. If smoking reduces labor productivity beyond health problems, then all the strategies to forbid smoking in the workplace should be complemented with serious efforts to reduce the incidence of smoking in those places. Second, the health consequences of smoking have two costs. One is the lost productivity and the other one is health expenditures. A cost benefit analysis of a program aimed at reducing the prevalence of smoking should then include both.

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