Further evidence about alcohol consumption and the business cycle*

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Abstract

The main goal of this paper is to test whether macroeconomic conditions affect alcohol consumption using data from the Behavioral Risk Factor Surveillance System for the period 1987–2008. While previous evidence have used pooled cross-section data and rich specifications controlling observed heterogeneity, we feel that both state dependence and unobserved heterogeneity can help in explaining individuals' behavior. We control for both effects by relying on the construction of pseudo-panel data from the different cross-sections available in the survey. Our results contain three messages. First, unemployment rates have significant effects on becoming drinker and the number of alcoholic beverages consumed but their cyclical or countercyclical effects depend very much on the period under consideration. Second, once unobserved effects and the dynamic nature of the demand models are accounted for, we do not detect business cycle effects independently of the period. Third, we feel that a methodological implication can also be inferred from our results: the importance of taking into account the nature of the zeros when estimating demand models for this good. As a result, inferences obtained without controlling for them should be interpreted with caution.

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1 Introduction

Concerns about the economic implications of the relationship between alcohol consumption and the labor market are well grounded. Most of the empirical literature has maintained the commonly held view that alcohol drinking is associated with lower earnings, lower employment rates, greater unemployment and productivity losses. Nevertheless, some authors (**??**) emphasize that the direction of the casuality between unemployment and alcohol consumption is not conclusive. While some studies have shown that unemployment is positively correlated with alcohol consumption (**?**), with alcohol abuse (**?**) and with diseases and psychological problems derived from alcohol abuse (**?**), other analyses suggest that the correlation is nonexistent or even negative (**?**, **?**).

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 consumption of alcohol (**?**). While some authors support the existence of a positive relation between financial strain and depression, understanding chronic financial strain as a situation in which is difficult to satisfy basic needs (**??**), others have found a positive relation between depression and alcohol consumption (**?**). Analyses within the second set argue that unemployment usually implies lower consumption through an income effect. This reduction could not happen when unemployment is transitory and the unemployed receive benefits or family support.

An important number of previous papers are based on the existence 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 works use individual data and relax the representative consumer assumption (**??** are two good examples). In this work we propose a further step to explicitly consider unobserved heterogeneity among individuals. Since panel data is not available we rely on cohorts built from independent cross-sections taken from the *Behavioral Risk Factor Surveillance System* (BRFSS from now on).

Another important issue in estimating consumption models for alcoholic drinks is the treatment of habits. For the same reasons that results could be misleading when non-considering unobserved effects, the omission of dynamics for 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 analysing the existence of rationality in the consumption of several goods from the seminal paper of **?**. **????** and **?** constitute some interesting examples.

Finally, we think that it is not 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 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 an 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 parameter estimates of the UR.

The objectives of the paper are threefold. The first and fundamental one is to show the effects of misspecification caused by missing unobserved heterogeneity and dynamics. In the case of the omission of unobserved effects even assuming they are uncorrelated with the regressors 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 **?** and **?** 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 in interpreting the results produced by more complex models. 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 unemployment is not a significant determinant of the decisions of becoming drinker and consuming alcohol, once the unrestricted specifications are estimated. 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 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.

sectionModel and relationship to previous literature

1.1 The model

Suppose that Y is an indicator of whether an individual is or not a drinker or 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 $Y = 1(Y^* > 0)$ for the binary choice being a drinker or not or $Y = \max(Y^*, 0)$ for the number of drinks consumed. $1(A)$ is the indicator of event A.

Consider a general linear model for the latent variable:

$$Y^*_{ismt} = X_{ismt}\beta + UR_{smt}\gamma + \alpha_s + \delta_m + \lambda_t + \eta_i + \varepsilon_{ismt} \tag{1}$$

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 variables, UR refers to unemployment rate, ε is an error term and α, δ, λ and η are state, month, year and individual unobserved factors.

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 **?**, in which we allow the possibility that the dependent variable be limited or qualitative (binary or a count).[1]

1.2 Relationship to previous literature

The identification of the effects of all determinants of participation and consumption becomes more complicated when we rely on time series or cross-section data. In the first case, we have to claim for the existence of a representative consumer and it is only possible to identify β and γ. For example, **??** 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,

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

Brenner's work has been very criticized by other authors as **??** or **?** arguing absence of rationality in the election of the unemployment rate lag structure, high collinearity and variability of the results conditioned on time period, country or proxies for health status chosen.

When using a single cross-section instead of time series data, a similar problem appears. In terms of (**??**) we only can estimate separately β and γ (also α and δ if we have information for month and state of the observation). If cross-section data correspond to countries, states or regions, the problems of infra-specification and existence of representative consumer are equivalent to the previous ones (Jahoda, 1991). In a photography of individual alcohol consumption, the advantage is that X will contain a wide range of demand determinants (income and socioeconomic characteristics). 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 et al., 1991). Some authors (Hammarström et al., 1988, for instance), 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.

Another alternative is to use fixed effects panel data models for states or countries:

$$Y_{smt} = X_{smt}\beta + UR_{smt}\gamma + \alpha_s + \delta_m + \lambda_t + \varepsilon_{smt} \tag{2}$$

where Y_{smt} is the dependent variable for state (country or region) s, month m and year t, UR_{smt} refers to the unemployment rate, X_{smt} is a vector of other explanatory variables and ε_{smt} is an error term. The terms δ_m and λ_t reflect monthly and annual shocks common to all states and α_s controls those factors that are constant across time, but different among states. In this setting, **?** evaluated the relationship between the age at the onset of alcohol consumption and mortal accidents, and **?** studied the impact of breath alcohol detection tests with respect to mortal traffic accidents. On the other hand, **??** studied the relationship between consumption of alcoholic drinks and the main death causes. It is true that a specification like (**??**) allows to mitigate some of the previous problems, but some authors (Freedom, 1999, for instance) have 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).

1.3 Identification issues and the cohort approach

The lack of panel data requires some assumptions to identify the parameters, or in other words, in their absence we cannot control the $\eta's$. If we assume $\eta_i = \eta$ for all i, Ordinary Least Squares (in the case Y were a continuous variable) would provide consistent estimates of the parameters. On the other hand, we only require absence of correlation among the $\eta`s$ 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. This last situation is analyzed by **?** and **?** using information from the BRFSS for periods 1984-1995 and 1987-1999, respectively, with the aim of testing the relation between unemployment and consumption of alcoholic drinks. Our first priority will be then to reply their exercises before presenting results controlling for fixed effects.

Since the BRFSS 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 (**??**) and so, the effect of unemployment on consumption would not be properly identified. We can deal with this problem by constructing pseudo-panels. **?** 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. While it is true that 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 (**?**, establishes 150 observations per cell), we can forget measurement errors because sample means approximate well enough their population counterparts.

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

$$\bar{Y}_{cmt} = \bar{X}_{cmt}\beta + \overline{UR}_{cmt}\gamma + \bar{\alpha}_s + \bar{\delta}_m + \bar{\lambda}_t + \bar{\eta}_c + \bar{\varepsilon}_{cmt} \qquad c = 1, ..., C \tag{3}$$

We define n_{cmt} as the size of cohort c in month m of year t. Every element of $\bar{X}_{cmt}$, 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 month m (or quarter) 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 $\bar{X}_{cmt}$. Therefore, (**??**) 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 $\bar{X}_c = (\sum_{m=1}^{M}\sum_{t=1}^{T} n_{cmt})^{-1} X_{cmt}$ be the average of the observed means for cohort c, and define $\bar{Y}_c$ analogously. Then:

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

$\hat{\beta}_{WG}$ will be biased in small samples but it will be consistent as n_{cmt} 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_{cmt}), which implies a trade-off between bias and variance of the estimator.

1.4 Consumption and habits: The myopic and rational addiction models

Once we have data which allows us to control for unobserved effects, we are also able to include dynamics in the specification of the models. Given the longitudinal dimension of our cohort data, we can extend our specification in several directions. In particular we consider the estimation of habit models. In the myopic version of the model (**??**) we introduce the lagged outcome as a regressor:

$$\bar{Y}_{cmt} = \theta \bar{Y}_{cmt-1} + \bar{X}_{cmt}\beta + \overline{UR}_{cmt}\gamma + \bar{\alpha}_s + \bar{\delta}_m + \bar{\lambda}_t + \bar{\eta}_c + \bar{\varepsilon}_{cmt} \qquad c = 1, ..., C \qquad (4)$$

However, there are nowadays a lot of papers analysing the existence of rationality in the consumption of several goods from the seminal paper of **?**. **?**, **?**, **?**, **?** or **?** constitute some interesting examples. The consumption dynamics assuming quadratic utility (see **?** can be expressed in this context as :

$$\bar{Y}_{cmt} = \theta \bar{Y}_{cmt-1} + \theta\mu \bar{Y}_{cmt+1} + \bar{X}_{cmt}\beta + \overline{UR}_{cmt}\gamma + \bar{\alpha}_s + \bar{\delta}_m + \bar{\lambda}_t + \bar{\eta}_c + \bar{\varepsilon}_{cmt} \qquad c = 1, ..., C \quad (5)$$

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 (**??**) is that the error terms is autocorrelated. In this case, neither lagged or forward values of the endogenous variables 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.

2 Data

The main dataset is the BRFSS for the 1987–2005 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[2]. 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 success in reducing the prevalence of behaviors that affect public health[3].

In the first survey (1984) information for only 15 states is available. However, since 1995 all the states and Columbia district 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,

[2]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)

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

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 1985-2007 is 3,360,075 observations[4].

2.1 Description of the variables

The survey reports several questions on alcohol consumption. First, respondents are asked whether they 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[5]. 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 five 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).
- Chronic consumption: binary indicator which takes one for male (female) having more than 60 (30) drinks during the last month[6].
- 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, **?** used the first two; **?** tried to capture the implications of alcohol abuse and used two measures very similar to the fourth and fifth. Finally, **?** and **?** used all indicators but the third. In our opinion, the third measure (mean consumption for drinkers) could have a lot of potential as an indicator as long as the probability of being a drinker and the consumption of drinks were affected by different determinants (or the same determinants with different effects).

We also use control of the 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 (also from the *Bureau of Economic Analysis*) and, as a price variable, beer state-specific taxes[7].

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

[5]The survey does not distinguish among types of drinks (except for 1987, 1988 and 2003), so it is not possible to introduce any weighting that refers to their different ethylic content.

[6]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 (**?**. 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 (**????**). 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. 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).

[7]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

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[8]. 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 is unavailable for 0.21% of the sample.

Table 1 contains descriptive statistics. For the period 1987–2005, 51% of the sample has 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.

2.2 Alcoholic drinks and unemployment: a first look

In Table 2 we compare our descriptive statistics with those of **?** and **?**. 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. We also present in Figure 1 of Appendix A, 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 procyclical pattern for most of the figures.

Although 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 procyclical profile, the situation completely changes when there are another sources of heterogeneity. We have done 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[9]. According to Figures 2 and 3, it seems that average consumption is procyclical only for men and women from 21 to 30. Although we cannot establish any causal relationship based on these correlations, economic conditions could have some effect on consumption at the intensive margin for some group of the population.

3 Empirical results

3.1 Estimates using individual data

As a first step to cover one of the objectives of this research, we present in Table 3 a comparison of our estimates with those obtained by **?** and Ruhm and Black (2002). For each of the five alcohol consumption indicators we have estimated equation (**??**) by weighted LS, using BRFSS final weights. As Ruhm and Black (2002) we have included in all the regressions controls for month, age and its square, gender, race/ethnicity, level of schooling, marital status and real per capita income. For the period 1987-1999 we have obtained similar results than **?**. Minor discrepancies

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

[9] 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.

can be explained by small differences in the unemployment variables. However, **?** 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 **?** 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.

Surprisingly, the result we get for the period 1985-2007 are very different, specially as regards the unemployment rate, offering support for a counter-cyclical effect of unemployment for the participation and consumption equations.[10] The positive coefficient of the unemployment rate variables is very robust to changes in the specification (for instance, to the inclusion of year dummies, and a quadratic trend). In fact, we find counter-cyclical effects of unemployment during the period 1985-2007 in all the indicators as opposed to the 1987-1999 period where only consumption variables were affected by the business cycle. As a result, we feel that a more profound analysis of the data is needed as we perform in the following sub-sections.

3.2 Results using cohort data

3.2.1 Definition of the groups

Once we have covered our first aim of comparing the results 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 et al., 1994).[11] For each synthetic individual we have consecutive observations by year and quarter, since we pool observations within a given quarter in order to increase the average number of observations within a cell.[12] The resulting sample has 4416 and 8832 observations in the first and second cases, respectively. The average sample size for each cohort in the year of birth-region aggregation is 716 and in the year of birth-region-gender one is 400. Thus, given this sample size, we can neglect the errors in variables problem according to the results in **?**.

3.2.2 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 (**?**). 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 could affect earnings (citethamilton1997, french1995. As instruments for the unemployment rate and real per capita income we

[10] This confirms one of the explanations offered by **?** in the sense that the results could be sensitive to the period of analysis as our results for different periods confirm.

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

[12] The errors in variables problem could be serious whenever the number of observations per cell is smaller than 150, which occurs if we group observations by month. We choose quarter instead of month to built cohorts and we will include quarterly dummies to control seasonality in consumption.

propose their respective values in the same quarter of the previous year. Since consumption exhibits seasonality the correlation between the regressor and the instrument is high, while we do not think it exhibits 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 are not able to reject the null of absence of systematic differences in the coefficients. Then, it seems both variables are exogenous under the identifying assumption of exogeneity of the other variables in the regression.

In Table 4 we present LS estimates for year of birth-region and yob-region-gender cohorts. All regressions include quarterly dummies, state fixed effects, age and its square, gender, race/ethnicity, the level of schooling, marital status and real per capita income. We present two different sets of results with and without cohort effects and two time periods: 1987-1999, that is the one used in **?**, and 1987-2005. These results show some common traits for all beverage consumption indicators. For the 1987-1999 time period, when cohort effects are omitted the unemployment rate appears to be significantly negative, being the case of binge drinking an exception (it was also an exception in the pooled cross-section results). The magnitude of the coefficient is, in all cases, very similar to that found in the pooled cross-section samples (see again Table 3). The magnitude of the coefficient of income is also similar. When cohort effects are introduced in the 1987-1999 sample, the unemployment rate remains significant in the same equations as before.

Real per capita income is as a rule significant (even for the participation decision), 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 negatively correlated with the preference for drinking, except for chronic drinkers. 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.

3.3 Dynamic models: habit and rational addition models

The second advantage of using longitudinal data is the possibility to introduce dynamics in the specification of the model. 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 **?** and **?**, **?**, **?**, **?** or **?** constitute some interesting applications. In Tables 5 we present myopic and rational addition model estimates for year of birth-region and yob-region-gender cohorts. Since least squares provide inconsistent estimates, we only present results based on instrumental variables. All regressions include quar-

terly dummies, state fixed effects, age and its square, gender, race/ethnicity, the level of schooling, marital status and real per capita income. We report results only for the period 1987-2005 for the participation equation and for two consumption equations, the unconditional and conditional number of drinks. Several messages emerge from these results. First, the general rational addiction model always reject the restricted myopic one. The effect of the consumption lead is very important for every specification both in the year of birth-region and yob-region-gender cohorts. 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 participation and in the conditional log number of drinks specifications. 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 drinkers is not sensitive to the business cycle. We provide an interpretation of these results in the following subsection. 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. Income elasticity is negative for participation, i. e. the greater the income the lower 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, but we test this is not the reason for loosing significance. We just run a regression of the unemployment rate on cohort effects in the sample of cohorts by age and we obtain an R^2 of 0.33. On the other hand, we might also think that when including month, year, state and cohort fixed effects the variation of the unemployment rate is not sufficient to properly identify its effects separately from other micro and macroeconomic determinants. In order to check it we have re-estimated all the models excluding individually each of the subsets of monthly, 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 seem to be 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[13]. 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.

4 Interpretation of the results

Our hypotheses about the relationship between alcohol consumption and the business cycle are: i) unobserved factors, the preference for drinking, for instance, can drive decisions about both being drinker or consuming as the business cycle does; ii) the presence of habits in the consumption

[13] All these results are available upon request.

of alcoholic drinks could generate spurious correlations which are not properly detected in cross-sectional studies. In this section we want to offer econometric and economic interpretations of the results obtained. We feel that it is necessary to separate decisions at the level of participation and consumption as results both using pooled cross-sections and pseudo panel confirm since some variables show different effects on the two decisions.

We are going to rewrite the equations for participation (P), per capita consumption of alcoholic drinks (C_c) and mean or *per drinker* consumption (C_M) including as explanatory variables the unemployment rate (UR), other socioeconomic or demographic determinants including income and taxes (X) and an error term for each equation, respectively (u, v, w).

$$\begin{aligned} P &= \alpha_0 + \alpha_1 UR + \alpha_2 X + u \\ \log(C_c) &= \beta_0 + \beta_1 UR + \beta_2 X + v \\ \log(C_M) &= \gamma_0 + \gamma_1 UR + \gamma_2 X + w \end{aligned}$$

Per capita consumption can be expressed as the product of participation rate and average consumption per drinker, where D is the number of drinks consumed, N is the population size and N_D is the number of drinkers (Jones, 1989).

$$C_c = \frac{D}{N} = \frac{N_D}{N} * \frac{D}{N_D} = P * C_M \tag{6}$$

If we use the per capita measure of alcohol consumption we will be underestimating the real value of the consumption of drinkers. Thus pooling positive and zero observations is going to affect parameter estimates and elasticities. The estimates of the per capita consumption equation are going to be larger (in absolute value) than those of the mean consumption equation, whenever the variables affect both decisions. This is true except in the case of changes of regime, when participation and consumption are affected by different variables, or even if they are the same ones but their influence over both decisions is different. So, under certain circumstances, it is necessary to consider the decisions of participation and consumption together. If zero demands were due to abstention and if participation and consumption decisions were affected equally by the same determinants, the problem would not be so serious. Moreover, if we tried to perform valid inferences at population level (i.e., aggregate demand, evaluation of costs derived from the consumption of alcohol), the fact of having positive and zero observations raises a problem since both groups of consumers belong to different demand regimes and have heterogeneous preferences (citetfry1994. In fact, preference heterogeneity among those with positive and zero consumption may be problematic due to changes in the income profile of drinkers. Changes in the percentage of drinkers for period 1987-2005 can cause important estimation problems, in terms of instability of parameters (see graphs in Appendix A). The way we look at both decisions is combining information on participation and consumption. Let's write equation (**??**) in logs as in our specification

$$\log C_c = \log C_M - \log P \tag{7}$$

Since we estimate separately $\log C_c$, $\log P$ and $\log C_M$, the estimates of the per capita consumption equation misspecifies the effect of participation. Once the effect of participation is included (in a naive way) in $\log C_M$ the effects of unemployment vanishes given the magnitude of both parameters in the two equations. So, results obtained pooling observations from the two regimes are capturing inadequately the whole effect.

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 1987–2003 from the BRFSS. Opposite to previous studies (**??** 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 1987-2005 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.

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A Figures

Figure 1. Mean consumption and unemployment by age cohorts. source: BRFSS

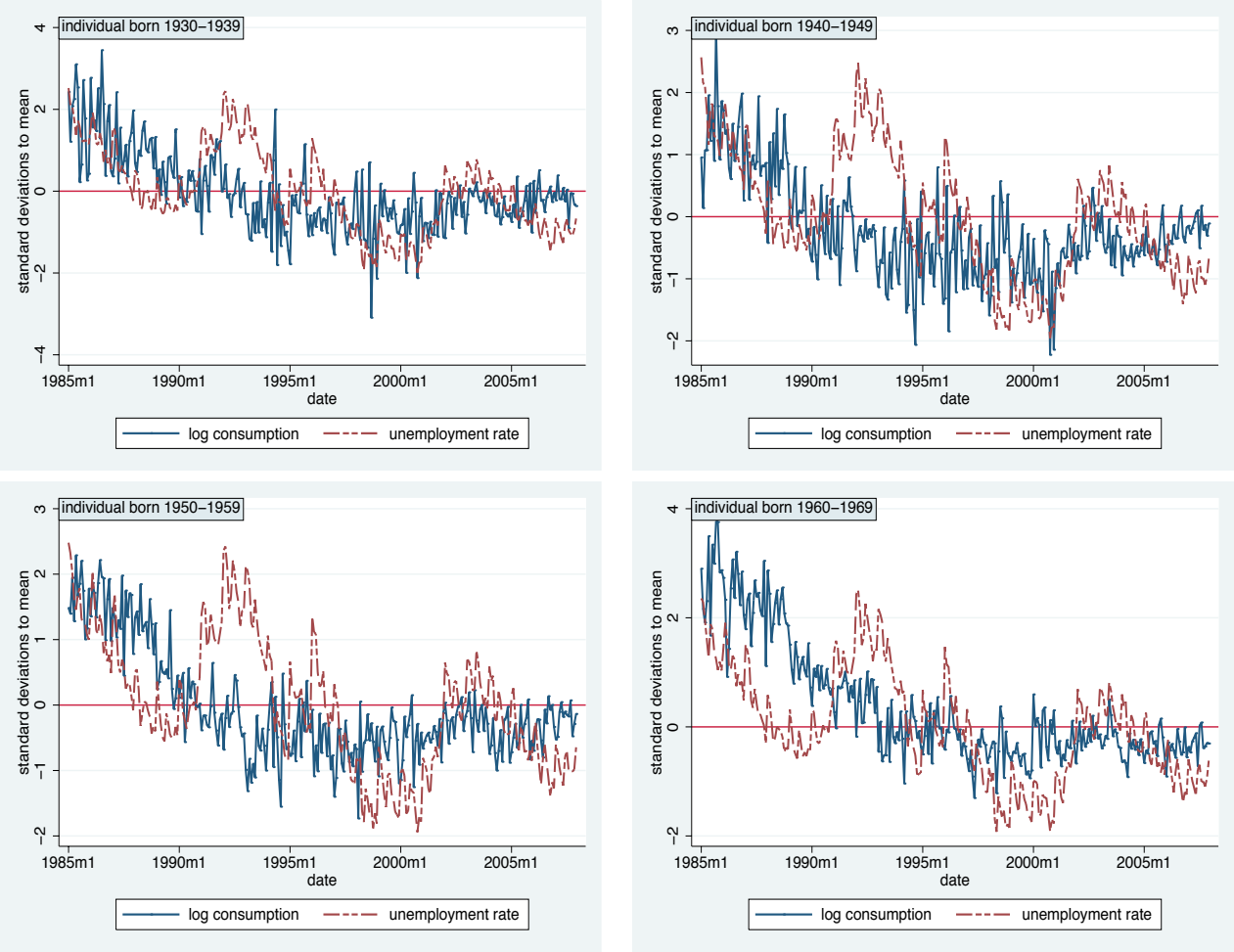


Figure 2. Mean consumption and unemployment by age gender cohorts. Men. source: BRFSS

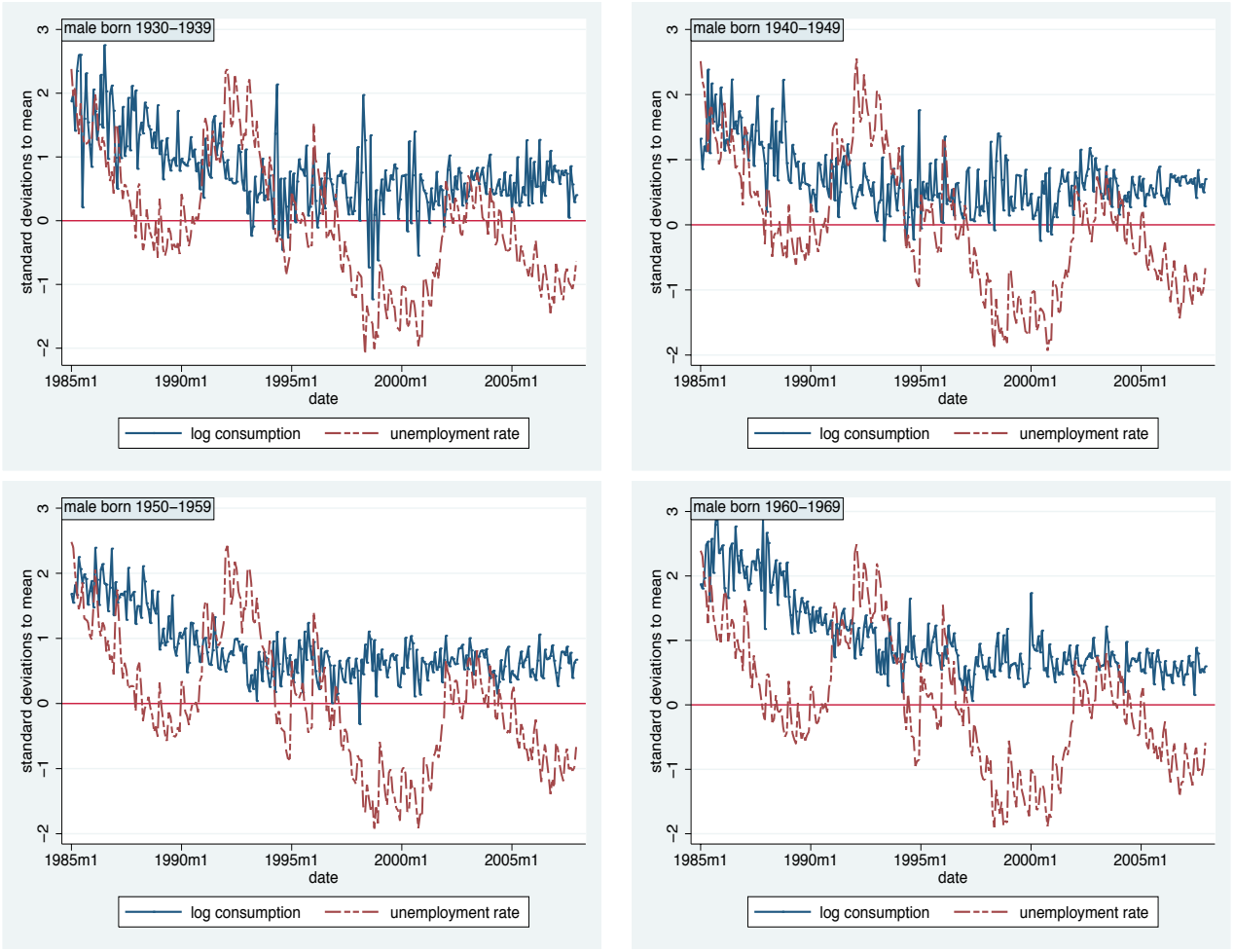
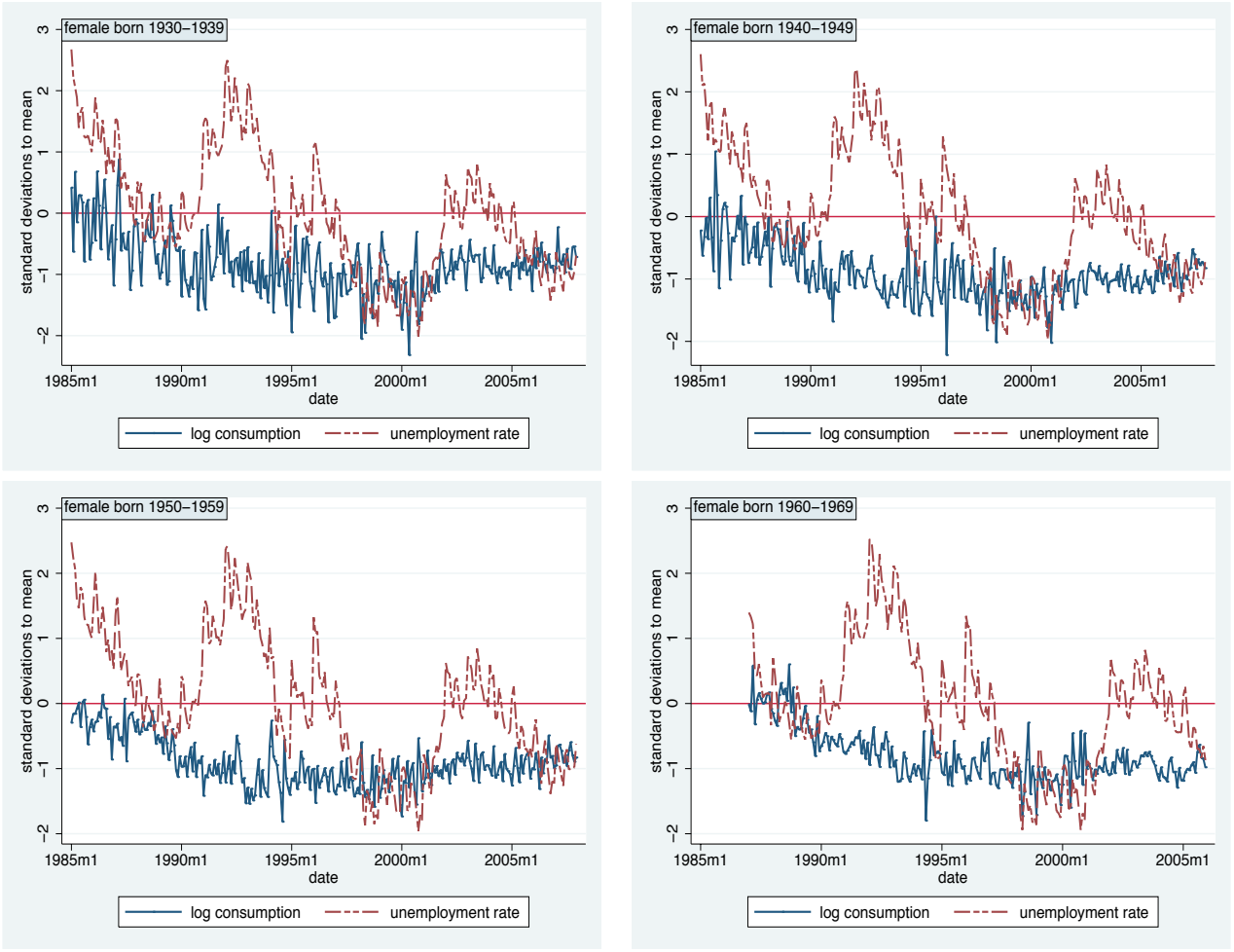


Figure 3. Mean consumption and unemployment by age gender cohorts. Women. source: BRFSS



B Tables

Table 1: Descriptive Statistics 1985-2007[a]

Variable	Definition	**Without Weights**		**With Weights**	
		Mean	**(Std. Dev.)**	**Mean**	**(Std. Dev.)**
Drinker	*1 if he/she has consumed one alcoholic beverage in the last month*	0.52	(0.5)	0.54	(0.499)
Mean consumption	*Average # of drinks per individual by month and state*	10.34	(26.325)	11.22	(27.613)
Consumption	*# number of drinks consumed by drinkers in the last month*	20.27	(34.018)	21.28	(35.103)
Binge drinking	*1 if he/she has consumed 5 or more drinks in the same occasion*	0.13	(0.335)	0.16	(0.363)
Chronic	*1 if she/she has consumed more than 60 drinks in the last month (30 drinks for women)*	0.07	(0.249)	0.06	(0.243)
Unemployment rate	*Monthly State unemployment rate*	5.07	(1.423)	5.55	(1.493)
Per capita real income	*Per capita real income in 1999 $*	27.09	(4.825)	25.25	(4.172)
Beer tax rate	*State tax rate per gallon in 1999 $*	1.70	(0.484)	1.76	(0.428)
Female	*1 if the individual is female*	0.60	(0.491)	0.52	(0.5)
Age	*Age in years*	47.73	(16.23)	43.55	(16.65)
Black	*1 if he/she is black*	0.08	(0.27)	0.08	(0.272)
Other	*1 if she/he belong to another ethnicity*	0.05	(0.218)	0.04	(0.2)
Race not reported	*1 if he/she doesn't report race*	0	(0.058)	0	(0.042)
Hispanic origin	*1 if he/she is hispanic*	0.06	(0.238)	0.09	(0.285)
Hispanic not reported	*1 if he/she doesn't report hispanic condition*	0	(0.052)	0	(0.041)
High School Dropout	*1 if High School not completed*	0.11	(0.318)	0.13	(0.339)
Some College	*1 if he/she has some college education*	0.27	(0.441)	0.27	(0.445)
College	*1 if the individual finished college*	0.30	(0.46)	0.27	(0.442)
Education not reported	*1 if educational level not reported*	0	(0.041)	0	(0.043)
Married	*1 if he/she is married*	0.59	(0.492)	0.64	(0.48)
Separated	*1 if he/she is separated*	0.16	(0.369)	0.11	(0.314)
Widowed	*1 if he/she is widowed*	0.09	(0.289)	0.06	(0.234)
Marital status not reported	*1 if maritial status not reported*	0	(0.048)	0	(0.04)
Observations		**3,084,131**			

[a] Data are from 1985 to 2007 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.