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The complementarity of teen smoking and drinking

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Abstract

Teen drinkers are over twice as likely as abstainers to smoke cigarettes. This empirical study provides evidence of a robust complementarity between these health behaviors by exploiting the ‘‘cross-price’’ effects. The results indicate that the movement away from minimum legal drinking ages of 18 reduced teen smoking participation by 3 to 5%. The corresponding instrumental variable estimates suggest that teen drinking roughly doubles the mean probability of smoking participation. Similarly, higher cigarette taxes and reductions in teen smoking are associated with a lower prevalence of teen drinking. However, the results which rely on cigarette taxes for identification are estimated imprecisely. © 1999 Elsevier Science B.V. All rights reserved.

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1. Introduction

Over the past several decades there has been a broad variety of aggressive public health initiatives aimed at curbing the prevalence of abusive drinking and tobacco use as well as their related consequences. There has been a particular

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interest in reducing alcohol and cigarette use among young adults. [1] This interest has motivated a large number of econometric studies that examined the policy responsiveness of teen smoking and drinking. [2] Most of these econometric evaluations have considered these consumption decisions in isolation. More specifically, relatively little attention has been paid to the substitutability or complementarity of such ‘‘sin’’ goods among teens. Furthermore, the empirical research that has addressed such issues has largely emphasized the substitutability of various drugs and how this might attenuate the desirability of policies that reduce the availability of a particular substance. For example, DiNardo and Lemieux (1996) report that the movement to higher minimum legal drinking ages (MLDA) led teens to substitute marijuana for alcohol. [3]

However, the consumption of alcohol and cigarettes by teens might constitute an important case where the goods are complements instead of substitutes. In particular, to the extent these goods are economic complements, the considerable public health efforts aimed at reducing the prevalence of teen smoking and drinking may generate important and unintended benefits. This empirical study provides evidence on this policy-relevant question by relying on the ‘‘cross-price’’ effects in models of teen alcohol and cigarette use. More specifically, this study evaluates the complementarity between teen smoking and drinking by exploiting the exogenous variation in the full teen prices of alcohol and tobacco generated by changes in cigarette taxes and state minimum legal drinking ages (MLDA). [4] These econometric evaluations are based on pooled cross-sections from the 1977–1992 Monitoring the Future (MTF) surveys of high school seniors. An important and unique feature of these data is that because they contain both time-series and cross-sectional variation, the empirical results presented here do not need to rely exclusively on the conventional cross-sectional identification

[1] There are several reasons for focusing on young adults. Teens may be more susceptible to policy manipulation than adults who have formed an established habit. Furthermore, most smokers begin their habit as teens (U.S. Department of Health and Human Services, 1994). Additionally, abusive teen drinking is linked to the leading cause of mortality among this age group, traffic fatalities (Rosenberg et al., 1996).

[2] Empirical studies of teen alcohol use include Grossman et al. (1987; 1993; 1994), Coate and Grossman (1988), Cook and Moore (1993), Kenkel (1993) and Dee (1999). Studies on teen cigarette use include Lewit and Coate (1982), Lewit et al. (1981), Chaloupka (1991), Chaloupka and Grossman (1996), Chaloupka and Weschler (1997), Wasserman et al. (1991) and Evans and Huang (1998).

[3] Thies and Register (1993), Chaloupka and Laixuthai (1997) and Pacula (1998) also provide evidence on the relationship between alcohol and marijuana use. More recently, Farrelly et al. (1999) evaluate the effects of alcohol, marijuana and tobacco policies on marijuana use among youths and young adults.

[4] The direct, ‘‘own-price’’ effects of these policy instruments on teen smoking and drinking participation are well-documented (see footnote 2). Evidence on the first-stage effects of these instruments is also presented here.

strategies which can confound the effects of state alcohol and tobacco policies with unobserved, state-specific determinants of these teen behaviors. [5]

The MTF data are first used to estimate the magnitudes of the stylized links between teen drinking and smoking. The results demonstrate that there are dramatically large and positive partial correlations between these consumption behaviors. For example, teens who drink are more than twice as likely as abstainers to smoke cigarettes. To the extent such a relationship is robust, it suggests that the initiation of a smoking habit can be understood as a major and largely overlooked welfare consequence of teen drinking. However, these naive empirical models may be highly misleading. The unobserved determinants that increase the likelihood of consuming alcohol are also likely to increase the probability of smoking participation. This study presents less ambiguous evidence on the relationships between teen alcohol and cigarette use by relying on the exogenous variation in these health behaviors generated by their policy determinants (i.e., cigarette taxes and minimum legal drinking ages). These identification strategies are implemented by evaluating reduced-form models which provide direct evidence of ''cross-price'' effects as well as by evaluating instrumental variables (IV) estimates which provide direct evidence on the structural relationship between these behaviors.

The results of these varied estimations provide consistent evidence that there is a strong complementarity between teen smoking and drinking. For example, reduced-form models indicate that the movement away from a minimum legal drinking age of 18 reduced teen smoking participation by 3 to 5%. The related instrumental variables (IV) estimates indicate that teen drinking roughly doubles the mean probability of smoking participation. These marginal effects are statistically significant and robust to a variety of specification changes. Furthermore, the magnitudes of these marginal effects are consistent with the empirical benchmarks established by the stylized partial correlations between teen drinking and smoking. [6] However, empirical models that estimate the effects of cigarette taxes on teen drinking participation provide less reliable evidence of this complementarity. In part, this may be due to important structural changes in teen smoking behavior over this period. Evans and Huang (1998) find that the tax responsiveness of teen smoking participation was dramatically higher during the 1985–1992 period than during the 1977–1992 period. Consistent with these results, teen drinking models based on only the 1985–1992 data uniformly indicate that higher cigarette taxes

[5] There is evidence that such unobserved state-specific attributes are important determinants of these teen health behaviors. For example, Evans and Huang (1998) find that the tax responsiveness of teen smoking participation is somewhat smaller in models that include state fixed effects. Additionally, Dee (1999) finds that the conventional links between beer taxes and teen alcohol use are not robust to the inclusion of state fixed effects.

[6] However, the omission of state fixed effects does lead to some estimates that are suspiciously larger than those based on the preferred specifications that include state fixed effects.

are associated with reductions in teen drinking. However, these marginal effects are statistically imprecise and, in some specifications, implausibly large. [7] In an important sense, the relative sensitivity of the results based on cigarette taxes is not surprising. Though there is considerable evidence that teen smoking participation is significantly tax-responsive, the magnitude of this responsiveness is relatively small and imprecise. In contrast, the nationwide increases in minimum legal drinking ages generated considerable variation in teen alcohol use and, thus, allow the complementarity between teen smoking and drinking to be evaluated more precisely.

2. Monitoring the future (MTF) surveys

The widely used MTF surveys, which are funded by the National Institute on Drug Abuse (NIDA), were designed to identify changes in important youth behaviors and attitudes through consistent questioning of successive youth cohorts. The MTF sample is based on a national three-stage probability design which begins with the selection of geographic areas based on the primary sampling units developed by the Survey Research Center at the University of Michigan for nationwide interviews. In the second stage, high schools within these areas were selected with a probability proportional to the size of their senior classes. In the third stage, several hundred seniors within each sampled high school were selected for interviews. In smaller schools, all seniors were interviewed. Typically, at least 15,000 seniors from roughly 130 schools annually provide information on the self-completed MTF questionnaire. [8] Each yearly survey of these high school seniors has included questions about alcohol and tobacco use. However, the public-use versions of these survey data cannot be matched to the state-level variation in alcohol and tobacco policies. In order to preserve respondent confidentiality, these data do not identify the state in which the selected school is located. To circumvent this difficulty, a special agreement was reached with the producers of the MTF data to provide certain key survey data along with state identifiers. This stylized data extract matches 1977–1992 MTF respondents to their states but, by necessity, contains relatively limited demographic information. More specifically, this data set identifies the proportion of respondents satisfying certain drinking and smoking definitions within a given state, survey year, race, gender

[7] Nonetheless, conservative estimates suggest that a cigarette tax increase as large as those recently rejected by Federal legislators could generate substantial reductions in teen alcohol use. For example, these estimates suggest that a cigarette tax increase of US$1.10 per pack would reduce heavy teen drinking by 13%. However, the imprecision of the reduced-form estimates provides an important caveat to such policy simulations.

[8] The standardized MTF questionnaires are administered in the classroom by trained personnel. The limited non-response is due largely to absenteeism. The biases associated with non-response appear to be quite small (Johnston et al., 1991).

and age cell as well as the number of observations within each cell. Responses within a given state and year were defined by gender, age (i.e., above or below the age of 18) and race/ethnicity (i.e., white non-Hispanics or not). This data set contains 3,941 cells representing the responses of 255,560 students in 44 states. [9]

These data are relatively novel in that they allow for the introduction of unambiguous controls for the potentially confounding and unobserved state-specific determinants of these behaviors. However, at least three potential limitations of these data should be noted. First, a small proportion of MTF respondents were excluded from this extract. Respondents who did not satisfy the limited racial/ethnic definition (white, black or Hispanic) were deleted. Additionally, in order to preserve respondent confidentiality, data cells with fewer than 5 respondents were also deleted. Fortunately, the extent of these exclusions is fairly small. This extract includes over 94% of all MTF respondents. [10] Furthermore, since the descriptive statistics and econometric results based on this extract consistently replicate prior findings based on unrestricted data, the modest aggregation and censoring of these MTF data do not appear to be problematic. A second notable limitation of the pooled MTF data is that, though they contain an unusually large number of observations, they were not explicitly designed to generate representative estimates for each sampled state. This is a frequent problem with the many demand studies that rely on cross-state policy variation and survey data that are typically representative only at higher levels of aggregation. However, this issue merits a special caveat here since the preferred specifications are somewhat unorthodox in that they include state fixed effects and effectively evaluate the within-state variation over time in teen smoking and alcohol use. Fortunately, there is some direct evidence to suggest that this limitation is not confounding the identification strategies employed in this study. Most notably, the results presented here and elsewhere (Dee, 1999; Dee and Evans, 1997) indicate that the influence of minimum legal drinking ages on the drinking reported by MTF respondents appears quite robust to the introduction of state fixed effects as well as to an extensive set of additional controls. [11] Similarly, in models that include state fixed effects, the influence of cigarette taxes on smoking participation among these respondents is quite robust to sharp increases in the set of additional regressors.

A third limitation of these MTF data is that, by necessity, they omit information on detailed socioeconomic priors that are also likely to influence teen smoking and

[9] Not all of the 44 states in this data set are represented in each survey year. Typically, 36 states are represented in each survey year.

[10] The public-use MTF surveys over the 1977–1992 period consisted of 271,012 respondents.

[11] This robustness also supports the conclusion that the sensitivity of the tax elasticity of teen drinking to state fixed effects reflects unobserved cross-state heterogeneity rather than data limitations. Preliminary work by the author with CDC survey data (which are representative within states over time) also supports this conclusion regarding the sensitivity of the conventional links between alcohol use and taxation.

drinking. The results presented here explore the sensitivity of the key results to these omissions by introducing a variety of related state-year controls. More specifically, the MTF data were matched to other state-year covariates that provide robustness checks when included as regressors in the subsequent models. For example, a variety of family characteristics, which are unavailable in this MTF extract, are likely to influence both teen alcohol and tobacco use. Since the within-state variation in alcohol and cigarette policies should be uncorrelated with such variables, their omission should not generate any misleading biases. Nonetheless, constructed state-year measures for parental education and family structure are included in some specifications.[12] The MTF data were also matched to state-year measures of macroeconomic activity (i.e., the unemployment rate and the real state personal income per capita) since the implied employment and income variation might exercise an important, if not confounding, influence on teen alcohol and tobacco consumption. Some of the empirical specifications reported here will also condition on other unobserved determinants of teen smoking and drinking through the use of state and year fixed effects, gender-specific and race-specific year fixed effects as well as state-specific trend variables.

The measures of teen alcohol and cigarette participation contained in this MTF extract are frequently employed in this literature. A ‘‘drinker’’ is a respondent who reports having had an alcoholic drink in the last 30 days. A ‘‘heavy drinker’’ reports having consumed 5 or more drinks in a row in the last 2 weeks.[13] A ‘‘smoker’’ reports any cigarette use within the past 30 days. Nearly 66% of these high school seniors report drinking participation; 36.7% identify themselves as heavy drinkers (Table 1). Just over 30% smoked cigarettes within the past 30 days. These data were matched by state and year to the applicable minimum legal drinking age (MLDA) and excise taxes on cigarettes and beer.[14] The cigarette and beer taxes reflect both state and Federal charges and are measured in real

[12] Five variables were created using pooled cross-sections from the 1977–1992 October Current Population Survey (CPS). More specifically, using households with enrolled children between 14 and 17 years-old, state-year measures for the proportions of households where the highest adult education is a high school dropout, high school graduate and some college and the proportions of households where the head of household or ‘‘reference’’ person is unmarried and widowed, separated or divorced.

[13] This drinking definition is particularly policy-relevant since this interaction of quantity and frequency is understood to be more strongly associated with related consequences.

[14] The data on cigarette excise taxes were taken from the Tobacco Institute’s annual publication, ‘‘The Tax Burden on Tobacco.’’ Data on excise taxes on beer were drawn from the Distilled Spirits Industry Council’s ‘‘History of Beverage Alcohol Tax Changes.’’ Data on minimum legal drinking ages refer to beer consumption and were drawn from the Distilled Spirits Industry Council’s ‘‘Minimum Purchase Age By State and Beverage, 1933–Present.’’ Grandfathered MLDA variation is not recognized separately since it may obscure discrete changes in enforcement. However, replications with alternative formulations or exclusions of states with grandfathering generate results similar to those reported here.

Table 1
Descriptive statistics, 1977–1992 MTF surveys

Variable	Mean (Standard Deviation)
Smoking (30-day cigarette participation)	0.301 (0.105)
Drinking (30-day alcohol participation)	0.657 (0.160)
Heavy drinking (5 or more alcohol drinks in a row in last 2 weeks)	0.367 (0.157)
White non-Hispanic	0.767 (0.423)
Male	0.493 (0.500)
Age 18 or older	0.466 (0.499)
Minimum legal drinking age (MLDA) of 18	0.196 (0.397)
Minimum legal drinking age (MLDA) of 19	0.139 (0.346)
Minimum legal drinking age (MLDA) of 20	0.029 (0.169)
Real cigarette tax (state and Federal excise taxes in 1982–1984 dollars per pack)	0.304 (0.078)
Real beer tax (state and Federal excise taxes in 1982–1984 dollars per gallon)	0.496 (0.199)
Number of observations	255,560

(1982–1984) cents per pack. Minimum legal drinking ages are represented here in two ways. One is a fully unrestrictive representation which consists of separate binary indicators for minimum legal drinking ages of 18, 19 and 20 (with an MLDA of 21 as the reference). However, the subsequent evaluations based on this approach demonstrate that it was the movement away from an MLDA of 18 that substantially influenced the prevalence of alcohol use among the MTF respondents. The preferred specifications include a simple binary indicator for respondents from states and years with an MLDA of 18. In interpreting the heterogeneous effects associated with changes in minimum legal drinking ages, the somewhat limited scope of the MTF survey should be emphasized as a caveat. In particular, it may well be that the movement between minimum legal drinking ages of 19, 20 and 21 did have a significant influence on the prevalence of alcohol use among populations not included in the MTF survey (e.g., high school dropouts as well as older teens).

3. Empirical specifications

This section discusses the empirical specifications employed in this study and presents evidence on the partial correlations between teen drinking and smoking participation based on the MTF data. Then this section discusses how more definitive evidence on the relationship between these consumption behaviors is identified through reduced-form evidence of cross-price effects and through instrumental variables (IV) estimates of how these consumption behaviors influence each other.

3.1. Teen smoking and drinking

The empirical results presented here are based on functional forms that utilize the MTF data largely as they were received. For example, an empirical benchmark for how teen drinking might influence smoking participation is identified, in part, through least-squares estimations of the following basic equation:

$$C_{\mathrm{ist}} = D_{\mathrm{ist}}\,\delta + W_{\mathrm{ist}}\,\Pi + T_{\mathrm{st}}\,\alpha + u_{\mathrm{s}} + \upsilon_{\mathrm{t}} + \epsilon_{\mathrm{ist}} \tag{1}$$

where C_{ist} and D_{ist} represent respectively the proportion of respondents in cell i in state s during year t that satisfy the given teen smoking and drinking definitions. The term, W_{ist}, represents the other cell-specific determinants of these behaviors. The terms, u_{s} and υ_{t}, represent state and year fixed effects and $\varepsilon_{\mathrm{ist}}$ represents a mean-zero random error. The variable, T_{st}, represents the level of real cigarette taxes in state s during year t. Benchmark estimates for how the teen drinking measures might influence smoking participation are derived from a similarly specified model where D_{ist} is the dependent variable and C_{ist} is a key regressor. [15] Efficient estimation of Eq. (1) is achieved through a weighted least squares (WLS) procedure where the weights are the number of respondents per cell. Other estimation procedures are also feasible with these data. For example, the grouped data could be expanded to individual-level observations and used to generate least-squares estimates of a linear probability model or maximum-likelihood estimates of probit or logistic models. However, the key results based on those procedures are very similar in magnitude and precision to those reported here. [16]

The results of estimating variants of Eq. (1) are reported in Table 2. These estimates uniformly indicate that there are large, positive links between teen smoking and drinking participation. For example, the results in the top panel of Table 2 are for models of smoking participation. These estimates indicate that teens who engage in drinking or heavy drinking are 29 to 38 percentage points more likely to smoke cigarettes. Given a mean level of smoking participation of 30.1%, such large marginal effects constitute increases of roughly 100%. These marginal effects are also statistically precise; the estimated coefficients are at least 20 times larger than their standard errors. Furthermore, the marginal effects are fairly robust across the different empirical specifications. Model (1) includes as additional regressors only the binary indicators for the demographic variables and year fixed effects. Model (2) introduces state fixed effects. Models (3) through (7)

[15] However, in those specifications, a binary indicator for an MLDA of 18 replaces the cigarette tax variable.

[16] These grouped data could also be used for WLS estimates of a log-odds model (Berkson, 1953; Maddala, 1983; Cox and Snell, 1989). However, because many cells contain no drinkers or smokers, ad-hoc corrections to the logistic transformation would be required.

Table 2
WLS estimates, teen cigarette participation and alcohol use, 1977–1992 MTF surveys

Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable: smoking							
Drinking	0.359	0.350	0.346	0.344	0.344	0.348	0.338
	(0.012)	(0.012)	(0.014)	(0.014)	(0.013)	(0.013)	(0.014)
R^2	0.371	0.501	0.507	0.511	0.527	0.537	0.559
Heavy drinking	0.376	0.309	0.292	0.291	0.297	0.309	0.299
	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
R^2	0.345	0.463	0.476	0.481	0.499	0.511	0.537
Dependent variable: drinking							
Cigarette use	0.499	0.423	0.393	0.392	0.399	0.408	0.406
	(0.017)	(0.017)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)
R^2	0.613	0.705	0.742	0.743	0.746	0.748	0.763
Dependent variable: heavy drinking							
Cigarette use	0.399	0.300	0.290	0.291	0.304	0.319	0.319
	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
R^2	0.682	0.736	0.752	0.753	0.755	0.760	0.771
State fixed effects	no	yes	yes	yes	yes	yes	yes
Socioeconomic covariates	no	no	yes	yes	yes	yes	yes
Macroeconomic covariates	no	no	no	yes	yes	yes	yes
Race/ethnicity-specific year fixed effects	no	no	no	no	yes	yes	yes
Gender-specific year fixed effects	no	no	no	no	no	yes	yes
State-specific time trends	no	no	no	no	no	no	yes

These estimations are based on the responses of 255,560 high school seniors grouped into 3,941 observations by state, year, age, race and gender and are weighted by the number of students grouped into each observation. Standard errors are reported in parentheses. All models include binary indicators for race/ethnicity, gender and age and year fixed effects. The models for cigarette use include real cigarette taxes as a regressor. The models for teen drinking and heavy drinking participation include as a regressor a binary indicator for an MLDA of 18.

successively add to this specification the socioeconomic and macroeconomic covariates as well as race-specific and gender-specific year fixed effects and state-specific time trends. The models of drinking and heavy drinking participation, which are reported in the middle and lower panels of Table 2 generate similarly large, precise and stable results. For example, teens who smoke are roughly 39 to 50 percentage points more likely to be drinkers, an increase of at least 59% in the mean prevalence of teen drinking. Similarly, teen smokers are approximately 29 to 40 percentage points more likely to be heavy drinkers, an increase of at least 79% in the mean prevalence of heavy teen drinking.

3.2. Reduced-form and IV estimates

The dramatic magnitudes of the relationships between teen smoking and drinking participation reported in the previous section suggest that the joint dependence of these consumption decisions may have dramatic policy relevance and are worthy of more careful scrutiny. The fundamental difficulty with attributing causality to these intriguing partial correlations is that they could plausibly reflect to an unknown degree the confounding influence of the unobserved factors that may jointly influence both alcohol and tobacco use (e.g., an idiosyncratic rate of time preference). An identification strategy for addressing such concerns would rely on events that generated plausibly exogenous variation in teen smoking and alcohol use. The preferred specifications reported here inform this important specification issue by relying on the within-state variation in cigarette taxes and the movement away from a minimum legal drinking age of 18. This approach to identification is employed to generate both reduced-form and instrumental variables (IV) estimates which provide related evidence of the possible complementarity between teen smoking and drinking.

The reduced-form models for the measures of teen alcohol use take the following form:

$$D_{\mathrm{ist}} = W_{\mathrm{ist}}\pi_1 + T_{\mathrm{st}}\alpha_1 + M_{\mathrm{st}}\gamma_1 + u_{1\mathrm{s}} + \upsilon_{1\mathrm{t}} + \epsilon_{1\mathrm{ist}} \tag{2}$$

where the regressors and error components are defined similarly to those in Eq. (1). As noted earlier, some of the reported estimates for Eq. (2) also evaluate alternative approaches to identification by including as regressors the real state and Federal excise tax on beer and a more unrestrictive representation of the MLDA variation. The reduced-form models for teen smoking participation are based on specifications similar to that in Eq. (2):

$$C_{\mathrm{ist}} = W_{\mathrm{ist}}\pi_2 + T_{\mathrm{st}}\alpha_2 + M_{\mathrm{st}}\gamma_2 + u_{2\mathrm{s}} + \upsilon_{2\mathrm{t}} + \epsilon_{2\mathrm{ist}} \tag{3}$$

The estimated ‘‘cross-price’’ effects in these reduced-form models (Eqs. (2) and (3)) can provide direct evidence on the possible complementarity between teen smoking and drinking. For example, if teen smoking and drinking were complements, we would expect an MLDA of 18 (which implied a sharp increase in teen drinking) to be associated with a higher prevalence of teen smoking (i.e., $\gamma_2 > 0$). Similarly, we would also expect higher cigarette taxes to be associated with reductions in teen alcohol use (i.e., $\alpha_1 < 0$).

However, it would also be informative to construct unbiased structural estimates of how these teen consumption behaviors influence each other. Some of the estimations reported here achieve this by employing the ‘‘cross-price’’ variables as instrumental variables. For example, teen exposure to an MLDA of 18 is employed as an instrument for identifying how teen alcohol use influences the prevalence of teen smoking participation. Similarly, cigarette taxes are employed as an instrument to identify how teen smoking participation influences teen drinking. These IV estimates can be understood as 2SLS estimators where the reduced-form models in Eqs. (2) and (3) constitute the first-stage models. As with the reduced-form models, the robustness of the IV estimates is evaluated partly by comparing the results across several specifications that introduce additional regressors. However, the stylized links presented in Table 2 also provide a useful opportunity to evaluate the plausibility of the IV results. More specifically, it is fairly reasonable to suspect that the partial correlations identified in Table 2 may overstate the true complementarity of teen smoking and drinking. This is because the shared but unobserved determinants of these behaviors (e.g., an idiosyncratic rate of time preference) are likely to have a positive covariance. This would imply that the estimates in Table 2 constitute upper bounds on the value of the corresponding IV estimates. Any IV estimates that are substantially larger than these stylized benchmarks may suggest the existence of important specification error. [17] However, a caveat should be emphasized. The power of these benchmarks values as evidence of specification error should not be overstated since they are predicated on the reasonable but possibly unjustified assumption that the unobserved determinants of teen smoking and drinking exhibit a non-negative covariance.

4. Reduced-form models: teen alcohol use

This section presents a variety of estimates based on Eq. (2): the reduced-form model for the measures of teen alcohol use. The results of estimating these models for teen drinking participation within the last 30 days are reported in Table 3. Models (1) and (2) report the results of specifications which omit state fixed effects. These models replicate conventional evaluations in this literature since they effectively rely on the cross-state variation in these tobacco and alcohol policies. As in the prior literature, the results of this identification strategy suggest that both lower beer taxes and lower MLDA imply significant increases in teen drinking participation. The coefficients on the cigarette tax variable also suggest

[17] This approach also implies bounds for the corresponding reduced-form estimates from Eqs. (2) and (3). Since the IV estimates are just identified, they equal the ratio of the reduced-form and first-stage estimates (i.e., an indirect least squares interpretation). Therefore, the related reduced-form bound simply equals the product of the first-stage estimate and the upper bound for the corresponding IV estimate. Dee and Evans (1997) discuss how such benchmarks can provide an important commentary on conventional reduced-form evidence linking reductions in teen alcohol availability with increases in educational attainment.

that teen drinking substitutes for smoking - higher cigarette taxes imply significant increases in teen drinking. However, these results also suggest the possible existence of important specification errors. For example, the coefficients on the cigarette tax variable are strikingly large given that they represent ‘‘cross-price’’ rather than ‘‘own-price’’ effects. Furthermore, the MLDA results from Model (1) do not exhibit a plausible monotonicity: an MLDA of 20 was associated with substantially more teen drinking than an MLDA of 19.

Models (3) and (4) introduce state fixed effects into the conventional cross-sectional specifications. The results of these models indicate that exposure to an MLDA of 18 increased drinking participation among MTF respondents by 3.7% — a statistically significant increase of over 12% in the mean prevalence of drinking. However, these models also indicate that minimum legal drinking ages

Table 3
WLS estimates: the reduced-form determinants of teen drinking participation, 1977–1992 MTF surveys

Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MLDA of 18	0.0677	0.0365	0.0362	0.0366	0.0383	0.0382	0.0254
	(0.0054)	(0.0048)	(0.0065)	(0.0054)	(0.0066)	(0.0055)	(0.0068)
MLDA of 19	0.0207	–	−0.0010	–	−0.0001	–	−0.0077
	(0.0053)		(0.0059)		(0.0059)		(0.0060)
MLDA of 20	0.0567	–	0.0062	–	0.0071	–	−0.0103
	(0.0098)		(0.0112)		(0.0113)		(0.0115)
Real cigarette tax	0.2156	0.3246	−0.0807	−0.0764	−0.0500	−0.0436	−0.0610
	(0.0235)	(0.0229)	(0.0417)	(0.0406)	(0.0434)	(0.0427)	(0.0440)
Real beer tax	−0.1567	–	0.0114	–	0.0219	–	0.0826
	(0.0106)		(0.0309)		(0.0320)		(0.0329)
R^2	0.630	0.608	0.717	0.717	0.718	0.718	0.722
State fixed effects	no	no	yes	yes	yes	yes	yes
Socioeconomic covariates	no	no	no	no	yes	yes	yes
Macroeconomic covariates	no	no	no	no	no	no	yes
Race/ethnicity-specific year fixed effects	no	no	no	no	no	no	no
Gender-specific year fixed effects	no	no	no	no	no	no	no
State-specific time trends	no	no	no	no	no	no	no

These estimations are based on the responses of 255,560 high school seniors grouped into 3,941 observations by state, year, age, race and gender and are weighted by the number of students grouped into each observation. Standard errors are reported in parentheses. All models include binary indicators for race/ethnicity, gender and age and year fixed effects.

of 19 and 20 had very small marginal effects that were statistically indistinguishable from the reference of an MLDA of 21. The estimated coefficients on the cigarette tax variable are negative and statistically precise in these models suggesting that teen smoking and drinking are complements. Notably, the coefficients on the beer tax variable have an implausible positive sign and are relatively small and statistically insignificant after the introduction of state fixed effects. Models (5) through (14) further evaluate the robustness of these results through dramatic increases in the set of other regressors. Models (5) and (6) introduce five constructed socioeconomic covariates measuring family structure and parental education. Models (6) and (7) add the two macroeconomic covariates, the state unemployment rate and real per capita personal income. Models (8) through (12) introduce year fixed effects that are race and gender-specific. Finally, Models (13)

(8)	(9)	(10)	(11)	(12)	(13)	(14)
0.0329	0.0257	0.0336	0.0259	0.0337	0.0417	0.0371
(0.0056)	(0.0068)	(0.0056)	(0.0068)	(0.0055)	(0.0098)	(0.0073)
–	−0.0083	–	−0.0081	–	0.0063	–
	(0.0060)		(0.0060)		(0.0074)	
–	−0.0118	–	−0.0121	–	0.0016	–
	(0.0114)		(0.0114)		(0.0125)	
−0.0441	−0.0756	−0.0592	−0.0750	−0.0584	−0.0115	−0.0302
(0.0432)	(0.0440)	(0.0432)	(0.0440)	(0.0432)	(0.0551)	(0.0548)
–	0.0811	–	0.0818	–	0.2307	–
	(0.0328)		(0.0327)		(0.0764)	
0.721	0.745	0.725	0.727	0.726	0.740	0.739
yes	yes	yes	yes	yes	yes	yes
yes	yes	yes	yes	yes	yes	yes
yes	yes	yes	yes	yes	yes	yes
no	yes	yes	yes	yes	yes	yes
no	no	no	yes	yes	yes	yes
no	no	no	no	no	yes	yes

and (14) purge other unobserved determinants that are varying within states over time through the use of state-specific trend variables. The results of these richer models uniformly indicate that exposure to an MLDA of 18 implied an increase in drinking participation. These estimates are both statistically precise and remarkably stable. These models also provide consistent evidence that exposure to an MLDA of 19 or 20 had relatively small and statistically insignificant effects on drinking participation among MTF respondents. Also, the coefficients on the cigarette tax variable in these specifications remains uniformly negative, suggesting complementarity between teen smoking and drinking. However, in the richer specifications, these marginal effects are smaller and statistically insignificant. The coefficients on the beer tax variable remain implausibly positive. These richer models provide no evidence that increases in beer taxes led to reductions in teen drinking participation. [18]

Table 4 presents the results of similar evaluations for heavy drinking participation among the MTF respondents. The pattern of these evaluation results are quite similar to those presented in Table 3 for drinking participation. For example, in conventional specifications that omit state fixed effects, we observe both MLDA and beer tax effects that are statistically significant. Furthermore, as in Table 3, these specifications also suggest the existence of large, positive and significant cigarette tax effects as well as an implausible monotonicity among the MLDA variables. However, the results of these heavy drinking models also exhibit a similar sensitivity to the introduction of state fixed effects. For example, the coefficients on the beer tax variable become uniformly and implausibly positive in the subsequent models. The cigarette tax coefficients in these models remain uniformly positive but become small and statistically insignificant after the introduction of state fixed effects. Similar to the results in Table 3, the imprecise or implausibly signed coefficients on the variables for an MLDA of 19 or 20 indicate that, for these MTF respondents, it was largely the movement away from an MLDA of 18 that influenced heavy drinking participation. [19] However, the estimated increases in heavy drinking associated with exposure to an MLDA of 18 are large, statistically precise and surprisingly stable. In the preferred specifications these estimates range from 2.7 to 3.4 percentage points — an increase of at least 7.4% in the mean prevalence of heavy drinking among teens.

The results in Tables 3 and 4 provide important support for one of the key identification strategies employed in this study. Exposure to an MLDA of 18

[18] However, the beer tax variable becomes significantly positive in these models. This may reflect the somewhat limited sample variation in beer taxes. These implausibly significant results do not appear in models restricted to respondents from states with beer tax changes (Dee, 1999). The general sensitivity of the conventional beer tax results is discussed in more detail below.

[19] In some models (7, 9 and 11), an MLDA of 19 is implausibly associated with significant reductions in heavy drinking.

implied sharp increases in the prevalence of both drinking and heavy drinking among teens. [20] But the estimated reduced-form impact of cigarette taxes in these drinking models did not provide strong evidence on the possible complementarity of teen smoking and drinking. However, these weak results are not entirely surprising. Evans and Huang (1998) report that the first-stage links between cigarette taxes and teen smoking participation are statistically weak in models that pool data from this entire 16-year period while they are quite robust in models that rely only on the 1985–1992 cohorts. [21] For this reason, a later section of this study revisits the issue of whether cigarette taxes and smoking participation influence the prevalence of teen alcohol use by relying only on the MTF data from the 1985–1992 cohorts. However, that sample restriction was not applied here or in some of the subsequent teen smoking models since a substantial proportion of the variation in minimum legal drinking ages occurred before 1985.

The results of these reduced-form models also led to the rejection of the variation in beer taxes as a plausible identification strategy. This may seem surprising to some readers given the widely reported results linking increased beer taxes with reductions in teen alcohol use. However, the direct evidence for these links has been exclusively based on cross-sectional (typically cross-state) identification strategies which could plausibly confound the state-specific determinants of beer taxes (e.g., cultural attitudes towards drinking) with the true effects of those taxes on teen usage. The results presented here demonstrate that the conventional beer tax elasticities are indeed sensitive to the introduction of state fixed effects which are jointly significant regressors that provide unambiguous controls for the unobserved state-specific determinants of these policies and related behaviors. Dee (1999) evaluates the robustness and implications of this sensitivity in more detail. An important caveat to the demonstrated sensitivity of the conventional beer tax results is that the introduction of state fixed effects removes much of the sample variation in beer taxes. Therefore, a cautious interpretation of this sensitivity is not that teen drinking is entirely unresponsive to tax incentives but rather that we have not observed sufficient within-state variation in these taxes to establish tightly bounded estimates. Nonetheless, it should also be noted that the limited within-state tax variation available to us still generates sufficient power to reject the tax elasticities based on the possibly confounded cross-sectional identifications. [22]

[20] Dee and Evans (1997) discuss further anecdotal and empirical evidence that supports the conventional view that the within-state variation in the movement away from an MLDA of 18 was independently given.

[21] They suggest this may be due to important structural changes in teen smoking behaviors.

[22] For example, the 95% confidence interval for the tax responsiveness of heavy teen drinking (Model (3), Table 4) includes negative values but easily excludes the large estimates implied by models that rely on the cross-state variation (Model (1)).

Table 4
WLS estimates: the reduced-form determinants of teen heavy drinking participation, 1977–1992 MTF surveys

Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MLDA of 18	0.0450	0.0269	0.0252	0.0305	0.0267	0.0314	0.0177
	(0.0049)	(0.0044)	(0.0061)	(0.0051)	(0.0062)	(0.0052)	(0.0064)
MLDA of 19	0.0028	–	−0.0088	–	−0.0080	–	−0.0124
	(0.0049)		(0.0055)		(0.0056)		(0.0057)
MLDA of 20	0.0507	–	0.0030	–	0.0041	–	−0.0065
	(0.0090)		(0.0106)		(0.0106)		(0.0108)
Real cigarette tax	0.1764	0.2487	0.0429	0.0508	0.0552	0.0638	0.0373
	(0.0215)	(0.0208)	(0.0392)	(0.0383)	(0.0409)	(0.0402)	(0.0416)
Real beer tax	−0.1014	–	0.0306	–	0.0348	–	0.0777
	(0.0098)		(0.0291)		(0.0301)		(0.0310)
R^2	0.676	0.665	0.739	0.739	0.740	0.740	0.742
State fixed effects	no	no	yes	yes	yes	yes	yes
Socioeconomic covariates	no	no	no	no	yes	yes	yes
Macroeconomic covariates	no	no	no	no	no	no	yes
Race/ethnicity-specific year fixed effects	no	no	no	no	no	no	no
Gender-specific year fixed effects	no	no	no	no	no	no	no
State-specific time trends	no	no	no	no	no	no	no

These estimations are based on the responses of 255,560 high school seniors grouped into 3,941 observations by state, year, age, race and gender and are weighted by the number of students grouped into each observation. Standard errors are reported in parentheses. All models include binary indicators for race/ethnicity, gender and age and year fixed effects.

Furthermore, the reduced-form links between teen traffic fatalities and beer taxes also appear sensitive to important robustness checks (Dee, 1999).

5. The determinants of teen smoking participation

The nationwide move to higher minimum legal drinking ages has had well-documented effects on the prevalence of teen drinking and on related consequences like traffic fatalities. The results in the previous section demonstrate that, for the high school seniors represented in the MTF surveys, it was largely the movement away from an MLDA of 18 that influenced alcohol use. This implies

(8)	(9)	(10)	(11)	(12)	(13)	(14)
0.0273	0.0187	0.0288	0.0191	0.0291	0.0356	0.0343
(0.0053)	(0.0064)	(0.0052)	(0.0063)	(0.0052)	(0.0092)	(0.0069)
–	−0.0128	–	−0.0125	–	0.0026	–
	(0.0057)		(0.0056)		(0.0070)	
–	−0.0099	–	−0.0107	–	−0.0021	–
	(0.0108)		(0.0107)		(0.0117)	
0.0531	0.0266	0.0425	0.0295	0.0455	−0.0238	−0.0418
(0.0408)	(0.0415)	(0.0408)	(0.0412)	(0.0405)	(0.0517)	(0.0514)
–	0.0798	–	0.0806	–	0.2232	–
	(0.0309)		(0.0307)		(0.0716)	
0.741	0.746	0.745	0.751	0.750	0.762	0.762
yes	yes	yes	yes	yes	yes	yes
yes	yes	yes	yes	yes	yes	yes
yes	yes	yes	yes	yes	yes	yes
no	yes	yes	yes	yes	yes	yes
no	no	no	yes	yes	yes	yes
no	no	no	no	no	yes	yes

that if teen smoking were a complement of teen drinking, exposure to an MLDA of 18 should be associated with increased smoking participation among the MTF respondents. Similarly, these results imply that exposure to an MLDA of 18 can be employed as an instrument for how teen drinking influences teen smoking participation. This section presents both reduced-form and IV estimates of the determinants of teen smoking participation based on this identification strategy.

5.1. Reduced-form estimates

If the naive estimates presented in the top panel of Table 2 represented unbiased evidence of how alcohol use actually influences teen smoking participa-

Table 5
WLS estimates, the determinants of smoking participation, 1977–1992 MTF surveys

Independent variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reduced-form estimates							
MLDA of 18	0.0215	0.0119	0.0117	0.0101	0.0095	0.0092	0.0166
	(0.0043)	(0.0050)	(0.0051)	(0.0052)	(0.0050)	(0.0050)	(0.0066)
Real cigarette tax	0.0543	0.0445	0.0536	0.0507	0.0592	0.0572	−0.0644
	(0.0204)	(0.0375)	(0.0393)	(0.0400)	(0.0392)	(0.0386)	(0.0491)
R^2	0.284	0.445	0.448	0.448	0.479	0.496	0.516
IV Estimates							
Drinking	0.5905	0.3243	0.3071	0.3084	0.2836	0.2718	0.4479
	(0.1100)	(0.1256)	(0.1228)	(0.1449)	(0.1655)	(0.1353)	(0.1630)
R^2	0.314	0.485	0.487	0.488	0.519	0.538	0.558
Heavy drinking	0.8009	0.3893	0.3737	0.3713	0.3299	0.3146	0.4850
	(0.1607)	(0.1555)	(0.1539)	(0.1796)	(0.1645)	(0.1587)	(0.1820)
R^2	0.283	0.369	0.473	0.473	0.508	0.531	0.543
State fixed effects	no	yes	yes	yes	yes	yes	yes
Socioeconomic covariates	no	no	yes	yes	yes	yes	yes
Macroeconomic covariates	no	no	no	yes	yes	yes	yes
Race/ethnicity-specific year fixed effects	no	no	no	no	yes	yes	yes
Gender-specific year fixed effects	no	no	no	no	no	yes	yes
State-specific time trends	no	no	no	no	no	no	yes

These estimations are based on the responses of 255,560 high school seniors grouped into 3,941 observations by state, year, age, race and gender and are weighted by the number of students grouped into each observation. Standard errors are reported in parentheses. All models include binary indicators for race/ethnicity, gender and age and year fixed effects.

tion, we would expect the reduced-form effect of an MLDA of 18 on heavy drinking participation to be roughly 1.2 percentage points. [23] This section presents direct evidence on this relationship by using the MTF data to generate WLS estimates of the reduced-form equation for teen smoking participation (Eq. (3)). The key results of those estimations are presented in the top panel of Table 5.

The evidence from these empirical models indicates that teen drinking and smoking are highly complementary. The results of Model (1), which includes year and demographic fixed effects but excludes state fixed effects, indicate that an MLDA of 18 increased teen smoking participation by over 2 percentage points. This estimate is somewhat larger (by more than one standard error) than the associated upper bound implied by the results in Tables 2 and 3 and may provide additional evidence on the necessity of including unambiguous state controls. The subsequent models, which introduce state fixed effects as well as a rich set of other regressors, generate plausibly smaller estimates. For example, the results of Model (2), which includes binary indicators for the available demographic information and state and year fixed effects, indicate that exposure to an MLDA of 18 increased teen smoking participation by 1.19 percentage points. This statistically significant marginal effect constitutes an increase of nearly 4% in the mean probability of smoking (0.0119/0.301). Furthermore, the estimated impact of an MLDA of 18 on smoking participation is quite robust across alternative specifications that introduce additional regressors (Models (3) through (7)). The estimated marginal effect is particularly large in Model (7), which includes state-specific trend variables. The point estimate from Model (7) indicates that exposure to an MLDA of 18 increased smoking participation by 1.66 percentage points (over 5%). [24] However, despite the magnitudes of such estimates, they are uniformly consistent with the upper bounds established by the corresponding first-stage estimates and the stylized correlations between teen drinking and smoking participation. [25] In sum, the evidence from the top panel of Table 5 provides novel evidence that teen drinking and teen alcohol availability are important determinants of teen cigarette use. The estimates in Table 5 also replicate the results reported by Evans and Huang (1998): in models that pool data from this entire 16-year period, cigarette taxes appear to have implausibly signed or statistically imprecise effects on teen smoking participation.

[23] The results presented in Tables 2 and 3 indicated that drinkers are roughly 35 percentage points more likely to smoke and that exposure to an MLDA of 18 increased the probability of drinking by 3.5 percentage points. Taken at face value, these results would imply that the reduced-form effect of an MLDA of 18 on teen smoking participation would be roughly 1.2 percentage points (0.35×0.035).

[24] The state-specific trend variables are jointly significant determinants of teen smoking participation. The p-value on the appropriate F-test is 0.0001.

[25] Some of these estimates are moderately larger than those suggested by the empirical benchmarks. However, these differences are statistically quite small (i.e., just a fraction of the relevant standard errors).

5.2. IV estimates

The corresponding instrumental variables (IV) estimates rely on the plausibly exogenous variation in teen alcohol use generated by exposure to an MLDA of 18 to identify the causal impact of drinking on teen smoking participation. The results of these estimations are reported in the bottom panel of Table 5. It should be noted that these IV estimates are, within the limitations of some rounding error, implied indirectly by the ratio of the reduced-form MLDA effects identified in the top panel of Table 5 and the first-stage effects identified in Tables 3 and 4. Model (1), which omits state fixed effects, generated a surprisingly large reduced-form estimate of how exposure to an MLDA of 18 influenced teen smoking participation. In this context, those estimates imply similarly large estimates of how alcohol use influences teen smoking participation. More specifically, these estimates suggest that drinking increases smoking participation by 59 percentage points and that heavy drinking increases smoking participation by 80 percentage points. These marginal effects are substantially larger than the naive estimates reported in Table 2 and, like earlier results, raise some concerns about the appropriateness of omitting state fixed effects.

The remaining results are based on models that include state fixed effects. These IV estimates uniformly suggest that alcohol use has large, independent effects on the prevalence of teen smoking participation. For example, these estimates indicate that drinking participation increases teen smoking participation by 27 to 45 percentage points — an increase of at least 90% in the mean probability of smoking. These estimates also indicate that heavy drinking participation increases teen smoking participation by 31 to 49 percentage points — an increase of at least 100% in the mean probability of smoking. These statistically significant estimates are quite robust to the introduction of additional regressors. Furthermore, the magnitudes of the large marginal effects are generally consistent with those based on the naive models reported in Table 2 (i.e., typically well within one standard error). In sum, like the related reduced-form results presented above, these novel results indicate that teen alcohol use has a surprisingly large and independent impact on the decision to smoke.

6. 1985–1992 MTF surveys

The prior results relied largely on the variation in minimum legal drinking ages in identifying the striking complementarity between teen smoking participation and alcohol use. An alternative and particularly policy-relevant approach to identifying this complementarity would be to rely on cigarette taxes — a policy instrument that appears to have generated plausibly exogenous variation in teen smoking. But the first-stage relationship between cigarette taxes and teen smoking participation is weak in models that pool MTF data from the entire 1977–1992 period. However, Evans and Huang (1998) also find that the tax responsiveness of teen smoking participation appears to have grown over this period, possibly reflecting structural changes in teen behaviors. In particular, they report robust and statistically significant effects of cigarette taxes on teen smoking participation using MTF data from the 1985–1992 period. The estimation results presented in this section exploit this relationship to provide additional evidence on the complementarity between teen smoking and drinking. [26] More specifically, this section presents reduced-form estimates of how cigarette taxes influence the smoking and drinking measures as well as the implied IV estimates of how teen smoking participation influences the prevalence of drinking and heavy drinking participation. Since the MLDA variation was very limited over this more recent period, it is omitted from these models.

The key estimation results of this approach to identification are presented in Table 6. The specification of these seven models corresponds exactly to the definitions in Tables 2 and 5 except for the omission of the limited MLDA variation. For example, Model (1) includes year and demographic fixed effects but omits state fixed effects. The reduced-form results of Model (1) suggest that cigarette taxes had a small and statistically insignificant effect on teen smoking participation but implausibly large, positive and statistically significant effects on each of the drinking measures. The implied IV estimates are also implausibly oversized. As noted earlier, such results provide a direct but incomplete commentary on the appropriateness of omitting state fixed effects. The subsequent models (2 through 7) incrementally introduce into these specifications state fixed effects as well as the set of other regressors which reflect socioeconomic and macroeconomic priors, unobserved race and gender-specific determinants and unobserved state-specific time trends.

The results in the first column of Table 6 demonstrate that, in these richer specifications, the estimated effects of cigarette taxes on teen smoking participation are uniformly negative and quite stable. More specifically, the estimated tax coefficients in these models range from roughly -0.098 to -0.112. However, these estimates are relatively imprecise, leading to somewhat weak statistical significance in some models. Nonetheless, these estimates do indicate that the within-state variation in cigarette taxes generated ''first-stage'' variation in the prevalence of teen smoking participation. To the extent that teen smoking and drinking are complements, the expectation would then be that increased cigarette taxes are also associated with reductions in the prevalence of teen drinking and heavy drinking participation. The next two columns of Table 6 provide direct evidence of those reduced-form relationships. The results of these reduced-form models uniformly indicate that higher cigarette taxes are indeed associated with

[26] The MTF extract employed in this study includes 1,933 cells from the 1985–1992 period representing the responses of 122,957 high school seniors.

Table 6
WLS estimates: reduced-form and IV models of teen smoking, 1985–1992 MTF surveys

Model	Reduced-form models			IV models	
	Estimated effects of real cigarette taxes on			Estimated effects of smoking on drinking measures	
	Smoking	Drinking	Heavy drinking	Drinking	Heavy drinking
Model (1)	0.0082	0.2497	0.2012	30.38	24.48
	(0.0262)	(0.0300)	(0.0269)	(95.10)	(76.63)
Model (2)	−0.1067	−0.1610	−0.1029	1.5095	0.9646
	(0.0488)	(0.0553)	(0.0498)	(0.6600)	(0.5153)
Model (3)	−0.0987	−0.1036	−0.0737	1.0491	0.7465
	(0.0511)	(0.0580)	(0.0524)	(0.6016)	(0.5328)
Model (4)	−0.1009	−0.0439	−0.0577	0.4348	0.5722
	(0.0528)	(0.0598)	(0.0542)	(0.5344)	(0.5138)
Model (5)	−0.0980	−0.0645	−0.0704	0.6579	0.7187
	(0.0530)	(0.0599)	(0.0545)	(0.5550)	(0.5501)
Model (6)	−0.0978	−0.0659	−0.0705	0.6738	0.7206
	(0.0525)	(0.0600)	(0.0545)	(0.5575)	(0.5452)
Model (7)	−0.1116	−0.1411	−0.1655	1.2638	1.4827
(0.0736)	(0.0834)	(0.0750)	(0.8413)	(0.9604)	

These estimations are based on the responses of 122,957 high school seniors grouped into 1,933 observations by state, year, age, race and gender and are weighted by the number of students grouped into each observation. Standard errors are reported in parentheses. All models include binary indicators for race/ethnicity, gender and age and year fixed effects. These model specifications correspond to those in Tables 2 and 5 except for the omission of MLDA variation.

reductions in both drinking measures. In some specifications, these reduced-form marginal effects are statistically significant. These findings are consistent with those reported in the previous section in that they suggest teen smoking and drinking are complements.

However, this evidence should be interpreted cautiously since it is highly qualified both by its lack of statistical precision as well as by concerns about the magnitudes of some evaluation results. These concerns about the magnitudes and imprecision of these results are illustrated succinctly in the final two columns of Table 6. These columns report the IV estimates of how teen smoking participation influences each of the two drinking measures based on the use of cigarette taxes as an instrumental variable. The corresponding but naive WLS estimates reported in the top panel of Table 2 indicated that teens who used alcohol were 30 to 35 percentage points more likely to smoke cigarettes.[27] The implied IV estimates reported in Table 6 are either substantially larger than these estimates or statistically indistinguishable from zero. The sensitivity apparent in these results is not entirely surprising and it does not contradict the results of the previous section:

[27] These stylized marginal effects are somewhat larger among the 1985–1992 cohorts.

that there is a strong complementarity between teen smoking and drinking. Instead, the inability of these reduced-form and IV estimates to provide more robust empirical evidence of this complementarity is actually quite plausible since cigarette taxes generated relatively little ''first-stage'' variation in smoking participation. In contrast, changes in minimum legal drinking ages generated sharp changes in teen alcohol use and allowed the complementarity of teen drinking and smoking to be identified robustly in models of smoking participation equations (Table 5).

The uniform evidence linking higher cigarette taxes to reductions in teen alcohol use is highly qualified by the lack of precision evident in Table 6. However, even the most conservatively small point estimates of this tax responsiveness suggest that these tax effects may be highly policy-relevant. For example, Federal legislators recently debated and rejected a cigarette tax increase of US$1.10 a pack (i.e., US$0.678 a pack in 1982–1984 dollars). The results in Table 6 (Model (4)) suggest that such a tax increase would reduce the prevalence of heavy drinking among teens by at least 3.9 percentage points (-0.0577×0.678). In other words, such a tax increase would generate a reduction of roughly 13% (0.039/0.331) in the mean probability of heavy drinking. While such a policy simulation is of course qualified by the lack of precision in the underlying econometric models, it nonetheless illustrates the largely overlooked empirical relevance of the complementarity between teen smoking and drinking and the unintended benefits of policies aimed at reducing their prevalence.

7. Conclusions

Policy-makers continue to devote considerable attention and resources to reducing the consumption of both alcohol and tobacco among young adults. However, these efforts and the corresponding empirical analyses have largely ignored the implications of the possible joint dependence of these consumption decisions. This study has presented empirical evidence on the complementarity between teen cigarette and alcohol use by exploiting the relevant ''cross-price'' effects in empirical models for smoking and drinking participation. The results indicate that teen drinking and smoking are highly complementary. In particular, the movement away from a minimum legal drinking age of 18 reduced teen smoking participation by 3 to 5%. The related instrumental variables (IV) estimates indicate that teen drinking roughly doubles the mean probability of smoking participation. These reduced-form and IV estimates are consistent with the magnitudes of the stylized partial correlations between teen smoking and drinking. The empirical evidence linking higher cigarette taxes to reductions in teen alcohol use also provided consistent evidence of this complementarity. However, these tax effects were imprecisely estimated. This evidence of strong, causal relationship between teen smoking and drinking has important implications. For example, our state experiments with increasing minimum legal drinking ages have provided robust empirical evidence that the initiation of a cigarette habit is an important welfare consequence of teen alcohol use. But, more generally, these results also indicate that efforts to reduce teen smoking and drinking participation can generate important and unintended benefits by reducing the consumption of complementary ‘‘sin’’ goods.

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