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Alcoholism, Work, and Income

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This article reports on an empirical analysis of the relationships between alcoholism and income and working. We show that the relationships between alcoholism and labor market success have important age or life-cycle dimensions. We present evidence that alcoholism may affect income more by restricting labor market participation than by affecting the wages of workers. Finally, we demonstrate that the effects of alcoholism on earnings depend on the extent to which one controls for other covariates associated with alcoholism; as such, we suggest that there may be important indirect as well as direct effects of alcoholism on labor market success.

> Individuals, besides, may sometimes ruin their fortunes by an excessive consumption of fermented liquors. [ADAM SMITH, *The Wealth of Nations*]

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I. Introduction

It is widely believed that alcoholism is a major social problem with potentially important economic consequences.[1] Alcoholism is a prevalent disorder in the United States, affecting about one in 20 individuals at any one time and one in 10 individuals at some point during their lives. Males are three times more likely than females to suffer from alcoholism.

A prevailing view on alcoholism is that it has depressant effects on earnings, income, and wages even after controlling for other important determinants of these labor market outcomes.[2] Despite Adam Smith's early observation and the current popularity of this view, its veracity has been challenged by the hypothesis that increased alcohol consumption—at least within some range—might actually be productive.[3] Some of this controversy is resolved immediately by distinguishing "alcohol consumption" and "alcoholism," but this distinction settles only part of the debate.

The main purposes of this article are to demonstrate the complexity of the alcoholism-income issue and to provide some evidence about its structure. First, we demonstrate that the relationships between alcoholism and labor market success vary over individuals' lifetimes. Second, we present some evidence that alcoholism may affect income more by reducing employment probabilities than by reducing wages. Finally, we demonstrate how the magnitude and significance of the effects of alcoholism on earnings depend on the extent to which one controls for alcoholism-related covariates.

Before proceeding, it is useful to review some background information on alcoholism. Throughout the article we use "alcoholism" as a convenient term to summarize both the "alcohol dependence" and "alcohol abuse" disorders as defined by the American Psychiatric Association (1980, 1987). Although much about alcoholism remains unknown, most experts would agree on some general characteristics of alcoholics (see NIAAA 1990). For instance, it is now widely accepted that alcoholics come from all socioeconomic, demographic, and occupational groups. Moreover, about 10% of males and 3% of females are actively alcoholic at any point in time. For males, young adulthood is when symptoms of alcoholism are most prevalent; symptoms tend to decline after this peak.[4] In addition, it is commonly observed that alcoholism tends to run in families, with the prevalent current

[1] U.S. Dept. of Health and Human Services, National Institute on Alcohol Abuse and Alcoholism (NIAAA 1990).

[2] Berry and Boland (1977) and Rice et al. (1990) are representative of such findings.

[3] See Berger and Leigh (1988) for evidence of beneficial labor market effects of alcohol, Cook (1991) for additional discussion of such results, and Shaper et al. (1988) for a discussion of the findings of a positive health effect and some evidence explaining why the positive effects may be incorrect interpretations.

[4] See Vaillant (1983), chap. 3, for an interesting discussion.

wisdom being that there is a strong genetic dimension to such tendencies (see NIAAA 1990).[5]

Despite these broad agreements, there remain many lively debates on issues such as the etiology of alcoholism, the effectiveness of alternative treatment methods,[6] and the life-cycle course of alcoholism problems. For instance, there have to date been insufficient longitudinal data to determine much about the lifetime course of alcoholism problems. More important for our purposes is the debate about whether alcoholism is, in a biomedical sense, a disease or, rather, is a complex set of health outcomes that arise from freely chosen behaviors. Alcoholism is widely considered in the medical literature to be a disorder with a significant genetic basis. However, such "disease models" have not always held sway and even today are not universally embraced.[7]

This background provides the setting for the remainder of the article. Section II discusses the data used in the empirical analysis and the characteristics of our sample. Section III presents a model and an econometric strategy for estimating the role of alcoholism in a human capital framework. Section IV provides some evidence on the age or life-cycle dimensions of the relationships between alcoholism and labor market outcomes. Section V presents a variety of econometric estimates of the role of alcoholism as

[5] It might also be noted that alcoholism is generally estimated to be quite prevalent in the homeless population (Institute of Medicine 1988) as well as in some areas of the institutionalized population. The prevalence rate in the homeless population is suggested to be 20%–45% (NIAAA 1990). Since this analysis is based on a residential sample, the interesting and important issue of the effects of alcoholism in the homeless and institutionalized populations will not be treated here. To the extent that these "nonresidential" populations have greater than average propensities to be alcoholic and lower than average labor market success, our results would likely tend to underestimate the economic impacts of alcoholism in the entire population.

[6] Evidence on the effectiveness of treatment is mixed (see, e.g., Hayashida et al. 1989; and NIAAA 1990). Indeed, lacking evidence on the efficacy of inpatient treatment, many third-party payers are no longer covering inpatient treatment. Many individuals seek repeated treatment and try a variety of different approaches, ranging from self-help groups, to inpatient group therapy, to drug treatment. Given remission after such sequences of treatment, it is not clear which, if any, treatment was effective, if the sequence itself mattered, or if individuals self-selecting into treatment were those relatively more likely to succeed. Moreover, according to a prevalent view, alcoholics who are not currently manifesting symptoms are "recovering" or "in remission," although they never completely recover; "once an alcoholic, always an alcoholic," even though symptoms may not be manifested currently.

[7] See, e.g., Fingarette (1988) who maintains that the disease view has been perpetuated by various special interest groups that benefit from alcoholism's classification as a disorder. Perhaps not surprisingly, his perspective is the minority in the medical literature. The "rational addiction" theory of Becker and Murphy (1988) should also be noted in this context.

a determinant of labor market success. Section VI concludes with a discussion of the findings and of unresolved issues.

II. Data and Sample Characteristics

This analysis is based on wave 1 of the New Haven, Connecticut, site of the Epidemiological Catchment Area (ECA) survey conducted under the auspices of the U.S. National Institute of Mental Health (NIMH). The New Haven ECA survey is part of a larger NIMH-funded data collection effort surveying individuals 18 years old and older to assess psychiatric disorders in a population-based sample. The ECA surveys were designed primarily to assess the distribution of mental disorders in a community setting.[8] Prior to the ECA surveys, there were no large U.S. samples assessing individuals' psychiatric disorders that contained reliable measures of these disorders, including alcoholism. The ECA data are particularly well suited for study of alcoholism, as they provide medically sophisticated measures of alcoholism and other mental disorders and, of particular importance for our study, information on labor market outcomes as well as socioeconomic and demographic characteristics of individuals and their households.[9]

Between 1980 and 1981, wave 1 of the New Haven survey was completed, yielding 5,034 observations, a 77.6% completion rate. The New Haven standard metropolitan statistical area (SMSA) was (approximately) the catchment area sampled, this area comprising 13 towns with a total adult population of 420,000. Two coordinate groups were sampled in this residential survey: all adults (18+), and individuals 65 and over. From the 5,034 observations in wave 1, we initially restrict our attention to males aged 22–64. The focus here is on males, both because they are far more likely to suffer from alcoholism than are females and because of the considerable body of accumulated research regarding the specification of earn-

[8] For details on the ECA surveys, see Reiger et al. (1984), Eaton and Kessler (1985), and Robins et al. (1981).

[9] We restrict attention in this analysis to the New Haven site because its data on labor market outcomes are much richer than the labor market data available from the other four survey sites: Durham, North Carolina; Baltimore, Maryland; St. Louis, Missouri; and Los Angeles, California. Prior to the ECA surveys, studies of the economic and social consequences of alcoholism had of necessity relied on data that were unsatisfactory in one way or another. Weaknesses have included self-diagnoses of alcoholism, data obtained from individuals' visits to medical care facilities, unavailability of important covariates, and others. For instance, in an often-quoted study, Berry and Boland (1977) relied on a data set that included only household (not individual) income data and data on alcoholism for only one individual per household. Moreover, until the ECA surveys were conducted, assessments of disorders on the basis of state-of-the-art psychiatric diagnostic criteria were not available in large community data sets; mental health was typically assessed only by direct self-reporting of a diagnosis in large data sets.

ings models for males.[10] As discussed further below, we select this age group in order to mitigate problems of incomplete educational spells (on the younger end) and retirement (on the older end). Given the oversampling of the elderly at the New Haven site, the upper-end restriction reduced our sample size considerably.[11]

Appendix A describes the dependent and independent variables, while table 1 displays the sample descriptive statistics. With the exception of alcoholism, the tables are largely self-explanatory.[12] Alcoholism is measured in several ways, the two major definitions used here being whether or not the individual ever in his lifetime met the criteria for diagnosis of alcoholism (ALC-EVER = 1 or 0) and whether or not the individual who ever met the criteria had symptoms in the past year (ALC-YEAR = 1 or 0). Assessment of alcoholism and other mental disorders in the ECA is via a professionally designed survey instrument, the Diagnostic Interview Schedule (DIS), which conforms to the American Psychiatric Association DSM-III and DSM-IIIR (Diagnostic Statistical Manual-3d ed., and 3d ed. rev., respectively) disorder criteria for diagnosis of alcohol abuse and alcohol dependence; see Appendix B for details.

III. Earnings, Human Capital, and Alcoholism

The tradition of including measures of individuals' health status as components of their human capital in wage and earnings functions is well established.[13] The basic framework posits an earnings function

[10] See Willis (1986) for a good survey of earnings function estimation, and Mullahy and Sindelar (1990*b*, 1991) for a discussion of gender differences in the effects of alcoholism and other mental health problems.

[11] The reduction in sample size from the original 5,034 observations to the 555 observations we use in much of the econometric analysis is due to the following set of restrictions:

a) Nonelderly (ages 18–64): 2,458 remaining observations;
b) Initial age cutoff (ages 22–64): 2,237 remaining observations;
c) Restriction to estimation sample (ages 30–59): 1,420 remaining observations;
d) Restriction to males: 604 remaining observations;
e) Miscellaneous missing data: 555 remaining observations.

[12] It might be noted, however, that the variables SCHOOLING and INCOME are created using interval midpoints. For SCHOOLING, 17 years was used for the open-ended upper interval "grad school." The 1980–81 survey asks the respondent to report income in the preceding year; income is thus expressed in 1979–80 dollars. The variable INCOME consists of both labor income and other income "brought into" the household by the individual. For this measure, "0.5" was used for the bottom interval "less than $1,000," and "120" was used for the upper open-ended interval "over $100,000." While this approach is admittedly ad hoc, it greatly simplifies the econometrics. We present some evidence in table 6 that explicitly accounting for the censoring of the income measure yields results that differ little from those obtained using the "fill in the upper end" method.

[13] See, e.g., Grossman (1972), Bartel and Taubman (1979, 1986), Mitchell and Butler (1986), and Frank and Gertler (1991).

Table 1
Sample Descriptive Statistics

Variable	Mean		Minimum		Maximum	
	Full Sample	Ages 30–59	Full Sample	Ages 30–59	Full Sample	Ages 30–59
Left-hand side:						
FULLTIME	.764	.838	0	0	1	1
INCOME	20.073	23.423	.500	.500	120.0	120.0
INCOME*	22.704	25.068	.500	.500	120.0	120.0
INCOME†	22.851	25.233	.500	.500	120.0	120.0
LOG-INCOME	2.717	2.912	−.693	−.693	4.787	4.787
LOG-INCOME*	2.959	3.067	−.693	−.693	4.787	4.787
LOG-INCOME†	2.962	3.070	−.693	−.693	4.787	4.787
TRANSFER RECIPIENT	.120	.090	0	0	1	1
FULL-NOTRANS	.737	.809	0	0	1	1
Right-hand side:						
ALC-EVER	.206	.204	0	0	1	1
ALC-YEAR	.106	.101	0	0	1	1
ALC-PRE19	.080	.059	0	0	1	1
ALC-1922	.064	.056	0	0	1	1
AGE	39.689	41.861	22	30	64	59
WHITE	.861	.858	0	0	1	1
HEALTHY	.887	.899	0	0	1	1
SCHOOLING	13.465	13.447	2	2	17	17
HIGH SCHOOL	.465	.436	0	0	1	1
COLLEGE	.359	.371	0	0	1	1
MARRIED	.658	.723	0	0	1	1
OTHER INCOME	5.584	4.532	0.0	0.0	58.5	57.5
ANTISOCIAL PERSONALITY	.090	.079	0	0	1	1
MENTALLY HEALTHY	.891	.906	0	0	1	1

NOTE.—N = 897 for full sample; N = 555 for subsample ages 30–59.
* Computed on subsample for which FULLTIME = 1, N = 685 or 465.
† Computed on subsample for which FULLTIME = 1 and TRANSFER = 0; N = 661 or 449.

$$y = y(H, K, X) + \varepsilon, \tag{1}$$

where y is some measure like log earnings; H is a vector of measures of the health components of human capital; K is a vector of nonhealth human capital measures (schooling, experience, etc.); X is a vector of other covariates (age, race, sex, etc.); and ε is a stochastic error, generally assumed to satisfy $E(\varepsilon | H, K, X) = 0$.

Following this tradition, we specify $H = (A, S)$, where A is a vector of measures of alcoholism and S represents other health outcomes. The econometric counterpart to (1) is specified to be linear:

$$y_t = z_t\alpha + \varepsilon_t, \tag{2}$$

where y_t is log income; z_t summarizes all exogenous variables, $z_t = (A_t, S_t, K_t, X_t)$; and α is a vector of unknown parameters. In addition, we specify

a probit equation (Heckman 1979) to describe the outcome of full-time participation in the labor market. The observed binary participation variable, I_t, is generated by a linear latent variable model whose error term is assumed to be homoscedastic normal, so that $I_t = 1(z_t\beta + \eta_t > 0)$, where $1(.)$ is the 0-1 indicator function. One standard result that will be of use below is that, in general,

$$E(y_t | I_t = 1, z_t) = z_t\alpha + \gamma\lambda_t$$
$$\neq z_t\alpha,$$

where λ_t is the inverse Mills ratio under an assumption of normality for ε_t, and γ is an unknown scalar parameter that is a function of $\text{cov}(\varepsilon, \eta)$. It is assumed throughout that there are available N independent observations on (y_t, I_t, z_t).

That other components of human capital (schooling, experience, marital status, etc.) may be correlated with, and to some degree determined by, the health component(s) is usually ignored in the context of earnings function estimation. To the extent that the health components are structural determinants of the nonhealth components (e.g., if schooling attainment depends structurally on health status), then it is very easy to understate empirically the total productivity of health capital, that is, dy/dH, since indirect effects operating through the nonhealth components would not generally be captured. A fundamental point can be made in the context of the human capital framework sketched above (reinforced empirically by results presented below in table 5). Suppose for simplicity that $y = y(A, K) + \varepsilon$ with $E(\varepsilon | A, K) = 0$, and consider how $E(y | A, K)$ varies with A:

$$dE(y | A, K)/dA = y_A + y_K dK/dA. \tag{3}$$

Equation (3) emphasizes what we consider to be a potentially important omission in many studies when measuring the productivity effects of alcoholism or, for that matter, of any disorder of interest. That is, the total effects of A on y are given not simply by the partial derivative y_A that holds all else constant but rather by the total derivative that allows K to vary in response to variations in A. Accordingly, both direct (y_A) as well as indirect ($y_K dK/dA$) channels of influence must be admitted as possibilities if all the effects of A on y are to be evaluated and well understood in the design of policies targeted to mitigate or prevent alcoholism-related problems. For instance, see Mullahy and Sindelar (1989, 1990*a*, in press) and Cook and Moore (1991) for recent discussions of the relationships between alcohol use and educational attainment.

It is also important to note that standard static earnings-health models like (1) typically assume that the health variables are econometrically ex-

ogenous. In a life-cycle context, such an assumption may be tenuous since health and labor market outcomes will be jointly determined in a health production context. Unfortunately, the data at our disposal do not suggest any reasonable instruments that would enable us to control for possible nonzero correlation between A and ε. Accordingly, mainly out of econometric necessity, we treat alcoholism as a predetermined or exogenous determinant of labor market and other sociodemographic outcomes, much like health status is typically treated in such models. This perspective on alcoholism is consonant with much of the medical literature that considers alcoholism a disease; whether this would be appropriate under a conscious choice perspective is a more tenuous matter. Our empirical results are thus reasonable to the extent that the exogeneity of alcoholism is a valid maintained assumption, but we readily admit that biased estimates of alcoholism's role in labor market success may arise should this assumption be invalid.[14] Since whether one has ever had an alcoholism problem is less likely to be correlated with contemporaneous unobservables than whether one is currently drinking to excess, we focus primarily on ALC-EVER as the alcoholism measure.[15]

IV. Life-Cycle Dimensions to Alcoholism Problems

This section compares how labor force participation and earnings profiles vary by age for alcoholics (defined in various ways) and nonalcoholics. This life-cycle perspective provides some insights into why earlier studies have come up with conflicting results about the effect of alcohol consumption or alcoholism on earnings, income, and wages. While some studies have found negative effects (Berry and Boland 1977), others have found no significant effects (Benham and Benham 1982), and still others have found positive impacts (Berger and Leigh 1988; Cook 1991).[16]

In this analysis, we are particularly interested in the youngest and oldest age categories as it is these groups for whom seemingly counterintuitive

[14] Even though our data do not allow us to meaningfully address the cause-effect issue in the empirical analysis, we offer the simple yet often overlooked point that a nonzero dK/dA implies that there is some correlation between K and A—causality may or may not be present. From a policy perspective, it is important to recognize that over the course of the life cycle A and K are likely to be interwoven in an intricate, complicated manner and that attributing "costs" to alcoholism without cognizance of such interrelationships—i.e., failing to account for possible indirect as well as the direct effects of alcoholism on earnings—is likely to lead to underestimates of such costs.

[15] The use of the DSM-IIIR measure of alcoholism also helps justify its exogeneity in labor market equations relative to the DSM-III measure, which includes symptoms of trouble at work (see App. B for additional discussion).

[16] For instance, in studies of alcohol consumption (*not* alcoholism), Berger and Leigh (1988) and Cook (1991) find positive effects of alcohol consumption on earnings.

relationships between alcoholism and labor market success might arise. For the youngest group, alcoholism may tend to increase labor market participation and thus increase earnings. One mechanism consistent with such a finding would be that those with alcohol problems would be more likely to have trouble in school and either drop out of school or work more hours even while attending school part time. They would thus be working more hours and accumulating more labor market experience; increases in both hours and wage rates would thus tend to result in greater earnings. At some later point, however, the effects of nonalcoholics' greater educational attainment would overtake the experience advantages of the young alcoholics.

The oldest age group could also exhibit seemingly inconsistent working and earning profiles. Over their lives, alcoholics may accumulate less financial capital (pensions, savings, etc.) than would nonalcoholics so that early retirement may not be a reasonable option. Instead, these aging alcoholics may continue employment later in life so that they may have higher contemporaneous labor market participation and income (but not necessarily wealth) as compared to their nonalcoholic counterparts who have begun to retire.

Table 2 compares the probability that an individual is employed full time (i.e., whether the individual worked for pay all 12 months in the

Table 2
FULLTIME Workers: Percentages by Age Group and Alcoholism Status

Ages	All Observations	ALC-EVER = 0	ALC-EVER = 1	ALC-YEAR = 0	ALC-YEAR = 1
All ages	.764	.775	719	.776	.663
N	897	712	185	802	95
Difference due to ALC = 1 relative to ALC = 0		−.056		−.113**	
Age subgroups:					
22–29	.675	.652	.738	.678	.657
N	243	178	65	208	35
Decrease or increase due to ALC = 1 relative to ALC = 0		+.086		−.021	
30–44	.845	.875	.733	.873	.643
N	348	273	75	306	42
Decrease or increase due to ALC = 1 relative to ALC = 0		−.142***		−.230***	
45–59	.826	.858	.684	.829	.786
N	207	169	38	193	14
Decrease or increase due to ALC = 1 relative to ALC = 0		−.174**		−.043	
60–64	.566	.565	.571	.568	.500
N	99	92	7	95	4
Decrease or increase due to ALC = 1 relative to ALC = 0		+.006		−.068	

** $p < .05$, for two-tailed tests of differences in proportions between ALC = 0 and ALC = 1.
*** $p < .01$, for two-tailed tests of differences in proportions between ALC = 0 and ALC = 1.

previous year, vacations excepted) for alcoholics and nonalcoholics by age group. The top row in the table suggests that, for all ages combined, alcoholism (occurring either within the past 12 months [ALC-YEAR] or at any point in one's lifetime [ALC-EVER]) has a dampening effect on full-time work propensity, although the difference is statistically significant only for alcoholism symptoms in the past year.

The remainder of the table shows how these relationships vary across age categories. The second through fifth rows suggest that there are important differences in full-time work propensity by alcoholism status and also show how the effects of alcoholism vary by age. The youngest and the oldest age groups both show a positive labor force participation response to alcoholism measured by ALC-EVER but not ALC-YEAR. Note that the differences are not statistically significant and that the sample of alcoholics is quite small in the oldest group. Conversely, for what might be considered the prime-aged males, ages 30–59, the effects of alcoholism are generally negative, significant and quite large, as seen in rows 3 and 4 (the exception being for current symptoms [ALC-YEAR] for individuals aged 30–44). Interestingly, when considering current symptoms only (ALC-YEAR), the differences are far less striking, suggesting the possibility that the damages associated with alcoholism are much more subtle, far-reaching, and indirect than simply whether or not an individual currently is manifesting symptoms. We take up an econometric examination of this issue in Section V.

Table 3 displays the results of a similar analysis, with the focus now on income. The top row of the table demonstrates that, for our sample of males, the effects of alcoholism on personal income are negative and fairly large. This result holds whether one considers the full sample (cols. 2–5) or restricts attention to the sample of males who are full-time workers (cols. 7–10), although the effects are somewhat larger for the full sample. For full-time workers, income is likely to be a better proxy for earnings and, therefore, for productivity.[17] Again, however, aggregation over the age-groups masks considerable heterogeneity in these relationships across the age-groups. Statistical significance aside for the moment, alcoholism (measured either way) appears to have little effect on the incomes of the youngest group, a positive effect for the oldest group, but important negative effects on the incomes of the prime-aged group in the middle.

This simple yet revealing examination of the data suggests that the effects of alcoholism vary in important ways over the life cycle.[18] Whether al-

[17] In Sec. V we assess in greater detail this question about the role of alcoholism as a determinant of income when the focus is on full-time workers only.

[18] Interpreting alcoholism as a "hazardous behavior," the finding that there are important age or life-cycle effects should not be surprising. See Ippolito (1981) and Ehrlich and Chuma (1990) for discussion.

Table 3
INCOME: Means (in Thousands of $), by Age Group, ALC-EVER Status, ALC-YEAR Status, and FULLTIME Status

	All Observations					FULLTIME = 1				
		ALC-EVER		ALC-YEAR			ALC-EVER		ALC-YEAR	
Ages	All Observations	= 0	= 1	= 0	= 1	All Observations	= 0	= 1	= 0	= 1
All ages	20.1	20.8	17.4	20.5	16.9	22.7	23.2	20.7	22.9	20.7
N	897	712	185	802	95	685	552	133	622	63
% difference due to ALC = 1 relative to ALC = 0		−16.3%***		−17.6%**			−10.8%		−9.6%	
Age subgroups:										
22–29	12.9	12.8	13.2	12.9	12.7	15.6	15.7	15.4	15.6	15.7
N	243	178	65	208	35	164	116	48	141	23
% decrease or increase due to ALC = 1 relative to ALC = 0		+3.1%		−1.6%			−1.9%		+0.6%	
30–44	23.5	24.2	21.1	24.3	18.1	25.2	25.2	25.3	25.4	23.3
N	348	273	75	306	42	294	239	55	267	27
% decrease or increase due to ALC = 1 relative to ALC = 0		−12.8%		−25.5%**			+0.4%		−8.3%	
45–59	23.3	24.7	16.8	23.3	21.9	24.8	25.7	19.6	25.0	22.3
N	207	169	38	193	14	171	145	26	160	11
% decrease or increase due to ALC = 1 relative to ALC = 0		−32.0%***		−6.0%			−23.7%***		−10.8%	
60–64	19.0	18.8	20.6	18.8	22.2	23.8	23.4	29.4	23.4	36.3
N	99	92	7	95	4	56	52	4	54	2
% decrease or increase due to ALC = 1 relative to ALC = 0		+9.6%		+18.1%			+25.6%		+55.1%**	

** $p < .05$, for two-tailed tests of differences in means, allowing for different variances.
*** $p < .01$, for two-tailed tests of differences in means, allowing for different variances.

coholism has positive, negative, or no effects on income and labor force participation depends critically on age. This result suggests that the various cross-sectional age-groups should be examined separately and should not merely be pooled in analyses.[19] Given the pronounced differences in how alcoholism relates to income and to full-time work propensity between the 30–59 age-group and both the younger and the older groups, most of the econometric analysis that follows will focus on this prime-aged group.[20] The overwhelming importance of these life cycle effects is unlikely to be captured fully by using the entire sample and resorting to the use of age polynomials in a regression model.[21]

Although we believe that this glimpse of the data is enlightening, it is insufficiently structured to address many questions one would like answered. Accordingly, we turn in the next section to a more structured econometric approach.

V. Econometric Estimates

In order to assess the relative magnitudes of the direct and indirect effects of alcoholism on labor market success, it is useful to consider a set of baseline estimates in which potentially important correlates of alcoholism have been omitted, thereby allowing the alcoholism coefficient estimate to absorb their impacts to the extent that such correlation is present. These results are presented in table 4, which compares estimates of LOG-INCOME (i.e., eq. [2]) and FULLTIME (eq. [3]) models for the entire sample 22–64 and for the 30–59 subsample. Consistent with the results in tables 2 and 3, we find that the magnitude of the effects of alcoholism depends on the age composition of the sample. For both LOG-INCOME and FULLTIME, the point estimates of the alcoholism effects are markedly larger and statistically more precise in the 30–59 sample. For this age group, ever having alcoholism has significant and large depressant effects on both income and labor force participation. Whether these are meaningful esti-

[19] The above discussion has been cast in a life-cycle context. It is possible, of course, that the effects are not age effects as much as they are cohort effects, but neither our modest sample, nor (to our knowledge) any other contributions in the literature (NIAAA 1990), have been up to the task of disentangling these elements. Accordingly, we will continue the discussion as if they are age effects but readily admit that competing hypotheses must be entertained.

[20] Using the specification reported in col. 6 of table 5, we conduct a Wald test to determine whether it is reasonable to pool the 30–44 and the 45–59 age-groups. The test statistic, distributed χ^2 with 9 df under the null, has a value of 10.27. Since the corresponding p-value exceeds .3, pooling would not appear to be unreasonable. In some preliminary estimates of income models for the younger group, we find that the alcoholism coefficient estimate is positive (as expected) but insignificant.

[21] See Murphy and Welch (1990) for an interesting discussion of the nonlinear structure of age effects in earnings models.

Table 4
Baseline LOG-INCOME and FULLTIME Specifications: Comparing Results for Alternative Age-Inclusion Criteria

	LOG-INCOME (OLS)		FULLTIME (Probit)	
Variable	Ages 22–64 (1)	Ages 30–59 (2)	Ages 22–64 (3)	Ages 30–59 (4)
ALC-EVER	−.173	−.312	−.186	−.465
	(2.43)	(3.33)	(1.58)	(3.01)
AGE	.165	.105	.221	.176
	(9.83)	(2.50)	(7.38)	(1.99)
AGE^2	−.002	−.001	−.003	−.002
	(9.18)	(2.34)	(7.33)	(2.00)
WHITE	.388	.490	.269	.419
	(4.43)	(4.47)	(2.00)	(2.35)
HEALTHY	.493	.522	.837	.948
	(4.65)	(3.33)	(5.81)	(4.90)
CONSTANT	−1.509	−.232	−4.466	−3.745
	(4.36)	(.26)	(7.33)	(1.98)
N	897	555	897	555

NOTE.—Asymptotic *t*-statistics are in parentheses, heteroscedasticity-consistent for ordinary least squares (OLS).

mates of structural effects, or merely the confounding indirect effects of omitted human capital covariates, will be considered below.[22]

The estimates presented in table 5 illustrate what might be thought of as the direct and indirect effects of alcoholism (specifically, ALC-EVER) on LOG-INCOME. In columns 2–6 we add to the covariates included in the column 1 specification additional variables that may be correlated with, and perhaps influenced by, alcoholism, thereby demonstrating how controlling for an increasingly more inclusive vector of human capital covariates in z_t affects inferences about the effects of alcoholism. (For reference, col. 2 of table 5 replicates the results from col. 2 of table 4.)

First, note that the coefficient estimates for HEALTHY, educational attainment (measured either by SCHOOLING or by the HIGH SCHOOL and COLLEGE dummies), MARRIED, and OTHER INCOME are statistically significant by conventional standards. The net effect on the ALC-EVER coefficient point estimate of including these variables is substantial.[23] Using $\exp(\hat{\alpha}_j) - 1$ to estimate the percentage change in $E(y_t|z_t)$ due to

[22] Note for both LOG-INCOME and FULLTIME the estimated AGE and AGE SQUARED effects for the 30–59 sample are less significant and somewhat smaller than they are for the full sample. Since the most steeply sloped segment of the age-income profile is likely to be at ages less than 30, this is not a surprising result.

[23] Entering individually each of the omitted human capital variables, schooling attainment as measured by the two school success dummies (HIGH SCHOOL and COLLEGE) had the largest single impact on the coefficient of ALC-EVER. The age 30–59 sample correlations of ALC-EVER with the human capital covariates

Table 5
OLS Model Estimates with Alternative Human Capital Covariates

	Dependent Variable: LOG-INCOME					
Variable	(1)	(2)	(3)	(4)	(5)	(6)
ALC-EVER	−.369	−.312	−.249	−.236	−.200	−.188
	(3.86)	(3.33)	(2.77)	(2.69)	(2.31)	(2.20)
AGE	.113	.105	.125	.121	.116	.109
	(2.61)	(2.50)	(3.11)	(3.02)	(2.92)	(2.77)
AGE^2	−.001	−.001	−.001	−.001	−.001	−.001
	(2.53)	(2.34)	(2.81)	(2.73)	(2.65)	(2.53)
WHITE	.568	.490	.349	.331	.280	.245
	(4.91)	(4.47)	(3.20)	(3.04)	(2.58)	(2.23)
HEALTHY	...	.522	.415	.414	.389	.392
		(3.33)	(2.59)	(2.57)	(2.51)	(2.49)
SCHOOLING	...	...	.065	...	.070	.072
			(5.67)		(6.39)	(6.69)
HIGH SCHOOL	...	...	...	.284	...	...
				(3.15)		
COLLEGE	...	...	...	.570	...	...
				(5.48)		
MARRIED	...	...	...	...	.319	.361
					(4.27)	(4.83)
OTHER INCOME	...	...	...	...	...	−.017
						(3.49)
CONSTANT	.074	−.232	−1.446	−.797	−1.453	−1.234
	(.08)	(.26)	(1.68)	(.94)	(1.72)	(1.50)

NOTE.—Subsample ages 30–59. N = 555. Heteroscedasticity-consistent t-statistics are in parentheses. OLS = ordinary least squares.

turning on the jth dummy variable, the direct effect of ALC-EVER shrinks from a 31% reduction in mean income (based on the col. 1 estimate) to a 17% reduction (based on the col. 6 estimate) when we control for all the other human capital covariates.

We interpret the coefficient of ALC-EVER in column 1 as an estimate of the full effect of alcoholism on income. In this case, the coefficient picks up all the effects of alcoholism—direct and indirect—that could occur through the omitted human capital variables. Interpreting the difference between the transformed ALC-EVER coefficient in column 1 and that in column 6 as the magnitude of the indirect effects of alcoholism suggests that such indirect effects are substantial, almost one-half the total effect. This suggests that studies will vary in their estimates of the labor market effects of alcoholism depending on the extent to which they control for covariates correlated with alcoholism, a simple yet critical point that goes largely unrecognized in the pertinent health literature.

are: SCHOOLING, −0.1449; HEALTHY, −0.1426; HIGH SCHOOL, 0.0066; COLLEGE, −0.1291; MARRIED, −0.1364; and OTHER INCOME, 0.0124. In related work, we have found negative and significant correlations between early onset of alcoholism and educational attainment (Mullahy and Sindelar, in press).

We turn now to consider a variety of alternative model specifications and estimation strategies that are designed to determine the robustness of what we view as our central results in table 5. These sensitivity analyses are presented in tables 6, 7, and 8.

In table 6, the model estimates displayed in column 1 are used to assess whether there are effects of current alcoholism symptoms on income beyond those attributable to ALC-EVER. Accordingly, both ALC-EVER and ALC-YEAR are included as covariates. We find that little additional explanatory information is contained in the ALC-YEAR variable. When only ALC-YEAR is included (col. 2), its effects are somewhat stronger

Table 6
Alternative Model Specifications and Estimators

	Dependent Variable: LOG-INCOME						
	OLS					SCLS	
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ALC-EVER	−.199	...	...	−.330	−.191	−.174	−.120
	(1.63)			(2.43)	(1.98)	(2.23)	(1.73)
ALC-YEAR	.021	−.150	...	...	...	...	...
	(.12)	(1.31)					
ALC-PRE19	...	...	−.099	...	...	...	...
			(.57)				
ALC-1922	...	...	−.175	...	...	...	...
			(1.29)				
ANTISOCIAL PERSONALITY	...	...	...	...	.010	...	...
					(.05)		
MENTALLY HEALTHY	...	...	...	...	.044	...	...
					(.30)		
AGE	.109	.105	.107	.114	.109	.113	.102
	(2.75)	(2.64)	(2.72)	(1.65)	(2.77)	(3.29)	(3.41)
AGE^2	−.001	−.001	−.001	−.001	−.001	−.001	−.001
	(2.51)	(2.39)	(2.47)	(1.44)	(2.53)	(3.02)	(3.17)
WHITE	.245	.234	.238	.154	.244	.203	.168
	(2.23)	(2.12)	(2.17)	(.79)	(2.21)	(2.16)	(2.00)
HEALTHY	.391	.419	.410	1.324	.377	.332	.242
	(2.48)	(2.66)	(2.53)	(3.26)	(2.37)	(2.38)	(1.95)
SCHOOLING	.072	.074	.075	.089	.072	.072	.068
	(6.62)	(6.60)	(6.78)	(4.16)	(6.70)	(7.24)	(7.48)
MARRIED	.362	.373	.383	.156	.360	.351	.318
	(4.84)	(4.94)	(5.02)	(1.16)	(4.78)	(4.95)	(4.70)
OTHER INCOME	−.017	−.018	−.018	−.009	−.017	−.016	−.014
	(3.49)	(3.50)	(3.54)	(1.50)	(3.46)	(3.58)	(3.16)
CONSTANT	−1.246	−1.223	−1.299	−2.333	−1.256	−1.216	−.759
	(1.49)	(1.46)	(1.57)	(1.53)	(1.51)	(1.66)	(1.17)
N	555	555	555	123	555	555	555

NOTE.—Subsample ages 30–59. Columns 1–3: alternative alcoholism measures; col. 4: father's schooling exactly 12 years for all observations; col. 5: inclusion of possible psychological comorbidities; cols. 6–7: symmetrically censored estimator. Heteroscedasticity-consistent *t*-statistics are in parentheses.

than when included with ALC-EVER, but not up to conventional criteria for statistical significance.

In rather different ways, the model estimates presented in columns 3 and 4 of table 6 attempt to control for individuals' "baseline" circumstances. Column 3 includes two dummy variables describing the timing of onset of alcoholism symptoms: symptoms occurring prior to age 19 (ALC-PRE19) and symptoms occurring between ages 19 and 22 (ALC-1922). While both point estimates have the anticipated negative sign, neither is statistically significant.

The model estimated in column 4 takes a somewhat different approach. The sample here is defined so that the educational attainment of the fathers of the ECA survey respondents is exactly 12 years, thus explaining the smaller sample size of $N = 123$. All individuals in this subsample can thus be said to have at least one (but certainly not all) initial conditions in common. Despite the small sample size, the point estimate of the ALC-EVER effect remains statistically significant and is considerably larger in absolute value than in the comparable specification in column 6 of table 5. On the basis of these estimates, we are led to conclude that some forms of unobserved heterogeneity in baseline or initial conditions are likely to be correlated with the included health covariates. How and to what extent this is so is an important research issue that demands considerably richer data than those available here.

In column 5 of table 6, we consider the possibility that estimates of the alcoholism coefficients may be corrupted by the omission of measures of other emotional or mental disorders that are correlated with alcoholism. Accordingly, we include in the vector of explanatory variables a dummy variable indicating whether the individual ever suffered from antisocial personality disorder—a potentially important comorbidity of alcoholism—as well as a dummy variable indicating whether the individual reports his mental and emotional health to be excellent/good versus fair/poor. Neither of these additional mental health covariates is statistically significant, and, more important, their inclusion has no material effects on the point estimate of the ALC-EVER coefficient or its significance (comparing col. 1 of table 6 to col. 5).

The last results in table 6 are an assessment of the sensitivity of our results to the recoding of the income variable. We consider two respecifications of the LOG-INCOME measure, each using different lower- and upper-censoring cutoffs. For values of income below some L we treat LOG-INCOME as if lower censored, for values of LOG-INCOME between L and some U we use the interval midpoints (as in the preceding analysis), and for values of LOG-INCOME greater than U we treat LOG-INCOME as if upper-censored. The new measures thus have the character of a two-limit Tobit model and should provide a useful check on the estimates presented above. To attain robustness beyond that offered by maximum

likelihood (ML) estimation of such a Tobit model under normality and homoscedasticity assumptions, we instead estimate a doubly censored version of the symmetrically censored least squares (SCLS) estimator proposed by Powell (1986).[24] In contrast to standard ML Tobit, consistency of Powell's SCLS estimator relies only on the ε_t having conditionally symmetric densities (in addition to some other technical regularity conditions discussed by Powell).

Two sets of SCLS estimates are estimated, and the results are presented in columns 6 and 7 of table 6. The appropriate comparison results in both cases are those in column 6 of table 5. In column 6, we set $L = \ln(.5)$ and $U = \ln(100)$ in order to account for censoring of just the lower and upper categories in the data. In column 7 we impose more stringent censoring, $L = \ln(5)$ and $U = \ln(50)$, given the possibility that the income data within the lower tail below \$5,000 and within the upper tail above \$50,000 may be most prone to misreporting and because the interval midpoints are less satisfactory proxies in the tails than in the center of the distribution. In the first case (col. 6), the results differ only trivially from the results in table 5. When the more stringent censoring is applied to the data (col. 7) the differences are somewhat more marked, yet the point estimate of alcoholism's effect is nonetheless in the same ballpark as in the previous set. We are led to conclude that few if any important inferences are being missed because of the censored income variables.

While the results in tables 5 and 6 permit a relatively straightforward interpretation of direct and indirect effects, it is unlikely that models such as these—linear in alcoholism—capture all the subtleties of the relationships between alcoholism and labor market success. To this end, we present an additional set of estimates of models in which the ALC-EVER measure is

[24] One characterization of the SCLS estimator is as the implicit solution $\hat{\alpha}$ of

$$\hat{\alpha} = \Big[\sum_{t=1}^{T} (\omega_{t1} + \omega_{t2}) z_t' z_t\Big]^{-1} \times \Big[\sum_{t=1}^{T} z_t'(\omega_{t1}\min\{y_t, 2z_t\hat{\alpha} - L\} + \omega_{t2}\max\{y_t, 2z_t\hat{\alpha} - U\})\Big],$$

where

$$\omega_{t1} = 1(L < z_t\hat{\alpha} < (U + L)/2)$$

and

$$\omega_{t2} = 1((U + L)/2 < z_t\hat{\alpha} < U).$$

permitted to interact with one or more of the other human capital covariates. While such interactions permit a much enhanced resolution of alcoholism's role in labor market success, they blur to some extent the distinction between what are direct and what are indirect effects.

The results of these interaction models are displayed in table 7. In the first column, ALC-EVER is interacted with the HIGH SCHOOL and COLLEGE dummies. The results here are striking: while the point estimate of the ALC-EVER coefficient remains significant, the significant interactions with both HIGH SCHOOL and COLLEGE are noteworthy. The heteroscedasticity-consistent Wald test statistics for the joint significance of the two interaction terms and for the ALC-EVER coefficient along with

Table 7
Alcoholism Interaction Models

	Dependent Variable: LOG-INCOME			
Variable	(1)	(2)	(3)	(4)
ALC-EVER	−.577	−.824	−1.036	−.596
	(2.92)	(2.05)	(1.76)	(2.05)
AGE	.106	.109	.107	.109
	(2.76)	(2.82)	(2.73)	(2.80)
AGE^2	−.001	−.001	−.001	−.001
	(2.53)	(2.57)	(2.52)	(2.55)
WHITE	.239	.243	.242	.247
	(2.21)	(2.22)	(2.21)	(2.26)
HEALTHY	.374	.370	.375	.227
	(2.42)	(2.39)	(2.41)	(1.19)
ALC-EVER × HEALTHY	...	...	...	.481
				(1.59)
HIGH SCHOOL	.166	...	...	...
	(1.77)			
COLLEGE	.453	...	...	...
	(4.18)			
ALC-EVER × HIGH SCHOOL	.480	...	...	...
	(2.11)			
ALC-EVER × COLLEGE	.658	...	...	...
	(2.92)			
SCHOOLING	...	.062	.061	.072
		(5.50)	(5.35)	(6.66)
ALC-EVER × SCHOOLING	...	.050	.053	...
		(1.71)	(1.82)	
ALC-EVER × AGE	...	...	.004	...
			(.45)	
MARRIED	.357	.366	.367	.375
	(4.79)	(4.91)	(4.90)	(5.10)
OTHER INCOME	−.018	−.018	−.018	−.018
	(3.60)	(3.62)	(3.61)	(3.61)
CONSTANT	−.398	−1.072	−1.009	−1.102
	(.49)	(1.33)	(1.23)	(1.33)

NOTE.—Subsample ages 30–59. $N = 555$. The table contains ordinary least squares estimates. Heteroscedasticity-consistent t-statistics are in parentheses. Column 1: schooling dummies and interactions; cols. 2–3: schooling and age interactions; col. 4: physical health status interaction.

the two interaction terms are 8.56 and 9.93, both corresponding to $p < .02$ for χ^2 variates with degrees of freedom (df) = 2 and df = 3, respectively.

The results for the HIGH SCHOOL variables are quite sensible: nonalcoholics who complete high school are best off, alcoholics who complete high school are next, nonalcoholics who fail to complete high school follow, while worst off are alcoholics who do not complete high school. (Note that, given the interactions of the dummy variables, the effects should be interpreted relative to the omitted category of nonalcoholic, non-high-school grads.) For the COLLEGE variables, the one possibly curious departure from this pattern is that the alcoholic college graduates marginally outperform their nonalcoholic counterparts.[25]

Columns 2 and 3 in table 7 present two analogous model estimates, the ALC-EVER interactions now being with SCHOOLING (col. 2) and with SCHOOLING and AGE (col. 3).[26] The positive point estimate on the interaction term in column 2 suggests the possibility that additional years of schooling may mitigate the otherwise adverse effects of alcoholism; a Wald test indicates that the ALC-EVER and ALC-EVER × SCHOOLING coefficient estimates are jointly significant at $p < .05$. Given this schooling effect, however, the results in column 3 suggest that there is little additional effect—positive or negative—due to an ALC-EVER × AGE interaction; indeed, the joint significance of the two interaction terms in the column 3 specification is low ($p > .18$).

Column 4 in table 7 considers an alcoholism interaction with physical health status. While the coefficient estimate on the interaction is not significant by usual standards, the point estimates (as they were for the HIGH SCHOOL variables) accord well with common sense: nonalcoholics in good health are best off, alcoholics in good health are next, nonalcoholics in poor health follow, while worst off are alcoholics in poor health. Wald test statistics for the joint significance of the linear and interaction ALC-EVER terms and for these along with the linear HEALTHY term are 5.99 ($p < .05$) and 14.81 ($p < .01$), these corresponding to χ^2 variates with df = 2 and df = 3, respectively. Given our relatively small sample size, it is no small task to tease out second-order effects. Nonetheless, the results in table 7 are sufficiently strong to suggest—at least for schooling attainment—that there is indeed some important interplay in how alcoholism and schooling jointly determine labor market outcomes.

[25] Cook and Moore (1991) consider how matriculation to and completion of college may be related to students' drinking behavior.

[26] We also estimated several alternative versions of the model, where quadratics in SCHOOLING and interactions between SCHOOLING and AGE were included. In none of these specifications, however, were the SCHOOLING^2 or the AGE × SCHOOLING coefficient estimates individually or jointly significant. These results are available on request.

Our final alternative specifications are motivated because individual income as recorded in the ECA represents income from all sources—labor earnings, nonlabor earnings, transfer payments, and so on. Accordingly, we attempt to obtain a measure of income that is likely to be closer to a measure of earnings that, we feel, would better measure the productivity effects of alcoholism. To derive such a measure, we first take the subsample of individuals who are full-time workers. Then we identify the subsample of observations reporting that they received no transfer payments in the form of social security, disability, welfare, or unemployment compensation. The intersection of the subsamples that report full-time work and no transfer receipt (FULL-NOTRANS = 1) is a subsample for which we feel the reported individual income measure better approximates earnings.[27] We model this process econometrically as a two-step Heckman selection process, with a probit model describing the determinants of FULL-NOTRANS, and a λ-corrected LOG-INCOME model estimated on the subsample for which FULL-NOTRANS = 1. It should be stressed that, because all variables in the FULLTIME model (interpreted as a labor supply model) must of necessity be included in the earnings equation (interpreted as labor supplied times the wage rate), the sample selection process that drives the selection into the population of full-time workers is identified only by the nonlinear functional form of the selection-correction term, λ.

The results of this exercise are presented in table 8. For both the baseline (cols. 1 and 2) and the augmented (cols. 3 and 4) models, the results suggest that the direct effects of ALC-EVER on income are working more through participation effects than through wage/productivity effects. That is, the ALC-EVER effect is statistically much stronger in the equation determining FULL-NOTRANS than in the equation determining LOG-INCOME conditional on FULL-NOTRANS = 1.[28]

The general finding is consistent with the results in table 1, where the income differences between alcoholics and nonalcoholics are greater for all individuals as compared to the differences conditional on working full time. Although evidence from both tables 1 and 8 is consistent with this interpretation, there are clearly many alternatives to examine before such a conclusion could be confirmed. Moreover, we share the concern common in applied microeconometrics when the selection model for conditional

[27] It might also be noted that when only full-time workers are sampled, earnings are more likely to proxy for wages since the variation in hours over the year is reduced considerably.

[28] Interestingly, while its significance is quite low, the point estimate on ALC-EVER in the LOG-INCOME model estimated conditional on FULL-NOTRANS = 1 differs little from its value when the full sample is used for estimation.

Table 8
Estimates for Fulltime Workers Receiving No Transfers: Probit FULL-NOTRANS and Heckman-Corrected LOG-INCOME for FULL-NOTRANS = 1

	Reduced Form		Full Model	
	FULL-NOTRANS = 1 (Probit)	LOG-INCOME (OLS)	FULL-NOTRANS = 1 (Probit)	LOG-INCOME (OLS)
Variable	(1)	(2)	(3)	(4)
ALC-EVER	−.402	−.151	−.286	−.181
	(2.67)	(.74)	(1.82)	(1.04)
AGE	.256	.104	.259	.230
	(3.05)	(.85)	(2.99)	(2.06)
AGE^2	−.003	−.001	−.003	−.003
	(3.10)	(.82)	(3.06)	(2.01)
WHITE	.497	.350	.255	.283
	(2.92)	(1.34)	(1.37)	(1.45)
HEALTHY	.836	.096	.764	.456
	(4.36)	(.19)	(3.84)	(1.08)
SCHOOLING	...	...	.056	.101
			(2.34)	(3.77)
MARRIED	...	...	.550	.542
			(3.74)	(2.31)
OTHER INCOME	...	...	−.028	−.033
			(3.12)	(2.55)
λ	...	.218	...	1.475
		(.18)		(1.50)
CONSTANT	−5.462	.410	−6.245	−4.581
	(3.03)	(.11)	(3.26)	(1.40)
N	555	449	555	449

NOTE.—Subsample ages 30–59. Asymptotic *t*-statistics are in parentheses.

LOG-INCOME is identified solely by the nonlinearity of the λ term. At a minimum, however, the results are suggestive of some avenues for future research pursuits.

The results that we present are in many respects not definitive. Other interpretations and confounding factors have not yet been eliminated. For example, alcoholism may itself be a symptom of deeper problems that may also result in reduced earnings, so that elimination of alcoholism per se would not necessarily imply that earnings would be of the same magnitude of otherwise similar individuals without alcoholism. Furthermore, the direction of causation cannot be determined with confidence; for example, lower earnings may certainly be a factor in the onset of alcoholism symptoms.[29] Of course, the altogether separate possibility that un-

[29] Vaillant (1983) quotes Enoch Gordis (now director of NIAAA): "Changes in personality or mood are now recognized to be largely the consequence of alcoholism, not its cause."

observed heterogeneity is ultimately driving all the outcomes must be admitted.

VI. Summary and Discussion

Studies of the effects of alcohol on earnings, income, and productivity have to date yielded conflicting results. The popular view that has been confirmed in several studies is that problem drinking has depressant effects on income. Recently, however, some studies have found insignificant effects or even positive effects of alcohol use. Part of the confusion owes to differences in the drinking measures used in these studies (alcohol consumption, alcoholism, etc.). That existing studies have employed different measures of labor market success (family income, individual income, individual earnings, wages) and/or have focused on different populations (e.g., workers only) only serves to compound the confusion.

The results reported in this article have several important implications and, we feel, provide at least a partial resolution to some of these apparently conflicting results. One is that inferences about the effects of alcoholism on income depend critically on the age-group being studied. Moreover, income alone may not be an accurate measure of well-being: Those alcoholics who earn more in youth, withdraw from school, and work more hours are not necessarily better off. Similarly, the older alcoholic who may have greater income and less leisure time is not necessarily in a preferred position.

Our results also suggest that alcoholism has a more significant impact on the likelihood of working than it does on how much earned when working (compare tables 2, 3, and 8). Studies may thus vary in their estimates of the impacts of alcoholism or alcohol consumption to the extent that their samples focus only on workers.

We also have shown that the extent to which one controls for variables correlated with alcoholism (e.g., schooling and marital status) has a considerable impact on the estimated effects of alcoholism. For instance, based on the results in table 5, the inclusion of covariates correlated with alcoholism reduces the estimated effects of alcoholism on income from 31% to 17%. Estimates of the magnitude of the effect of alcoholism on earnings may thus differ across studies depending on the extent to which one controls for such covariates. The full effect of alcoholism may be estimated by omitting such correlated variables. However, one could equally well be interested in estimating the effect of alcoholism on earnings after controlling for the indirect effects. The different estimates correspond to different lines of inquiry.

To summarize, while this article has not solved all margins of the alcohol-income puzzle, we feel it has suggested some important directions for future research. These would include—but not be limited to—further examination of alcoholism's life-cycle course, direct versus indirect effects

on labor market outcomes, interactions in the schooling-alcoholism process, and differential impacts on wages and participation.

Appendix A
Variable Definitions

FULLTIME	= 1 if individual worked 12 months for pay in previous year (including paid vacations), = 0 else
INCOME	= how much of household's total income before taxes for past year, including salaries, wages, social security, welfare, and any other income, was earned or brought in by individual (÷1,000)
LOG-INCOME	= natural log of INCOME
TRANSFER RECIPIENT	= 1 if individual reported receiving transfer payments in the form of social security, disability, welfare, or unemployment compensation, = 0 else
FULL-NOTRANS	= 1 if FULLTIME = 1 and TRANSFER RECIPIENT = 0, = 0 else
ALC-EVER	= 1 if symptoms of alcoholism present in past year if ever met the criterion, = 0 else
ALC-PRE19	= 1 if earliest symptoms of alcoholism present at age 18 or earlier, = 0 if earliest symptoms later or never
ALC-1922	= 1 if earliest symptoms of alcoholism present between ages 19 and 22, = 0 if earliest symptoms at other time or never
AGE	= age in years
AGE SQUARED	= AGE squared
WHITE	= 1 if race is white, = 0 if race is nonwhite
HEALTHY	= 1 if individual reports physical health excellent or good, = 0 if reports if reports fair or poor
SCHOOLING	= years of completed schooling
HIGH SCHOOL	= 1 if $12 \leq$ SCHOOLING ≤ 15, = 0 else
COLLEGE	= 1 if SCHOOLING ≥ 16, = 0 else
MARRIED	= 1 if currently married, = 0 else
OTHER INCOME	= other household income, measured as the greater of zero or household income minus personal income (in thousands)

ANTISOCIAL PERSONALITY	= 1	if ever met criterion for antisocial personality, = 0 else
MENTALLY HEALTHY	= 1	if individual reports emotional/mental health excellent or good, = 0 if reports fair or poor

Appendix B

Additional Details on the Definition of Alcoholism

Assessment of alcoholism in the ECA is via a professionally designed survey instrument, the Diagnostic Interview Schedule (DIS). The DIS consists of a battery of questions on symptoms. These symptoms are used to obtain diagnoses consistent with the American Psychiatric Association's (APA) Diagnostic Statistical Manual (DSM) criteria for diagnoses of alcohol abuse and dependence. Alcohol abuse and dependence are two separate but related disorders according to the APA criteria. Diagnosis of alcohol dependence requires an individual to have symptoms in at least three of nine diagnostic criteria and diagnosis of alcohol abuse requires that they have symptoms in at least one of two categories (APA 1987). We focus on whether the individual met the criteria for either dependence or abuse or both, and refer to this throughout the article as "alcoholism."

Questions in the DIS refer to symptoms such as had blackouts when drinking, heard things that weren't really there, had fits or seizures after cutting down on drinking, had the shakes, wanted to stop drinking but could not, continued to drink despite serious physical illness, needed a drink as soon as woke up, gone on benders for a couple of days, school or job troubles due to drinking, family objected to your drinking, fights while drinking, and arrested while drinking.

If an individual ever met the criteria for alcohol abuse or dependence, then the age at which he or she first had any symptom and age at which he or she last had a symptom (e.g., in the last year) were recorded. From this information we formulated two variables that are used in much of the analysis and two variables that we use in sensitivity analysis; all four are binary variables. The two main variables are ALC-EVER and ALC-YEAR. The variable ALC-EVER indicates whether the individual ever had the cluster of symptoms that met the criteria for alcohol abuse and dependence. The variable ALC-YEAR indicates for those who ever met the criteria for diagnoses whether they had suffered from any symptoms in the past year. The other two variables, ALC-PRE19 and ALC-1922, are indicators of when (i.e., before age 19 or between ages 19 and 22) the first symptoms occurred for only those who ever met the criteria.

The APA criteria have typically been used in a clinical setting by psychiatrists. Only recently with availability of the ECA data have diagnoses of mental health disorders been made in a survey of the general population. Using general population data avoids the self-selection problem in which only individuals who seek treatment are

observed. Using the DIS diagnostic approach reduces the potential for self-reporting biases with regard to alcoholism. That is, individuals are not merely asked "Are you an alcoholic?" or "Do you suffer from alcoholism?" as has been the case in some other surveys. Nonetheless, the input information for the DIS is provided by the respondent and thus may involve response bias. Fortunately, the ECA's DIS-based diagnosis of alcoholism has been found to have good correspondence with alternative diagnostic approaches (Anthony et al. 1985).

The DSM-III standards (APA 1980) for alcohol abuse and dependence were used in coding the ECA data. However, we revised the diagnosis of alcoholism using the symptom list in conjunction with the more recent DSM-IIIR criteria (APA 1987). This revision could have been important since the DSM-IIIR definition drops some of the labor market behavior symptoms used in diagnosis (see above), clearly a problem when using the diagnosis as an explanatory variable in labor market outcome models. However, for our category of alcohol abuse and/or dependence, the designation of "alcoholic" did not change for any observation in our sample for any of the four definitions discussed above.

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