SCHOOL-BASED PEER EFFECTS AND JUVENILE BEHAVIOR

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Abstract—We use a sample of tenth-graders to test for peer-group influences on the propensity to engage in five activities: drug use, alcohol drinking, cigarette smoking, church going, and the likelihood of dropping out of high school. We find strong evidence of peer-group effects at the school level for all activities. Tests for bias due to endogenous school choice yield mixed results. We find evidence of endogeneity bias for two of the five activities analyzed (drug use and alcohol drinking). On the whole, these results confirm the findings of previous research concerning interaction effects at the neighborhood level.

I. Introduction

MANY social scientists argue that social interactions play an important role in determining behavioral and economic outcomes. Wilson (1987, 1996), for example, argues that youths are collectively socialized through their contact with adults. Coleman (1990), Crane (1991), Becker (1996), and Durlauf (1997) posit contagious effects in which the probability that a youth behaves in a certain manner depends positively on the prevalence of such behavior among the youth's peers. And Anderson (1991) describes the allure of a "street culture" that values drug use and fosters delinquency.

The presence of neighborhood and peer effects is frequently offered as justification for policies that seek to integrate neighborhoods and public schools. The empirical literature, however, is far from conclusive concerning the magnitude of these effects, as well as the relative importance of the various forms of social interactions experienced by youths. Two methodological issues, in particular, stand out: first, it is difficult to distinguish among the various possible forms of social interactions; second, endogeneity problems are ubiquitous in this realm and may lead to overestimation of peer influences.

Manski (1995) identifies two broad forms of social interactions: in the first, youth behavior is influenced by the exogenous characteristics of the youth's reference group; in the second, youth behavior is influenced by the prevalence of that behavior in the group. An example may help clarify this distinction. According to the first hypothesis, a youth's propensity to drop out of school will be affected by the mean parental education within the youth's reference group; according to the second, a youth's propensity to drop out will be affected by the proportion of the youth's peers who drop out. Distinguishing between these two effects, labeled by Manski (1995) as "contextual" and "endogenous" effects, is important because they imply different responses to policy intervention.[1] Whereas endogenous effects give rise to bidirectional influences (and hence the possibility of social multipliers), contextual influences do not imply amplified responses to exogenous shocks.

Manski (1995) also raises a third possibility. Spurious estimates of peer-group effects may be erroneously interpreted as true endogenous or contextual effects. Spurious effects arise when youths in the same reference group behave similarly because they share a common set of unobserved characteristics. This may occur if families endogenously sort across neighborhoods and school districts. More precisely, if families sort themselves across school districts according to their willingness and ability to pay for better peer influences, and if such parental "conscientiousness" is unobserved, the estimates of peer influences will be biased upward. Although a few studies explicitly account for this source of bias (Aaronson, 1998; Rosenbaum, 1993; Evans, Oats, & Schwab, 1992), the majority of studies do not.

In this paper, we evaluate the importance of school-based peer influences in determining youth behavior. We use a sample of tenth-graders drawn from the National Education Longitudinal Survey (NELS) to test for peer-group influences in five different activities: drug use, alcohol drinking, cigarette smoking, church going, and dropping out of school. Our empirical strategy is designed to address the two methodological concerns discussed above: distinguishing endogenous from contextual effects and distinguishing real peer influences from spurious effects.

Our focus on schools rather then neighborhoods as the relevant sphere of interaction is intended to limit the importance of contextual effects. We argue that students are less exposed to the family background of their school peers than they are exposed to the family background of peers residing in the same neighborhood. Based on this contention, we argue that observable social interaction effects at the school level are more likely to be driven by bidirectional peer influences (rather than contextual effects) than are social interaction effects estimated at the neighborhood level.

To address the issue of identification, we use information on household mobility to conduct a test for endogenous peer groups, an idea previously suggested by Glaeser (1996). We argue that endogeneity bias of peer-group effect estimates should be less severe for long-term residents, because their

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[1] Nineteenth-century scientists grappling with the causes of epidemic fevers also drew the distinction between "endogenous" and "contextual" effects, although they used, of course, a different terminology. Around 1860, there were two competing views concerning the causes of epidemic fevers: contagion, the disease substance believed to be generated in the sick organism, which spreads the disease by contact, and miasma, the disease substance that invades an organism from the outside (see Barrett, 1996).

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residential and school decisions were made taking into account past, rather than present, school quality and peer-group composition. Indeed, to the extent that schools change with time and that endogenous sorting across schools is pervasive, peer-effect estimates should be higher for recent movers than for long-term residents. Estimating separate equations for long-term residents and recent movers and testing for differential effects provides then a simple test of endogeneity of school choices.

We find strong evidence of social interaction effects for all activities analyzed. These effects remain after controlling for several personal and school characteristics, family background variables, and several measures of parental involvement in the youth's daily life. On the other hand, we do find a relatively larger peer-group effects for youth from "recent-mover" families for two of the five activities analyzed (drug use and alcohol drinking), although the difference is statistically significant only for drug use. This provides mixed evidence concerning the extent to which endogenous sorting across schools inflates the estimates of peer influences.

We also implement a simple nonparametric test of social interactions in the spirit of Glaeser et al. (1996). The results of this test strongly suggest the presence of social interactions. We find, in particular, that, for all variables analyzed, the variance of school averages is much higher than would be expected in the absence of social interactions. The same result is obtained after extensively controlling for school heterogeneity. On the whole, this alternative approach reinforces the findings of substantial school-based peer effects.

II. Past Research and Empirical Methodology

A. Past Research

Although the sociological literature has placed great emphasis on the importance of social interactions, economists have traditionally downplayed interactions not mediated through markets. Recently, however, several attempts have been made to formalize the role of social interactions in human behavior and in the formation of preferences. Becker (1996), for example, proposes a "social capital" component to the utility function that depends on both one's choices and the choices of one's peers.[2] In Becker's theory, "an increase in a person's social capital increases his demand for goods and activities that are complements to the capital and reduces the demand for those that are substitute" (p. 13) Accordingly, "a teenager may begin to smoke, join a gang, and neglect his studies mainly because his friends smoke, are gang members, and do not pay attention to school" (p. 13).

Several explanations of social interactions that do not directly appeal to preferences have also been proposed.[3] One can imagine, for example, a situation in which drug use is harshly punished and the probability of detection declines as more people use drugs. Under these circumstances, drug use by one's peers will surely reduce one's chances of getting caught, thus raising one's propensity to use drugs. (See Sah (1991) for a model along these lines.) Alternatively, one can imagine a situation in which drug use is not only prevalent but is also perceived as a matter of status. Under these circumstances, deviators (those who dare to say no) are likely to be punished through ostracism or merciless bullying. This will in turn create strong incentives to conform and so will raise the propensity to use drugs. (See Akerlof (1997) and Bernheim (1994) for formalizations of this idea.)

Informational externalities can also give rise to social interactions. For example, if there is uncertainty about the relative payoffs of staying in school vis-à-vis dropping out, one may use the previous decisions of one's peers to make inferences. Under some circumstances, it will be optimal to follow the herd, that is, to drop out if everybody is dropping out (Bikhchandani, Hirshleifer, & Welch, 1992). If this is the case, peer-group effects will arise even though conformity itself does not necessarily entail a reward, pecuniary or otherwise.

The empirical research on the effects of social interactions on socioeconomic outcomes can be roughly divided into two groups. The first group is preoccupied mainly with contextual effects, reflecting the long-standing interests of sociologists in background. The second group is preoccupied mainly with endogenous effects, reflecting the renewed interests of economists in externalities.

There are several empirical studies of contextual interactions. Mayer (1991) and Evans et al. (1992) estimate the effect of the average socioeconomic status of a school's student body on dropping out, teen pregnancy, and a few other social outcomes. And O'Regan and Quigley (1996) study the relationship between neighborhood poverty rates and youth employment and "idleness" rates. (See also Brooks-Gunn, Duncan, Kelabanov, and Sealand (1993), Corcoran, Gordon, Laren, and Solon (1992), and Crane (1991).)

There are also several studies of endogenous interactions. Case and Katz (1991) use data on inner-city Boston youth to estimate the effects of neighborhood prevalence of crime, drug and alcohol use, childbearing out of wedlock, and church attendance on the probability that an individual

[2] Coleman (1990) defines social capital as "resources that inhere in family relations and in community social organizations and that are useful for the cognitive or social development of a child or young person" (p. 300).

[3] Wilson (1996) draws a similar distinction between social interactions driven by preferences or values and those driven by rational responses. According to Wilson, "accidental or nonconscious" behavioral transmission occurs when a youth's exposure to certain behavioral traits are "so frequent that they become part of his or her own outlook" whereas "situationally adaptive" behavioral transmission occurs when the actions of peers in a youth's reference group provide rational models concerning how to respond to neighborhood-specific situations.

youth engages in such activities. Borjas (1995) investigates the relationship between "ethnic capital," defined as the mean skill level within one's ethnic group of the generation of one's parents, and educational attainment. Kremer (1997) estimates the effects of parents' and neighbors' educational attainment on the educational attainment of neighborhood youth.

B. *Empirical Methodology*

Our empirical strategy aims to establish whether an individual propensity to engage in certain deviant or social behavior is affected by the prevalence of that behavior among the individual's school peers. We choose schools as the relevant sphere of interaction for several reasons. First, schools provide a setting within which youth are forced to interact with a fixed, well defined (in terms of school, grade, and track) set of peers. Unlike statistical proxies for neighborhoods such as census tracts (O'Regan & Quigley, 1996) or city blocks (Case & Katz, 1991), the geographic and social boundaries of interaction are here precise and unambiguous.

Second, because students interact primarily during school hours, estimated social interaction effects are more likely to reflect the influence of the behavior of peers rather than the influence of peer background factors (for example, neighborhood traits or parental behavior). To be more concrete, if the parents of one's school peers use drugs, this may affect one's propensity to use drugs mainly through the increased probability that one's peers use drugs. In contrast, if the parents of one's neighborhood peers are drug users, this may affect one's propensity to use drugs through direct observation of peer parental behavior or through the greater availability of drugs in the neighborhood. If this line of reasoning is correct, empirical estimates of social interaction effects using schools as the reference group should more likely reflect the influence of peer behavior (or Manski's endogenous effects) than peer background factors (Manski's contextual effects).

Finally, evidence from survey data indicates that the overwhelming majority of youths draw their main peers from schools. Calculations from the parent component of the NELS reveal that 65% of the tenth-graders attended school with their best friend and 94% attended school with at least one of their three closest friends. Calculations from the student component of the survey reveal that 83% of the respondents stated that meeting friends is the main reason why they go to school. Theoretically, friendships are likely to result from a fumbling search process in which heterogeneous people look for "right matches" among their acquaintances. If schools offer a larger pool of potential friends for a tenth-grader than do neighborhoods, students will establish, on average, more durable friendships with schoolmates than with neighbors.

Our empirical specification follows closely that of Case and Katz (1991). We model individual behavior with the simple linear equation

$$Y = c + X\beta + \alpha\bar{Y}_s + \epsilon, \tag{1}$$

where Y is a binary outcome, X is a vector of personal and family characteristics, $\bar{Y}_s$ is the average incidence of Y in school s, and ϵ is a random component independent across individuals. Note that the average background characteristics of students at school s, $\bar{X}_s$, do not directly affect Y in this specification. (Of course, they indirectly effect behavior through peer interactions.)

We replace $\bar{Y}_s$ (the average incidence of Y in the entire reference group) by its sample analog (the average incidence of Y in the available sample of students in school s). Further, we expand the model to include some relevant school characteristic to avoid spurious estimates of peer-group effects stemming from omitted school variables. Accordingly, we estimate the model

$$Y_{is} = c + X_{is}\beta + W_s\phi + \alpha\bar{Y}_{-is} + \epsilon_{is}, \tag{2}$$

where Y_{is} is the probability that student i, in school s, will be involved in Y; X_{is} is a vector of personal and family characteristics; W_s is a vector of school characteristics; $\bar{Y}_{-is}$ is the proportion of students in school s engaged in activity Y after excluding individual i; and ϵ_{is} is a random disturbance.

There are several potential sources of endogeneity bias in the estimation of equation (2). First, although equation (2) hypothesizes that average behavior affects individual behavior, individual behavior also affects the average of the group. As a result, individual error terms will be correlated with $\bar{Y}_{-is}$, and OLS estimates will be biased. Second, if relevant school variables are omitted, the error terms of all youths in the same school will be correlated, and OLS estimates of peer-group effects would be biased. Finally, if families sort across schools according to their willingness to invest in their children's future and this willingness is unobserved, OLS estimates will be biased upward.

Our correction for the first source of bias is straightforward. Under the assumption that contextual effects are non-existent, there should be no direct relationship between individual i's outcomes and the average background characteristics of individual i's peers ($\bar{X}_{-is}$). Under these conditions, $\bar{X}_{-is}$ provides a natural set of instruments for average peer behavior ($\bar{Y}_{-is}$). Arguably, the same set of instruments can be used to correct for the possible omission of relevant school characteristics—at least insofar as omitted school variables are not systematically correlated with the average socioeconomic conditions of the school's student body.[4]

[4] That would be the case, for example, if the omitted variable is the presence of counseling services in the school and such variable is uncor-

Although this test addresses the simultaneity problem, it does not address the issue of endogenous sorting of households across schools. We use differences in household residential mobility to gauge the extent to which peer-group effect estimates are distorted by endogenous school choices. Specifically, we argue that, if endogenous sorting is widespread, estimates of peer-group effects for families that recently moved into a new neighborhood should be larger. Hence, household mobility should provide an indirect way to evaluate the magnitude of this type of bias.

A final methodological point that we must address concerns the fact that the "grouped" nature of our explanatory variable may bias the estimates of the parameter standard errors. Moulton (1990) has shown that, when the true specification of the residual variance-covariance matrix follows a grouped structure, estimates of the standard errors from simple OLS will be biased downwards. Consequently, we estimate all models below using a Huber-White robust estimator in which the residual covariance matrix is clustered by school.

III. Data Description

The National Education Longitudinal Study (NELS) is sponsored by the National Center of Education Statistics and carried out by the Bureau of the Census. The survey began in 1988 with a sample of roughly 1,000 schools and 26,000 eighth-graders. The survey employs a two-stage sampling frame, first choosing a sample of schools and then sampling student within schools. Schools with large minority enrollments and minorities within schools were slightly oversampled. Follow-up surveys with some modifications to the questionnaire and some additions of schools and students occurred in 1990, 1992, and 1994, when the original cohort was in the tenth grade, the twelfth grade, and in the second year after high-school graduation, respectively. The survey collects information from students, parents, teachers, and school principals, and hence contains a myriad of information about personal and family characteristics as well as detailed descriptions of the schools.

We use the first follow-up of tenth-graders to study the determinants of the following self-reported behaviors: drug use, alcohol drinking, cigarette smoking, church attendance, and dropping out.[5] The sample is restricted to students in schools for which the NELS collected at least five observations. We impose this restriction to ensure a minimum number of observations from which to compute average outcomes and average socioeconomic characteristics. The mean sample size per school is 13.3 students with a maximum of 43, a minimum of 5, and a standard deviation of 5.3. The final sample includes 12,300 students and 928 schools.

related with the average socioeconomic characteristics of the school's body.

[5] For twelfth-graders, school codes were not released, and so it is impossible to match students with their classmates. For eighth-graders, no questions were asked on the five variables we study in the paper.

TABLE 1.—VARIABLE DEFINITIONS AND DESCRIPTIVE STATISTICS NELS FIRST FOLLOW-UP, 1990

Variable	Definition	Sample Size	Mean
Drug use	0–1 dummy that equals 1 if student used cocaine or smoked marijuana during the last year.	11,222	0.144
Alcohol drinking	0–1 dummy that equals 1 if student drank alcohol during the last month.	11,230	0.411
Cigarrete smoking	0–1 dummy that equals 1 if student currently smokes more than one cigarrete daily.	12,418	0.175
Church attendance	0–1 dummy that equals 1 if student goes to church at least once a month.	12,422	0.615
Dropping out	0–1 dummy that equals 1 if student dropped out while in 11th or 12th grade.	13,290	0.119

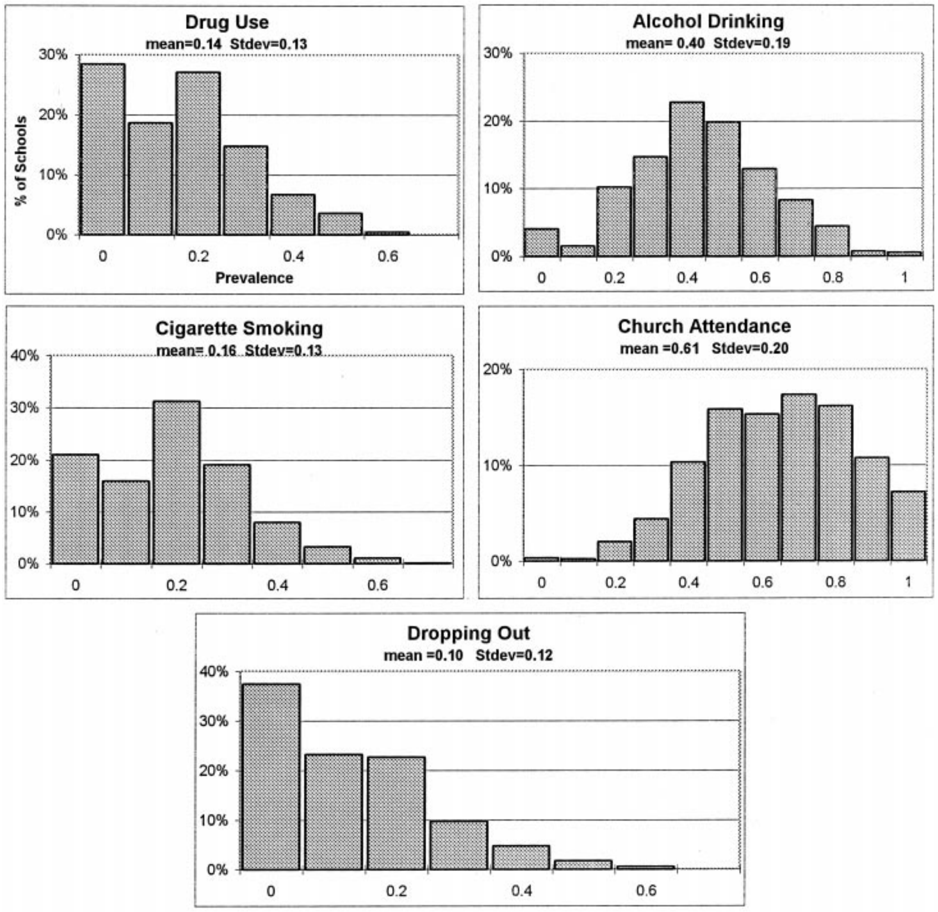
Table 1 presents definitions and summary statistics for the five dependent variables. For these computations, all observations were weighted using the provided sample weights. As shown, alcohol consumption is the most prevalent of the four delinquent behaviors. Church attendance is also quite prevalent yet far from universal.

Figure 1 presents the distributions of prevalence rates across schools for all five activities presented in table 1. The distributions for alcohol use and church attendance are roughly centered around the sample average and are single-peaked. For the remaining three behavioral outcomes (drug use, cigarette smoking, and dropping out), the distributions have high concentration around zero; additionally, for drug use and cigarette smoking, two peaks "bracket" the sample means. For all five behavioral outcomes, there is substantial variation across schools in prevalence rates.

In a recent review article, Haveman and Wolfe (1995) suggest that any studies dealing with juvenile behavior should consider three different sets of variables: behavioral and attitudinal attributes of parents (such as drug and alcohol abuse and religious commitment), behavior and attainments of siblings, and characteristics and qualities of the schools. We use this list as a starting point in choosing our model specification. Table 2 presents the means and definitions of the control variables used in the paper. These variables fall into three broad categories: personal variables, variables describing family background and parental involvement in the youth's life, and variables describing the general characteristics of the youth's school.

Because all youths in the sample are of similar age, we control only for their race and sex. Concerning family background characteristics, we include the following controls: whether the youth resides in a single-parent household, parental educational attainment, whether either of the youth's parents have used drugs any time during the last two years, and a composite socioeconomic status variable based on parental education, occupation, and family income. Fur-

FIGURE 1.—DISTRIBUTIONS OF PREVALENCE RATES ACROSS SCHOOLS



ther, we control for several measures of parental involvement and control, including variables indicating how often parents help their children with their homework, attend school meetings, and whether parents attempt to find out how their children spend their money and where they go at night. Finally, we control for whether the youth has a sibling that dropped out of school in the past.

The final set of controls listed in table 2 also includes proxies for the disciplinary systems of each high school. Specifically, we construct a set of dummy variables indicating whether suspension for first offenses and expulsion for second offenses are administered for drug use, alcohol drinking, and cigarette smoking. Finally, we include two dummy variables indicating whether the school is a Catholic school and whether the school is located outside a metropolitan statistical area (MSA).

IV. Empirical Results

A. *OLS and 2SLS Estimation Results*

Table 3 displays OLS estimates of equation (2) for each of the five behavioral outcomes listed in table 1.[6] The peer-group effect estimates are listed across the top of the table. Before discussing the social-interaction effects, a brief discussion of the performance of the control variables is necessary.

Concerning the two personal background controls, female students are less likely to self-report drinking alcohol and more likely to self-report smoking cigarettes and attending church. Black youths, on the other hand, are less likely to self-report using drugs, drinking alcohol, and smoking cigarettes. The lower abuse rates for blacks are consistent with the findings of Case and Katz (1991).

Parental-involvement variables are substantial and significant and have the expected signs in all equations. Parental drug use has strong positive effects on the probability that a youth uses drugs, drinks, and smokes. As shown, drug use by parents increases the probabilities of drug, alcohol, and tobacco consumption by their children by 19.4%, 13.2%, and 10.2%, respectively.[7] Growing up in a single-parent family raises substantially both the probability of any form of substance abuse and the probability of dropping out of

[6] Weighing each observation by the corresponding sample size of each school yields very similar results. Probit results are also very similar.

[7] These numbers are roughly consistent with the sociology literature (Kandel 1980, pp. 245–57) and somewhat higher than those reported by Case and Katz (1991).

TABLE 2.—DESCRIPTION OF CONTROL VARIABLES NELS FIRST FOLLOW-UP, 1990

Variable	Description	Mean
Female	0–1 dummy variable.	0.510
Black	0–1 dummy variable.	0.110
How often parents help with homework	1–4 dummy variable. 1: never; 2: rarely; 3: sometimes; 4: often.	2.332
How often parents attend school meetings	1–3 dummy variable. 1: never; 2: once or twice; 3: more than twice.	1.666
Parents try to find out how student spend his/her money	1–5 dummy variable. 1: don't know; 2: not at all; 3: just a little; 4: some; 5: a lot.	3.644
Parents try to find out where student goes at night	1–5 dummy variable. 1: don't know; 2: not at all; 3: just a little; 4: some; 5: a lot.	4.210
Parent with drug problems	0–1 dummy variable that equals 1 if at least one of the parents have used drugs during the last two years.	0.130
Single-parent family	0–1 dummy variable that equals 1 if at least one of the parents live outside home.	0.347
At least one of the parents has college degree	0–1 dummy variable.	0.282
Socioeconomic status	Composite based on parent's education level, parent's occupations and family income (see Rock & Pollak, 1995)	−0.051
A sibling dropped out in the past	0–1 dummy variable that equals 1 if at least one sibling left school before graduation (no siblings → 0).	0.143
How often parents go to church with student	1–4 dummy variable. 1: never; 2: rarely; 3: sometimes; 4: frequently.	2.905
Suspension first time caught with:		
Drugs	0–1 dummy variable that equals 1 if out-school suspension after first time possession of drugs.	0.702
Alcohol	0–1 dummy variable that equals 1 if out-school suspension after first time possession of alcohol.	0.786
Cigarettes	0–1 dummy variable that equals 1 if out-school suspension after first time possession of cigarettes.	0.450
Expulsion second time caught with:		
Drugs	0–1 dummy variable that equals 1 if expulsion after second time possession of drugs.	0.773
Alcohol	0–1 dummy variable that equals 1 if expulsion after second time possession of alcohol.	0.647
Cigarettes	0–1 dummy variable that equals 1 if expulsion after second time possession of cigarettes.	0.196
Catholic school	0–1 dummy variable.	0.065
School located outside MSA	0–1 dummy variable.	0.316

high school. Church attendance by teenagers hinges heavily on whether their parents go to church. And whether a sibling drops out of school is a strong predictor of dropping out by other family members.

School policies do not affect drug, alcohol, and tobacco consumption. The validity of this result, however, could be questioned on the basis that there is not enough variation in the explanatory variables to identify the corresponding coefficients. In fact, if all schools suspend first-violators and expel repeated violators, it will be impossible to identify the effects of school policies. This criticism does not apply here, however, because schools do differ widely in their punishment strategies. (See table 2.) Catholic schools have a substantial negative effect on dropping out and drug use, which is a result consistent with the previous findings of Evans and Schwab (1995). Curiously, there seems to be a positive connection between attending Catholic schools and drinking alcohol.

Turning to the peer-group effects, all point estimates are large, positive, and highly significant. The largest peer effect is found for drug use, and the smallest, yet substantial, estimate is found for cigarette smoking. The estimates imply, for example, that moving a teenager from a school where none of his classmates use drugs to one where half use drugs would increase the probability that she will use drugs by approximately thirteen percentage points. Similar experiments would yield increases in the corresponding probabilities of nine percentage points for alcohol consumption, eight percentage points for cigarette smoking, eleven percentage points for church attendance, and eight percentage points for dropping out of school. Moreover, peer-group effects appear to be large in comparison to the effects of some of the family background variables. Thus, moving a student from a school at which the dropout rate is 80% to a school with a dropout rate of 50% would completely offset the higher risk of dropping out associated with being in a single-parent family. On the whole, the OLS results of table 3 strongly suggest that peer influences play a significant role in a variety of youth behaviors ranging from drug consumption to church attendance.

As noted earlier, the presence of peer effects opens the door to "social multipliers" in that exogenous changes in socioeconomic conditions may have higher effects than those implied by the estimated coefficients. Consider the following hypothetical example. Two school districts are

TABLE 3.—OLS REGRESSIONS RELATING FAMILY AND PEER INFLUENCES TO SOCIOECONOMIC OUTCOMES. NELS FIRST FOLLOW-UP, 1990

Explanatory Variable	(1) Mean	(2) Drug Use	(3) Alcohol Drinking	(4) Cigarette Smoking	(5) Church Attendance	(6) Dropping Out
Peer-group average		0.254	0.186	0.158	0.218	0.164
		(7.17)	(5.40)	(4.40)	(9.12)	(5.06)
Female	0.510	−0.002	−0.039	0.042	0.032	0.006
		(−0.24)	(−3.37)	(4.90)	(3.50)	(1.07)
Black	0.110	−0.056	−0.166	−0.142	0.000	0.016
		(−9.10)	(−9.10)	(−13.22)	(0.02)	(1.28)
How often parents help with homework	2.332	−0.024	−0.036	−0.008	0.024	0.001
		(−4.69)	(−5.48)	(−1.72)	(4.70)	(0.42)
How often parents attend school meetings	1.666	−0.019	−0.031	−0.032	0.035	−0.007
		(−3.77)	(−3.73)	(−5.66)	(5.52)	(−1.76)
Parents try to find out how student spend his/her money	3.644	−0.017	−0.024	−0.017	0.011	−0.001
		(−4.06)	(−3.99)	(−3.86)	(2.40)	(−0.25)
Parents try to find out where student goes at night	4.210	−0.006	0.001	−0.016	0.009	−0.013
		(−1.50)	(0.15)	(−3.61)	(1.76)	(−3.95)
Parent with drug problems	0.130	0.195	0.132	0.102	−0.033	0.001
		(12.31)	(7.25)	(6.76)	(−2.34)	(0.05)
Single-parent family	0.347	0.048	0.066	0.048	−0.022	0.031
		(5.11)	(5.21)	(5.17)	(−1.99)	(4.31)
At least one of the parents has a college degree	0.282	−0.027	−0.065	−0.025	0.016	−0.001
		(−2.40)	(−3.62)	(−2.02)	(1.17)	(−0.16)
Socioeconomic status	−0.051	0.013	0.052	0.003	0.011	−0.034
		(1.71)	(4.78)	(0.37)	(1.17)	(−5.42)
A sibling dropped out in the past	0.143	0.022	0.022	0.061	−0.067	0.082
		(1.70)	(1.30)	(4.45)	(−4.38)	(6.63)
How often parents go to church with student	2.905				0.192	
					(42.55)	
Suspension first time caught with:						
Drugs	0.702	−0.002				
		(−0.27)				
Alcohol	0.786		−0.015			
			(−1.13)			
Cigarettes	0.450			0.001		
				(0.15)		
Expulsion second time caught with:						
Drugs	0.773	0.009				
		(1.01)				
Alcohol	0.647		−0.008			
			(−0.75)			
Cigarettes	0.196			0.020		
				(1.88)		
Catholic school	0.065	−0.023	0.065	0.045	0.067	−0.024
		(−2.23)	(−2.86)	(2.72)	(4.22)	(−3.9)
School located outside MSA	0.316	−0.016	0.026	0.014	0.019	−0.001
		(−2.07)	(2.13)	(1.58)	(2.08)	(−0.11)
Student uses drugs	0.144					0.108
						(9.13)
Student drinks alcohol	0.411					0.017
						(2.88)
N		7,530	7,504	8,224	8,556	8,785
R^2		0.0837	0.0499	0.0564	0.2892	0.0833

t-statistics are in parentheses. Huber-White consistent standard errors were used for calculating the *t*-statistics. Observation with missing values were dropped from the sample.

identical in all respects but one: in the first district, the fraction of students whose parents have had drug problems equals the national average of 13%, whereas in the second the corresponding fraction is 40%. How does this difference affect the probability of, say, drug use for a typical tenth-grader? A simple calculation shows that for a class size of 25 students and under the assumption that all other independent variables are equal to the national averages in both districts, an average tenth-grader in the second district will be 7.1 percentage points more likely to use drugs.[8] We can, in turn, decompose this percentage into two parts: a direct effect due to the higher probability

[8] For the first school district, the probability of drug use of a typical student was computed as $Y = (I - \hat{\gamma}A)^{-1}(\hat{c} + X\hat{\beta} + W\hat{\phi})$, where the hatted variables are the estimated coefficients, X and W are matrices containing the covariates evaluated at the sample mean characteristics, and A is a matrix with zeros in the main diagonal and 1/24, $1/(n - 1)$, elsewhere. For the second district, we just change the corresponding column of X to incorporate the higher fraction of parents with drug problems.

TABLE 4.—PEER INFLUENCES AND FAMILY STRUCTURE

	(1) Drug Use	(2) Alcohol Drinking	(3) Cigarette Smoking	(4) Church Attendance	(5) Dropping Out
Peer-Group Average	0.224	0.197	0.145	0.211	0.106
t-statistics	(6.01)	(5.02)	(4.04)	(7.95)	(3.504)
Peer-Group Single-Parent Family	0.094	−0.033	0.039	0.025	0.158
t-statistics	(1.32)	(−0.55)	−0.68	(0.55)	(2.53)
Sample size	7,530	7,504	8,224	8,556	8,775
R^2	0.084	0.050	0.056	0.289	0.085

Huber-White consistent standard errors were used for calculating the *t*-statistics.

of drug use by parents and an indirect effect due to the corresponding increase in the proportion of peers using drugs. For this example, the direct effect amounts to 5.3 points and the indirect effect to 1.8 points, which entails a multiplier of 1.34.

An interesting extension of the results presented in table 3 is to test for differential peer effects for students from different family backgrounds. Some evidence indicates that children from single-parent homes are more susceptible to peer influences (Steinberg, 1987). The idea is that peers gain prominence when family ties are weak and adult role models non-existent. We test this idea by adding an interaction term between the peer-group average and our dummy for single-parent families to the previous specification. The results of this exercise are shown in table 4. To conserve space, we present only the coefficient estimates for the peer-group variable and the interaction term. Only for dropping out of high school do we find some clear evidence that peer-group effects are indeed more important among youth coming from single-parent families. Whereas the evidence is ambiguous for drug use, for the other variables peer-group effects are not larger for single-parent households.

Although the previous results indicate strong family and peer influences, the sources of bias discussed above must still be addressed. Recall that the OLS estimates in table 3 are likely to be biased due to both the bidirectionality of outcomes and the potential omission of some relevant school attributes. Table 5 presents the two-stage least-squares (2SLS) estimates using a set of average background characteristics as instruments.[9] For the most part, the point estimates for the personal, family, and school variables are very similar to the OLS estimates and will not be discussed further. The 2SLS peer-group effects estimates are similar to the OLS estimates for cigarette smoking, dropping out, and drug use, and higher for church attendance and, especially, for alcohol drinking. On the whole, these results seem to show that simultaneity problems, if anything, bias the estimates of peer influences downward.

The bottom panel of table 5 presents tests for the collective significance of the instruments in the first-stage regressions (F-statistics) and tests for the overidentifying restrictions (*chi*-square statistics). We comfortably reject the null hypothesis of the absence of collective significance of the instruments. We also fail to reject the overidentifying restrictions. These results lend some additional credence to our choice of instruments.

B. *Differential Mobility and the Endogeneity of Peer Groups*

Any empirical analysis of peer influences must recognize that youths do choose their peers; yet it also must recognize that, once the "sphere of interaction" is defined, youth choices are greatly constrained. We implicitly assume throughout this paper that teenagers enrolled in a particular school do not have control over mean peer influences. Families, however, have control over their children's social group in general and their schools in particular. This may give rise to endogeneity problems: parents that place great emphasis on the education of their children (and the peer influences to which they are exposed) may devote substantial resources—in the form of both time and outright expenditures—towards their children's education and development. To the extent that variation in such parental conscientiousness is unobservable yet correlated with average peer-group behavior, estimated peer effects will be biased upward.[10]

Past research has addressed this endogeneity bias with several alternative methodological designs. Rosenbaum (1993) uses the Chicago Gatreaux desegregation program in which poor families were relocated from inner-city public housing to private rental units located either within the city of Chicago or in the surrounding suburban communities in a somewhat random fashion. Aaronson (1995) exploits intersibling variation in neighborhood surroundings for families that change residences. Both Rosenbaum and Aaronson find evidence of substantial neighborhood effects. Evans et al. (1992), using metropolitan-wide measures of socioeconomic background characteristics as instruments for mean school socioeconomic characteristics, find that accounting for endogenous sorting eliminates the substantial peer-effect esti-

[9] The instruments used in the preceding analysis are the average across i's classmates of the two parental-involvement variables and the two parental control variables, the proportion of i's classmates whose parents have had drug problems and have some college education, and the proportion of i's classmates who live in single-parent families.

[10] Arguably, endogeneity problems are less serious in this paper than in previous research because we were able to control for parental involvement and control variables that have been customarily omitted in the past.

TABLE 5.—TSLS REGRESSIONS RELATING FAMILY AND PEER INFLUENCES TO SOCIOECONOMIC OUTCOMES. NELS FIRST FOLLOW-UP, 1990

Explanatory Variable	(1) Mean	(2) Drug Use	(3) Alcohol Drinking	(4) Cigarette Smoking	(5) Church Attendance	(6) Dropping Out
Peer-group average		0.322	0.354	0.156	0.287	0.165
		(3.98)	(2.73)	(1.27)	(9.42)	(2.30)
Female	0.510	−0.002	−0.037	0.042	0.033	0.007
		(−0.22)	(−3.18)	(4.89)	(3.58)	(1.20)
Black	0.110	−0.054	−0.153	−0.142	0.001	0.015
		(−4.41)	(−7.56)	(−11.23)	(0.05)	(1.21)
How often parents help with homework	2.332	−0.024	−0.035	−0.008	0.025	0.001
		(−4.74)	(−5.24)	(−1.71)	(4.71)	(0.38)
How often parents attend school meetings	1.666	−0.019	−0.031	−0.032	0.035	−0.007
		(−3.75)	(−3.89)	(−5.59)	(5.55)	(−1.79)
Parents try to find out how student spend his/her money	3.644	−0.017	−0.023	−0.017	0.011	0.000
		(−4.07)	(−3.81)	(−3.84)	(2.31)	(−0.14)
Parents try to find out where student goes at night	4.210	−0.006	0.001	−0.017	0.009	−0.014
		(−1.42)	(0.13)	(−3.62)	(1.82)	(−4.47)
Parent with drug problems	0.130	0.194	0.130	0.103	−0.033	0.001
		(12.27)	(7.02)	(6.79)	(−2.33)	(0.14)
Single-parent family	0.347	0.047	0.066	0.047	−0.022	0.031
		(5.00)	(5.22)	(5.05)	(−2.04)	(4.25)
At least one of the parents has a college degree	0.282	−0.025	−0.060	−0.025	0.016	−0.001
		(−2.26)	(−3.29)	(−2.00)	(1.17)	(−0.07)
Socioeconomic status	−0.051	0.012	0.048	0.003	0.011	−0.034
		(1.64)	(4.33)	(0.32)	(1.24)	(−5.22)
A sibling dropped out in the past	0.143	0.021	0.022	0.060	−0.067	0.083
		(1.61)	(1.29)	(4.37)	(−4.33)	(6.65)
How often parents go to church with student	2.905				0.190	
					(40.96)	
Suspension first time caught with:						
Drugs	0.702	−0.002				
		(−0.28)				
Alcohol	0.786		−0.012			
			(−1.08)			
Cigarettes	0.450			0.001		
				(0.17)		
Expulsion second time caught with:						
Drugs	0.773	0.008				
		(1.02)				
Alcohol	0.647		−0.005			
			(−0.51)			
Cigarettes	0.196			0.020		
				(1.83)		
Catholic school	0.065	−0.019	0.058	0.044	0.052	−0.025
		(−1.89)	(2.90)	(2.63)	(3.32)	(−3.45)
School located outside MSA	0.316	−0.014	0.024	0.013	0.015	0.000
		(−1.68)	(2.19)	(1.46)	(1.74)	(−0.08)
Student uses drugs	0.144					0.108
						(9.1)
Student drinks alcohol at least monthly	0.411					0.017
						(2.92)
N		7,515	7,491	8,203	8,536	8,749
R_2		0.083	0.047	0.056	0.288	0.085
F-statistic		143.98	64.36	73.26	1417.06	193.81
chi-square		0.752	2.247	2.461	2.561	0.875
(% upper tail)		0.980	0.690	0.873	0.634	0.972

t-statistics are in parentheses. Huber-White consistent standard errors were used for calculating the *t*-statistics. The *F*-Statistic tests the collective significance of the excluded instruments in the first-stage regressions. The *chi*-square statistics test for the overidentifying restrictions.

mates from single-equation models. Recent work by Rivkin (1997), however, suggests that using metropolitan-wide aggregate data as instruments exacerbates rather than reduces specification error.

In this paper, we follow an estimation strategy suggested by Glaeser (1996). We break the sample into two groups: youths whose families moved during the last two years and all other youths.[11] Assuming that the correlation between unobserved parental heterogeneity and average peer-group behavior is higher for those who have recently moved, we would expect stronger peer-group effect estimates for "mov-

[11] It should be noted that, for most of the students, these two years comprise the crucial transition from junior high to high school.

TABLE 6.—COMPARISON OF PEER INFLUENCES BETWEEN LONG-TERM AND SHORT-TERM RESIDENTS

	(1) Drug Use	(2) Alcohol Drinking	(3) Cigarette Smoking	(4) Church Attendance	(5) Dropping Out
Family moved	0.408	0.228	0.114	0.224	0.157
t-statistics	4.47	3.00	1.40	3.89	2.19
Sample size	1034	1039	1129	1153	1247
Family didn't move	0.233	0.179	0.167	0.220	0.165
t-statistics	6.81	4.90	4.40	8.57	4.70
Sample size	6473	6442	7068	7376	7504
F-stat	5.59	0.48	0.53	<0.01	0.05
p-value	0.02	0.49	0.47	0.95	0.90

Huber-White consistent standard errors were used for calculating the *t*-statistics.

ers" than for "stayers." This assumption can be justified under two premises: that there are unanticipated changes in neighborhood (and school) characteristics, and that there are nonnegligible relocation costs.

Table 6 presents the estimates of peer influences for the subsamples of movers and stayers. As shown, for both drug use and alcohol drinking, the estimates are indeed greater for movers. The estimates for church attendance and dropping out are quite similar for both subsamples, and the estimate for cigarette smoking is actually higher for stayers. To test whether the estimates across the two subsamples are statistically equivalent, we reestimate the model constraining the peer effects coefficients for movers to the corresponding values for stayers. The bottom row of table 6 presents the results of a test of the restrictions. As shown, we obtain significantly higher estimates for movers only for drug use.

All in all, the results of this exercise yield mixed evidence concerning the endogeneity of peer groups. Although for drug use we find peer-group effects estimates to be statistically distinguishable between movers and stayers in a manner consistent with endogeneity bias, we find no evidence of bias for the other four outcomes analyzed.[12]

C. Omitted School Characteristics

As noted earlier, omitting relevant school characteristics may create correlation between our prevalence measure of peer behavior and the random disturbances (ϵ_{is}) for all students. This may lead us to attribute common behavior among youth to peer influences whereas, in truth, students behave alike because they face a common (unobserved) institutional environment.

Although the instrumental-variable strategy implemented earlier may partially eliminate this problem, some serious doubts persist as to whether the instruments are uncorrelated with the omitted school variables. An alternative way to address this problem is to control for as many school characteristics as possible. This option is especially attractive in this case because the NELS contains more than 500 different variables describing many school characteristics. Thus, we expand the specification of equation (2) to include the following school variables: whether students receive information on alcohol and drug abuse, whether students are prohibited from leaving school grounds during school hours, whether religious organizations are available in school, whether the school has a dropout prevention program, whether the school uses parent workshops, whether the school promotes parental involvement and support, whether parents are notified of students absences, whether visitors at the school sign in at the school main office, whether the school is public, the number of full-time science teachers, and the number of days in the school year.

Table 7 compares the peer-group effect estimates between the old and the expanded specifications. As shown, controlling for myriad observable school characteristics does not affect the peer-group effects in any substantial way. Although this does not dispel altogether the possibility that some unobserved school attributes may play a role, it provides (along with the 2SLS estimates) compelling evi-

[12] This "mixed" result may reflect that, even if school choice is endogenous, peer "quality" is not necessarily so because it will be virtually impossible for the parents to control their children's social group along several different dimensions (drug use, cigarette smoking, academic performance, religious commitment, and so forth).

TABLE 7.—ESTIMATES OF PEER INFLUENCES WITH COMPREHENSIVE SCHOOL CHARACTERISTICS

	(1) Drug Use	(2) Alcohol Drinking	(3) Cigarette Smoking	(4) Church Attendance	(5) Dropping Out
New Controls					
OLS	0.240	0.148	0.128	0.213	0.175
t-statistic	(6.01)	(3.65)	(3.05)	(7.32)	(3.45)
Previous Results					
OLS	0.254	0.186	0.158	0.218	0.164
t-statistic	(7.17)	(5.40)	(4.40)	(9.12)	(5.06)

Huber-White consistent standard errors were used for calculating the *t*-statistics.

TABLE 8.—ALTERNATIVE MEASURES OF PEER INFLUENCES GLAESER ET AL. (1996) METHODOLOGY

Variable	(1) $y(1 - y)$	(2) Variance	(3) Index 1	(4) Index 2
Drug use	0.124	0.252	2.02	1.81
Alcohol drinking	0.244	0.470	1.93	1.72
Cigarrete smoking	0.146	0.252	1.73	1.61
Church attendance	0.237	0.552	2.33	1.33
Dropping out	0.095	0.183	1.92	1.48

Sample weights were used in all the calculations. The variance of *ws* was defined as

$$\text{Variance} = \frac{\sum_s (y_j - \bar{y})^2 N_j}{\#\text{ of schools}}$$

dence that the peer-group effect estimates are not mainly driven by unobserved school attributes.

V. A Nonparametric Test of Peer-Group Effects

In the previous section, we present parametric estimates of peer-group effects at the school level. Here, we step back from the empirical model defined in equation (1) and (2) and provide a simple nonparametric test of social-interaction effects. As shown in figure 1, there are sizable differences in prevalence rates across schools for the five behavioral outcomes under scrutiny. This variation stems from differences in the socioeconomic backgrounds of students, differences in school attributes, random variation, and social interactions. A simple test of social-interaction effects is to ask whether the observed variation in prevalence rates across schools is consistent with the absence of social interactions or, in other words, whether this variation reveals the presence of positive covariances among individual's outcomes. (See Glaeser et al. (1996) for a detailed description of this approach.)

We begin the analysis by defining the variable, $w_s = (y_s - \bar{y})\sqrt{N_s}$, where y_s is the proportion of students at school s involved in, say, drug use, $\bar{y}$ is the average of y across all individuals in the sample, and N_s is the sample size. Next, we assume (temporarily) that there are neither social interactions nor relevant socioeconomic differences across schools. Under these assumptions, an obvious estimate of the variance of w_s across schools is $\bar{y}(1 - \bar{y})$. If there are social interactions, the covariances across individuals are not zero and the variance of w_s will be greater than $\bar{y}(1 - \bar{y})$. Thus, a comparison between $\bar{y}(1 - \bar{y})$ and the actual variance of w_s should provide some indication of the degree of social interactions. Obviously, a more accurate comparison would first adjust for school heterogeneity along two dimensions: school attributes and socioeconomic characteristics of the school's student body.

Table 8 presents two indices of social interactions based on the observed dispersion across schools of the five variables under consideration. The first column gives the value of $\bar{y}(1 - \bar{y})$ and the second the actual variance. (The sample comprises 928 schools.) The third column gives the ratio of column 2 and 1, which, as mentioned before, can be interpreted as an index of social interactions.[13] This index can be used to assess the extent to which social interactions vary across social outcomes. Interestingly, the results are roughly consistent with our previous estimates in that we find high peer influences for drug use and church attendance, and relatively lower peer influences for cigarette smoking and dropping out. (See table 3.)

The last column of table 8 presents a second index of social interactions. Here we allow for the presence of relevant differences across schools. To do this, we orthogonalized w_s with respect to both school attributes and average socioeconomic characteristics of the school's student body. (See Glaeser et al. (1996, pp. 526–530) for a detailed explanation.) The results are qualitatively the same as before with one exception: the implied level of interactions is relatively lower for church attendance this time around.

The values of the variance index—although consistent with the presence of social interactions—are much lower than a similar set of values reported by Glaeser et al. (1996) in their study of crime and social interactions at the city level. This result is a bit puzzling, because we shouldn't expect large differences in the degree of social interactions between criminal activities and the activities studied here. If we use the total number of students in the school instead of the observed samples when computing the indices, we get higher values but still much lower than the crime indices. We believe that this discrepancy casts serious doubts on the structural interpretation of the variance indices put forward by Glaeser et al. (1996, p. 537).

To sum up, there appears to be, for all five variables under analysis, greater variability across schools than would be expected in the absence of social interactions. As long as one is willing to accept that unobservables do not play a crucial role, this can be interpreted as evidence in favor of peer-group effects. Needless to say, the latter assumption is crucial and may well prompt the skeptical reader to dismiss the evidence. Durlauf (1997) has made the point that, "one's prior beliefs, if strongly enough predisposed against the importance of group effects, may be unaffected by the available evidence" (p. 14).

VI. Conclusions

Spatially mediated interactions are an important part of modern economics from growth theory to labor economics. As important as the recognition that space matters is the recognition that the relevant sphere of interaction varies with the problem at hand. Thus, cities are, arguably, the appropriate units to study the transfer of knowledge among productive units (Lucas, 1988). Neighborhoods, for their part, have been shown to play a prominent role in the accumulation of human capital (Borjas, 1995; Kremer,

[13] A value of 1.14 will be sufficient to reject the hypothesis that the observed variance equals $\bar{y}(1 - \bar{y})$ at the 1% level. (This test is based on a *chi*-square distribution with 928 degrees of freedom.)

1997). Here, we have attempted to show that schools constitute the relevant spheres of interaction to study the accumulation of social capital among teenagers.

Lucas (1988) has argued that human capital should be understood "as a force, not directly observable, that we postulate in order to account in a unified way for certain things we can observe" (p. 35). Lucas has also pointed out that the large body of empirical research on human capital (now spanning more than thirty years) is what enables us to "see" human capital, so to speak. Similarly, social capital can be thought of as a useful abstraction that allows us to explain certain phenomena in a unified way. But if we are to see social capital (that is, if we are to have something more concrete than a handy metaphor), we will need a larger body of empirical research than what is available today.

Optimistically, this paper has contributed to a better visualization of the by now diffuse picture of social capital. We show that social interactions play a prominent role on drug use, alcohol drinking, cigarette smoking, church attendance, and dropping out. Undoubtedly, the issues regarding the accumulation and depreciation of social capital are sure to remain a fertile ground for future research.

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