SOCIAL MULTIPLIERS IN SEXUAL INITIATION DECISIONS AMONG U.S. HIGH SCHOOL STUDENTS*

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In this article, I use a national sample of high school students to test for several types of social influences on the decision to have sexual intercourse. I find evidence of endogenous social interactions (social multipliers), where the propensity of an individual choosing to have sex varies with the average behavior in his or her school. Additionally, the magnitude of the social multipliers and several other interesting risk factors differ by gender and by race. These findings might help explain the large variation in sexual initiation across schools in the United States. These results also add to the debate over school vouchers and ability grouping because social multipliers imply changes in school-wide rates of sexual behavior with moderate changes in school-body composition. In this way, school vouchers and ability grouping might exacerbate the situation of high rates of teenage pregnancy and out-of-wedlock births in some communities. To show the potential benefits and costs of public policies that cause students to change schools, I present the results of several simulation exercises that predict the school-level changes in rates of sexual initiation following changes in school composition.

In recent years, a large literature has attempted to credibly determine the importance of social effects in individual decision-making (see Durlauf 2004 for a comprehensive review). Adolescent decision-making and risky behaviors have been of particular interest (Duncan et al. 2005; Evans, Oates, and Schwab 1992; Gaviria and Raphael 2001). One reason for this interest is evidence that adolescents are more prone than children or adults to be driven by social concerns when making decisions (O'Donoghue and Rabin 2001). For example, Bryk and Thum (1989) found the normative climate of a high school to be important in the decision to drop out of high school. In part, the importance of social concerns for adolescents is due to the fact that children spend less time with adults and more time with friends as they age (Halpern-Felsher et al. 1997). There are also varying opinions of how sophisticated adolescents are as decision-makers. For example, Walker (2001) found that adolescents are good at predicting future events when the event is salient to them (e.g., teen pregnancy and parenthood). Wolfe, Wilson, and Haveman (2001) presented evidence that teenagers respond to the perceived costs associated with nonmarital birth by reducing risky behaviors. Manski (1993a) was more skeptical of the abilities of adolescents to make decisions. This skepticism has been validated in some experimental settings, where adolescents typically are shown to be more inclined than adults toward risky behavior and peer influence plays an important role in explaining risky behavior during adolescence (Gardner and Steinberg 2005).

The United States has among the highest rate of teenage sexual activity and the highest rate of teenage childbearing among industrialized countries (see Teitler 2002 for current trends). In the case of teenage sexual initiation, it is unclear how important social influences are on the decisions of adolescents, but there is some evidence. For example, Teitler

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and Weiss (2000) used self-reported measures of norms for sexual behavior and found them to be important predictors of sexual initiation. The presence of such social effects might help explain the large variation in this behavior across schools. For educators, parents, and policymakers, gaining a better understanding of the importance of the different types of influences on this behavior is crucial because of the many links between teenage sexual activity and other outcomes, including the risk of contracting sexually transmitted diseases. Additionally, teenage pregnancy has been linked to lower human capital accumulation and subsequent wages (Klepinger, Lundberg, and Plotnick 1999) and to various other outcomes. Early fertility also has intergenerational consequences. Children of teenage mothers are less likely to receive good prenatal care, are relatively disadvantaged, and are more likely to repeat the behavior (Trussell 1988).

There are several theories of how social influences affect adolescent sexual behavior. Information sharing among students may change the perceived costs of having sex. A social norm may develop so that in order to fit in with one's peers, a student might feel compelled to have sex. Regardless of the mechanism, the presence of social effects has been offered as a potential justification for several kinds of policy interventions. For instance, some types of social effects imply that interventions on a subset of individuals will have a "spillover effect" on other individuals. In contrast, Manski (1993b) pointed out that many nonsocial theories can explain the observation that individuals in the same group have similar outcomes. In these cases, public policies would not be expected to have the same kind of spillovers on the untreated.

While many theories link social environments to individual behaviors, in practice there are several problems in empirically establishing credible evidence of social effects. One substantial econometric problem to overcome in examining social influences is disentangling the different types of social effects (Manski 1993b). This is particularly important in proposing public policies because the effects of policies depend on the types of social effects present in the environment under study. A second econometric problem is the potential endogeneity of peer influences due to residential location choices made by parents. There are several ways of mitigating the effects of this problem when attempting to empirically establish the importance of the social environment of adolescent sexual initiation. This will be discussed in the next section.

This article adds to the literature on the importance of social effects of peers in adolescent decision-making as well as the potential unintended consequences of public policies that change the composition of schools. Using the nationally representative National Education Longitudinal Study (NELS) data set of high school students, I test for peer group influences in the probability of reported sexual initiation by the tenth grade. The results show evidence of different types of social effects on this outcome, and these effects differ by race and gender. Taken at face value, the social effects are important in magnitude, implying that increasing the proportion of sexually active peers by 10 percentage points increases the probability that an individual chooses to initiate sexual activity by 3 percentage points. This effect is almost half the size of growing up in a single-parent family or suffering a disruptive event (e.g., parental divorce) in the prior two years. Finally, I discuss several types of policy interventions and use simulation exercises to predict their effects on the sexual behavior of students. These exercises show that public policies that change the composition of schools (e.g., school voucher programs) could have unintended consequences on the rates of sexual initiation in schools.

LITERATURE REVIEW

Two of the primary econometric difficulties in estimating social effects are disentangling the types of social effects and the potential endogeneity of the peer group due to parental residential decisions. Going back to work by Charles Manski (1993b, 2000), there is a distinction among the following types of social effects: *endogenous interactions*, wherein

the propensity of an agent to behave in some way varies with the behavior of the group; *contextual effects*, wherein the propensity of an agent to behave in some way varies with the exogenous characteristics of the group members; and *correlated effects*, wherein agents in the same group tend to behave similarly because they have similar individual characteristics or face similar institutional environments.

As Manski pointed out, distinguishing among these effects is important for several reasons; among them is the implication for policy interventions. For example, endogenous interactions can imply social multiplier effects because an intervention on one individual is predicted to affect other individuals in the same reference group. In contrast, contextual changes do not imply the same multiplied responses to an intervention. Manski also directed attention to the problems associated with individuals in a common environment experiencing common unobserved attributes (correlated effects), which are nonsocial and need to be taken into account when examining true social effects. With so many problems of inference in these types of models, it may seem improbable to be able to attain defensible estimates to be used for relevant policy questions. In fact, Manski showed that in many cases, it is impossible to separately identify the three important kinds of effects on behavior without very strict and implausible assumptions.

The first econometric difficulty with examining social effects is that an individual's peer group is likely endogenous. For example, parents choose their child's school and thus choose their child's peer group. The problem of the endogeneity of peer groups has been dealt with in the literature in several ways, depending on the type of data used in the analysis. Various researchers have used random assignment, fixed effects, two-staged least squares, and comparisons across residential mobility in order to produce unbiased estimates. Rosenbaum (1993) used a random assignment treatment (Gatreaux desegregation program) in which poor families were relocated from public housing to private housing in a different neighborhood, and found substantial neighborhood effects (see also Sacerdote 2001). Aaronson (1998) used a data set with sibling characteristics to difference out family fixed effects and still found prevalent neighborhood effects. Evans et al. (1992) explicitly modeled the sorting process, used a two-staged estimation approach, and found no social effects on teenage pregnancy or dropping out of high school.

In this article, I use residential mobility to examine the extent of the endogeneity bias. Gaviria and Raphael (2001), in an analysis of risky behaviors using the NELS, argued that the bias of estimates should be less severe for long-term residents because these residents took into account past, rather than present, school quality and peer group composition when they made residential and school decisions. To the extent that schools change with time and that endogenous sorting across schools is pervasive, peer-effect estimates should be higher for mobile students than for immobile residents. Estimating separate equations for immobile and mobile residents and testing for differential effects provides a simple test of endogeneity of school choices. In my own view, the direction of the bias of examining mobile versus immobile residents is ambiguous, although comparing the two groups can give a sense of the magnitude of the endogeneity bias.

Disentangling the contextual and endogenous social effects, however, has been the subject of less work in the literature. In most cases, researchers do not mention the need to distinguish between the two types of effects.[1] Other researchers make restrictive assumptions in order to uncover the endogenous social effect parameters. For example, Gaviria and Raphael assumed that no contextual (school-level) effects are present. They defended this assumption by arguing that students are less exposed to the family background characteristics of other students than to the family background characteristics of their neighbors. In this article, I relax the restrictive assumption that no contextual effects are present and

1. This applies to nearly all work prior to 1993, when Manski (1993b) made a seminal contribution to the distinction as the "reflection problem."

include as instrumental variables only those contextual variables that are not rejected by an overidentification test. While not perfect, this methodological approach should produce more reliable results than did previous work.

METHODOLOGY

The empirical framework that I use in this article attempts to disentangle the different types of social interactions and has been used by several researchers in a modified form (e.g., Gaviria and Raphael 2001). Of particular interest is whether a student's propensity to have sex is affected by the choices of others in his or her school. The choice of school as the relevant sphere of interaction follows much recent work (Gaviria and Raphael 2001; McEwan 2003; Soetevent and Kooreman 2005) but is in contrast to previous work on peer and neighborhood effects.[2] Recent work that is particularly relevant for supporting my focus on the school as the sphere of interactions is that by Teitler and Weiss (2000), who compared school and neighborhood effects on sexual behavior for students in Philadelphia and found that neighborhood effects are virtually eliminated when school effects are introduced.

The empirical specification used in this article was introduced by Case and Katz (1991), and many researchers have followed the so-called linear in means model:

$$Y = c + \mathbf{X}\beta + \overline{\mathbf{X}}\delta + \alpha\overline{Y} + \varepsilon, \tag{1}$$

where Y is the outcome (sexual initiation), $\mathbf{X}$ is a vector of individual and family characteristics, $\overline{\mathbf{X}}$ is a vector of average peer characteristics, $\overline{Y}$ is the average incidence of Y in the school, and ε is a random component independent across individuals. In the language of Manski (1993a, 1993b, 2000), $\overline{\mathbf{X}}$ are contextual variables, and $\overline{Y}$ is an endogenous variable. To estimate the model, $\overline{\mathbf{X}}$ and $\overline{Y}$ are replaced with their sample analogs (the average incidence of sexual initiation of students in each school). Following Gaviria and Raphael (2001), the model is expanded to include school characteristics to avoid spurious estimates of social effects from the correlations from omitted school variables.

$$Y_{is} = c + \mathbf{X}_{is}\beta + \overline{\mathbf{X}_{-is}}\delta + \mathbf{W}_s\phi + \alpha\overline{Y_{-is}} + \varepsilon_{is}. \tag{2}$$

Here, Y_{is} is the probability that student i in school s will report having had sexual intercourse by the tenth grade; $\mathbf{X}_{is}$ is a vector of family and individual characteristics; $\overline{\mathbf{X}_{-is}}$ is a vector of average characteristics of students in school s excluding individual i; $\mathbf{W}_s$ is a vector of school characteristics; and $\overline{Y_{-is}}$ is the rate of school-level sexual initiation excluding individual i. As Manski (1993a, 1993b, 2000) pointed out, the types of social effects estimated from Eq. (2) imply different policy interventions. If δ is estimated to be nonzero, this is consistent with role model effects from the environment. These types of effects do not, however, indicate that there will be collective gains from changing the composition of the student body through busing or other reallocation of students. While reorganizing students will have *distributional* effects across schools, the overall incidence of sexual initiation will remain unchanged. In contrast, if α is estimated to be greater than zero, this is consistent with a positive social multiplier effect. This implies that policies that affect a subset of students will have indirect effects on other students in the same school. Furthermore, reallocating students across schools can lead to an overall decline of sexual initiation. This type of social multiplier might explain some of the current large variation across schools in sexual initiation, as well as pockets of teenage pregnancy and out-of-wedlock births in some communities, and is the focus of this article.

2. Harding (2003) used census tracts, Evans et al. (1992) used metropolitan-level data, and Case and Katz (1991) used city block–level data.

There are several econometric issues in Eq. (2) that must be addressed. First, there is a simultaneity issue because the individual outcome affects average behavior ($\overline{Y}$), and average behavior affects the individual outcome—this is what Manski (1993b) referred to as the *reflection problem.* Second, to the extent that families choose schools, the average behavior variable is a choice of parents rather than an exogenous variable. Third, there are likely to be omitted variables that are correlated among those in the same school. Gaviria and Raphael (2001) corrected for the first issue by assuming that $\delta = 0$ and using the average characteristics as instrumental variables. I use a similar strategy in this article, although I test this assumption and restrict the instruments to those that are not rejected as valid instruments; the rejected variables are left as regressors.[3] To examine the magnitude of the second issue of sorting, I follow the suggestion of Glaeser (1996) and compare the social multiplier (α in Eq. (2)) by residential mobility of the students to examine the potential endogeneity bias. If there is little difference in the estimated social effects between students who move in order to attend another high school and students who do not move, then this is evidence that the endogeneity bias is minimal. Finally, I use a Huber-White robust estimator in which the residual covariance matrix is clustered by school in order to account for the sampling scheme used in the data. While this methodology can potentially account for several econometric difficulties in estimating social effects, it is difficult to be sure that the results are free from the correlated effects mentioned above. As in much nonexperimental empirical research, there is no way to know whether there are adequate controls in the estimating equations so that unobserved factors that are common to individuals in the same schools are not biasing the results. The results should be viewed with this caveat in mind.

DATA

The NELS is sponsored by the National Center for Education Statistics. This nationally representative survey started in 1988 with a sample of almost 1,000 schools and over 24,000 eighth graders, although subsequent waves of data (in 1990, 1992, 1994, and 2000) have a sizable reduction in sampling. Sexual histories are not reported until the 1994 wave, and there are outcome data for 11,035 individuals. Additionally, surveys were administered to parents, teachers, and school administrators and linked to the student data. Like Gaviria and Raphael (2001), I restrict the sample to students in schools that had at least five sampled students in the NELS in order to compute the average outcomes in schools. This restriction decreases the number of observations to 9,525. The average number of individuals (randomly) sampled per school is over 13 with a maximum of 40. Dropping individuals with missing data on race, gender, and test scores leaves 9,148 observations. Eliminating individuals with missing school-level data or missing data on family characteristics leaves 6,697 observations. Finally, I also drop from the analysis those students who changed schools between tenth and twelfth grade because I do not have school identification codes for these individuals from which to calculate school-level characteristics; this restriction leaves 5,896 students in over 850 schools for the analysis sample. Summary statistics are presented in Table 1.

Unfortunately, the sample restrictions were more likely to remove students in disadvantaged schools from the analysis, and it is difficult to gauge the implications of the sample restrictions for the final results. On one hand, it may be that social influences are most powerful for disadvantaged students so that the results using this restricted sample would represent a lower bound on the potential of changing sexual initiation behaviors. On the other hand, disproportionately dropping the highest-risk individuals from the sample could lead to incorrect inferences about the ability of public policies to affect behavior. When

3. Another way of stating this is that covariates are used for instruments that, conditional on other individual- and school-level characteristics, are not statistically related to the outcome at the individual level. These variables continue to be statistically related to the outcomes at the school level, though, and are thus used as instruments.

Table 1. Summary Statistics of NELS Data (N = 5,896)

Variable	Mean	*SD*	Minimum	Maximum
Outcome				
Had sex by tenth grade	0.37	0.48	0	1
Individual-Level Variables				
Male	0.47	0.50	0	1
Family size	4.23	1.36	1	10
Parents married	0.84	0.37	0	1
White	0.76	0.43	0	1
Black	0.07	0.25	0	1
Hispanic	0.10	0.30	0	1
Other race	0.10	0.29	0	1
Rural	0.36	0.48	0	1
Test score (eighth grade)	54.05	11.26	33	100
No tragic events	0.42	0.49	0	1
Family income ($10,000)	43.29	35.43	2	200
Parental involvement	2.03	1.16	0	4
School Characteristics				
Public school	0.87	0.34	0	1
Total enrollment	1,099	650	200	2,500
Had sex education in eighth grade	0.17	0.37	0	1
Had sex education in tenth grade	0.67	0.47	0	1
Class-Level Variables				
Average test score	53.46	6.98	37	100
Average income	42.28	22.66	2	200
Male (%)	47.62	17.85	0	100
Black (%)	6.87	15.91	0	100
Hispanic (%)	9.91	19.19	0	100
Average parental involvement	1.93	0.52	0.13	3.80
Married households (%)	82.56	14.50	0	100
Average Outcome				
Sexual initiation rate (%)	39.34	19.05	0	100

viewing the results, readers should keep in mind the limitations from the sample restrictions used in the analyses.

The empirical results include several types of covariates. In addition to standard demographic variables such as gender, race, family size and income, and rural status, the specifications include prior test scores to proxy for academic endowment, parental involvement in school activities to proxy for unobserved parental guidance and involvement in the individual's life,[4] and an indicator for tragic events in the prior two years. Since I am

4. Parental involvement captures whether parents attended school meetings, spoke with teachers, visited the student's classes, or attended a school event. Sui-Chu and Willms (1996) showed that parental involvement across multiple domains is related to eighth-grade achievement. Since parental involvement is not the primary emphasis

most interested in examining social influences on adolescent outcomes rather than family circumstances per se, I aggregate tragic events into a single measure to control for these circumstances for parsimony.[5]

ESTIMATION RESULTS

Table 2 displays the results of several specifications. All regressions are linear probability models on the binary outcome of whether the individual reported having sex by the tenth grade.[6] Column 1 shows the results from a model of only individual- and school-level variables. Males are almost 12 percentage points more likely than females to report having had sex by the tenth grade. Blacks are more likely and "other" ethnic groups are less likely to report the outcome than whites and Hispanics. Individuals with higher ability (measured by eighth-grade test scores), higher family incomes, larger families, and more parental involvement are less likely to report sexual activity by the tenth grade. Individuals who have experienced tragic events between eighth and tenth grade are more likely to report sexual activity, which might reflect omitted family variables or individuals seeking to deal with these events.[7]

Students attending public schools are more likely to have had sex, and, interestingly, students who report attending sexual education classes during the tenth grade are *more* likely to initiate sex.[8] Trussell (1988) cited this relationship as a reason some parents are against having sex education. In contrast to these results, Averett, Rees, and Argys (2002) found no relationship between sex education requirements at the state level and individual sexual activity. Kirby (2001) pointed to the need to gauge the qualities of the sex education programs to forecast their effects, which may reconcile the mixed findings in the literature. Finally, individuals with married parents are less likely to report having had sex by the tenth grade.[9]

Column 2 shows a regression that is run on the full set of variables, including the average characteristics and behaviors in the school for each individual. There is very little difference between the results in columns 1 and 2, except that the coefficient for students in public schools shrinks and becomes statistically insignificant. This likely reflects the addition of previously omitted school-level variables. The average income of fellow students is negatively associated with own sexual behavior, which might reflect a measure of school resources. It is interesting that while being Hispanic is not associated individually with having sex, the percentage of Hispanics in the school is negatively associated with this outcome. Finally, the potential of a social multiplier effect (α from Eq. (2)) of sexual behavior is found because individual outcomes are associated with peer-level behaviors. But since individual outcomes affect the average outcome, and vice versa, the specification in column 2 suffers from a simultaneity problem.

Column 3 displays the results of attempts to solve the "reflection problem" by using an instrumental variables approach. The reflection problem (Manski 1993b, 2000) refers to the fact that researchers using observational data have information only on the

in this article, I include a more parsimonious measure rather than include the multiple factors for parental involvement presented by Sui-Chu and Willms.

5. This measure includes parental death, divorce, parental job loss, welfare receipt, residential move, sibling pregnancy or dropout, individual illness, and other measures.

6. As noted earlier, robust variance estimators are used to account for the heteroskedasticity that is introduced by using linear probability models.

7. Further analysis (available upon request) suggests that divorce and parental job loss are the primary factors that are associated with teenage sexual behavior. Russell (2002) discussed the child development literature, in which stress theory suggests that stressors or changes in family life encourage children to take on adult roles prematurely.

8. This result lacks precision in the basic regression results but has statistical significance in most of the later results.

9. Pierret (2001) also found differences in risky behaviors across family structures.

Table 2. Determinants of Sexual Initiation by Tenth Grade: Individual, Family, and School Factors

Covariates	OLS	OLS, With Classmate-Level Covariates	2SLS
Male	0.119**	0.117**	0.116**
	(9.65)	(9.36)	(9.31)
Family Size	–0.018**	–0.017**	–0.017**
	(3.91)	(3.77)	(3.74)
Married	–0.066**	–0.068**	–0.069**
	(3.76)	(3.89)	(3.91)
Black	0.142**	0.113**	0.118**
	(5.79)	(3.84)	(4.60)
Hispanic	0.012	0.037	0.039
	(0.59)	(1.42)	(1.49)
Other	–0.069**	–0.061**	–0.059**
	(3.26)	(2.96)	(2.88)
Rural	0.023	0.009	0.008
	(1.31)	(0.59)	(0.59)
Test Score (eighth grade)	–0.005**	–0.005**	–0.005**
	(7.72)	(7.30)	(7.26)
No Tragic Event	–0.054**	–0.053**	–0.053**
	(4.17)	(4.13)	(4.10)
Family Income (1,000s)	–0.001**	–0.000	–0.000
	(2.92)	(1.33)	(1.31)
Parental Involvement	–0.018**	–0.016**	–0.017**
	(3.24)	(3.00)	(3.16)
Public School (tenth grade)	0.073**	0.028	0.027
	(3.37)	(1.27)	(1.27)
Enrollment (tenth grade)	–0.000	–0.000	–0.000
	(0.56)	(0.29)	(0.11)
Had Sex Education in Eighth Grade	0.028	0.027†	0.027
	(1.64)	(1.66)	(1.64)
Had Sex Education in Tenth Grade	0.023	0.025†	0.025†
	(1.63)	(1.85)	(1.87)
Average Income		–0.001*	–0.001*
		(2.57)	(2.33)
Hispanic (%)		–0.001*	–0.001*
		(2.36)	(2.53)
Had Sex (%)		0.002**	0.003*
		(5.14)	(2.08)
Constant	0.715**	0.649**	0.589**
	(14.68)	(8.33)	(5.87)
Number of Observations	5,899	5,896	5,896
R^2	0.07	0.08	0.08

Note: Robust *t* statistics are shown in parentheses.

†Significant at the 10% level; *Significant at the 5% level; **Significant at the 1% level.

individual and aggregate (school-level) outcomes at the same point in time. Therefore, researchers have no way of judging whether the individual outcomes are affecting the aggregate outcomes, or vice versa—and the conceptual model of social interactions implies that the relationship is bidirectional. This difficulty implies a simultaneity bias in the ordinary least squares (OLS) regression of individual outcomes on group outcomes, as both outcomes affect each other.

In order for the instrument to solve the reflection problem, a variable is needed that (1) at the individual level is related to the individual outcome, (2) at the aggregate level is unrelated to the individual outcome and error term, and (3) at the aggregate level is related to the aggregate outcome (Durlauf 2004). The chosen instruments are school-level average characteristics (the proportion with married parents, the proportion black, the proportion male, and average parental involvement) that do not affect individual outcomes (conditional on other covariates).[10] Column 3 of Table 2 shows two-stage least squares (2SLS) regressions for the outcome. There are minor changes in the individual-level characteristics. The social multiplier (α from Eq. (2)) is still statistically significant and has increased slightly in magnitude.[11] The magnitude of the social multiplier is comparable to, although a little higher than, results for drug use, alcohol use, cigarette smoking, dropping out, and church attendance reported by Gaviria and Raphael (2001). Taken at face value, the results imply that increasing the school-level rate of sexual activity by 10 percentage points will increase the likelihood that an individual initiates sex by 3 percentage points.[12] Before examining policy interventions, I present results separately for gender and race, as well as by mobility, to examine potential endogeneity bias.

Table 3 presents results based on gender, race, and mobility. Columns 1 and 2 present the results based on gender. Many of the individual-level variables are similar for male and female adolescents. However, black and Hispanic males are much more likely than others to report sexual behavior. Additionally, females who report a tragic event between eighth and tenth grade are over 8 percentage points more likely to report having sex, while there is no such effect for males. This is some evidence of the motivation to pursue premarital sex for females. Further, females are more likely to have sex if they receive sexual education in the tenth grade, which supports the idea of sexual education lowering the perceived costs of having sex. Oettinger (1999) also found positive effects of sexual education on females only. It is very interesting, though, that I also find that social multiplier effects are present only in males, with the coefficient over twice as large as any behavior reported by Gaviria and Raphael (2001). This is consistent with the notion that peer pressure to have sex is amplified for male adolescents as well as with some experimental results showing that peer pressure differentially affects the behaviors of males (Brown, Clasen, and Eicher 1986; Gardner and Steinberg 2005). This finding is of additional interest, given the small number of studies that examine the sexual initiation decisions of males (Levine 2001). Overall, the evidence suggests that the risk factors of sexual initiation differ greatly by gender.

Columns 3–6 report the results of 2SLS based on race. Black and Hispanic males are again more likely to report having had sex. There are also many interesting nonresults. Having married parents is important only for white and black individuals. Test scores are important only for whites. Parental involvement is not associated with lowering the risk of sex for blacks or "other" races. The average "ability" of classmates increases the

10. I test the overidentifying restrictions by using a procedure suggested by Wooldridge (2002:122–23), which is a version of the Hausman test statistic. This test fails to reject the null of exogenous instruments. Although this procedure is superior to assuming no contextual effects, it is clearly less optimal than selecting instruments based on theory or formal reasoning.

11. In unreported results, I found that including predicted income for missing income observations did not change the results.

12. Using sample weights did not substantially change the results. Tables that display these results are available upon request from the author.

Table 3. Determinants of Sexual Initiation by Tenth Grade, by Gender, Race, and Mobility[a]: 2SLS Results

Covariates	Male	Female	White	Black	Hispanic	Other Race	Mobile	Immobile
Male			0.090**	0.312**	0.218**	0.076†	0.085*	0.118**
			(6.31)	(6.08)	(5.21)	(1.90)	(2.16)	(8.88)
Married	–0.080**	–0.063**	–0.061**	–0.157**	0.080	–0.069	–0.054	–0.069**
	(3.00)	(2.65)	(2.80)	(3.29)	(1.24)	(1.11)	(1.16)	(3.55)
Black	0.207**	0.052					0.058	0.125**
	(5.28)	(1.46)					(0.78)	(4.48)
Hispanic	0.089*	–0.008					0.093	0.026
	(2.35)	(0.23)					(1.27)	(0.97)
Other	–0.056†	–0.061*					–0.098	–0.057**
	(1.78)	(2.24)*					(1.61)	(2.67)
Test Score	–0.007**	–0.003**	–0.006**	–0.001	–0.003	–0.002	–0.002	–0.005**
	(6.93)	(3.58)	(7.42)	(0.30)	(1.20)	(1.09)	(1.23)	(7.32)
No Tragic Event	–0.009	–0.085**	–0.048**	–0.090†	–0.010	–0.067†		
	(0.46)	(5.05)	(3.21)	(1.83)	(0.22)	(1.73)		
Family Income (1,000s)	–0.000	–0.000	–0.000	–0.000	–0.003**	0.000	–0.000	–0.000
	(1.16)	(0.73)	(0.59)	(0.31)	(3.49)	(0.24)	(0.15)	(1.56)
Parental Involvement	–0.020*	–0.017*	–0.019**	0.024	–0.033†	–0.000	–0.018	–0.017**
	(2.51)	(2.36)	(3.07)	(1.35)	(1.73)	(0.02)	(1.08)	(3.03)
Had Sex Education in Eighth Grade	0.027	0.022	0.032†	–0.004	–0.077	0.072	0.000	0.032†
	(1.08)	(0.94)	(1.76)	(0.07)	(1.20)	(1.42)	(0.00)	(1.81)
Had Sex Education in Tenth Grade	0.008	0.038*	0.016	0.050	0.065	0.037	0.075†	0.018
	(0.42)	(2.09)	(1.05)	(0.84)	(1.32)	(0.74)	(1.85)	(1.27)
Average Income	0.000	–0.002**	–0.001†	–0.002	–0.000	–0.002	0.000	–0.001**
	(0.26)	(3.21)	(1.81)	(1.19)	(0.19)	(1.14)	(0.14)	(3.07)
Hispanic (%)	–0.001	–0.001	–0.001	0.001	–0.002*	–0.002	0.000	–0.001**
	(1.55)	(1.62)	(1.01)	(0.85)	(2.12)	(1.49)	(0.01)	(2.62)
Had Sex (%)	0.006**	0.001	0.004**	–0.001	–0.007	0.008	0.005	0.003†
	(2.84)	(0.29)	(2.70)	(0.25)	(0.84)	(1.19)	(1.26)	(1.83)
Constant	0.624**	0.634**	0.509**	0.730**	1.117*	0.352	0.443†	0.628**
	(4.06)	(4.13)	(4.58)	(2.87)	(2.03)	(0.54)	(1.67)	(5.35)
Number of Observations	2,746	3,150	4,459	407	588	564	707	5,189
R^2	0.07	0.06	0.06	0.15	0.05	0.07	0.03	0.08

Note: Robust *t* statistics are shown in parentheses.

[a]I also control for family size, rural status, whether the school is public, the school's enrollment, and the average test score at the school. Results are available on request.

†Significant at the 10% level; *Significant at the 5% level.; **Significant at the 1% level.

probability of whites reporting having had sex but decreases the probability for Hispanics and blacks (although the latter is not precisely measured). Finally, there do not appear to be any social multiplier effects for Hispanics or blacks, and the biggest effects are for whites. This result is consistent with Teitler and Weiss's (2000) finding that school environments affect students in primarily white schools.

Finally, columns 7–8 report the results separately for those who report moving between eighth and tenth grade and those individuals who stay in the same residence. Glaeser (1996) suggested comparing these two groups in order to examine the potential bias resulting from parental selection of peer groups by residential choice.[13] There is some evidence of endogeneity bias. Although I cannot reject that the coefficients are the same, the mobile students have a higher social multiplier effect (higher α). This could reflect unobserved family factors related to mobility, but it is consistent with a story of students who move to a new school feeling an elevated need to fit in and being more susceptible to peer pressure. The observation that sex education is positively related with having had sex only for mobile students is also consistent with this second story of adhering to new school norms after changing schools, where the student is informed of the school norm by the availability of sex education. Now that I have illustrated the probable existence of social multipliers in sexual behavior among teenagers, I outline the effects of policy interventions by using simulation exercises.

EFFECTS OF PUBLIC POLICIES ON RATES OF SEXUAL INITIATION: SIMULATION RESULTS

The primary policy intervention considered in this article is changing the composition of schools. In particular, I examine the aggregate effects of adding to, subtracting from, and switching the number of high- and low-risk students in schools on the predicted percentage of individuals who report sexual activity. While the simulations of the effects of changes in the composition of schools do not follow from specific public policies, the results are relevant to a number of policies that change where students attend school, including school voucher programs and busing.

School vouchers are usually means-tested and are designed to allow students in poorly performing schools the opportunity to attend private schools by offsetting the required school fees. Since the take-up of school vouchers has been shown to be related to desirable characteristics of the students and parents (Campbell, West, and Peterson 2005), the students in low-performing schools who use the vouchers to move to other schools are expected to be better students. Thus, school vouchers have the potential of leaving the low-performing schools in worse shape if the students who remain have peers with a high risk of sexual initiation.

I examine this policy issue by predicting the effects of changes in school composition on school-level rates of sexual activity. In practice, the predicted percentage of individuals within each school who are sexually active is estimated using Eq. (2):

$$Y_{is} = c + \mathbf{X}_{is}\beta + \overline{\mathbf{X}_{-is}}\delta + \mathbf{W}_s\phi + \alpha\overline{Y_{-is}} + \varepsilon_{is}.$$

The aggregate proportion of sexually active individuals within a school—the so-called social equilibrium—can be found by taking expectations of both sides of the equation and rearranging:[14]

$$E(Y_{is}) = \frac{1}{1-\hat{\alpha}} E\left[\hat{c} + \mathbf{X}_{is}\hat{\beta} + \overline{\mathbf{X}_{-is}}\hat{\delta} + \mathbf{W}_s\hat{\phi}\right]. \quad (3)$$

Thus, we can see that changes in the students in a school can change the expected aggregate sexual behavior through the *direct effect* of their own characteristics (through changes in $\mathbf{X}$ and $\overline{\mathbf{X}}$) and an *indirect effect* through the social multiplier (α) that magnifies the effects of

13. Tragic events are not used because moving is included in this category.
14. The hatted variables are the estimated coefficients from the regression analysis.

the individual's characteristics.[15] I randomly pick three schools from the data set to show the results of the simulations and to get a sense of the heterogeneity of the effects of the same interventions across different schools. All results are presented in Table 4.

The first policy examines the effects of adding five high-risk students to each school.[16] The percentage of individuals reporting sexual activity is expected to increase after the intervention for two reasons: a *direct effect* from the newly added individuals because of their own behavior, and an *indirect effect* from the effects on the social environment of the school that changes the behavior of other students. The predicted social multiplier of this policy is approximately 1.1, which is the ratio of the total effect to the direct effect. This number represents the magnitude of the indirect effect of adding high-risk students. For comparison, Gaviria and Raphael (2001) estimated a multiplier of 1.34 for drug use for adolescents.

The second policy shows the effects of adding five low-risk students to each school. This intervention is expected to lower the predicted aggregate percentage reporting sexual activity. The reason there is a social multiplier greater than 1 after this intervention is that even though the additional students are low-risk, they still have positive probabilities of being sexually active. This represents the negative effects of adding *any* students to a school, which might be called a resource-constraint effect. Comparing the predicted social multipliers between interventions 1 and 2 allows an examination of the potentially explosive effects on reported sexual activity for schools in bad and declining neighborhoods. In particular, the debate over school vouchers usually does not incorporate the social multiplier effects on the remaining students when some "bad schools" lose their best students.

The next two policy interventions examine the effects of replacing high-risk students with low-risk students and vice versa.[17] The third policy intervention predicts the effects of taking away the five highest-risk students and adding five low-risk students. This could happen if we could somehow reduce the risks of the highest-risk students down to the level of the lowest-risk students in each particular school.[18] An alternative scenario in which one might expect these compositional changes within a school is if the neighborhood that supplies the students is undergoing changes (e.g., gentrification). This simulation shows the effects at the school level of the highest-risk students moving out of the neighborhood (and school) and five low-risk students taking their place at the school. There again is a direct effect that is a result of the former high-risk students being switched with low-risk students. But the indirect effect shows the impact on the untreated students in the school. For example, in school 1, the composition change decreases the predicted rate of sexual initiation by 4% to 25%, but the effect on the social norms of the school (the indirect effect) decreases the prediction by an additional 4% to a little more than 20%.

A final simulation I examine is to take away five low-risk students and add five high-risk students in each school. This could happen as a neighborhood changes from predominantly nonpoor to predominantly poor, where the best students leave for better schools, and the schools receive an influx of higher-risk students. Alternatively, this scenario could reflect the introduction of a magnet school in a school district, which might

15. The procedure I use in the policy simulations is similar to that used in Gaviria and Raphael (2001).

16. "Adding" here means cloning the characteristics of the five students who have the highest predicted probability of reporting sexual activity in each school.

17. This is done by dropping the observations for the five individuals with the highest predicted risk and cloning the lowest-risk individual five times.

18. Two facts about this intervention should be kept in mind. First, the intervention is assumed to be quite powerful in that it can lower the risk of high-risk students to that of low-risk students. Second, although this intervention is done on five students, the number of observations for each school ranges from 14 to 25, so 5 students from a class of 25 represents an intervention on 20% of the school body.

Table 4. Simulation Results of Changes in the Composition of Students for Three Schools: Compositional (direct) Effects and Social (indirect) Effects on Sexual Initiation Rates

	School 1 (N = 25)	School 2 (N = 15)	School 3 (N = 14)
Pre-intervention			
School-level sexual initiation (%)	29	33	38
Interventions			
Add five high-risk students			
School-level sexual initiation (%)	36	39	42
Direct effect of intervention (%)	33	35	38
Indirect effect of intervention (%)	3	4	4
Multiplier (total effect / direct effect)	1.10	1.12	1.11
Add five low-risk students			
School-level sexual initiation (%)	27	28	32
Direct effect (%)	26	27	31
Indirect effect (%)	2	2	1
Multiplier	1.03	1.04	1.04
Replace five low-risk with five high-risk students			
School-level sexual initiation (%)	20	23	30
Direct effect (%)	25	25	31
Indirect effect (%)	–4	–2	–1
Multiplier	0.82	0.91	0.98
Replace five high-risk with five low-risk students			
School-level sexual initiation (%)	40	45	47
Direct effect (%)	33	35	38
Indirect effect (%)	7	10	9
Multiplier	1.21	1.27	1.23

select the lowest-risk students into the new school (i.e., "cream-skimming").[19] Again, we can see a dramatic direct *and* indirect effect on the school-level rate of sexual initiation. For example, in school 1, the compositional change in the school directly increases the predicted rate of sexual initiation from 29% to 33%. The larger effect, though, is that the students in the school now interact with higher-risk students, and this social (indirect) effect increases the school rate of sexual initiation another 7% to 40%.

While these simulations are interesting exercises, there are some limitations to their relevance for predicting the effects of specific public policies. The most important limitation of the approach in this article is that the social effects are a "black box" in that the *mechanism* is unknown, which leaves multiple hypotheses of why individuals are influenced by their peers. One explanation for this effect is that the school norm for sexual initiation in a school is changed through the policy intervention on the subset of high-risk students, so that all students in the school are less likely to initiate sex. An alternative

19. Dills (2005) used the introduction of a magnet school that selects the highest-ability students in an attempt to examine peer effects in academic achievement. Like the present article, she found that the loss of good students lowers the outcomes of the students left behind.

mechanism for the effects of the intervention is information sharing among students. For example, the high-risk students (who are low-risk after the intervention) no longer share information about their high-risk behavior after this intervention. Other stories are also consistent with the social multiplier effects. The simulation exercises are suggestive, however, of the potential benefits of finding policies that reduce risky behaviors in individuals and also the benefits of finding better ways of predicting future risky behaviors in preadolescence and developing interventions for those individuals.

CONCLUSIONS

Overall, the results are consistent with the presence of several types of social effects on the decisions of adolescents to report having had sex by tenth grade. Although an attempt is made to control for many aspects at the individual, family, and school levels, omitted variables are ubiquitous in empirical work on adolescent choices. Because the data begin in most cases in the eighth grade, preadolescent factors cannot be fully taken into account. Future work on other data sets is important to test the results of this article. Additionally, the endogeneity of the peer group both by choice of school and within schools is of concern. I attempted to mitigate this concern by using residential mobility decisions to compare estimated social effects and allowing correlated errors across individuals in the same school, but additional work should be done on this topic.

With these caveats in mind, there is fairly consistent evidence of social multiplier effects in the sexual initiation decisions of adolescents. These effects appear to differ in importance by gender and by race. They seem to be most important for males and whites. There is also evidence of differences in the importance of individual, school, and peer characteristics by gender and race. I examined several policy interventions, including adding low-risk and high-risk students to schools as well as switching low- and high-risk students between schools. The general result is that moderate differences in school composition can have large effects on the overall rates of teenage sexual initiation. This finding is relevant to the debates over school vouchers and ability grouping, which are policies that change the composition of schools and the composition of peer groups within schools, respectively. These policies may exacerbate the already poor outcomes of students in low-performing schools and lead to persistent rates of high pregnancy and out-of-wedlock births in some communities. The policy simulations also suggest that targeting resources toward high-risk individuals might have the added effect of positively affecting other individuals through the social multiplier effect and should be the subject of more research.

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