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Social Multipliers in Sexual Initiation Decisions among U.S. High School Students

Jason Fletcher[•]
University of Wisconsin-Madison

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Abstract

This paper uses 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 school. Additionally, the magnitude of the social multipliers and several other interesting risk factors differ by gender and race. These finding might help explain the large variation in sexual initiation across schools in the United States. Additionally these results add to the debate over school vouchers and ability grouping. Large multipliers imply large 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 teenage pregnancy rates and out-of-wedlock births in some communities.

[•] jmfletcher@wisc.edu . Graduate Student in Applied Economics. Mail: 429 Lorch Street, Taylor Hall, University of Wisconsin, Madison, WI 53706

Introduction

In recent years there has been a large literature attempting to credibly determine the importance of social effects in individual decision-making.[1] Adolescent decision-making and risky behaviors have been of particular interest (see Duncan et al 2003, Gaviria and Raphael 2001, and Evans et al 1992). One reason for this is evidence that adolescents are more prone to be driven by social concerns when making decisions.[2] There are also varying opinions of how sophisticated adolescents are as decision-makers.[3]

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 not clear how important social effects are on the decisions of adolescents.[4] The presence of such effects might, however, help explain the large variation in this behavior across schools (see Figure 1 below). 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. For example teenage pregnancy has been linked to lower human capital accumulation and subsequent wages (Klepinger et al 1999) and various other outcomes. Early fertility also has

[1] See Durlauf (2004) for a comprehensive review.
[2] O'Donahue and Rabin (2001) review much of the literature and point out that youths focus more on social consequences of a given behavior. In particular, youths are more concerned about peer reactions and doing behaviors that form identity and independence regardless of consequences. Additionally, as children develop, they spend less time with adults and more time with friends (Halpern-Felsher et al. 1997). In the case of dropping out, Bryk and Thum (1989) find the normative climate of a high school to be important for the outcome.
[3] Manski (1993b) focuses on the heterogeneity in adolescent decision-making processes. In contrast, Walker (2001) finds that adolescents are good at predicting future events when the event is salient to them (e.g. teen pregnancy and parenthood). Wolfe et al (2001) present evidence that teenagers respond to perceived costs associated with nonmarital birth by reducing risky behaviors.
[4] Teitler and Weiss (2000) use self reported measures of norms for sexual behavior and find them to be important predictors of sexual initiation.

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).

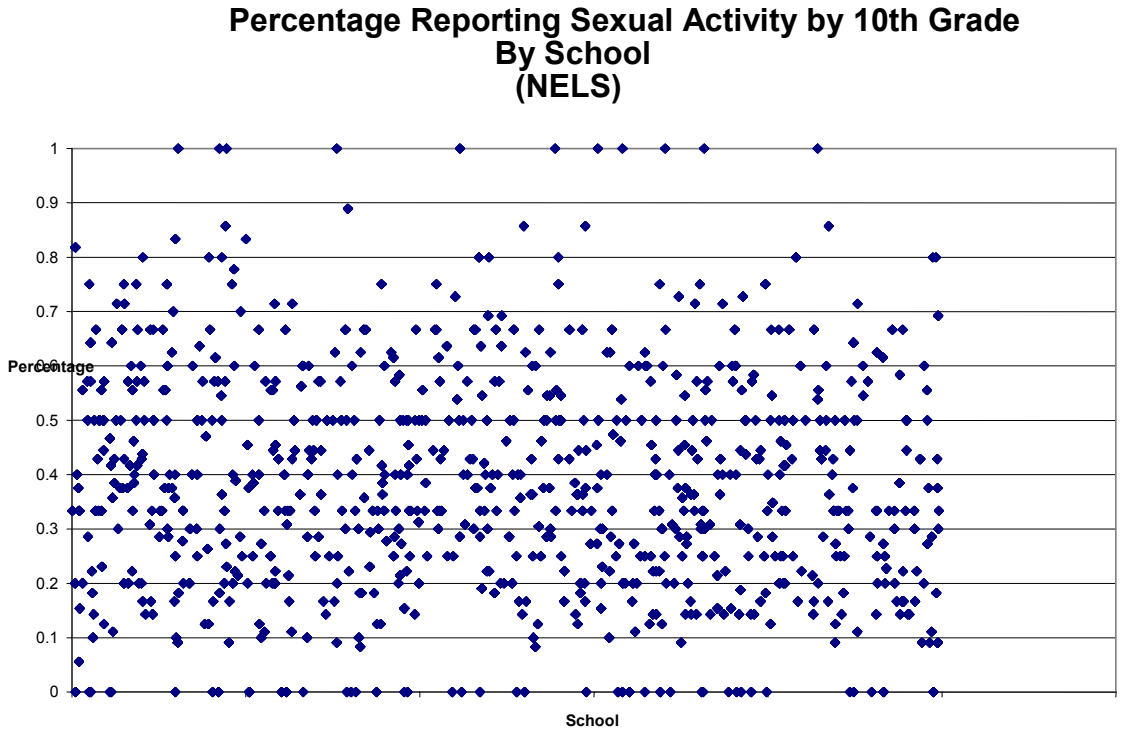
There are several theories of the ways social influences affect adolescent sexual behavior. Information sharing among students may change the perceived costs of having sex. A social norm may develop so that in order to be a part of the "in crowd" or fit in with his peers, a student might feel compelled 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 spill over onto other individuals. In contrast, Manski (1993a) points out that many non-social theories can explain the observation that individuals in the same group have similar outcomes. In these cases, some policy interventions would not be expected to have the same kind of spillovers on the untreated.

While there are many theories that 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 multipliers and other effects is disentangling the different types of social effects (Manski 1993a). This is particularly important if policy interventions are being proposed 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, which will be discussed in the next section.

This paper adds to the literature on the social effects of peers in adolescent decision-making. Using the nationally representative National Education Longitudinal Survey (NELS) data set of high school students, I test for peer group influences in the probability of reported sexual initiation by the 10th grade. The results show evidence of different types of social effects on this outcome, and these effects differ by race and gender. Additionally, residential mobility is used to test for endogeneity bias in the results (Glaeser 1996). Finally, I discuss several types of policy interventions and predict their effects on the sexual behavior of students.

Figure 1[5]



[5] Even though schools with lower income students have higher rates of sexual activity, there is substantial variation across all schools. See Figure 1A in appendix.

Literature Review

The two 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 (1993a, 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.[6]

As Manski points out, distinguishing among these effects is important for several reasons; among them is the implication for policy interventions. For example, endogenous interactions can imply multiplier effects because an intervention on one agent will spill over onto the interacting agents. In contrast, contextual changes do not imply the same multiplier effect responses to an exogenous shock. Manski also directs attention to the problems associated with individuals in common environment experiencing common unobserved effects (correlated effects), which are non-social 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.

[6] Correlated effects are implied from a Tiebout (1956) type model of residential sorting.

The potential source of spurious social effects from residential sorting 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) uses a random assignment treatment (Gatreaux desegregation program) in which poor families are relocated from public housing to private housing in a different neighborhood and finds substantial neighborhood effects (see also Sacerdote 2001). Aaronson (1995) uses a data set with sibling characteristics to difference out fixed effects and still finds prevalent neighborhood effects. Evans et al. (1992) explicitly model the sorting process and use a two-staged estimation approach and find no social effects on teenage pregnancy or dropping out of high school.

The method utilized in this essay is to use residential mobility to determine the potential bias from correlated effects. Gaviria and Raphael (2001), in analysis of risky behaviors using the NELS, argue that the bias of estimates should be less severe for long-term residents because their residential and school decisions were made taking into account past, rather than present, school quality and peer group composition. 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 a simple test of endogeneity of school choices. In my own view, the direction of the bias of examining movers versus non-movers is ambiguous, although comparing the two groups can give a sense of the magnitude of the 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.[7] Other researchers make restrictive assumptions in order to uncover the endogenous social effect parameters. For example, Gaviria and Raphael assume no contextual effects are present. They defend this assumption by arguing that students are less exposed to the family background characteristics of other students than to family background characteristics of his neighbors.

Methodology

The empirical framework used in this paper attempts to disentangle the different types of social interactions and has been used by several researchers (e.g. Gaviria and Raphael 2001). Of particular interest is whether an individual's propensity to have sex is affected by the choices of others in his school. The choice of school as the relevant sphere of interaction follows recent work by Gaviria and Raphael (2001) but is in contrast to much of the previous work on peer and neighborhood effects.[8] One principle argument for focusing on schools rather than neighborhoods is that schools are a better defined environment, particularly for adolescents.[9] In contrast, several definitions of the relevant geographic neighborhood have been used in the literature. A second argument for focusing on schools is the 'forced' nature of the interactions. While even a suitably defined neighborhood gives no indication of the level of actual peer interaction, the

[7] This applies to nearly all work prior to 1993 when Manski (1993a) made a seminal contribution to the distinction as the "reflection problem."
[8] Harding (2003) uses census tracts, Evans et al. (1992) uses metropolitan level data, and Case and Katz (1991) use city block level data.
[9] Teitler and Weiss (2000) compare school and neighborhood effects on sexual behavior for students in Philadelphia and find that neighborhood effects are virtually eliminated when school effects are introduced.

school environment usually is thought to include a high level of peer interaction. Additionally, Gaviria and Raphael (2001) cite evidence from the National Education Longitudinal Survey (NELS) that most students attend school with their best friends and state that meeting friends is the main reason they go to school.

The empirical specification used in this paper was introduced by Case and Katz (1991) and many researchers have followed the so called linear in means model (Manski 1993a):

$$Y = c + X\beta + \overline{X}\delta + \alpha\overline{Y} + \varepsilon \quad (1)$$

where Y is the outcome (sexual initiation), X is a vector of individual and family characteristics, $\overline{X}$ is a vector of 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 (1993, 2000), $\overline{X}$ are contextual/exogenous variables, and $\overline{Y}$ is an endogenous variable. To estimate the model, $\overline{X}$ 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.

$$Y_{is} = c + X_{is}\beta + \overline{X_{-is}}\delta + W_s\phi + \alpha\overline{Y_{-is}} + \varepsilon_{is} \quad (2)$$

Here Y_{is} is the probability that student *i* in school *s* will report having had sexual intercourse by the 10th grade; X_{is} is a vector of family and individual characteristics, $\overline{X_{-is}}$ is a vector of average characteristics of students in school *s* excluding individual *i*, W_s is a vector of school characteristics, and $\overline{Y_{-is}}$ is the average outcome excluding individual *i*. As Manski (1993, 2000) points out, the types of social effects estimated

from (2) imply different policy interventions. If δ is estimated to be non-zero, this is consistent with role model effects or resource 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 student. 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 an intervention on a subset of students will have indirect effects of students who do not receive the intervention. Furthermore, reallocating students across schools can lead to an overall decline of sexual initiation. Additionally, this type of social multiplier might explain some of the current large variation across schools in sexual initiation and pockets of teenage pregnancy and out-of-wedlock births in some communities.

There are several econometric issues in equation (2) that must be addressed. First, there is a simultaneity issue since the outcome affects average behavior and average behavior affects the outcome. Second, to the extent that families sort across schools, the average behavior variable is a choice of parents rather than an exogenous variable. Third, there are most likely omitted variables that are correlated among those in the same school. Gaviria and Raphael (2001) correct for the first issue by assuming that $\delta = 0$ and using the average characteristics as instrumental variables. A similar strategy is used in this paper, 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.[10] To examine

[10] 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 remain to be statistically related to the outcomes at the school level, though, and are thus used as instruments. Results in appendix

the magnitude of the second issue of sorting, I follow the suggestion of Glaeser (1996) and compare the social multiplier (α in equation 2) by residential mobility 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 endogeneity bias is minimal. Finally, a Huber-White robust estimator is used 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 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 National Education Longitudinal Study (NELS) is sponsored by the National Center of Education Statistics. The survey started in 1988 with a sample of almost 1,000 schools and over 24,000 eighth graders, although subsequent waves of data have a sizable reduction in sampling.[11] Four follow-up surveys occurred in 1990, 1992, 1994, and 2000. 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 observations collected by the NELS in order to compute the average outcomes in schools.[12] The mean sample size per school (class) is

[11] Since sexual histories are not reported until the 1994 wave, there is outcome data for 8,400 individuals.
[12] The number of observations falls from 10,000 to 8,900 after this restriction.

almost 13 with a maximum of 37. The final sample includes almost 6,000 students.[13] Summary statistics are presented in Table 1.

Table 1
Summary Statistics of NELS Data

Variable	Obs	Mean	Std. Dev.	Min	Max
Outcome					
Had Sex by 10th Grade	5896	0.37	0.48	0	1
Individual Level Variables					
Male	5896	0.47	0.50	0	1
Family Size	5896	4.23	1.36	1	10
Parents Married	5896	0.84	0.37	0	1
White	5896	0.76	0.43	0	1
Black	5896	0.07	0.25	0	1
Hispanic	5896	0.10	0.30	0	1
Other Race	5896	0.10	0.29	0	1
Rural	5896	0.36	0.48	0	1
Test Score (8th Grade)	5896	54.05	11.26	33	100
No Tragic Events	5896	0.42	0.49	0	1
Family Income (10K)	5896	43.29	35.43	2	200
Parental Involvement	5896	2.03	1.16	0	4
School Characteristics					
Public School	5896	0.87	0.34	0	1
Total Enrollment	5896	1099	650	200	2500
Sex Education 8th Grade	5896	0.17	0.37	0	1
Sex Education 10th Grade	5896	0.67	0.47	0	1
Class Level Variables					
Average Test Score	5896	53.46	6.98	37	100
Average Income	5896	42.28	22.66	2	200
Average Male	5896	47.62	17.85	0	100
Average Black	5896	6.87	15.91	0	100
Average Hispanic	5896	9.91	19.19	0	100
Average Parent Involvement	5896	1.93	0.52	0.13	3.80
Average Married	5896	82.56	14.50	0	100
Average Outcome					
Average Sex (Rate)	5896	39.34	19.05	0	100

The empirical results include several types of covariates. In addition to gender, race, family size and income, and rural status, the specifications include prior test scores to

[13] The number of observations falls to 6,800 because of missing income and family size variables. Missing data on sex education, tragic events, and parental marital status variables cause the number of observations to fall to 5,896.

proxy for academic endowment, parental involvement[14] in school activities to proxy for unobserved parental guidance and involvement in the individual's life, and an indicator for tragic events in the prior two years.[15]

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 10th grade.[16] Column 1 shows the results from a model of only individual and school level variables. Males are almost 12 percentage points more likely to report having had sex by the 10th grade. Blacks are more likely and 'other' ethic groups are less likely to report the outcome than whites and Hispanics.[17] Individuals with higher ability (8th grade test scores), higher family incomes, larger families, and more parental involvement are less likely to have had sex by the 10th grade. Individuals who have experienced tragic events between 8th and 10th grade[18] are more likely to report sexual activity, which might reflect omitted family variables or individuals seeking to deal with these events. Students attending public schools are more likely to have had sex, and, interestingly, students who report attending sexual education classes during the 10th grade

[14] Levine (2001) suggests a need to incorporate parental involvement in order to examine the effects of academic achievement on risky behaviors (net of parental investment).
[15] This measure includes parental death, divorce, parental job loss, welfare receipt, residential move, sibling pregnancy or dropout, individual illness, and other measures.
[16] As noted above, robust variance estimators are used to account for the heteroskedasticity that is introduced by using linear probability models.
[17] Cawley (2001) finds no difference for blacks and whites.
[18] 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) discusses the child development literature in which stress theory suggests that stressors or changes in family life encourage children to take on adult roles prematurely.

are *more* likely to initiate sex.[19] Finally, individuals from intact families are less likely to report having had sex by the 10th grade.[20]

In Column 2, a regression 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 Column 1 and 2, except that the coefficient for students in a public school shrinks and becomes statistically insignificant. This probably 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. Interestingly, 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 of sexual behavior is found because individual outcomes are associated with peer-level average behaviors. But since individual outcomes affect the average outcome and vice-versa, the specification in column 2 has a simultaneity problem.

Columns 3 and 4 attempt to solve the simultaneity problem by using instrumental variables. The chosen instruments are school-level average characteristics (average married, black, male and parental involvement) that do not affect individual outcomes (conditional on other covariates).[21] Columns 3 and 4 show unweighted and weighted 2SLS regressions for the outcome.

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[19] This result lacks precision in the basic regression results, but has statistical significance in most of the later results. Averett et al (2002) finds no relationship between sex education requirements at the state level and individual sexual activity. Trussell (1988) cites this relationship as a reason some are against having sex education. Kirby (2001) points to the need to gauge the qualities of the sex education programs to forecast their effects.

[20] Pierret (2001) also finds differences in risky behaviors across intact versus non-intact families.

[21] I test the over-identifying restrictions using a procedure suggested by Wooldridge (2002, pp. 122-3), which is a version of the Hausman test statistic. This test fails to reject the null of exogenous instruments.

Table 2
Results of OLS and 2SLS

	OLS	OLS	2SLS	2SLS
	Individual and School	All Xs	Unweighted	Weighted
Male	0.119	0.117	0.116	0.126
	(9.65)**	(9.36)**	(9.31)**	(9.04)**
Family Size	-0.018	-0.017	-0.017	-0.016
	(3.91)**	(3.77)**	(3.74)**	(2.93)**
Married	-0.066	-0.068	-0.069	-0.072
	(3.76)**	(3.89)**	(3.91)**	(3.53)**
Black	0.142	0.113	0.118	0.161
	(5.79)**	(3.84)**	(4.60)**	(5.37)**
Hispanic	0.012	0.037	0.039	0.027
	(0.59)	(1.42)	(1.49)	(0.85)
Other	-0.069	-0.061	-0.059	-0.017
	(3.26)**	(2.96)**	(2.88)**	(0.60)
Rural	0.023	0.009	0.008	-0.008
	(1.31)	(0.59)	(0.59)	(0.46)
Test Score (8th Grade)	-0.005	-0.005	-0.005	-0.005
	(7.72)**	(7.30)**	(7.26)**	(6.54)**
No Tragic Event	-0.054	-0.053	-0.053	-0.062
	(4.17)**	(4.13)**	(4.10)**	(3.94)**
Family Income (000s)	-0.001	-0.000	-0.000	-0.000
	(2.92)**	(1.33)	(1.31)	(1.62)
Parental Involvement	-0.018	-0.016	-0.017	-0.024
	(3.24)**	(3.00)**	(3.16)**	(3.73)**
Public School	0.073	0.028	0.027	0.040
	(3.37)**	(1.27)	(1.27)	(1.72)+
Enrollment	-0.000	-0.000	-0.000	0.000
	(0.56)	(0.29)	(0.11)	(0.89)
Sex Ed 8th Grade	0.028	0.027	0.027	0.011
	(1.64)	(1.66)+	(1.64)	(0.60)
Sex Ed 10th Grade	0.023	0.025	0.025	0.027
	(1.63)	(1.85)+	(1.87)+	(1.87)+
Average Income		-0.001	-0.001	-0.001
		(2.57)*	(2.33)*	(2.51)*
Average Hispanic		-0.001	-0.001	-0.001
		(2.36)*	(2.53)*	(3.23)**
Average Had Sex		0.002	0.003	0.003
		(5.14)**	(2.08)*	(1.67)+
Constant	0.715	0.649	0.589	0.638
	(14.68)**	(8.33)**	(5.87)**	(5.46)**
Observations	5899	5896	5896	5896
R-squared	0.07	0.08	0.08	0.08

Robust t-statistics in parentheses
+ significant at 10%; * significant at 5%; ** significant at 1%

There are minor changes in the individual-level characteristics. The social multiplier is still statistically significant and has increased slightly in magnitude.[22] The magnitude of the social multiplier is comparable, although a little higher, to results for drug use, alcohol use, cigarette smoking, dropping out, and church attendance reported in Gaviria and Raphael (2001). Before examining policy interventions, I present results separately for gender and race as well as 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 to report sexual behavior than others. Additionally, females who report a tragic event between 8th and 10th 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 pursuing premarital sex for females. Further, females are more likely to have sex if they receive sexual education in the 10th grade, which supports the idea of sexual education lowering the perceived costs of having sex.[23] Very interestingly, though, I also find that social multiplier effects are only present in males, with the coefficient over twice as large as any behavior reported in Gaviria and Raphael (2001).[24] This is consistent with peer pressure to have sex being amplified for male adolescents. Overall, the evidence suggests 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 sex. There are also many interesting non-results.

[22] In unreported results, I found that including predicted income for missing income observations did not change the results.
[23] Oettinger (1999) also finds positive effects of sexual education on females only.
[24] This finding is of additional interest given the small number of studies that examine the sexual initiation decisions of males (Levine 2001).

Intact families are only important for white and black individuals. Test scores are only important for whites. Parental involvement is not associated with lowering risk of sex for blacks or 'other' races. The average 'ability' of classmates increases the probability of whites reporting having had sex but decreases the probability for Hispanics and blacks (although the latter is not precisely measured). Finally, there does not appear to be any social multiplier effects for Hispanics or blacks, and the biggest effects are for whites.[25]

Finally, columns 7-8 report the results separately for those who report moving between 8th and 10th grade and those individuals who stay in the same residence. Glaeser (1996) suggests comparing these two groups in order to examine the potential bias resulting from parental selection of peer groups by residential choice.[26] Hispanics who move and blacks who do not are more likely to report having sex. Additionally, there is some evidence of endogeneity bias. Although I can not reject that the coefficients are the same, the mobile students have higher endogenous effects. Now that I have illustrated the probable existence of social multipliers in sexual behavior among teenagers, I outline the effects of policy interventions.

[25] Teitler and Weiss (2000) find that school environments affect students in primarily white schools.
[26] Tragic events are not used since moving is included in this category.

Table 3
2SLS Results by Gender, Race, and Mobility

	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)**
Family Size	-0.015	-0.020	-0.024	0.010	0.012	-0.026	-0.029	-0.016
	(1.97)*	(3.30)**	(4.26)**	(0.67)	(0.81)	(1.88)+	(2.01)*	(3.29)**
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	0.004				-0.098	-0.057
	(1.78)+	(2.24)*	(0.11)				(1.61)	(2.67)**
Rural	-0.012	0.035	0.010	0.043	0.040	-0.027		
	(0.62)	(1.53)	(0.65)	(0.74)	(0.65)	(0.46)		
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 (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)**
Public School	0.033	0.020	0.017	0.001	0.075	-0.052		
	(1.13)	(0.61)	(0.73)	(0.01)	(0.77)	(0.74)		
Enrollment	0.000	0.000	0.000	0.000	-0.000	-0.000		
	(0.02)	(0.14)	(1.10)	(0.55)	(0.61)	(0.61)		
Sex Ed 8th 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)+
Sex Ed 10th 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 Score	0.002	0.002	0.004	-0.004	-0.006	0.002	0.001	0.002
	(1.08)	(0.92)	(2.78)**	(1.30)	(1.29)	(0.38)	(0.38)	(1.30)
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)**
Average 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)**
Average 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)**
Observations	2746	3150	4459	407	588	564	707	5189
R-squared	0.07	0.06	0.06	0.15	0.05	0.07	0.03	0.08

Simulations

The primary policy intervention considered in this paper is changing the composition of schools. In particular, I examine the aggregate effects on the predicted percentage of individuals who report sexual activity from adding to, subtracting from, and switching the number of high and low risk students in schools. All results are presented below in Table 4. The first policy examines the effects of adding five high risk students to each school.[27] 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.[28] This number represents the magnitude of the indirect effect of adding high risk students

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 above one 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 intervention 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

[27] "Adding" here means cloning the characteristics of the five students who have the highest predicted probability of reporting sexual activity in each school.
[28] Gaviria and Raphael (2001) estimate a multiplier of 1.34 for drug use in adolescents.

does not incorporate the social multiplier effects on the remaining students when some bad schools lose their best students.

Table 4
Simulation Results of Changing Allocation of Students

		School 1	School 2	School 3
	Obs	25	15	14
Pre Intervention				
	Total Outcome	29%	33%	38%
Interventions				
Add 5 High Risk				
	Total Outcome	36%	39%	42%
	Direct Effect	33%	35%	38%
	Indirect Effect	3%	4%	4%
	Multiplier	**1.10**	**1.12**	**1.11**
Add 5 Low Risk				
	Total Outcome	27%	28%	32%
	Direct Effect	26%	27%	31%
	Indirect Effect	2%	2%	1%
	Multiplier	**1.03**	**1.04**	**1.04**
Switch 5 Low Risk for 5 High Risk				
	Total Outcome	20%	23%	30%
	Direct Effect	25%	25%	31%
	Indirect Effect	-4%	-2%	-1%
	Multiplier	**0.82**	**0.91**	**0.98**
Switch 5 High Risk for 5 Low Risk				
	Total Outcome	40%	45%	47%
	Direct Effect	33%	35%	38%
	Indirect Effect	7%	10%	9%
	Multiplier	**1.21**	**1.27**	**1.23**

The next two policy interventions examine the effects of replacing high risk students with low risk students and vice-versa.[29] This is done to get a sense of the upper bound of how explosive the aggregate rates for schools can be after interventions such as school vouchers. This also can give a glimpse of some potential effects of magnet schools or even ability grouping within schools, where there might be substantial differences in allocation of students across schools. In particular, this policy experiment suggests that segregating students through ability grouping might increase levels of sexual activity in schools.

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 before 10th 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. Since the data begin in most cases in the 8th grade, preadolescent factors can not be fully taken into account.[30] Future work on other data sets is important to test the results of this paper. Additionally, the endogeneity of the peer group both by choice of school and within schools is of concern. An attempt is made to mitigate this concern by using residential mobility decisions to compare estimated social effects but additional work should be done on this topic.

[29] This is done by dropping the observations for the five individuals with the highest predicted risk and cloning the lowest risk individual 5 times.

[30] For example, Longmore et al (2001) find that preadolescent parental monitoring is associated with sexual initiation.

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 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. Several policy interventions are examined, 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 within schools. 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.

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Appendix

Figure 1A