Peer Effects for Adolescent Substance Use:

Do They Really Exist?

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Abstract

Methods for estimating peer effects face some difficult identification problems. This study proposes and tests two identification methods that arguably address these issues. The strategies focus on peer effects corresponding to particular policy interventions. The first corresponds to the removal of a substance user from a peer group; the second corresponds to the mixing of children with schoolmates who are several years older. The results suggest that these peer effects are small compared to typical estimates in the literature, but in some cases are still large enough to be policy-relevant.

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Peer Effects for Adolescent Substance Use: Do They Really Exist?

I. Introduction

When the concept of peer effects or peer influences is mentioned, what behaviors come to mind first? For many readers, the answer is undoubtedly substance use, such as smoking and drinking. For adolescents in particular, it is almost accepted as fact that peers exert strong effects on each other's substance use behavior. Yet in practice it is extremely difficult to perform an empirical test that verifies these effects.

Most existing studies of peer effects for substance use conclude that they are significant. Despite a variety of creative approaches, however, these studies are subject to a set of tricky identification problems. Most notably, estimated peer effects may be confounded by the presence of correlated unobservable factors, due to selection or shared environments, within peer groups. These correlated unobservables could be responsible for the similarities in behaviors found among peers, as opposed to true peer effects. Even if we look at our own experiences as adolescents, can we say definitively whether our substance use behavior was determined largely by our peer groups? Maybe we merely selected peers with similar propensities to engage in substance use. Or maybe we shared common environments which led to similar behaviors.

This study proposes and tests two identification strategies that are arguably unbiased by these factors. Both strategies are natural experiments of sorts. The first looks at two plausibly exogenous changes in peer environment: a friend moving away, or a friend graduating while the individual remains at a school (because the individual is in a lower grade). The experiment essentially asks the following question: if in period 1 you and I are identical in observable characteristics and each of us has a friend who smokes, and then between periods 1 and 2 your friend moves away (or graduates) but mine does not, do you smoke less than I do in period 2?

This experiment can be thought of as equivalent to a policy intervention that removes a substance user from a friend group.

The second experiment takes advantage of the fact that 8th graders and 7th graders in the U.S. attend schools with a variety of grade spans. In recent years the most common arrangement for 8th and 7th graders has been a middle school with grades 6-8, but many have instead been in schools with grades 8-12, 7-12, or K-12, for example. Given that substance use rates increase significantly with grade level, a potentially useful instrument for peer group environment is whether an 8th grader (or 7th grader) is in a school with much older adolescents (e.g. grades 7-12) or not (e.g. grades 6-8). In this case, the equivalent policy intervention would be, for example, to change from a school system with separate schools for grades K-5, 6-8, and 9-12 to one with schools for grades K-6 and 7-12.

Understanding the impacts of these interventions has direct implications for real-world policies. If we were to discover, for example, that the removal of substance users from peer groups leads to significantly improved behavior for the remaining peers, then we might want to adjust accordingly the practice of expelling or transferring these adolescents from schools, or alternatively we might place a higher priority on reducing their substance use. Similarly, parents might want to adjust their "policies" regarding the composition of their children's peer groups. Likewise, if we were to find that the mixing of 8th graders with older adolescents in school significantly worsens the 8th graders' outcomes in terms of substance use, then we might want to alter the grade composition of such schools or implement protective interventions.

These questions are especially important because a large body of research has shown that smoking, drinking, or using marijuana as a youth is highly predictive of these same behaviors and more serious drug use later in life (e.g. Lynskey, Heath, Bucholz, Slutske, Madden, Nelson, et al, 2003). The serious long term health consequences of substance use are well-documented. In addition to long term problems, adolescents are at risk for shorter term problems such as impaired driving and decreased academic performance.

The results for the first natural experiment provide some evidence for peer effects. Most notably, the removal of a user-friend appears to reduce the probability that a child initiates substance use. The second experiment does not suggest peer effects. Taken as a whole, the results of this study imply that the particular types of peer effects examined are, at most, much smaller than typical estimates in the literature, but still may be quite meaningful in magnitude from a policy perspective. Furthermore, this finding is unlikely to be a peculiarity of the data, as the data are also used to replicate common empirical techniques from the literature and yield similar results (large estimated peer effects).

This study focuses narrowly on the types of peer effects embodied by the two natural experiments. It is important to recognize, however, that these types of peer effects represent a subset of many possibly important contextual determinants of substance use. A broad and rich literature indicates that substance use is determined by a wide variety of social context factors, including characteristics of schools, families, partners, neighborhoods, media, and local social capital (Aveyard, Markham, & Chang, 2004; Dedobbeleer, Béland, Contandriopoulos, & Adrian, 2004; Eisenberg & Wechsler, 2003; Meas & Lieven, 2003; Monden, van Lenthe, De Graaf, & Kraaykamp, 2003; Frohlich, Potvin, Chabot, & Corin, 2002; Weitzman & Kawachi, 2000). The presence of so many important and interrelated factors makes causal inference related to peer effects particularly challenging.

Section II discusses the existing peer effects literature, highlighting the typical identification problems as well as some of the approaches used to address them. Section III then briefly describes the data sets used for this study: the National Longitudinal Survey of Adolescent Health (AddHealth) and the National Educational Longitudinal Survey (NELS). Section IV illustrates the identification methods, and Section V presents the results. Section VI concludes by discussing implications and suggesting future research paths.

II. Existing Literature

Methodological work by Manski and Moffitt provides a good starting point for reviewing the empirical peer effects literature. Their work highlights potential sources of bias that analyses must address in order to present convincing results. As they detail, finding a strong correlation in behavior among peers is hardly enough to establish the existence of peer effects. Manski (1993) provides a technical summary of the various conditions under which peer effects can be identified, and Moffitt (1998) describes the three identification issues that pose problems for estimating peer effects.

Presenting Moffitt's points slightly differently for simplicity, one could classify the identification problems into three items: 1) simultaneity (or "reflection"); 2) endogenous membership (or "selection"); and 3) shared unobserved environmental factors. Reflection refers to the issue that if an individual is influenced by peers, then the peers are probably influenced by the individual too. Peer behavior would thus be correlated with the error term in a regression with individual behavior as the dependent variable. Selection into peer groups produces correlated unobservable characteristics among peers that may lead to similar behaviors. Also, sharing a common environment often implies that peers experience similar unmeasured environmental factors that can contribute to similar behaviors (e.g. having the same teacher, or experiencing the same drug enforcement rules).

The empirical literature on peer effects for substance use and other risky behaviors employ a variety of empirical strategies, and the vast majority of studies conclude that there are large peer effects. However, it would be hard to argue that any of the studies is immune to all of the major sources of bias described above, as many of the authors acknowledge. One can divide this literature into three groups.

The first group focuses on cross-sectional correlation in behavior among peers as evidence for social interactions. For example, Glaeser, Sacerdote, & Scheinkman (1996) look at the distribution of crime rates across geographical units, and Crane (1991) looks at the distribution of teenage pregnancy and dropping out across neighborhoods. Both studies find evidence for peer effects using careful statistical analysis and a range of reasonable assumptions, but selection and shared unobservable environmental factors are still potentially large sources of bias. Of the many other studies in this category, a number use the same AddHealth data set used in this study (e.g. Alexander, Piazza, Mekos, & Valente, 2001; Duncan, Boisjoly,& Harris, 2001; Regnerus, 2002).

The second group of studies also looks at cross-sectional correlation in behavior among peers, and these studies attempt to address the selection problem and the simultaneity problem by using instrumental variables. For example, Gaviria & Raphael (2001) look at substance use, church attendance, and dropping out by 10th graders, and peer groups are defined by school. They address the simultaneity problem by instrumenting peer behavior with peer characteristics such as parental involvement and education variables. They have an impressive set of school level controls, but there is no way of knowing if their empirical model leaves out some important shared environmental factors, nor whether selection is adequately addressed. One could imagine unmeasured factors such as local availability of drugs, enforcement of school rules, and supervision on school grounds as being important factors.

While the first two groups of studies examine cross-section data, a third group of studies employs longitudinal data in an attempt to confront the selection issue. In these studies, the empirical strategy is carried out in the following way: peer effects are defined as the degree to which initial peer behavior predicts subsequent change in individual behavior; and selection is defined as the degree to which initial individual behavior predicts subsequent changes in peer group composition and behavior. Examples include Ennett & Bauman (1994), Fisher & Bauman (1988), Wills & Cleary (1999), Curran, Stice, & Chassin (1997), Bauman, Carver, & Gleiter (2001), and Haynie (2002). Each of these studies looks at groups of friends as the peer group units, and each study finds evidence for peer effects. Note that the last two papers listed above, like this study, use AddHealth data.

The strategy used in these studies has intuitive appeal, particularly because it has the potential to say something about the time scale in which peer effects act. Furthermore, it might avoid a potentially serious problem with cross-sectional studies: if peer effects operate as a stock as well as a flow, then cross-sectional studies could easily confound current peer effects with peer effects accumulated in the past, because current peer groups are likely to be similar to past peer groups.

Despite this possible advantage of longitudinal data, it is unlikely that the empirical strategy properly addresses the selection problem. Consider the idea in these studies that peer effects are defined as the degree to which initial peer behavior predicts subsequent change in individual behavior. If adolescents select friends based in part on a partially unobserved propensities to smoke or undertake otherwise risky behaviors, then we will observe a positive correlation between initial peer smoking and the changes in individual smoking, as the unobserved propensities become expressed in the second period. Thus, we would estimate "peer effects" even though peer effects may not exist at all.

The primary goal of this study is to measure two particular kinds of peer effects for adolescent substance use in ways that avoid the important sources of bias discussed above. The contexts studied here correspond to two particular policy interventions, as noted earlier. The close connection between the natural experiments and potential interventions may make this exercise particularly informative from a policy standpoint.

III. Data

In many respects the data set used in this study is well-suited for analyzing peer effects. The National Longitudinal Survey of Adolescent Health (AddHealth) allows one to look at peer groups defined by school and grade as well as by friendships. It is a large national sample of students in grades 7-12, and contains detailed information about students, parents, schools, and student's neighborhoods.

AddHealth data was collected by surveys in three stages. In the first stage, which took place in late 1994 and early 1995, about 90,000 students from 142 different schools were surveyed at their schools. These 90,000 some students represent all students in attendance at the 142 schools on the day of the survey. The survey collected information on student's socioeconomic characteristics, grades, extracurricular activities, attitudes, substance use, and friends. Regarding substance use, students reported how many days per month over the last year they smoked cigarettes, and how many days they drank alcohol. Regarding friends, the students were asked to nominate up to five close male friends and five close female friends from the school roster. Data for friends can be linked using the nominations.

In the second stage AddHealth conducted detailed in-home surveys with about 20,000 adolescents from all 142 schools between late 1994 and late 1995. All students on the school rosters were eligible to be selected for these Wave 1 in-home surveys, including those who were absent on the day of the initial in-school survey. The bulk of the Wave 1 in-home sample was formed by randomly choosing about 15 students from each school-grade-sex stratum. The Wave 1 in-home group also includes the complete sampling of two large schools, which is helpful for studying peer effects.

The Wave 1 in-home data set contains similar types of information to that of the in-school sample, but it is more comprehensive and detailed. For substance use it includes information on smoking, drinking, binge drinking, and marijuana smoking. Summary statistics for these variables by grade are listed in Table 1. In Wave 1 the students are asked to nominate up to five male friends and five female friends, as in the in-school survey.

A year after Wave 1 AddHealth re-interviewed about 15,000 of the Wave 1 respondents in a Wave 2 survey. The approximately 5,000 adolescents who were interviewed in Wave 1 but not in Wave 2 were either over-sampled adolescents in Wave 1 (e.g. disabled, or twins) or adolescents who were in 12th grade in Wave 1 and graduated from secondary school by Wave 2. In Wave 2 adolescents were asked a set of questions nearly identical to those from Wave 1.

In addition to the three stages of student surveys, AddHealth compiled information from school administrators, parents, and Census data on students' neighborhoods. Administrators provided information on school characteristics and policies. For each of the Wave 1 in-home respondents, a parent (usually the mother) answered questions about her background, her spouse's background, and the parents' relationships with the adolescent. The neighborhood data include socioeconomic variables such as mean income and unemployment for the block in which the respondent lives.

In addition to AddHealth, this study uses the National Educational Longitudinal Survey (NELS) for a segment of the analysis. The base year of the panel is used, 1988, when all of the 25,000 some respondents were in eighth grade. As discussed in the following section, NELS is more suited to the older/younger school experiment than AddHealth because its sample is taken from a much greater number of schools: about 1,000 compared to 142.

IV. Estimation Methods

A. Removal of user-friend natural experiment

$$\Delta Y_i = B_0 + B_1 (\Delta \overline{Z}_i^P) + B_2 \overline{Y}_{i,0}^P + B_3 X_{i,0} + e_i \tag{4}$$

The above equation summarizes the "removal of user-friend" natural experiment. This identification method isolates a plausibly exogenous change in individual i's peer group environment, $\Delta \overline{Z}_i^P$, between Wave 1 and Wave 2 of the survey, and assesses the impact of that change on the change on individual behavior, ΔY_i. Control variables include baseline peer behavior, $\overline{Y}_{i,0}^P$, and individual characteristics, $X_{i,0}$. For estimation purposes, the equation is modified in the standard way to set up a probit regression, as with all other regressions in this study.

$\Delta \overline{Z}_i^P$ is a continuous variable which represents the proportional change in the average behavior of the individual's friends *due to* the moving of friends between Wave 1 and Wave 2. Moving away is defined as changing residence to a different census tract. Census tracts are sub-units of counties typically consisting of about 10,000 people. Mover regressions with a stricter definition of moving, in which the adolescent must relocate to a new county, yield similar results (available on request). $\Delta \overline{Z}_i^P$ is constructed by first calculating the average behavior in Wave 1 of all individual i's friends, and then subtracting that value from the average behavior in Wave 1 of i's friends *excluding* those who move between Wave 1 and Wave 2, and finally dividing this difference by the first term: $\Delta \overline{Z}_i^P = \dfrac{\overline{Y}_{i,0}^{P,-M} - \overline{Y}_{i,0}^{P}}{\overline{Y}_{i,0}^{P}}$.

Given that $\Delta \overline{Z}_i^P$ is intended to measure a change in peer environment, one might wonder why the first term in its definition, $\overline{Y}_{i,0}^{P,-M}$, is calculated using Wave 1 values instead of Wave 2 values. The reason is that if Wave 2 values are used then $\Delta \overline{Z}_i^P$ is likely to be endogenous due to shared unobserved environmental factors. As defined, $\Delta \overline{Z}_i^P$ is equivalent to the change in peer environment due to the moving of friends, assuming that non-mover friend identities and behaviors remain the same between Wave 1 and Wave 2. Including the changes in friend identities and friend behaviors in the definition of $\Delta \overline{Z}_i^P$ would introduce an endogenous component that could confound the estimates.

In essence $\Delta \overline{Z}_i^P$ is an instrument for the change in peer environment. A two stage least squares model with this instrument is not estimated because AddHealth provides information on friend nominations for only a small sub-set of the respondents in Wave 2. Therefore one cannot construct the endogenous RHS variable of interest, the change in peer environment. Instead this analysis focuses on a single equation with the instrument replacing the endogenous RHS variable. The validity of the instrument, in turn, depends upon the extent to which it predicts the endogenous RHS variable of interest and is independent of the error term in the main regression equation. The results section that follows presents some simple evidence regarding these conditions.

The removal of user-friend natural experiment is also evaluated with an analogous graduator-friend approach. The strategy is to examine whether changes in individual i's substance use are related to changes in the substance use of his peer group due to (older) friends' graduating between Wave 1 and Wave 2. $\Delta\overline{Z}_i^P$ is defined in analogous fashion to how it is defined in the mover-friend experiment. The analysis is restricted to observations for which individual i himself does *not* graduate between Waves, in order to minimize the sources of change in environment besides the movement of friends. Thus, a friend who is one or more grades ahead of the individual and graduates between waves is equivalent to a mover-friend in the first setup.

B. "Older" schools versus "younger" schools natural experiment

$$Y_i = B_0 + B_1 O_i^S + B_2 X_i + e_i \tag{5}$$

The second natural experiment is of a very different sort. It uses cross-sectional data and focuses on schools as the peer group. It exploits the fact that 8th graders and 7th graders in the United States can be found in schools with adolescents in higher grades, "older schools" ($O_i^S = 1$), and also in schools with adolescents in lower grades, "younger schools" ($O_i^S = 0$). In the AddHealth data set, for example, 8th graders and 7th graders come from schools with the following grade spans: 8-12, 7-12, K-12, K-13+, 6-8, 7-9, 5-8, 5-7, and K-8. Which type of school these adolescents attend, "older" or "younger," can be used as an instrument for their peer group environments.

This instrument corresponds to the policy intervention of reconfiguring the grade spans of schools within a district. For example, the intervention could be changing a district from separate schools with grades K-5, 6-8, and 9-12 to just two schools with grades K-6 and 7-12.

Again, the extent to which the instrument truly mimics an exogenous policy intervention depends on the validity of the instrument, which is examined in the results sections.

A drawback of using AddHealth for this natural experiment is that it has a modest number of schools in its sample (142). Because the instrument varies at the school level and it is important to control for a variety of school characteristics, in effect there is relatively little sample variation. For that reason the analysis is also applied to a sample of 8th graders from NELS data. NELS contains over 1,000 schools in its sample, and has information on smoking behavior for 8th graders (but not on drinking or marijuana use).

V. Results

A set of auxiliary results merit attention before turning to the main results: replicating several of the existing methods with the AddHealth data yields results in line with those of the literature (tables available on request). For example, using the standard cross-sectional regression approach gives the following "peer effect" coefficients: 0.64 for smoking; 0.73 for drinking; 0.36 for binge drinking; and 0.43 for marijuana use. These coefficients remain similar when the average peer behavior is instrumented by peer characteristics, as in Gaviria & Raphael (2001). The magnitudes can be interpreted as follows, as an example: for each one percent increase in peer smoking prevalence, an individual becomes 0.64 percent more likely to smoke. These results alleviate the concern that AddHealth may be very different from prior data sets that have been used to estimate peer effects. Also, the results serve as a point of comparison for interpreting results using the natural experiments proposed in this study.

A. Removal of user-friend natural experiment

Table 2 demonstrates the amount of sample variation with which the first natural experiment can be estimated. Under the looser definition of movers, in which any move to a new census tract counts, there are 494 adolescents, or about 3 percent of the panel sample, who have at least one mover friend. With the stricter definition, in which only moves across counties count, there are 173 adolescents with mover friends. This section reports results for the looser definition of movers; the corresponding regressions with the stricter definition yield very similar results (available on request). Looking at grad friends instead of movers, one can see that there are 1,390 non-grad adolescents with at least one grad friend, or about 12 percent of the non-grad sample.

A potential concern is that, given the limitations of the data, these natural experiments do not have enough power to detect sizeable peer effects. In particular, one may wonder how much impact the removal of a user-friend has on an individual's peer environment, given that a peer group typically consists of several friends. To evaluate this concern, one can calculate the average impact on peer substance use level due to the removal of a user-friend, for those individuals who have user-friends who move or graduate. In the case of movers, the average impact is -64 percent for smoking, -75 percent for drinking, -88 percent for binge drinking, and -72 percent for marijuana use. In the case of graduators, the respective numbers are -66, -70, -78, and -88. Although peer groups may be incompletely reported, these numbers suggest that the removal of a user-friend does generally have a very large impact on the average peer environment. This can be explained by two factors: first, only a small fraction of adolescents uses substances frequently, as implied by Table 1; second, the peer groups being examined are based on nominations of "close friends" and only consist of one or a couple of friends in many cases.

The main results for the "removal of user-friend" experiment are reported in Table 3. There is some evidence of peer effects: for marijuana use in the mover experiment, and for smoking and binge drinking in the graduator experiment. The magnitudes of the coefficients can be interpreted as follows, for example. A 72 percent reduction in peer marijuana use due to a friend's moving (the average change due to a marijuana using friend's moving, as noted above) is associated with an 8.4 percent (0.1167 * 0.72) reduction in the probability that a non-marijuana user in Wave 1 will begin using marijuana by Wave 2. Similarly, a 66 percent reduction in peer smoking due to a friend's graduating corresponds to a 3.2 percent (0.478 * 0.66) reduction in the probability of beginning to smoke, and a 78 percent reduction in peer binge drinking due to graduating corresponds to a 4.0 (0.0519 * 0.78) reduction in the probability of beginning to binge drink.

How do these estimated effects compare to those in previous studies? The comparison is inexact because most previous studies are cross-sectional estimates. Then again, the comparison is appropriate in that previous studies typically give a dynamic interpretation to their results ("if the individual is exposed to a peer group with X percent more substance use, then the individual's probability of use increases by Y percent"). The numbers here are several times smaller than those obtained in most prior estimates, many of which find around a 1 percent increase in individual behavior for every 2 percent increase in peer behavior.

While the estimates may appear small compared to prior findings, they are certainly not small from a policy perspective. It is unlikely that parents or school administrators would consider them trivial. Consider, for example, that the average 11th grader non-smoker has a 17.5 percent probability of beginning to smoke by 12th grade. If this adolescent has an older smoker friend who leaves the school due to graduation during that time, then the probability of beginning to smoke is hypothesized to fall to 14.3 percent (17.5-3.2).

It should be noted, however, that the evidence for peer effects in Table 3 is mixed at best. Most of the specifications yield statistically insignificant coefficients. This fact could well be due to the relatively low power of the experiments, as the standard errors are relatively large compared to what might be considered meaningful effects. At the least, one firm conclusion is that even at the upper limit of 95 percent confidence intervals, the estimated peer effects are quite small compared to most prior estimates.

In the regressions discussed above, the moving and graduating of friends serve as instruments for changes in peer environments. Tables 4 and 5 provide some information concerning the validity of these instruments. The first table indirectly evaluates the exogeneity of the instruments. It addresses whether "treatment" adolescents – those whose average peer substance use changes due to friends' moving or graduating are systematically different than the "controls" – those without such changes. For most variables the groups do not have statistically different means. However, adolescents with mover friends have higher parents' education, and are less likely to drink or use marijuana at baseline than adolescents without mover friends. Non-grad adolescents with grad friends on average are in higher grades, are more likely to be female, and have more educated parents than non-grad adolescents without grad friends, but they exhibit no statistically significant differences in substance use and other variables.

The handful of differences observed in the preceding paragraph is cause for concern, but they do not necessarily invalidate the instruments. Note that the regressions in Table 3 control for each of the observable individual characteristics, including baseline substance, which is controlled for by looking at *changes* in behavior. The differences in means in Table 4 are problematic to the extent that they imply unobservable differences in determinants of *changes* in substance use. It is not clear how such a bias would emerge, based on the differences observed.

For the second key property of instruments, their value in predicting the endogenous RHS variable, one can look at more direct evidence. While AddHealth does not provide much information about who is friends with whom in Wave 2, it does ask respondents in both Waves the following question: “Out of your three best friends, how many smoke at least one cigarette per day?” Analogous questions are asked for “drink at least once per month” and “use marijuana at least once per month.” Table 5 tests whether the moving and graduating of friends between Waves predicts changes in respondents’ answers to these questions between Waves. We would expect the moving of friends who do behavior X to predict a negative change in the answers to the questions. It is less clear whether the moving of friends who do not do behavior X should predict a positive change in the answers.

The results in Table 5 are encouraging. In the mover regressions, two of three coefficients have the expected positive signs, with the smoking coefficient being significant at the 5 percent level (and the marijuana coefficient significant at the 10 percent level). For the graduator experiment, all three coefficients have the expected signs (third row in table), with the marijuana coefficient statistically significant at the 5 percent level.

B. “Older” schools versus “younger” schools natural experiment

Typically 8th graders and 7th graders attend schools with grades 6-8 or 7-9. For the purpose of this natural experiment the former is considered a “younger” school and the latter is neither “younger” nor “older” (and is therefore excluded). How common are “older” schools (e.g. grades 7-12) for 8th and 7th graders? Table 6 shows that in fact a substantial portion of these students attend “older” schools: 1,828 (16 percent of) 8th graders in 24 schools, and 1,807 (15 percent of) 7th graders in 22 schools.

Note that K-12 and K-13+ schools are counted as “older” schools in this analysis. The assumption behind this choice is that older adolescents are much more likely to exert peer effects for risky behaviors than younger adolescents, if peer effects exist at all. In any case, the natural experiment excluding K-12 and K-13+ schools yields similar results (available on request).

The regression results in Table 7 show little evidence in support of peer effects. Each cell corresponds to a separate regression, and only the coefficient for the older/younger school dummy (equal to 1 for “older”, 0 for “younger”) is presented. None of the coefficients on the “older” school dummy are in the expected (positive) direction at the 95 percent confidence level. The only coefficients significantly different from zero are negative (for marijuana use).

To connect these results more clearly to prior peer effects estimates, a two stage probit estimation is also performed in which the “older” school dummy serves as an instrument for average peer (schoolmate) behavior (results available on request). As expected, in the first stage the “older” school dummy strongly predicts higher average schoolmate substance use (t statistics are over 50). The second stage results show again that the “older” school experiment does not provide support for strong peer effects. For each substance use behavior, the upper bound of the 95 percent confidence interval for the estimated peer effect is well below typical estimates in the literature and the lower bound of the interval is well below zero.

One concern with this experiment is that some parents probably anticipate the negative influence of placing their adolescents in schools with older adolescents, and thus choose instead to place at-risk adolescents in “younger” schools. To the extent that being at-risk is unobservable, this behavior by parents would bias the coefficient on “older” schools downwards. This sorting issue is probably most problematic for private schools, which are more frequently chosen rather than assigned. For that reason the regressions in Table 7 are repeated, excluding private schools from the sample, and the results are essentially unchanged (available on request).

As with the mover-friend and grad-friend experiments, it is important to check the validity of the instruments to the extent possible. First one can look at whether adolescents in "older" schools differ in observable ways from adolescents in "younger" schools, and also whether "older" schools differ from "younger" schools in term of school characteristics. For most of the 20 variables examined the estimated means are not significantly different (table available on request). However, the following differences are statistically significant. As compared to 8th graders in "younger" schools, 8th graders in "older" schools are six percent more likely to be white and four percent more likely to live with a dad or other male guardian. As compared to 7th graders in "younger" schools, 7th graders in "older" schools are seven percent more likely to be white, have a 0.15 higher average GPA (out of 4.0), and are seven percent more likely to live with a dad or male guardian. As for school level characteristics, 8th grader "older" schools on average have larger class sizes, are less likely to be suburban (and more likely to be rural), and are more likely to be private than 8th grader "younger" schools. For 7th grader schools the same differences exist, plus the fact that "older" schools are less likely to be in the South than "younger" ones.

The differences described above suggest at least that "older" schools differ from "younger" schools in terms of certain observable characteristics. Repeating the exercise with the NELS data (not shown here) reveals a similar pattern, although there are much fewer variables to examine. The most notable differences between "older" schools and "younger" schools are the proportion of white students (84 percent versus 67 percent) and the proportion of public schools (50 percent versus 80 percent). However, as discussed previously, these differences only suggest an endogeneity problem with the instruments to the extent that they imply *unobserved* differences in determinants of substance use.

The second key feature of the instrument, the extent to which it predicts peer group environment, is clearly satisfied if adolescents interact with schoolmates from other grades. Some casual evidence in support of this assumption is that in AddHealth more than a one fourth of reported close friendships are across grades. From Table 1 one can easily see how dramatically higher the average level of substance use at an "older" school would be compared to a "younger" school.

The discussion above suggests that "older" schools probably provide 8th or 7th graders with worse peer environments (in terms of substance use) than "younger" schools, but the two types of schools might have some unobservable differences that must be addressed for the natural experiment to be valid. An approach to dealing with this issue is to compare 9-12 graders in "older" schools to 9-12 graders in the typical high school setup (grades 9-12 only). We would expect to see differences in substance use between these two comparison groups, controlling for observable characteristics, if there are fundamental, unobservable differences in "older" versus "younger" schools that are related to substance use. The results in Table 8 show that in fact there are such differences. Controlling for individual and school characteristics, 9-12 graders in "older" school setups are less likely to smoke (95 percent significance level), less likely to drink (90 percent level), and less likely to use marijuana (95 percent level).

It is possible that these general differences in substance use for "older" versus "younger" schools offset any increased use by 8th and 7th graders in "older" schools due to peer effects. To evaluate this story the regressions in Table 9 are estimated. Here all grades 7-12 are included, and the "older" school dummy serves to control for unexplained differences between "older" and "younger" schools that exist for all grades. The key RHS variables, then, are the interactions between the "older" school dummy and the 8th grade dummy, and between the "older" school dummy and 7th grade dummy. Again, the results indicate no solid evidence for positive peer effects in this natural experiment. Marijuana use for 8th graders is actually significantly lower in "older" schools, even after controlling for the general "older" school difference.

One final angle to consider in the "older" versus "younger" school approach is to focus on the jump in substance use between 8th and 9th grade. If peer effects from much older schoolmates are important, then one would expect substance use to increase more from 8th to 9th grade in typical school systems (grades 6-8 middle school, then 9-12 high school) than in combined systems (of which K-12 and 7-12 are the most common). This hypothesis is evaluated in Figure 1 for each of the four substance use behaviors. These numbers are based on cross-sectional regressions that control for all of the covariates included in earlier regressions. The results indicate that the 8th-to-9th grade jump in substance use is comparable for the two different school system types in the cases of smoking, drinking, and binge drinking. In the case of marijuana use, surprisingly the jump is actually greater for the combined system.

Cross-sectional comparisons such as these are vulnerable to the critique that there are unaccounted compositional differences by grade across the two different school system types, despite the inclusion of a detailed set of covariates. Thus, a useful check is to use the AddHealth one-year panel data to examine individual changes in substance use from 8th to 9th grade for adolescents in the two different school system types. Regressions controlling for the same covariates as earlier show that adolescents in the typical (6-8/9-12) school system do not experience any greater increase from 8th to 9th grade in smoking, drinking, or binge drinking than their counterparts in combined systems (results available on request). However, adolescents in the typical system are five percent more likely than their counterparts to increase their marijuana use from 8th to 9th grade, perhaps signifying some small evidence for peer effects.

Collectively, the results discussed above are mostly consistent with the conclusion that 7th and 8th graders are not adversely affected by being in schools with older adolescents, who have much higher substance use rates. However, the results might still be reasonably explained with a story that does not preclude the *potential* for peer effects from older to younger adolescents. For example, perhaps schools with grades 7-12 provide special protection for their youngest adolescents from the pressures and temptations to use substances. These schools might anticipate the peer effects from older students and take special measures to offset them. Indeed, anecdotal evidence suggests that some of these schools place physical barriers such as fences between their youngest and oldest students. Perhaps such protections effectively mitigate potential adverse peer effects. Evaluating this scenario would require even more detailed information about schools than AddHealth or NELS provide.

VI. Conclusion

What do we make of the results, as a whole? First, to be clear, this paper's results do not show that peer effects are generally small or unimportant for adolescent substance use. The natural experiments examine peer effects in specific contexts that correspond to specific policy interventions. It might be the case that peer effects are relatively small or do not exist in these contexts and at the same time they do exist in other ways not examined. For example, peer effects might act over longer periods than the time frame of the first experiment. Or in the case of the second experiment, adolescents in other grades might not exert peer effects even though adolescents in the same grade do.

For the particular kinds of peer effects studied here, the evidence suggests that, if anything, the effects are weaker than commonly estimated. Removing substance users from peer groups appears to have some impact on the probability of beginning substance use for the remaining adolescents in the group. Placing 8th and 7th grade adolescents in schools with high school age adolescents (grades 9-12) does not appear to lead to an increase in substance use on the order that would be predicted by prior studies, although some smaller effect would be consistent with the upper limits of the estimated confidence intervals.

This study clearly has potential limitations. Most notably, while the instruments used appear promising, the informal tests of their respective validities suggests at least that they are far from random interventions in terms of observable characteristics of adolescents. Thus it is possible that differences in unobserved characteristics could bias the estimations. Fortunately Add Health provides longitudinal information and a rich set of controls at the individual and school level, both of which help to attenuate this problem. The method used in Table 9 to evaluate the "older" school natural experiment, for example, also may attenuate the problem.

The empirical analysis of peer effects has recently taken a turn towards more defensible methods in other applications, such as investment behavior and academic achievement (Duflo & Saez, 2003; Hoxby, 2000; Hanushek, Kain, & Rivkin, 2002). This study aims to continue this trend in the context of adolescent substance use. In the future, convincing studies based on observational data will probably require detailed longitudinal information on substance use behaviors, which is currently in short supply, and will likely need to look at other natural experiments, such as the rezoning of school districts or college roommate assignments. In addition, as some authors have already suggested (Moffitt, 2001, Manski, 2000, Duncan &

Raudenbusch, 2001), randomized experiments hold perhaps the most promise for studying peer effects.

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Table 1: Proportions of adolescents using substances in last month, by grade

	smoked cigarettes at least four times per week during last month			drank alcohol at least once per "month or two" in past year			binge drank (5+ drinks in one occasion) at least once per "month or two" in past year			smoked pot at least once in last 30 days		
grade	mean	standard deviation	N	mean	standard deviation	N	mean	standard deviation	N	mean	standard deviation	N
7	0.04	0.17	2703	0.11	0.30	2711	0.05	0.21	2712	0.06	0.22	2683
8	0.06	0.23	2699	0.19	0.39	2712	0.1	0.29	2715	0.1	0.29	2672
9	0.13	0.32	2867	0.29	0.45	3611	0.16	0.36	3614	0.16	0.36	3552
10	0.13	0.32	3939	0.35	0.47	3961	0.2	0.39	3959	0.17	0.37	3883
11	0.16	0.35	3777	0.41	0.48	3800	0.26	0.42	3791	0.19	0.39	3721
12	0.2	0.36	3336	0.49	0.50	3349	0.32	0.44	3344	0.2	0.38	3293

Notes: a) data from AddHealth in-home Wave 1 sample.

Table 2: Number of observations for adolescents of different types

	Adolescents with at least 1 "mover" friend	Adolescents without any "mover" friends	Non-grad adolescents with at least 1 "grad" friend	Non-grad adolescents without any "grad" friends
all	494 (173)	14243 (14564)	1390	9737
and at least 1 "mover" ("grad") friend…				
smokes 4 + days/wk	92 (38)		250	
drinks 1+ day/M	120 (40)		448	
binge drinks 1+ day/M	77 (21)		313	
uses pot 1+ times/M	93 (35)		244	
Saturated sample only:				
all	262 (89)	3432	777	1991
and at least 1 "mover" ("grad") friend…				
smokes 4 + days/wk	44 (14)		160	
drinks 1+ day/M	45 (9)		277	
binge drinks 1+ day/M	33 (6)		207	
uses pot 1+ times/M	54 (19)		149	

Notes: a) numbers in parentheses are for stricter definition of "mover" (adolescent must move to different county, not merely to different census tract, to be counted).

Table 3: Results for "removal" (moving or graduating) of user-friend experiment

	Mover experiment		Graduator experiment	
model:	1	2	3	4
method:	probit	probit	probit	probit
dependent variable:	begin behavior?	quit behavior	begin behavior?	quit behavior
smoking:	N = 1707	N = 576	N = 1476	N = 516
Proportional change in average peer behavior due to friends' removal	-0.0334	0.1114	0.0478*	0.0173
	(0.0322)	(0.1689)	(0.0221)	(0.0714)
drinking:	N = 1222	N = 1075	N = 1069	N = 941
Proportional change in average peer behavior due to friends' removal	0.0831	-0.0189	0.0448	0.0349
	(0.0705)	(0.0980)	(0.0388)	(0.0320)
binge drinking:	N = 1694	N = 603	N = 1484	N = 523
Proportional change in average peer behavior due to friends' removal	0.0589	0.0621	0.0519*	0.0210
	(0.0375)	(0.1025)	(0.0141)	(0.0633)
pot using:	N = 1941	N = 308	N = 1689	N = 282
Proportional change in average peer behavior due to friends' removal	0.1167*	-0.1612	-0.0136	-0.0452
	(0.0530)	(0.1449)	(0.0232)	(0.0268)

Notes: a) saturated school sample only; b) other RHS variables: initial (Wave 1) average friend behavior, white, female, grade, doesn't live with mom, doesn't live with dad, religious involvement, income, parents' education, block median income, block unemployment rate, block percent HS grads, time between interviews; c) * -- significantly different from 0 at the 5-percent level; d) for probits, reported coefficients represent estimated derivative of probability with respect to independent variable; e) White-Huber robust standard errors (clustered by school) reported in parentheses.

Table 4: Evaluating "mover" and "grad" instruments

	kids with at least one mover friend			kids w/o any mover friends		(non-grad) kids w/ at least one grad friend			(non-grad) kids w/o any grad friends	
	N = 262			N = 3432		N = 777			N = 1991	
	mean	standard dev.		mean	standard dev.	mean	standard dev.		mean	standard dev.
Individual characteristics, Wave 1 in-home										
grade	9.44	1.44		9.61	1.49	10.07	1.24	*	9.43	1.35
female	0.54	0.50		0.51	0.50	0.54	0.50	*	0.49	0.50
white	0.62	0.49		0.58	0.49	0.58	0.49		0.61	0.49
lives with mom	0.94	0.24		0.94	0.23	0.96	0.20		0.95	0.23
lives with dad	0.76	0.43		0.74	0.44	0.76	0.43		0.77	0.42
parents' avg education	1.89	1.08	*	1.71	1.12	1.71	1.09		1.76	1.11
smokes?	0.11	0.32		0.12	0.33	0.14	0.34		0.12	0.33
drinks?	0.23	0.42	*	0.30	0.46	0.33	0.47		0.28	0.45
binge drinks?	0.13	0.34		0.18	0.38	0.21	0.41	*	0.16	0.37
uses pot?	0.10	0.30	*	0.16	0.36	0.17	0.37		0.15	0.35

Notes: a) * -- indicates that the surrounding means are statistically different at the 95 percent confidence level; b) saturated sample only; c) substance use variables defined as in Table 1.

Table 5: Correlation between "mover" ("grad") instrument and peer environment change

method:	probit	probit	probit
behavior X:	smoking (1+ cigarette/day)	drinking (1+ times/"month or two")	pot (1+ times/month)
dependent variable:	Decrease in "out of 3 best friends, how many do behavior X?"?	Decrease in "out of 3 best friends, how many do behavior X?"?	Decrease in "out of 3 best friends, how many do behavior X?"?
N	3458	3442	3458
number of "mover" friends who do behavior X	0.1417*	-0.0643	0.0901
	(0.0577)	(0.0612)	(0.0712)
number of "mover" friends who do not do behavior X	0.0086	-0.0004	0.0001
	(0.0193)	(0.0353)	(0.0253)
N (non-grads only)	2680	2666	2681
number of "grad" friends who do behavior X	0.0312	0.0032	0.0564*
	(0.0238)	(0.0176)	(0.0164)
number of "grad" friends who do not do behavior X	-0.0144	0.0143	-0.0074
	(0.0129)	(0.0068)	(0.0078)

Notes: a) saturated school sample only; b) other controls: grade, white, female; c) * -- significantly different from 0 at the 95 percent level; d) binge drinking not included here because kids were not asked "out of 3 best friends, how many…" for that behavior; e) Huber-White clustered (by school) standard errors in parentheses.

Table 6: Number of schools (and students) of each type

	grade span	*7-12*	*8-12*	*6-13+*	*7-13+*	*K-12*	*K-13+*	*total*
8th graders in "older" schools		8 (1151)	2 (177)	1 (47)	1 (110)	11 (339)	1 (4)	24 (1828)
7th graders in "older" schools		8 (1245)		1 (67)	1 (129)	11 (360)	1 (6)	22 (1807)
	grade span	*7-8*	*6-8*	*K-8*	*5-8*	*5-7*		*total*
8th graders in "younger" schools		11 (2533)	35 (6354)	7 (319)	1 (110)			54 (9316)
7th graders in "younger" schools		11 (2570)	35 (6775)	7 (308)	1 (160)	1 (105)		55 (9918)

Table 7: Do 8th and 7th graders in "older" schools engage in more substance use?

model:		1	2	3	4	5
method:		probit	probit	probit	probit	probit
dependent variable:		smokes? (4+ times per week)	smoke 1+cigarette per day? (NELS data)	drinks? (1+ time per "month or two")	binge drinks? (1+ times per "month or two")	uses pot? (1+ time per month)
key RHS variable:	school level controls?			*8th graders only*		
8th graders: dummy variable for going to school w/ older kids	no	-0.00003 (0.0104) (N=7581)	0.00019 (0.0048) (N=19310)	-0.0174 (0.0169) (N=7576)	-0.0001 (0.0200) (N=2328)	-0.0471* (0.0100) (N=2297)
8th graders: dummy variable for going to school w/ older kids	yes	-0.004 (0.0156) (N=6988)	0.0085 (0.0053) (N=19283)	-0.0163 (0.0187) (N=6983)	0.0086 (0.0227) (N=2304)	-0.0402* (0.0122) (N=2273)
				7th graders only		
7th graders: dummy variable for going to school w/ older kids	no	0.0056 (0.0088) (N=7140)		0.0082 (0.0181) (N=7121)	0.0077 (0.0141) (N=2380)	-0.0151 (0.0148) (N=2363)
7th graders: dummy variable for going to school w/ older kids	yes	0.0061 (0.0122) (N=6337)		0.021 (0.0218) (N=6627)	0.007 (0.0142) (N=2372)	-0.0211 (0.0129) (N=2355)

Notes: a) other RHS variables: female, age, white, doesn't live with mom, doesn't live with dad; school level controls: regional dummies (south, midwest, west (northeast omitted)); urbanicity dummies (urban, suburban (rural omitted)); average class size; private school dummy. b) for regression using NELS data (column 2), individual level controls are female, white, region, and school level controls urban/suburban/rural, private/private-religious/public; c) * - - significantly different from 0 at the 95 percent level; d) there are substantially fewer observations for binge drinking and pot use because these variables were only collected for the "in-home" sample (a random sub-sample of the full "in-school" sample); e) for probits, reported coefficients represent estimated derivative of probability with respect to independent variable; Huber-White clustered (by school) standard errors reported in parentheses.

Table 8: Comparing 9-12 graders in "older" schools (e.g. grades 7-12) to 9-12 graders in conventional setup (9-12 only)

model:	1	2	3	4
method:	probit	probit	probit	probit
observations:	39996	39902	10557	10390
grade span of sample:	9-12	9-12	9-12	9-12
dependent variable (DV):	smokes? (4+ times per week)	drinks? (1+ time per "month or two")	binge drinks? (1+ times per "month or two")	uses pot? (1+ time per month)
mean of DV:	0.159	0.357	0.207	0.167
"older" school dummy variable	-0.0111 (0.0165)	-0.0359 (0.0212)	-0.0006 (0.0183)	-0.0513* (0.0110)
R squared	0.072	0.045	0.063	0.062

Notes: a) same notes as in Table 7 apply.

Table 9: Do 8th and 7th graders in "older" schools engage in more substance use? (controlling for "older" school fixed effect)

model:	1	2	3	4
method:	probit	probit	probit	probit
observations:	53659	53545	15477	15258
grade span of sample:	7-12	7-12	7-12	7-12
dependent variable (DV):	smokes? (4+ times per week)	drinks? (1+ time per "month or two")	binge drinks? (1+ times per "month or two")	uses pot? (1+ time per month)
mean of DV:	0.135	0.300	0.171	0.144
older school dummy	-0.0093	-0.0361	0.0042	-0.0382*
	(0.0147)	(0.0202)	(0.0164)	(0.0097)
older school dummy * 8th grade	0.0025	0.0017	-0.018	-0.0427*
	(0.0214)	(0.0363)	(0.0273)	(0.0150)
older school dummy * 7th grade	0.0083	0.0017	0.0032	-0.0248
	(0.0319)	(0.0363)	(0.0361)	(0.0333)
R squared	0.093	0.085	0.098	0.084

Notes: a) sames notes as in Table 7 apply.

Figure 1: Covariate adjusted substance use rate by grade (relative to 7th grade)