What is a Peer? The Role of Network Definitions in Estimation of Endogenous Peer Effects

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Abstract

We employ a standard identification strategy from the peer effects literature to investigate the importance of network definitions in estimation of endogenous peer effects. We use detailed information on friends in the National Longitudinal Study of Adolescent Health Survey (Add Health) to construct two network definitions that are less *ad hoc* than the school-grade cohorts commonly used in the educational peer effects literature. We demonstrate that accurate definitions of the network seriously impact estimation of peer effects. In particular, we show that peer effects estimates on educational achievement, smoking, and drinking are substantially larger with our more detailed measures than with the school-grade cohorts. These results highlight the need to further understand how friendships form in order to fully understand implications for policy that alters the peer group mix at the classroom or cohort level.

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I. Introduction

The potential effect of peers and social networks on individual behavior is a source of debate in many policy contexts. Economists have explored effects of peers on school participation decisions (Cipollone and Rosolia, 2007; Gaviria and Raphael, 2001; Bobonis and Finan, 2009), on worker productivity (Mas and Moretti, 2009), on choice of medical school specialty (Arcidiacono and Nicholson, 2005), on utilization of prenatal care (Aizer and Currie, 2004), and on retirement savings behavior (Duflo and Saez, 2003) among others. Previous research addresses the difficult econometric issues in identifying social interactions. However, very few papers directly address the ad-hoc manner in which peer groups are often defined.

An informal sampling of the literature in educational peer effects shows the frequent use of school-grade cohorts as the peer group of interest (Appendix A). However, it is unclear whether school-grade cohorts are the true peer group in operation or whether they merely influence the composition of closer friendship ties, which in turn affect peer outcomes. There are relatively few examples of papers that do not use school-grade cohorts or classrooms as the relevant peer group. One important exception is work by Carrell, Fullerton, and West (2009) which uses random assignment to squadrons in the U.S. Air Force Academy to identify peer effects. In this work, the use of the squadron as the relevant peer group is carefully justified. Foster (2006) also uses an alternative definition of peer group, "all students residing in rooms that are on the same wing of a residence hall floor as the given student," though it is less clear why this should be the correct peer group of interest. A fuller discussion of the policy relevance of such findings follows below.

In this paper, we use the National Longitudinal Study of Adolescent Health (Add Health) to explore the way in which peer group definitions impact estimates of the effect of adolescent peers on propensities to achieve good grades in school, to smoke, and to drink. Our contributions are threefold. The first contribution is the estimation of peer effects using both self-reported friend groups and school-grade cohorts. To our knowledge, comparisons of estimates across network definitions within the same dataset have not yet been made in the literature. Our results suggest that behavior observed at the school-grade cohort level is essentially a reduced-form approximation of a two-step process in which students first sort themselves into peer groups and then behave in such a way that determines an outcome. As such, we argue the use of school-grade cohorts to estimate the full influence of peers on outcomes is flawed.

Our results underscore the need to explore models of peer influence that explain the effect of school-grade cohort composition on individual friendship ties. One such model (Weinberg, 2007) shows that under endogenous friendship formation, the composition of the school-grade cohort affects individual behavior non-linearly and depends on individual characteristics and behavior. Under such conditions, it is unclear what traditional peer effects estimates using school-grade cohorts imply for policy.

We further claim that the use of school-grade cohorts can lead to a number of serious econometric issues including: omitted variables bias, collinearity and weak instruments. For example, if the researcher specifies the network as a school-grade cohort, but does not address the substantial heterogeneity across schools by including a set of school dummy variables, the estimate of the endogenous peer effects will be contaminated by (among other factors) omitted school characteristics that are both positively correlated with the network's and the pupil's behavior. In the language of Manski (1995), these are called correlated effects. Researchers often address this source of bias by controlling for fixed school characteristics but in so doing, reduce much of the variation in the network behavior.

Because of this collinearity between the network's behavior and the school fixed effects, we show that it is difficult to detect endogenous effects even if they are present. We believe this may be why some recent studies such as Foster (2006) and Arcidiacono and Nicholson (2005) fail to detect a relationship between own behavior and network background characteristics. Furthermore, school fixed effects may address the problem of correlated effects, but they do not address simultaneity bias, also known as "the reflection problem" (Manski, 2005). To address this problem, researchers often use instrumental variables analysis. However, if the instrument is specified at the level of the school-grade cohort and if school-fixed effects are included in the estimations, the instrument will be weak. We provide evidence for this assertion.

The second contribution of this paper is that it is the first (of which we are aware) to explore alternative definitions of peer groups in a nationally representative sample of adolescents. Three papers which use alternative definitions of peer group, Sacerdote (2001), Kremer and Levy (2008), and Duncan et al (2005), use roommates at Dartmouth college and two large state universities respectively as the peer groups of interest. The two papers cited above, Carrell et al (2009) and Foster (2006), examine a sample of college students at two select institutions of higher education, the U.S. Air Force Academy and the University of Maryland respectively. However, no such papers study alternative definitions of peer groups on nationally representative samples of adolescents. The third contribution is to add to a relatively sparse (though growing) literature that examines the effect of peers on important adolescent health outcomes such as drinking and smoking. Hanushek et al (2003) notes, "due to limited outcome measures… all empirical work examines academic achievement. Many of the policy discussions and parental concerns focus on other outcomes including teen pregnancy, drug use… to name a few." Our paper helps fill this gap in the literature.

Unfortunately, the effort to estimate peer effects is complicated by a number of important empirical issues. These empirical issues are outlined most comprehensively in Charles Manski's seminal work (1995) and we borrow heavily from his framework to model our problem in the following sections. In particular, it is difficult to separately identify the endogenous peer effect from other contextual and correlated effects. For example, when we observe correlations between individual and peer group GPA's, we are not always able to discern whether this correlation arises because 1) individuals who get good grades tend to associate with friends who also get good grades, or 2) individuals are influenced by their peers to get good grades. If the first, policy affects only the outcomes of the targeted individuals, if the second, policy has an impact on outcomes that is magnified by a social multiplier effect.

Though we address these identification issues, the primary goal of this paper is *not* to solve the numerous empirical problems and precisely identify causal peer effects. We focus on the errors in estimation brought on by defining the peer group incorrectly. We do this by demonstrating that estimates vary widely based on differing definitions of the peer group. The widely varying estimates resulting from different network definitions highlight the need to carefully define and justify use of particular definitions when estimating peer effects.

Policy Implications

Educational policymakers are particularly interested in quantifying the effect of peers on adolescent behavior because of the long-term consequences of adolescent choices. As a result, the existence and size of peer effects hold important implications in a number of educational policy debates.[1] In comparing these effects across varying definitions of 'peer group', we see that definitions are important to identifying correct effects for policy. We show that peer effects

[1] Discussions on school choice policies and school integration address concerns over the influence of high-performing students on their lower-performing counterparts and vice versa (for example, Cullen, Jacob, and Levitt, 2006; Angrist and Lang, 2004). Both proponents and detractors of tracking policies within elementary and secondary schools ask these same questions (for example, Lefgren, 2004). The current debate over optimal school grade configurations asks whether or not older students have a negative impact on the educational outcomes of their younger peers (Bedard and Do, 2005). Discussions of single-sex versus co-educational classrooms involve questions of gender-based peer effects (Whitmore, 2005). And special education policymakers express concern over the effect of special education peers on the educational outcomes of their regular education counterparts and vice versa (for example, Hanushek, et al 2002). Furthermore, an important current strand of research in public policy, child psychology, and education documents concern that treatments which isolate and segregate youths engaged in risky behaviors may exacerbate the problem if these teens teach, encourage and reward further deviant behavior in their peers (for example, Dodge, Dishion, Lansford, 2006).

estimates on educational achievement, smoking, and drinking are consistently and substantially larger and more precisely estimated with our more detailed measures than with the school-grade cohorts. In fact, even when effects of school-grade cohorts are zero, we find significant effects of friends on outcomes. Because we believe the most significant peer influences occur at the level of individual friendship ties, estimates of school-grade cohort effects do not capture the true parameter of interest. Rather, correct estimates of peer influence depend on two separate effects: the sorting of students into friend groups and the subsequent effect of friends on their peers. The use of school-grade cohorts leads to reduced-form estimates of these combined effects. Our paper focuses on the second effect; we find significant friend effects even in the presence of much weaker and often non-existent school-grade cohort effects.

These results highlight the need to further understand how friendships form in order to fully understand implications for policy that alters the peer group mix at the classroom or cohort level. It may be true that school-grade cohorts are the policy tool over which policymakers have the most control. However, if the most significant peer influences occur at the level of individual friendship ties, the efficacy of educational policies that change the mix of peers at the school-grade cohort level will ultimately depend on how students sort into friend groups. For example, suppose students sort into friend groups based solely on geographical proximity. Then friend groups consist of students who ride the same bus together, who sit next to each other in the same classrooms, or who eat lunch together in the same period. In such a setting, a school policy that sorts students into classrooms or school-grade cohorts will affect formation of the relevant peer groups. As a result, peer effects estimates may be positive, not because the student is affected by the entire school-grade cohort but because the school-grade cohort affects his friend group.

On the other hand, if students choose friends based solely on gender or ethnicity, a shared family background, or shared interests without regard to proximity, policy that sorts students into classrooms or school-grade cohorts has a diminished or non-existent impact on student friend group formations. In such a setting, peer effects estimates at the school-grade cohort level may be nonexistent, not because of nonexistent peer effects, but because school-grade cohorts do not capture the relevant peers. Put another way, our results imply that policies that use peer effects to improve student outcomes must address friendship formation. For example, a school desegregation policy that does not lead to integrated friendships has limited peer effects. Our paper illustrates why developing models of friendship formation (such as Currarini, Jackson, and Pin, 2009) is crucial for the advancement of this literature.

The remainder of the paper is organized as follows: section II describes the data, section III summarizes the empirical approach and its accompanying estimation issues, and section IV describes the construction of peer group definitions in detail. In section V we discuss the main findings of the paper, and section VI concludes.

II. Data

We use data from the National Longitudinal Study of Adolescent Health (Add Health). The Add Health survey was conducted by the Carolina Population Center and is available for a nationally representative sample of students who were in seventh through twelfth grades in 1994. The 145 schools in the Wave I survey consist of pairs of sister schools. That is, if a particular high school is included in the survey, the corresponding feeder junior high or middle school is also included. If a school spanning seventh through twelfth grades is chosen for the survey, no sister school is included. We use data from the In-School survey which consists of 90,118 students. Of this sample, 4491 students are missing personal identifiers. Because this information is needed to collect information on peer behavior, these observations are dropped from our analysis. This brings our sample size to 85,627.

The Add Health survey is particularly useful for our purposes because of the extensive data on friendship networks. Using this information, we are able to define peer groups more accurately and precisely than has been possible in many previous studies. In each of the surveys, students are asked to nominate five female friends and five male friends. In almost all cases, students report fewer than five male and five female friends indicating they are not constrained in their choice of friends in their network by the ten-friend limit. These friend nominations include both friends in the same school as well as friends from outside of school.

Because we do not have information on friends outside of the respondent's school, we are unable to include them in our measures of average peer group behavior. However, the vast majority of friend nominations are to other students in the same school; on average only 15% of friend nominations are to friends outside of the respondent's school. There are also sizeable numbers of nominations to friends that are not found on the school rosters. In the In-School Wave I sample, for example, approximately eight percent of nominations are not found on the school rosters. This is due to the mixed use of nicknames and official names, students who are new to the school, or errors in school records. There are 19,005 students who have no friend information due to one of the above reasons or because they did not nominate any friends. These observations are dropped. As a result, our final sample consists of 66,622 students.

Students in each wave are asked detailed questions about their choices to smoke cigarettes and drink alcohol. They are also asked about their performance in school, including grades in English, math, science, and social studies. Using this information, we construct three variables: the number of times in the past year the student drank alcohol, the number of times the student smoked and the student's grade point average.[2] We measure the effects of peers on these three outcome variables.

Descriptive statistics and sample sizes for outcome variables and all other control variables are reported in Table 1A. For each variable employed in the analysis, we report two sets of descriptive statistics. The first corresponds to the original In-School data and the second corresponds to our extract. We see that students in our sub-sample reflect the students in the full dataset along most characteristics. The students in our extract smoke and drink slightly less than those in the overall sample, although the standard deviations on these variables are quite large and some variation is expected.

III. Estimation

Identification Issues

Manski (1995) notes one approach to estimating peer effects is given by:

$$y = \alpha + \beta\, E[y \mid x] + E[w \mid x]'\gamma + w'\lambda + u\,, \qquad (1)$$

where y is the outcome of interest (GPA, smoking, sexual behavior, or drinking), x is a vector of group characteristics, w is a vector of individual characteristics, and u is an idiosyncratic error term. Following Manski (1995), we assume that $E[u \mid x, w] = x'\delta$ and rewrite equation (1) as follows:

$$E[y \mid x, w] = \alpha + \beta\, E[y \mid x] + E[w \mid x]'\gamma + w'\lambda + x'\delta \qquad (2)$$

We observe that the behavior of individuals in groups can be conceptually separated into three strands of effects: *contextual effects*, *correlated effects*, and *endogenous peer effects*. *Contextual effects* ($\gamma \neq 0$) arise when "the propensity of an individual to behave in some way varies with the distribution of

[2] The variable for average number of drinks per year is a categorical variable ranging from 0 for never to 6 for nearly every day or every day. We transform it to be a cardinal variable which takes on a value of 0 for "never drank", 1.5 for "1 or 2 days in the past month," 7 for "3 to 12 times in the past 12 months," 30 for "two to three times a month," 78 for "1 or 2 days per week," 208 for "3 to 5 days per week," and 365 for "nearly every day." The recoded variable counts how many days the person drank alcohol in the past year. The grade point average is calculated by averaging grades from English, math, science, and social studies using a four point scale.

background characteristics in the group". For example, the tendency of student achievement to vary with socioeconomic background would be considered a contextual effect. *Correlated effects* ($\delta \neq 0$) describe "the propensity of individuals in the same group to behave similarly because they face similar institutional environments or have similar individual characteristics". For example, students in the same school may tend to achieve similarly because they face the same teachers and curriculum. *Endogenous peer effects* ($\beta \neq 0$) refer to the propensity of an individual to behave "in (ways that vary) with the prevalence of that behavior in that group". For example, a student may influence his friend to get good grades and the friend in turn may induce the student to get good grades.

As noted earlier, we are interested in identifying endogenous effects separately from correlated and contextual effects because of the potential policy implications of positive endogenous effects. In the presence of endogenous effects, policy will have a social multiplier effect. Absent endogenous effects, policy will have no such effect.

Empirical Approach

Our core empirical specification of equation (1) is:

$$y_{is} = \bar{y}_{is}\beta + x_{is}\lambda + \delta_s + \varepsilon_{is}, \qquad (3)$$

where y_{is} denotes an outcome for individual *i* in school *s* and x_{is} denotes a vector of individual *i*'s observable characteristics or observed heterogeneity, δ_s is a school effect and ε_{is} is a time-variant unobserved component to individual behavior.[3] The variable, $\bar{y}_{is}$, denotes the average behavior of individual *i*'s peers. We discuss the construction of $\bar{y}_{is}$ in greater detail in section IV. The parameter

[3] Note, we include δ_s in the specification because we believe omitting the school effects will lead to quite serious omitted variables bias. This need not pose any inconsistency between equations (2) and (3) if one includes the school fixed effects in w .

of interest β estimates the extent to which peers influence an individual's behavior. Note that we are deliberately omitting the average peer group background characteristics from equation (3).

Our first set of estimations uses OLS and includes a set of school and grade dummies and a comprehensive set of covariates[4] in the regression equation. These are our benchmark estimations. The inclusion of school fixed effects helps to mitigate endogeneity bias stemming from omitted correlated effects. However, it does not account for any unobserved individual-level heterogeneity within schools that is omitted from x_i but is also correlated with average group behavior. In addition, the procedure does nothing to solve the biases resulting from the simultaneous determination of average peer behavior and the student's outcome, also known as the reflection problem.

Our second set of estimations uses average background characteristics of the group member's parents to instrument for average group behavior while controlling for a complete set of school dummies and exogenous covariates. In order to identify equation (3) using instrumental variables estimation, we assume the absence of contextual effects (i.e. $\gamma = 0$). This type of exclusion restriction is discussed by Manski (1995) and is used quite frequently in the peer effects literature (*e.g.* Gaviria and Raphael, 2001).[5]

We provide a simple illustrative example that explains the conditions under which our instrument is valid. First, consider a simple model describing the behaviors of two peers:

$$y_1 = y_2\beta + F(z_1) + u_1 \quad \text{(4a)}$$

[4] The set of control variables includes dummy variables for own parental education, living with their father and mother, gender, race, grade and health status.

[5] Another practice which is common in this literature is to regress own outcomes on averages of group background characteristics (e.g. Arcidiacono and Nicholson 2005 and Foster 2006). Essentially, this is the reduced form of what we do. Our approach allows for estimates that are more easily interpretable. Provided the identifying assumptions hold, we identify β, whereas the other studies identify $\beta\pi$ where π denotes the reduced-form relationship between average peer behavior and the contextual effects. Because $\beta\pi$ is harder to interpret than β, we prefer our approach.

and

$y_2 = y_1\beta + F(z_2) + u_2$ (5a)

where the variable's y_i are the outcomes and the variables z_i are exogenous parental characteristics (i.e. orthogonal to $u_{i)}$. In this game, the peers do not choose each other. Equations (4a) and (5a) can be viewed as reaction functions in a two-player game. In this simple model (in which we have assumed no contextual effects), the exogenous parental characteristics of the friend can serve as instruments for the friend's behavior.

Now let's consider a more general version of the model in which the students can choose to be friends. Let $\bar{u}_i = \upsilon_i(u_1, z_i)$ denote the reservation utility to student *i* to not befriending the other student. We assume that it depends on her unobserved characteristics and her parental background. Next, let $v_{i,-i} = \varpi(u_i, u_{-i}, z_i)$ denote the utility of a match between student *i* and student *–i*. Then a generalized model is given by:

$y_1^* = y_2^*\beta + F(z_1)^* + u_1^*$ (4b)

And

$y_2^* = y_1^*\beta + F(z_2)^* + u_2^*$ (5b).

where we adopted the notation that $x_i^* = x_i * 1(v_{i,-i} > \bar{u}_i)$. Equations (4b) and (5b) tell us that if students choose their friends based on their friend's behaviors but not their parental background then peer parental background characteristics will still be a valid instrument for peer behavior.

In the set of IV estimations, we run two separates groups of results using two separate sets of instrumental variables. The instruments in the first group are averages of dummy variables for whether or not the mothers and fathers of the peer group members have college degrees. The second are averages of dummy variables for whether or not the student lives with his mother and father. For each of our estimations, we have two instrumental variables (one for each parent) and one endogenous variable and so can conduct over-identification tests.

In addition, we maintain that parental background characteristics within the peer group will affect own behavior only through peer behavior but will have no *direct* effect on own behavior, at least for the majority of the sample. So, for example, peer parental education influences peer smoking which, in turn, influences, own smoking. However, peer parental education is assumed to have no direct impact on own smoking. Though one can certainly conceive of scenarios in which our identifying assumption will break down, we posit that the primary avenue through which peer parental background characteristics affect own behavior is through peer behavior.

As discussed above, another threat to our identification is that students may choose friends based on their peer's parent's background characteristics. If so, these contextual effects belong in the main regression equation. Omitted contextual effects create selection bias in our estimates. We address this selection issue by including a comprehensive set of own individual characteristics.[6] Moreover, peer behavior is included in the main estimation equation; because peer behavior is an excellent proxy for peer parental background, we submit that this solves much of the problem of omitted contextual effects. Once we include a comprehensive set of individual characteristics and control for peer behavior, we are less worried about the prospect of peer background characteristics entering the main model. In addition, all models are saturated with a complete set of school and grade dummy variables, which addresses a major source of heterogeneity in the data.

Finally, to further shed light on some of the identification concerns discussed above, we conduct Hansen's J-test. We concede that over-identification tests do not prove the validity of the instruments. However, they provide additional reassurance that our identifying assumption is reasonable.

[6] The set of control variables includes dummy variables for own parental education, living with their father and mother, gender, race, grade and health status.

IV. Defining the Networks

The estimations discussed above are further complicated by measurement error in $\bar{y}_{is}$ stemming from two possible sources: reporting errors in peer behaviors and errors in the definition of peer groups. Reporting error refers to the classical measurement error resulting from mistakes in reporting and recording data. The definitional component of measurement error stems from incorrect or imprecise definition of peer groups. One of the main weaknesses of the peer effects literature is this inability to precisely define the peer group. Manski (1995) states, "Researchers studying social effects rarely offer empirical evidence to support their specifications of reference groups." Definitions of peer groups are often ad-hoc (see Appendix A) and depend more on availability of data than a theoretically justified definition of "peers".

To better understand the role that network definitions play in the identification of endogenous peer effects, we estimate equation (3) using four different network definitions. Our first definition of the peer group (we call this definition "Inner Ring Friends") uses information from the Add Health friendship network to construct average levels of smoking, drinking and grade point averages across self-nominated friends. This definition only includes friends directly nominated by the respondent and is limited to at most five male and five female friends. In Table 1B, we see that the average network size for this definition is 5.17.

Our second network definition (which we call "Outer Ring Friends") uses all friends of inner ring friends, excluding all inner ring friends. We also exclude self from the outer ring. Also, if a student appears in the outer ring more than once (i.e. has been nominated by more than one inner ring friend), we only count him once in our constructed average peer behavior variable. In Table 1B, we see that the average size of the outer ring is 9.91.

Our third network definition (which we call "Extended Friends") is the union of the inner and outer rings. In Table 1B, we see that the average size of the extended friends network is 15.09. Note that the maximal network size of the extended friends network in our data is 42, so these networks can be quite large.

Our fourth definition of the peer group is a school-grade cohort. This is the most commonly used definition in the educational peer effects literature and includes all individuals in the same grade within the respondent's school. In our estimations, we again exclude the individual from his own network.

We use the above definitions to calculate average peer outcomes for each student in our sample. Table 1C reports the mean and standard deviation for average peer behavior for the three outcomes that we consider. We see that average outcome variables are very similar across network definitions.

Finally, we consider the degree of variation in peer average outcomes within schools. In Figure 1, we calculate the difference between $\bar{y}_{is}$ and the school-level or average of $\bar{y}_{is}$. We then plot the non-parametric density estimates of these differences for three separate outcome variables: GPA, smoking, and drinking. Each plot contains three densities each corresponding to three network definitions: inner ring friendship ties, extended friendship ties, and school-grade cohorts. We see that there is little variation when networks are defined as school-grade cohort but substantial variation when networks are defined based on inner ring friend nominations and extended friendship ties. This emphasizes significant problems with using the school-grade cohort definition to estimate peer effects. On one hand, inclusion of school dummies allows researchers to account for the unobserved school-level heterogeneity that is almost certainly correlated with both own and group behavior. On the other hand, it limits variation in the peer group variable substantially and leads to less efficient results and a possible failure to detect endogenous peer effects even when they are present. In other words, tests based on these definitions have low power. Finally and more importantly, these definitions potentially cause instruments to be weak due to collinearity between the instruments (if they are also defined at the school grade cohort level) and the school fixed effects.

V. Results

Tables 2, 3, and 4 present peer effects estimates for our three outcome variables: grade point average, smoking, and drinking, respectively. OLS estimates are in tables 2A, 3A, and 4A while instrumental variables estimates are in tables 2B, 3B, and 4B. As stated earlier, the OLS regressions include school and grade fixed effects and control for a comprehensive set of individual characteristics. The school fixed effects mitigate problems with omitted correlated effects but do not solve endogeneity biases stemming from individual-level omitted variables that are correlated with network characteristics. In addition, the OLS regressions do not attempt to resolve biases resulting from the simultaneity or reflection problem.

The IV regressions in tables 2B, 3B, and 4B use peers' parental background characteristics to instrument for average peer group behavior. We run two separate sets of IV regressions using two separate sets of instruments: first, mother's education and father's education, second, whether or not the mother lives at home, and whether or not the father lives at home. In order to maintain validity of our instruments, we assume that peers' parental characteristics affect own outcomes only through peer behavior. In other words, friends' parents' background influences friends' behavior, which in turn influences own behavior. But friends' parents' background does not influence own behavior directly. We may also worry that students choose friends based on parental characteristics. In Manski's language, these are contextual effects and will lead to biases if omitted. To account for this selection, we include a comprehensive set of individual control variables. Furthermore, we include, peer behavior in the main estimation equation, an excellent proxy for peer parental background.

Broadly stated, there are four main patterns of results in the OLS and IV estimates. First, the estimates using school grade cohorts are (with one exception in Table 3B, column 7) significantly smaller than results using the inner ring friends. This is true across all three outcome variables and across both OLS and IV estimates. The magnitude of the differences in the OLS point estimates between inner ring estimates and school-grade cohort estimates is modest for grade point average (for example, .45 or .39 versus .38) but quite large for smoking (.59 or .52 versus .25) and drinking (.25 or .23 versus .04). These differences hold and are amplified in the IV results. For example, Table 2B shows that IV estimates for the effect of the inner ring friends on own grade point average range from .65 to .83. However, similar IV estimates using school grade cohorts are not statistically significant and point estimates are very small in magnitude (-.09 or .03). Results for smoking and drinking outcome variables are similarly large, positive and statistically significant for inner ring friends while zero or negative and not statistically significant for school-grade cohorts. It is also important to point out the F-tests that our excluded instruments are zero in the first stage regression are uniformly much smaller for school-grade cohorts than for the other definitions suggesting that weak instruments may be a problem with this definition.

Second, the estimates using inner ring friends are more precisely estimated than the estimates using school grade cohorts. For example, in Tables 2B, 3B, and 4B, the t-statistics on the inner ring estimates are (with one exception, Table 4B, column 3) all larger than three and range as high as 24.70, while those on the school grade cohorts are (with one exception, Table 3B, column 7) all less than one in magnitude. These results are consistent with Figure 1 which shows a markedly smaller amount of within-school variation in the outcome variables across school-grade cohorts. Third, estimated effects of outer ring friends on outcomes are uniformly smaller in magnitude than the estimated effect of inner ring friends. Again, this is true across all three outcome variables and across both OLS and IV estimates. In columns 3 and 4 of Tables 2B, 3B, and 4B, we see the point estimates are significantly smaller for outer ring friends than inner friends (.07 and .23 versus .80 and .65 for grade point average; -.43 and .03 versus 1.16 and .86 for smoking; and .14 and .13 versus .58 and .55 for drinking). Furthermore, the estimates on the outer ring friends are often statistically indistinguishable from zero. These results indicate a distinct falling off of peer influence in increasingly distant friendships. They also point to distinct sub-patterns in spheres of peer influence that may be lost in traditional school-grade cohort estimates.

Fourth, the estimated effect of extended friends is larger than any of the other effects taken individually. For example, Tables 2B, 3B, and 4B show that effects of extended friends (inner ring plus outer ring friends taken together) on own outcomes and behavior are large, positive, and highly statistically significant. However, even these estimates obscure the different pattern of influence between inner ring and outer ring friends. Taken as a whole, these estimates of effects of extended friends on own behavior may be interpreted as indicating significant peer influence on grade point average as well as smoking and drinking behavior. However, policymakers may be interested to know that inner ring friends drive the estimates, while outer ring friends have little or no influence.

VI. Conclusions

In this paper, we explore the role of peer group definitions in the estimation of endogenous peer effects in GPA, smoking, and drinking using the Add Health Survey. Under appropriate identifying assumptions we provide evidence of endogenous peer effects in school performance and the propensity to smoke and drink. But we also find that the magnitudes and precision of estimates differ quite widely depending on the definition of peer groups. Furthermore, we provide evidence that errors in defining the peer network correctly may lead to underestimates of peer effects in many contexts. Because the definition of peer groups is often ad-hoc in the existing literature, we find these results highlight the need to justify use of particular definitions.

In particular, we contend that the use of school-grade cohorts as the definition of peer network in the school peer-effects literature is problematic. We claim that estimates based on the school-grade cohorts are essentially reduced-form approximations of a two-step process in which students first sort themselves into friend groups and then are influenced by these friends to choose behaviors which lead to outcomes. We see in our results that friends do matter. So what is important to policymakers may not be whether or not peer effects exist, but rather an understanding of how policies affect formation of these friend groups.

Though it may be true that school-grade cohorts are the policy tool over which policymakers have the most control, it is untrue that policy based on estimates of peer-effects at the school-grade cohort level will necessarily be most effective. Due to the lack of a canonical model describing how students choose friends, it is unclear whether estimates of a linear model based on school-grade cohorts provide an adequate approximation of the more complicated process. In order for researchers to determine if school-grade cohort effects provide an adequate approximation, it is necessary to compare these to peer effects estimates based on actual peer networks. Our work presents a first step in this direction.

Appendix A. Peer group definitions in the education literature.

Study	Definition of Peer Group
Gaviria and Raphael, 2001	School cohorts
Angrist and Lang, 2004	School-grade cohorts
Arcidiacono and Nicholson, 2005	School-grade cohorts
Carrell, Malmstrom and West, 2008	School-grade cohorts
Hanushek, Markman, Kain and Rivkin, 2001	School-grade cohorts
Hoxby, 2000	School-grade cohorts
Lavy, Paserman, and Schlosser, 2007	School-grade cohorts
Burke and Sass, 2006	Classroom
Vigdor and Nechbya, 2004	Classroom
Duncan et al, 2005	Roommates
Sacerdote, 2001	Roommates
Foster, 2006	Residents of same wing of a residence hall floor
Carrell, Fullerton, Gilchrist, and West, 2007	Squadron

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Table 1A. Summary Statistics

Variable	In-School Survey	Our Extract	Variable	In-School Survey	Our Extract
Age	14.99	14.91	Live with Mother	0.92	0.93
	(1.72)	(1.71)		(0.27)	(0.26)
	N=89712	N=66454		N=87140	N=65418
Black	0.19	0.18	Live with Father	0.76	0.77
	(0.39)	(0.38)		(0.43)	(0.42)
	N=90118	N=66621		N=86854	N=65289
White	0.61	0.64	Self-Reported Health Status[2]	2.09	2.08
	(0.49)	(0.48)		(0.94)	(0.92)
	N=90118	N=66621		N=85441	N=64479
Male	0.50	0.47	GPA	2.86	2.89
	(0.50)	(0.50)		(0.79)	(0.78)
	N=89387	N=66255		N=56844	N=43675
Mother's education[1]	0.32	0.32	# days drank last year	21.11	17.66
	(0.47)	(0.47)		(60.78)	(52.13)
	N=70026	N=53944		N=83952	N=63531
Father's education[1]	0.38	0.38	# days smoked last year	48.98	44.39
	(0.49)	(0.49)		(115.50)	(109.87)
	N=56640	N=43852		N=83952	N=63392

Notes: Standard deviations in parentheses.
This table summarizes network information for the in-school samples.
[1] Indicator for whether or not parent has a college education.
[2] This is a categorical variable used to describe the pupil's own health. A one is excellent and a five is poor.

Table 1B. Average numbers of friends in the network

Network Definition	Average Friend Number	Max # of Friends
Friends – Inner Ring	5.17 (2.62)	10
Friends – Outer Ring	9.91 (6.21)	33
Extended Friends – Inner + Outer	15.09 (8.55)	42
School Grade Cohorts	143.59 (139.93)	697

Note: Standard deviations are reported in parentheses.

Table 1C: Peer Group Averages for Outcome Variables

	GPA	Smoking	Drinking
Friends – Inner Ring	2.91 (0.59)	46.41 (77.8)	19.91 (35.17)
Friends – Outer Ring	2.92 (0.45)	45.36 (54.19)	20.41 (22.92)
Extended Friends – Inner + Outer	2.92 (0.45)	45.36 (54.19)	20.59 (24.45)
School Grade Cohorts	2.89 (0.30)	43.24 (26.19)	17.16 (9.10)

Note: Standard deviations are reported in parentheses.

Table 2A: GPA – OLS Results

	(1)	(2)	(3)	(4)
Average GPA			-	
- Inner Ring	0.45 (38.12)	0.39 (35.82)	-	-
- Outer Ring	-	0.25 (17.21)	-	-
- Extended Friends	-	-	0.60 (30.09)	-
- School Grade Cohorts	-	-	-	0.38 (8.59)
Male Dummy	-0.19 (-22.43)	-0.19 (10.17)	-0.20 (-22.76)	-0.21 (-22.41)
White Dummy	0.10 (8.05)	0.09 (6.85)	0.09 (7.12)	0.14 (9.59)
Black Dummy	-0.05 (-2.61)	-0.02 (-1.32)	-0.04 (-2.12)	-0.11 (-5.16)
Asian Dummy	0.23 (10.67)	0.20 (10.17)	0.21 (9.65)	0.31 (10.65)
SRHS = Excellent	0.28 (5.66)	0.26 (5.05)	0.32 (5.79)	0.41 (7.42)
SRHS = Very Good	0.23 (4.60)	0.21 (4.08)	0.26 (4.71)	0.34 (6.16)
SRHS = Good	0.07 (1.35)	0.05 (1.06)	0.09 (1.73)	0.13 (2.48)
SRHS = Fair	-0.08 (-1.58)	-0.09 (-1.73)	-0.06 (-1.04)	-0.04 (-0.77)
One Parent w/ College Degree?	0.20 (23.32)	0.18 (22.34)	0.20 (23.06)	0.26 (23.12)
Lives with Mother?	0.15 (6.56)	0.14 (6.33)	0.14 (6.23)	0.20 (7.89)
Loves with Father?	0.09 (8.77)	0.08 (7.70)	0.09 (9.75)	0.12 (11.47)
R2	0.3018	0.3129	0.2844	0.2225
N	35649	34841	36881	37423

Notes: All standard errors adjust for clustering by schools. All models include a comprehensive set of school and grade dummies. t-statistics are reported in parentheses. Sample sizes are smaller than the original because of substantial missing gpa information.

Table 2B: GPA – IV Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average GPA								
- Inner Ring	0.83 (24.70)	0.75 (14.57)	0.80 (9.75)	0.65 (4.22)	-	-	-	-
- Outer Ring	-	-	0.07 (0.67)	0.23 (1.19)	-	-	-	-
- Extended Friends	-	-	-	-	0.91 (23.01)	0.90 (18.45)	-	-
- School Grade Cohorts	-	-	-	-	-	-	-0.09 (-0.38)	0.03 (0.25)
Excluded Variables[1]	IV1	IV2	IV1	IV2	IV1	IV2	IV1	IV2
F-Test for Weak Instruments	189.18 p=0.0000	154.79 p=0.0000	-	-	168.87 p=0.0000	133.57 p=0.0000	4.62 p=0.0114	8.51 p=0.0003
Hansen's J-Statistic								
- Hetero Robust[2]	0.879 p=0.3485	0.081 p=0.7762	-	-	-	-	0.219 p=0.6396	0.706 p=0.4008
- Hetero Non-Robust	0.984 p=0.3212	0.097 p=0.7555	2.435 p=0.2960	1.973 p=0.3729	3.656 p=0.0559	1.079 p=0.2989	0.222 p=0.6375	0.707 p=0.4008
R2	0.2527	0.2674	0.2657	0.2886	0.2743	0.2708	0.2168	0.2193
N	33918	35604	33212	34804	36505	36866	37416	37423

Notes: All standard errors adjust for clustering by schools. All models include the same set of controls as the OLS regressions. t-statistics are reported in parentheses.

[1] IV1 uses the average number of mothers and fathers in the network with college degrees as instruments. IV2 uses the average number of pupils in the network living with mothers and fathers as instruments. Note that every column except for columns 3 and 4 has two excluded instruments. Columns 3 and 4 have four excluded instruments because we calculated the averages for both the inner and outer rings separately.

[2] Some of the heteroskedastic robust J-statistics could not be calculated because some of the covariance matrices could not be inverted.

Table 3A: Smoking – OLS Results

	(1)	(2)	(3)	(4)
Average Smoking			-	
- Inner Ring	0.59	0.52	-	-
	(36.93)	(34.13)		
- Outer Ring	-	0.25	-	-
		(13.68)		
- Extended Friends	-	-	0.78	-
			(30.84)	
- School Grade Cohorts	-	-	-	0.25
				(4.13)
Male Dummy	4.88	5.00	5.24	5.25
	(4.65)	(4.63)	(4.74)	(4.42)
White Dummy	8.56	7.13	8.16	15.45
	(4.27)	(4.98)	(4.18)	(5.41)
Black Dummy	-12.38	-9.08	-10.27	-28.35
	(-6.71)	(-4.98)	(-5.43)	(-9.50)
Asian Dummy	1.32	2.06	1.25	-1.78
	(0.62)	(1.02)	(0.59)	(-0.63)
SRHS = Excellent	-115.87	-115.30	-117.15	-140.67
	(-12.98)	(-12.95)	(-12.47)	(-12.53)
SRHS = Very Good	-106.77	-106.62	-107.72	-128.14
	(-12.23)	(-12.24)	(-11.68)	(-11.78)
SRHS = Good	-77.00	-78.02	-76.45	-86.93
	(-9.92)	(-9.40)	(-8.74)	(-8.64)
SRHS = Fair	-43.95	-45.23	-42.92	-45.35
	(-5.23)	(-5.41)	(-4.82)	(-4.53)
One Parent w/ College Degree?	-2.05	-1.22	-2.14	-7.25
	(-2.28)	(-1.38)	(-2.39)	(-5.90)
Lives with Mother?	-12.96	-10.74	-12.54	-22.00
	(-3.91)	(-3.20)	(-3.98)	(-5.98)
Lives with Father?	-7.66	-7.19	-8.06	-12.08
	(-6.42)	(-6.27)	(-6.60)	(-8.42)
R2	0.2799	0.2911	0.2568	0.1262
N	53854	52513	54014	54371

Notes: All standard errors adjust for clustering by schools. All models include a comprehensive set of school and grade dummies. t-statistics are reported in parentheses.

Table 3B: Smoking – IV Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average Smoking								
- Inner Ring	0.86 (13.44)	0.87 (12.47)	1.16 (3.16)	0.86 (4.05)	-	-	-	-
- Outer Ring	-	-	-0.43 (-0.96)	0.03 (0.10)	-	-	-	-
- Extended Friends	-	-	-	-	0.95 (14.53)	1.03 (12.32)	-	-
- School Grade Cohorts	-	-	-	-	-	-	1.00 (6.25)	-0.28 (-0.25)
Excluded Variables[1]	IV1	IV2	IV1	IV2	IV1	IV2	IV1	IV2
F-Test for Weak Instruments	30.16 p=0.0000	49.34 p=0.0000	-	-	19.74 p=0.0000	31.01 p=0.0000	3.04 p=0.0512	0.61 p=0.5444
Hansen's J-Statistic								
- Hetero Robust[2]	0.159 p=0.6902	-	0.644 p=0.7249	-	-	-		
- Hetero Non-Robust	0.176 p=0.6748	0.432 p=0.5111	0.759 p=0.3836	3.746 p=0.1537	0.005 p=0.9447	4.730 p=0.0297	0.023 p=0.8795	2.821 p=0.093
R2	0.2638	0.2490	0.1966	0.2591	0.2583	0.2446	0.1230	0.1260
N	50283	53712	49084	52392	53153	53962	54362	54371

Notes: All standard errors adjust for clustering by schools. All models include the same set of controls as the OLS regressions. t-statistics are reported in parentheses.

[1] IV1 uses the average number of mothers and fathers in the network with college degrees as instruments. IV2 uses the average number of pupils in the network living with mothers and fathers as instruments. Note that every column except for columns 3 and 4 has two excluded instruments. Columns 3 and 4 have four excluded instruments because we calculated the averages for both the inner and outer rings separately.

[2] Some of the heteroskedastic robust J-statistics could not be calculated because some of the covariance matrices could not be inverted.

Table 4A: Drinking – OLS Results

	(1)	(2)	(3)	(4)
Average Drinking			-	
- Inner Ring	0.25	0.23	-	-
	(16.39)	(16.20)		
- Outer Ring	-	0.16	-	-
		(8.57)		
- Extended Friends	-	-	0.35	-
			(14.11)	
- School Grade Cohorts	-	-	-	0.04
				(0.58)
Male Dummy	10.20	10.20	10.59	10.91
	(15.47)	(15.21)	(15.44)	(15.87)
White Dummy	-0.73	-0.31	-0.63	-0.47
	(-0.88)	(-0.39)	(-0.79)	(-0.52)
Black Dummy	-2.16	-1.66	-2.21	-2.51
	(-1.77)	(-1.41)	(-1.92)	(-1.89)
Asian Dummy	-1.37	-0.28	-1.05	-2.81
	(-0.98)	(-0.21)	(-0.76)	(-1.88)
SRHS = Excellent	-72.57	-72.75	-72.99	-74.19
	(-9.13)	(-8.94)	(-9.19)	(-9.39)
SRHS = Very Good	-70.80	-71.15	-71.27	-72.14
	(-8.86)	(-8.70)	(-8.93)	(-9.10)
SRHS = Good	-62.53	-63.09	-62.85	-62.92
	(-7.90)	(-7.77)	(-7.93)	(-8.03)
SRHS = Fair	-51.02	-52.27	-51.58	-50.87
	(-6.38)	(-6.39)	(-6.45)	(-6.42)
One Parent w/ College	-0.54	-0.46	-0.57	-1.25
Degree?	(-1.05)	(-0.94)	(-1.10)	(-2.28)
Lives with Mother?	-2.56	-2.41	-2.11	-3.54
	(-2.00)	(-1.94)	(-1.63)	(-2.72)
Loves with Father?	-3.20	-2.75	-3.13	-3.69
	(-4.97)	(-4.25)	(-4.99)	(-5.72)
R2	0.0898	0.0940	0.0869	0.0632
N	53736	52404	53911	54269

Notes: All standard errors adjust for clustering by schools. All models include a comprehensive set of school and grade dummies. t-statistics are reported in parentheses.

Table 4B: Drinking – IV Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average Drinking								
- Inner Ring	0.75	0.57	0.58	0.55	-	-	-	-
	(5.07)	(6.82)	(1.31)	(3.55)				
- Outer Ring	-	-	0.14	0.13	-	-	-	-
			(0.28)	(0.55)				
- Extended Friends	-	-	-	-	0.89	0.64	-	-
					(7.12)	(5.77)		
- School Grade Cohorts	-	-	-	-	-	-	-1.58	-1.66
							(-0.15)	(-1.02)
Excluded Variables[1]	IV1	IV2	IV1	IV2	IV1	IV2	IV1	IV2
F-Test for Weak Instruments	16.96	29.22	-	-	13.45	21.22	0.05	2.33
	p=0.0000	p=0.0000			p=0.0000	p=0.000	p=0.953	p=0.101
Hansen's J-Statistic								
- Hetero Robust[2]	0.197	-	0.512	-	0.604	-	-	-
	p=0.6573		p=0.7741		p=0.4371			
- Hetero Non-Robust	0.222	0.020	0.560	0.370	0.802	0.192	0.343	0.057
	p=0.6375	p=0.8875	p=0.7558	p=0.8311	p=0.3705	p=0.6613	p=0.5581	p=0.4503
R2	0.0086	0.0502	0.0605	0.0563	0.0482	0.0731	0.0418	0.0397
N	50180	53597	48991	52284	53056	53860	54260	54269

Notes: All standard errors adjust for clustering by schools. All models include the same set of controls as the OLS regressions. t-statistics are reported in parentheses.

[1] IV1 uses the average number of mothers and fathers in the network with college degrees as instruments. IV2 uses the average number of pupils in the network living with mothers and fathers as instruments. Note that every column except for columns 3 and 4 has two excluded instruments. Columns 3 and 4 have four excluded instruments because we calculated the averages for both the inner and outer rings separately.

[2] Some of the heteroskedastic robust J-statistics could not be calculated because some of the covariance matrices could not be inverted.

Figure 1. Within-school variation in GPA, Smoking, Drinking