Bad Apples, Goody Two Shoes and Average Joes: The Role of Peer Group Definitions in Estimation of Peer Effects

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Abstract

The potential influence of peers and social networks on individual outcomes is important to a variety of educational policy debates including school vouchers, special education, middle school grade configurations and tracking. Researchers usually address the identification problems associated with credibly estimating peer effects in these settings but often do not account for ad-hoc definitions of peer-groups. In this paper, we use extensive information on peer groups to demonstrate that accurate definitions of the peer network seriously impact estimation of peer effects. We estimate the effect of peers' smoking, drinking, sexual behavior and educational achievement on a teen's propensity to engage in like-minded behavior and address the major identification problems that plague estimation of these effects.

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JEL codes: I12, I20

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, 2003; Gaviria and Raphael, 2001; Bobonis and Finan, 2006), on worker productivity (Mas and Moretti, 2006), on choice of medical school specialty (Arcidiacono and Nicholson, 2005), on utilization of prenatal care (Aizer and Currie, 2005), and on retirement savings behavior (Duflo and Saez, 2002) 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). It is unclear whether school-grade cohorts are the true peer group in operation or whether school-grade cohorts merely influence the composition of closer friendship ties, which in turn affect peer outcomes. In this paper, we use the Adolescent Health Survey (Add Health Data) 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, to drink and use drugs, and to engage in risky sexual behavior. One benefit of using the Add Health Data is the existence of self-reported friend groups. Using this information, we are able to estimate social interaction effects using precisely defined friend groups and compare to estimates using school-grade cohort or classroom averages. We call this the definitional component of measurement error and show that it can be quite large.

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 these definitions are important to identifying correct effects for policy. If the most important peer influences occur at the level of individual friendship ties, a finding of positive school-grade cohort effects does not capture the true parameter of interest. In fact, the true effect will depend on the sorting of students into friend groups and cannot be directly inferred from the school-grade correlations. Therefore, school-wide policies based on estimates of school-grade cohort effects will not necessarily be effective.

Unfortunately, the effort to estimate peer effects is complicated by a number of other empirical issues as well. 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 focus on the definitional component of measurement error, we also address these identification issues.

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

[1] Discussions on school choice policies address concerns over the influence of high-performing students on their lower-performing counterparts and vice versa (for example, Cullen, Jacob, and Levitt, 2006). 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).

describes the construction of peer group definitions in detail. In sections V and VI we discuss the main findings of the paper, and section VII concludes.

II. Data

We use data from the National Longitudinal Adolescent Health Survey (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. We use data from two waves of the survey: Wave I was conducted in 1994-95 and Wave II in 1996. The Wave I survey consists of an In-School questionnaire which was filled out by 90,118 students in 145 schools in 80 communities. A subset of 20,745 students was then chosen for an in-depth In-Home survey. The Wave II survey includes an In-Home questionnaire which was completed by 14, 738 students.

The 145 schools in the Wave I survey consist of pairs of sister schools. That is, if a particular high school was included in the survey, the corresponding feeder junior high or middle school was also included. If a school spanning seventh through twelfth grades was chosen for the survey, no sister school was included.

Students in each wave were asked detailed questions about their choices to smoke, drink alcohol, and engage in risky sexual behaviors. They were also asked about their performance in school, including grades in English, math, science, and social studies. Using this information, we construct four variables: the number of cigarettes the student smokes in an average day, the number of times in the past year the student drank alcohol, whether or not the student has ever had sex, and the student's average grade point average.[2] We measure the effects of peers on

[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

these four outcome variables. Descriptive statistics for the outcome variables and all other control variables are reported in Table 1A.

Peer Networks

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 that 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. Assuming friends outside of school have a different peer impact on respondents than friends in the same school, this missing information will bias our estimates. 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 may be due to the mixed use of nicknames and official names, students who are new to the school, or errors in school records. We drop these observations from the analysis.

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drank", 365 for "nearly every day", 30 for "two to three times a month", etc. The recoded variable counts how many days the person drank alcohol in the past year.

The first and third rows of Table 1B report summary statistics on the average number of friends in the Add-Health peer networks for the In-Home and In-School files. We do not include friends who attend other schools or whose information can not be found in the surveys. The average number of nominations in the In-Home and In-School data is 1.27 and 3.91 respectively. Because the In-Home file surveys a substantially smaller subset of the In-School population, the data for a given respondent's nominated friend is often not available in the In-Home sample. This is the primary reason for the discrepancy in the average size of the In-Home peer groups versus the In-School peer groups.

III. Estimation

Identification Issues

The standard 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 Manksi (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 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

An empirical counterpart to equation (1) is:

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

where y_{ist} denotes an outcome for individual *i* in school *s* at time *t* and x_{is} denotes a vector of individual *i*'s observable characteristics or observed heterogeneity, δ_s is a school effect and ε_{ist} is a time-variant unobserved component to individual behavior. $\bar{y}_{ist}$ denotes the average behavior of individual *i*'s peers. We discuss the construction of $\bar{y}_{ist}$ in greater detail in section IV. The parameter of interest β estimates the extent to which peers influence an individual's behavior.

Our first set of regressions includes a set of school dummies and a comprehensive set of observed covariates in the regression equation. This is similar to the procedure used by

Arcidiacano and Nicholson (2005). 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 also correlated with average group behavior. In addition, the procedure does nothing to solve the biases resulting from potential correlation between ε_{ist} and $\bar{y}_{ist}$. Both of these concerns suggest that this procedure will yield an upper bound on β.

Our second set of regressions includes 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). The instrumental variables are averages of dummy variables for whether or not the mothers and fathers of the peer group members have college degrees. We maintain that the educational attainment of the respondent's friends' parents affects his behavior only through his friends' influence and never directly. Meanwhile, the inclusion of the school dummies and exogenous covariates continues to address biases associated with the correlated effects. If either the exclusion restriction or the instruments are invalid, the estimates of β will be biased.[3] Because we expect a positive correlation between the unobserved individual effects and the average background characteristics of peers, the IV estimate of β may again biased upwards.

[3] While it is possible, in principle, to estimate the model in first differences while instrumenting for the difference in group behavior with the level of the instrument at baseline as in Arellano Bond (1991), such a procedure is in practice not very useful because these instruments become very weak once the endogenous variable is differenced.

The estimation is complicated further by potential measurement error in $\bar{y}_{ist}$. Measurement error in this context comes from two possible sources: reporting errors in peer behaviors and errors in the definition of peer groups. Reporting error refers to the standard 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. In other words, the researcher may define the group to be G^{*}_{ist} when in reality, the group is G_{ist}. One of the main weaknesses of the peer effects literature is its 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 (Appendix A) and depend more on availability of data than a theoretically justified definition of "peers".

IV. Defining the Networks

To better understand the role that network definitions play in the identification of endogenous peer effects, we estimate equation (3) using three different network definitions. Our first definition of the peer group (we call this definition "Friends") uses information from the Add Health friendship network to construct average levels of smoking, drinking, sex, and grade point averages across nominated friends. This definition only includes friends directly nominated by the respondent and is limited to at most five male and five female friends. The summary statistics for average network outcomes are in row 1 of Table 1C. We also report

summary statistics for the maximum and minimum of network behavior as defined by $\max_j\{y_{jst} : j \in G_{ist}\}$ and $\min_j\{y_{jst} : j \in G_{ist}\}$.[4]

Our second network definition (which we call "Extended Friends") uses all nominated friends from the first definition as well as all friends of nominated friends. Formally, this network is defined as

$$G_{ist}^{E} \equiv \left(\bigcup_{j \in G_{ist}} G_{jst} \right) \bigcup G_{ist} / i .$$

G_{ist} includes all friends from the first network definition. $\bigcup_{j \in G_{ist}} G_{jst}$ includes all friends of friends. The term $/i$ excludes the individual from his own network. We construct a weighted average over the within group behavior: $y_{ist} = \left(N_{ist}^{E}\right)^{-1} \sum_{j \in G_{ist}^{E}} \omega_{jist} y_{jst}$ where ω_{jist} is the number of times that individual j appears in the sets G_{kst} for $k \in G_{ist}$ and $N_{ist}^{E} = \sum_{j \in G_{ist}^{E}} \omega_{jist}$. This weighting procedure gives more weight to individuals who are "more present" in the extended network and, thus, allows for a "mover and shaker" effect.

The averages for N_{ist}^{E} for the In-Home and In-School files are reported in rows two and four of Table 1B. The size of these networks is substantially larger than with the first network definition. In the In-Home data, the average network size goes from 1.27 for the friends network to 4.53 for the extended friends network. In the In-School data, it moves from 3.91 for the friends network to 26.83 for the extended friends network. The summary statistics for the average, maximum and minimum of behaviors in these networks are given in the second row of

[4] Note that because our outcome variable on sexual behavior is binary, we do not report descriptive statistics on its maximum and minimum.

Table 1C. Note that because the first network definition is a proper subset of this definition, the maxima over these networks get bigger and the minima get smaller.

Our third definition of the peer group is a school-grade cohort. This is the most commonly used definition in the educational peer effects literature. Summary statistics of the mean, max and min of the network behavior are reported in row 3 of Table 1C. When calculating the descriptive statistics for these networks, we again exclude the individual from his own network.

Within Individual Variation in Peer Behavior

Next, we consider the degree of variation in peer average outcomes within individuals or schools. Without enough variation in these outcomes, our estimates will yield large standard errors. We find that with school fixed-effects regressions, this problem is more pronounced when we define peers as a school-grade cohort and less pronounced when we define peers using friendship ties or extended friendship ties.

In Figures 1 and 2, we calculate the difference between $\bar{y}_{ist}$ and the school-level or individual-level average of $\bar{y}_{ist}$. We then plot the non-parametric density estimates of these differences for three separate outcome variables: GPA, sexual behavior and smoking. Each plot contains three densities each corresponding to the three network definitions: friendship ties, extended friendship ties, and school-grade cohorts. Figure 1 shows the plot of within-individual variation across outcomes and network definitions. We see that for all three outcomes and all three network definitions, there is very little variation. This suggests that a panel data fixed-effects estimator will be inefficient. Therefore, we do not present our results for individual fixed effects regressions and focus on the results from the school fixed effects regressions. In Figure

2, we plot within-school variation and see striking differences across network definitions. Similar to the densities in Figure 1, there is little variation when networks are defined as school-grade cohorts. However, contrary to the densities in Figure 1, there is substantial variation when networks are defined based on friend nominations and extended friendship ties.

The results in Figure 2 raise an interesting issue when 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.

V. Primary Results

Table 2 shows first-stage correlations for our instrumental variables regressions. Overall the correlations are quite high, though they are somewhat less so for the smoking and sex outcome variables. Also, the instrumental variable is more strongly correlated with average peer group behavior when we use friends and extended friends as the peer group measure and much weaker when we use school-grade cohorts as the peer group measure. We report our main regression results in Table 3. We consider four outcomes: GPA, smoking, sexual behavior and drinking. We report results for two different estimation methods: school fixed effects with individual controls, and instrumental variables estimation with school fixed effects and individual controls. Each regression requires data on the individual's own behavior and data on at least one friend in the network. Because of the large degree of missing information in the

Add-Health data, these requirements substantially restrict our sample sizes in many cases, especially in the case of regressions using the In-Home data.

Rows 1 and 2 of Table 3 present results for networks defined as friends and extended friends, respectively. We see positive and significant estimates of β in all columns though results are a bit weaker for the IV regressions in most cases. For each of the outcome variables, we see that the school fixed effects estimates are smaller than the instrumental variables estimates. This is perhaps counterintuitive because we expect the estimate former to be biased by both the reflection problem and individual-level unobserved heterogeneity while the latter should only be biased by individual-level heterogeneity that is correlated with the instrument. One explanation for this might be the existence of reporting (measurement) errors in the endogenous variable. Under appropriate conditions, IV will eliminate the attenuation bias that is associated with this measurement error. If so, IV will yield larger coefficient estimates. This may happen even when there are other sources of bias in the OLS estimates that would bias the OLS results upwards.

Row 3 reports results for regressions using school-grade cohorts as the relevant peer group measure. When we compare these results to those in rows 1 and 2, we see that the estimates are much weaker across all outcome variables when using school grade cohorts. In fact, the estimates for sexual behavior and drinking are negative and insignificant. In general, the results in row 3 are less efficient and the point estimates are more unstable. For example, the school fixed effects estimates for the smoking variable are negative and significant (row 3, column 3), but become positive and significant in the instrumental variables regression (row 3,

column 4). Similarly, for the drinking variable, the OLS point estimate is positive and the IV point estimate is negative (row 3, columns 7 and 8).[5]

One possible explanation of the weaker and less stable estimates in row 3 is that the school-grade cohorts are crude approximations of the pupil's actual network. It is possible that this prevents us from capturing much of the endogenous peer effect. The weaker results and lack of stability may further be symptomatic of inefficiency stemming from less within-school variation (see Figure 2). Also, the instrumental variables estimates in row 3 are particularly weak due to near collinearity of the instruments. The required rank condition is therefore only barely satisfied (Bound, Jaeger and Baker 1995).

This raises an important point concerning the use of school-grade cohorts as a measure of peer group. This definition of the peer group precludes using both school dummies and instrumental variables that only vary at the school level. As a result, researchers face a difficult choice between addressing the school-level correlated effects but not the reflection problem, addressing the reflection problem but not the school-level correlated effects or addressing both but falling victim to weak instruments. In contrast, the use of the two definitions based on the Add-Health friend files allows us to address both the reflection problem and the school-level correlated effects without falling prey to weak instruments.

Next, we compare rows 1 and 2 of Table 3. We see that for all four outcome variables, the estimates using the extended friends networks are larger than those using the smaller friends networks. These results indicate the possible presence of non-linearities in peer effects. For example, extending the network may include additional pupils in the network who are not directly friends with the respondents but who, nevertheless, provide further avenues of influence. To investigate this idea further, we examine whether adolescents are particularly influenced by

[5] We also examine all of these results broken down by gender but do not see any salient patterns.

not just the average peer in their friend network, but by the behavior of peers at the extremes of the networks, the so-called bad apples or goody two shoes of the group.

VI. Bad Apples and Goody Two Shoes

In this section we ask whether or not adolescents are more or less sensitive to the behaviors of their peers at either of the extremes of the network. The question is relevant to many educational policy debates. For example, one common criticism of school vouchers is that the policy allows "cream-skimming". The fear is that better-performing schools will skim top pupils away from lower-performing schools. If students are heavily influenced by peers at the extremes of their networks this "cream-skimming" will have a particularly negative impact for students who are left behind. Another example of the relevance of this question is a hard line policy on expulsion that tends to eliminate pupils from the bottom of the networks. If adolescents are not particularly influenced by the bad apples in their networks this policy is less defensible than otherwise.

We estimate a modification of equation (3):

$$y_{ist} = \bar{y}_{ist}\beta_{ave} + y_{ist}^{max}\beta_{max} + y_{ist}^{min} + \beta_{min} + x_i\lambda + \delta_s + \alpha_i + \varepsilon_{ist} \qquad (4)$$

where $y_{ist}^{max} = \max_j\{y_{jst} : j \in G_{ist}\}$ and $y_{ist}^{min} = \min_j\{y_{jst} : j \in G_{ist}\}$. The parameters β_{max} and β_{min} give the average student's sensitivity to the behavior of the "bad apple" and the "goody two shoes" of the network. With some abuse of notation, the marginal effect of a change in the behavior at the extremes is given by

$$ME_{max} = N_{ist}^{-1}\beta_{ave} + \beta_{max}$$

and

$$ME_{min} = N_{ist}^{-1}\beta_{ave} + \beta_{min}$$

where, N_{ist} is the number of pupils in the network as before. These marginal effects have two components. The first operates through the effect on mean behavior and is given by $N_{ist}^{-1}\beta_{ave}$. The second determines the sensitivity of own behavior to the extremes and is given by the parameters β_{max} and β_{min}. These parameters give the effect of increasing the maximum or minimum of behavior in the network while holding average behavior constant. If the pupil is less sensitive to an increase in behavior at the maximum (minimum) of the distribution than he is to a change closer to the center then we should observe that $\beta_{max} < 0$ ($\beta_{min} < 0$).

We estimate these models using OLS with a complete set of school dummies. We consider all three network definitions. We consider are GPA, smoking and drinking outcomes, but not sexual behavior. Because our variable for sexual behavior is a binary outcome and because most networks have at least one person who purports to have had sex and one person who does not, examining the effect of the maximum and minimum is not useful. We acknowledge that OLS with school dummies leaves us vulnerable to empirical problems as discussed in Section III. However, the use of more robust methods such as IV with school effects is hampered by a lack of defensible instruments for changes in the extremes of the network distribution. Accordingly, these results are only suggestive. However, they do suggest that the definition of peer groups have a significant impact on estimation of peer effects.

We report results for estimation of equation (4) in Table 4. For regressions using peer group measures based on friends and extended friends networks, we find that both $\beta_{max} < 0$ and $\beta_{min} < 0$. For school achievement, smoking and drinking, this suggests that own behavior is less responsive to a change in the behavior of either the goody two shoes or the bad apple than it is to the behavior of people who are more average in these behaviors. In contrast, the estimates

using the school grade cohorts suggest the opposite, namely, that $\beta_{max} > 0$ and $\beta_{min} > 0$. However, we caution against a too strict interpretation of these results noting that there is a considerable lack of variation in the distribution of behavior across school-grade cohorts within schools.

We can use the estimates of effects on school achievement in Column 1 of Table 4 to examine the hypothetical impacts of school vouchers and expulsion on own behavior. School vouchers will tend to extract the goody two shoes from the network and send him to another school where ostensibly the opportunities are better. In contrast, a hard-line policy on expulsion will tend to extract the bad apple from the network. First, suppose that the goody two shoes leaves a group with four members but is replaced by another pupil who is still the maximum of the distribution but has a GPA that is 0.1 points lower. This will have a negative effect on the pupil's GPA with magnitude: $0.25 * 0.1 * \hat{\beta}_{ave} + 0.1 * \hat{\beta}_{max} = 0.1 * \left(\frac{0.80}{4.00} - 0.11 \right) = 0.009$. Second, suppose that the bad apple is expelled but is replaced by another pupil who is still the minimum but has a GPA that is 0.1 points higher. This will have a positive effect on the pupil's GPA with magnitude: $0.25 * 0.1 * \hat{\beta}_{ave} + 0.1 * \hat{\beta}_{min} = 0.1 * \left(\frac{0.80}{4.00} - 0.17 \right) = 0.003$. These simple calculations suggest that own behavior may not be very sensitive to what is happening at the extremes of the networks. The suggestion is that the costs of school vouchers and the benefits of expulsion may not be very large.

VII. Conclusions

In this paper, we explore the role of peer group definitions in the estimation of endogenous peer effects in GPA, drinking, smoking and sexual behavior using the National

Longitudinal Adolescent Health Survey. Under appropriate identifying assumptions we provide evidence of endogenous peer effects in school performance and the propensity to have sex, drink and smoke. But we also find that the magnitudes of estimates differ quite widely depending on the definition of peer groups. Because definition of peer groups is often ad-hoc in the existing literature, we find this variation in estimates further highlights the need to justify use of particular definitions.

We find that estimates that are based on the pupil's friend nominations tend to be larger than those that are based on school-grade cohorts. We also argue that use of school-grade cohorts makes it difficult for researchers to address both the reflection problem and school-level correlated effects due to limited within-school variation in peer network variation. Moreover, these network definitions result in weak instruments if the instruments are also defined at the school-grade cohort level due to the near collinearity of the instrument with a set of school dummies. Finally, we provide evidence that measurement error in peer behavior and peer network definition may lead to underestimates of peer effects in many contexts.

We concluded by investigating how the behavior of the pupil responds to changes in the behavior of his peers at the extremes of his network. We provide evidence that sensitivity to behavior at the extremes may not be as large as it is to behavior towards the middle. This suggests that the externalities associated with manipulating the extremes of the peer network will not be as large as those that come from manipulating the center of the network. However, we caution that these results are only suggestive and that more work is necessary before more substantive conclusions can be reached.

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, 2007	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

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Table 1A. Summary of Adolescent Characteristics in 1994 Survey Year.

Variable (Survey)	Average for all adolescents	Variable	Average for all adolescents
Age (School)	15.00	GPA (School)	2.86
	(1.71)		(.79)
Black (School)	.19	GPA (Home)	2.81
	(.39)		(0.75)
White (School)	.61	# cigs per day (Home)	4.30
	(.49)		(7.14)
Male (School)	.50	Ever have sex (Home)	0.40
	(.50)		(0.49)
Mother's education[1]	.32	# days drank last year	20.44
(School)	(.47)	(School)	(59.18)
Father's education[1]	.38		
(School)	(.49)		

Notes: Standard deviations in parentheses.
This table summarizes network information for the in-school sample. Unless otherwise noted, averages are based on a sub-sample of approximately 60,000 observations.
[1] Indicator for whether or not parent has a college education.

Table 1B. Average numbers of friends in the network

	Network Definition	Average Friend Number
In-Home	Friends	1.27
		(1.67)
In-Home	Extended Friends	4.53
		(8.97)
In-School	Friends	3.91
		(3.10)
In-School	Extended Friends	26.83
		(24.72)

Table 1C: Peer Group Averages

	GPA (In-School Survey)			GPA (In-Home Survey)			# Cigs Per Day (In-Home Survey)			# Days Drank Last Year			Ever Have Sex?
	Mean	Max	Min	Mean	Max	Min	Mean	Max	Min	Mean	Max	Min	Mean
Friends	2.91	3.39	2.36	2.88	3.06	2.70	4.13	5.75	2.92	19.91	63.51	3.37	0.40
	(0.59)	(0.64)	(0.76)	(0.68)	(0.72)	(0.76)	(6.37)	(8.83)	(5.81)	(35.17)	(99.01)	(22.17)	(0.44)
Extended Friends	2.92	3.74	1.80	2.87	3.24	2.47	3.88	8.14	1.75	20.52	155.38	0.95	0.41
	(0.46)	(0.45)	(0.66)	(0.62)	(0.70)	(0.79)	(5.40)	(11.39)	(4.24)	(24.86)	(137.64)	(13.05)	(0.40)
School-Grade	2.84	3.99	1.14	2.69	3.90	1.54	3.52	19.70	0.08	19.86	339.87	0.18	0.38
Cohort	(0.30)	(0.08)	(0.30)	(0.38)	(0.24)	(0.52)	(2.25)	(17.68)	(0.81)	(10.56)	(73.12)	(7.91)	(0.20)

Note: This table reports the mean and standard deviation for the mean, max and min of the network's behavior. Standard deviations are in parentheses.

Table 2. First-Stage Correlations.

	GPA	Smoking	Sex	Drinking
Friends	182.31	1.46	4.39	19.01
	[0.000]	[0.2377]	[0.0145]	[0.000]
Extended	170.85	9.61	2.61	14.00
Friends	[0.000]	[0.0001]	[0.0779]	[0.000]
School-Grade	12.81	4.66	1.54	0.08
Cohorts	[0.000]	[0.0112]	[0.2182]	[0.9210]

Each cell of this table reports the F-test and associated p-value in brackets of a test of the null that the excluded instruments from our IV regressions are significant in the first state regressions. All regressions include all of the exogenous covariates from the main regression equation.

Table 3. Average Peer Effects

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	GPA		Smoking		Sex		Drinking	
	School FE	IV + School FE	School FE	IV + School FE	School FE	IV + School FE	School FE	IV + School FE
	Network Definition: Friends							
Peer	0.46	0.83	0.17	0.37	0.26	0.26	0.26	0.64
Ave.	[37.13]	[21.66]	[2.60]	[1.37]	[6.35]	[1.17]	[15.74]	[3.92]
R2	0.2961	0.2565	0.1341	0.2549	0.2200	0.1566	0.0797	0.0353
NT^2	25934	24938	743	654	2750	2354	37918	35906
	Network Definition: Extended Friends							
Peer	0.61	0.91	0.13	8.42	0.28	0.72	0.39	0.82
Ave.	[28.26]	[21.69]	[1.66]	[0.38]	[5.46]	[3.06]	[16.68]	[6.10]
R2	0.2853	0.2772	0.1194	0.0000	0.2166	0.1739	0.0814	0.0554
NT^2	26740	26532	824	744	2751	2450	38015	37551
	Network Definition: School-Grade Cohorts							
Peer	0.41	0.08	-0.34	1.10	-0.12	-0.08	0.12	-1.94
Ave.	[8.04]	[0.45]	[-2.96]	[2.71]	[-1.25]	[-0.09]	[1.38]	[-0.27]
R2	0.2102	0.2112	0.1018	0.0408	0.1810	0.1955	0.0598	0.0375
NT^1	32419	32419	3174	3161	8192	8159	46275	35906
Survey	School	School	Home	Home	Home	Home	School	School

Notes: All regressions include grade and, when appropriate, gender dummies. T-statistics are in brackets. All standard errors adjust for clustering on schools. Reported sample sizes for the individual fixed-effects regressions includes individuals for whom there was only a single observation. All regressions include additional controls such as health status as well as race dummies and parental education.

[1] Refers to individual-time observations.

Table 4. Bad Apples and Goody Two Shoes

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
		GPA			Smoking			Drinking	
Network Definition	Friends	Extended Friends	School-Grade Cohorts	Friends	Extended Friends	School-Grade Cohorts	Friends	Extended Friends	School-Grade Cohorts
Peer Ave.	0.80	0.97	0.31	0.67	0.56	-0.77	0.48	0.55	-0.03
	[28.62]	[43.64]	[5.02]	[4.32]	[5.36]	[-3.85]	[14.04]	[17.55]	[-0.39]
Peer Max	-0.11	-0.20	0.33	-0.11	-0.11	0.13	-0.06	-0.03	0.04
	[-6.64]	[-12.07]	[4.97]	[-1.79]	[-8.74]	[3.06]	[-7.40]	[-8.98]	[8.59]
Peer Min	-0.17	-0.16	0.20	-0.35	-0.31	0.96	-0.23	-0.33	0.88
	[-15.54]	[-16.51]	[10.22]	[-5.23]	[-3.10]	[6.13]	[-7.56]	[-6.25]	[29.25]
Survey	School	School	School	Home	Home	Home	School	School	School
R2	0.2580	0.2528	0.1312	0.0937	0.0747	0.1098	0.0634	0.0645	0.0494
NT[1]	41104	42605	53621	1205	2304	9630	62085	62337	78914

Notes: All regressions include grade and gender dummies. T-statistics are in brackets. All standard errors adjust for clustering on schools.
[1] Refers to individual-time observations.

Figure 1. Within-individual variation in GPA, Sex, Smoking

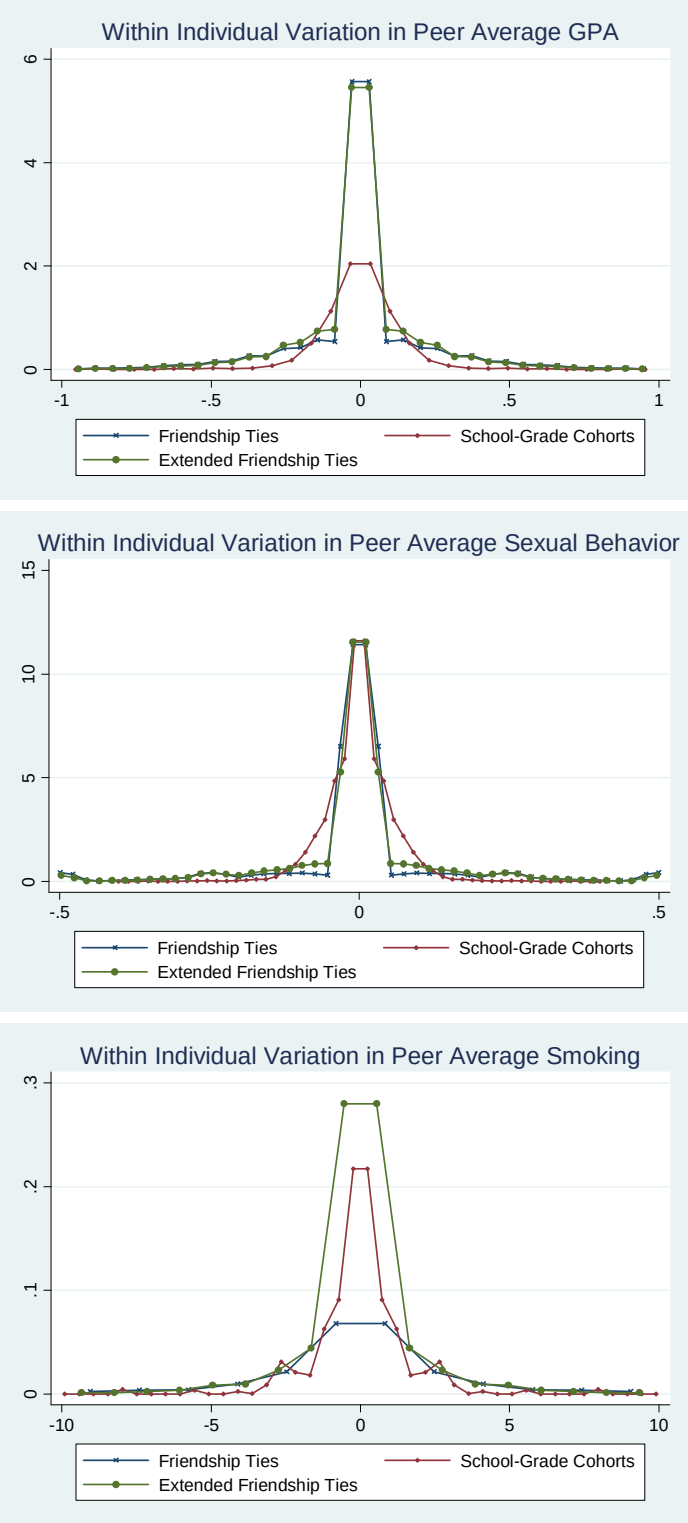

Figure 2. Within-school variation in GPA, Sex, Smoking