CHAPTER 4

Peer Effects in Education: How Might They Work, How Big Are They and How Much Do We Know Thus Far?

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Contents

Abstract

This chapter summarizes the recent literature on peer effects in student outcomes at the elementary, secondary, and post-secondary levels. Linear-in-means models find modest sized and statistically significant peer effects in test scores. But the linear-in-means model masks considerable heterogeneity in the effects experienced by different types of students. Using nonlinear models, one prevalent finding is larger peer effects in which high ability students benefit from the presence of other high ability students. Studies that stratify students by race and ability often find that students are affected both by the racial composition of their peers and by the achievement of their same-race peers. At the university level, several studies find modest sized effects from dormmate and roommate background on own academic performance. For both university and high school students, the measured peer effects on "social" outcomes such as drinking are larger than the effects on academic outcomes. Many authors find substantial peer effects in drinking, drug use, and criminal behavior. This chapter suggest areas for future investigation and data collection.
JEL classification: J0, I2

Keywords

Peer Effects
Social Networks

ISSN 0169-7218, DOI: 10.1016/S0169-7218(11)03004-8

Education
Higher Education

1. INTRODUCTION AND OVERVIEW

Peer effects in education have recently received a great deal of attention from researchers: five of the most popular articles are cited collectively more than 2000 times.[1] The potential importance of peers in the educational process has long been noted with the influential Coleman Report (1966), Coleman (1968) being one well-known example. As educational researchers make halting progress toward a deeper understanding of the educational production function, many researchers and teachers have argued that peer composition is as important a determinant of student outcomes as other widely cited inputs including teacher quality, class size, and parental involvement. This chapter reviews the empirical evidence on peer effects in elementary, secondary and post-secondary education and concludes that within certain contexts and for certain outcomes, peer effects are indeed a powerful determinant of why students turn out the way they do.[2]

Motivating social scientists' interest in peer effects is not a difficult task. If peer influences are a major factor in generating outcomes that include test scores, college going, career choice, drug use, or teen pregnancy, then every parent, teacher, and policy maker will care about the size and nature of peer effects.[3] Epple and Romano (1998) show that the size and nature of peer effects have large implications for the sorting of students into schools and the distributional consequences of tuition voucher programs.

Defining what is meant by a *peer effect* is slightly more challenging. This chapter uses a broad definition of peer effects to encompass nearly any externality in which peers' backgrounds, current behavior, or outcomes affect an outcome. By limiting peer effects to externalities, market-based or price-based effects are excluded. For example if the families in a particular county contribute to a demand shock for private schooling which raises or lowers the dollar cost of private schooling to the individual, that is clearly a market-based effect and not a peer effect. Externalities that work through class size are also excluded.[4]

What are included as peer effects are any other externalities that spill over from peers' or peers' family background or current actions. For example, if a student's classmates have higher

[1] Google Scholar search of "peer effects and education."

[2] Epple and Romano (forthcoming) contains a comprehensive survey of theoretical models of peer effects and the implications of these models.

[3] Harris (1998) famously argued that it is *exclusively* peers and not parental nurturing that determines child outcomes. While her thesis is almost surely an overstatement, the papers reviewed in this chapter will suggest that for certain outcomes and certain student groups peers matter a great deal.

[4] The main reason for this exclusion is that most economists consider the class size literature and the peer effects literature to be addressing separate questions. Note that class size (like spending) may be an input that is relatively easily set by policy makers while peer achievement or peer racial composition may be more difficult to alter.

incoming ability and the student learns directly from her classmates, that is a peer effect. If the classmates have higher incoming ability and this enables the teacher to teach at a higher level or a more demanding pace, that is also a peer effect. If the student is disruptive and consumes more of the teacher's attention, thereby reducing her classmates' test scores, that too is a peer effect (Lazear (2001)). If the classmates' high current achievement motivates the student (through competition) to work harder, that is also a peer effect. If the student develops an interest in athletics or in shoplifting because of her peers, those are also peer effects. Manski (1993) defines endogenous effects as those that emanate from peers' current outcomes whereas exogenous effects are those that emanate from peers' backgrounds.

Suppose that a student's outcomes are improved because his peers' families are actively involved in insisting on teacher accountability or in finding a superstar principal. Such an effect is considered a peer effect, even though these influences can also be simultaneously classified as effects of teacher quality or principal quality. In short, there are a large number of channels through which peer effects might operate. Identifying the precise channel through which a given peer effect operates is a Herculean task and in many cases is asking too much of the data. But researchers have been successful both in demonstrating the existence of peer effects and measuring the magnitudes of some of these effects. Many of these same papers have suggested plausible mechanisms through which peer effects work.

In the last 15 years, economists have begun to provide credible measurement and identification on the nature and size of peer effects. One method frequently employed in modern studies is to rely upon some form of exogenous variation in the assignment of students to schools, classrooms, or dorms (in the study of peer effects at the university or college level). The other common method is use models with both school and student fixed effects in an effort to control for the nearly inevitable self selection of students into schools and classrooms. Below several dozen studies of both types are reviewed.

The picture that emerges is both fascinating and relatively coherent. At first glance, results from the myriad peer effects studies would seem to be all over the map. Within elementary and secondary schools, many studies find modestly large effects of peer background on own test scores (Hoxby (2000b), Vigdor (2006), Vigdor and Nechyba (2007), Betts and Zau (2004), Boozer and Cacciola (2001), Hanushek, Kain, Markman, and Rivkin (2003)). However, Burke and Sass (2008) find little evidence that the peer average background affects the average student's achievement. Angrist and Lang (2004) find that the busing of Metco students into suburban Boston schools has little effect on the test scores of students in the receiving schools. And Imberman, Kugler, and Sacerdote (2009) find only modest linear-in-means effects from the arrival of Katrina evacuees on achievement in receiving schools in Louisiana and Houston, Texas.

These apparently contradictory results can be reconciled if we accept Hoxby and Weingarth's (2005) argument that the *linear-in-means*[5] model of peer effects is not necessarily the right model nor is it the most interesting one. Instead one can allow for

[5] See below for definitions of the various models.

peer effects to differ by a student's own achievement and by whether changes in peer group composition are generated by adding students at the top, middle, or bottom of the ability distribution. Hoxby and Weingarth find that students at the bottom of the test score distribution benefit significantly from the addition to their classroom of students who are themselves at the 15th percentile of past test scores. Conversely, students at the top decile of the test score distribution benefit strongly from the addition of other classmates who are also at the top. Achievement for students in the middle tends to less affected by peer composition.

Burke and Sass (2008), Lavy, Paserman, and Schlosser (2007) and most recently Imberman, Kugler, and Sacerdote (2009) reinforce these findings. Lavy, Paserman, and Schlosser (2007) find that high ability high school students in Israel benefit from the presence of other high ability students. Burke and Sass find at most small effects in linear-in-means models, but large effects when they allow the effect to differ by own achievement and the type of peer group change contemplated. Imberman, Kugler, and Sacerdote use the unexpected arrival of Hurricane Katrina evacuees as a shock to peer groups and find that high achieving students benefit the most from the arrival of high achieving peers and are hurt the most by the arrival of low achieving peers.

The literature on peer effects in university and college settings also features an appearance of contradictory results which can be reconciled upon deeper examination. Many studies of peer effects in college rely on roommates and dormmates since these are often the peer groups which can be easily identified and in some cases there is quasi-random assignment of students to room and dorm groups. Sacerdote (2001), Zimmerman (2003), and Stinebrickner and Stinebrickner (2006) find that roommates' background and current achievement affect own achievement. Here current achievement is measured by college grade point average (GPA) and background is measured by incoming test scores and high school class rank or high school GPA.

Foster (2006) and Lyle (2007) find no evidence that roommates' or hallmates' background affects own college GPA. But the effects found in the original roommates studies are modest enough that it easy to believe that differences across institutional settings and differences across student bodies would either eliminate roommate and dormmate influences or make such influences difficult to detect. Using data from the U.S. Air Force Academy, Carrell, Fullerton, and West (2008) examine peer effects in an unusual context in which the full peer group is known and the institution forces a great deal of peer interaction. In that setting, they find large peer effects.

Perhaps the more interesting result from the literature on peer effects in higher education is the fact that while academic achievement (college GPA) is affected modestly by roommates and dormmates, the effects on more "social" outcomes are large. Duncan, Boisjoly, Kremer, Levy, and Eccles (2005) find that males who themselves binge drank in high school have a fourfold increase in their number of college binge drinking episodes (per month) when assigned a roommate who also reported binge drinking in high school.

Similarly Sacerdote (2001) finds that a student is much more likely to join a fraternity or sorority when surrounded by roommates or dormmates who join.

Finally, there is a burgeoning literature on peer effects in crime, drug use, and teenage pregnancy among high school and middle school students. Like the college literature on peer effects in social outcomes, the peer effects on drug use, criminal behavior, and teen pregnancy for these younger students are estimated to be quite large. (Gaviria and Raphael (2001), Case and Katz (1991), Kling, Ludwig, and Katz (2005)). For example, Gaviria and Raphael (2001) find that moving a student from a school in which 13% of the peers' parents have a drug problem to a school in which 40% of the parents have a drug problem increases the student's own drug use by 7 percentage points relative to a mean drug use rate of 14%.

2. MODELS OF PEER EFFECTS

The most commonly estimated model in the peer effects literature is the linear-in-means model in which the outcome Y is some function of a student's background characteristics, her peers' average background characteristics, and the student's peers' average outcome. More formally this can be written as:

$$Y_i = \alpha + \beta 1 * \overline{Y}_{-i} + \gamma 1 * X_i + \gamma 2 * \overline{X}_{-i} + \varepsilon_i \tag{4.1}$$

where Y_i represents the student's outcome, $\overline{Y}_{-i}$ represents her peers' average outcome, X_i is a vector of the student's background characteristics, and $\overline{X}_{-i}$ is a vector of her peers' average background characteristics. This model has the virtue of simplicity. Equation (4.1) encompasses both endogenous effects from the peers' current outcomes and exogenous effects from the peers' background. This model of course constrains the size of either peer effect ($\beta 1$ or $\gamma 2$) to be the same regardless of where the student falls within the distribution of student background or ability.

And by definition all peer effects work through the mean. Effects from any other aspects of the distribution of the peers' background are ruled out. For example, effects from mean-preserving increases in the variance of the peers' ability are assumed to be zero as are the potential effects that might work through the most able or least able peer.

Despite its popularity in use, there are two major problems with the linear-in-means model in practice. (These problems are pointed out most notably by Hoxby (2000b) and Hoxby and Weingarth (2005).) First, from a social welfare point of view the model is not that interesting since the model constrains the net effect from reassignment of peers to different classrooms or groups to be zero. Suppose that an exceptionally good student from classroom A is exchanged for an exceptionally bad student from classroom B. Assume the classrooms are of equal sizes. From a social welfare or total output prospective, the positive peer effects for the students in B are exactly offset by the negative peer effects for the students in classroom A.

Second, from an empirical point of view, researchers have found that peer effects are not in fact linear-in-means. If anything, there tend to be complementarities of the type that would support schools using various forms of tracking policies. Some of the available evidence suggests that the most able students benefit from having more high ability students around, while the least able students are actually harmed by adding high ability peers and removing lower ability peers. Hoxby and Weingarth (2005), Burke and Sass (2008), Lavy, Paserman, and Schlosser (2007), Cooley (2009), and Imberman, Kugler, and Sacerdote (2009) all find this form of complementarity in elementary and secondary schools using test scores as the outcome. Hanushek and Rivkin (2009) find that test score growth for high achieving black students is helped by increases in the proportion of whites in their school and grade. At the university level, Sacerdote (2001) finds some evidence that high ability students benefit each other more than high ability students benefit average or low ability students.

Fortunately Equation (4.1) can be expanded to allow both student *i*'s position in the ability (or background characteristic) distribution to matter and to allow for different peer effects to stem from different possible changes to the peer group. The identification problems that plague nonlinear estimations are not much worse than the fundamental problems of identification inherent to the linear-in-means model. (See the next section for a discussion.)

Duncan et al. (2005) and Sacerdote (2001) take a very simple approach to testing for possible nonlinearities in peer effects. These two papers group student *i* into one of several possible categories and *i*'s peers (in this case roommates) into categories and then include in the regression all possible interactions of student *i*'s type and *i*'s roommate's type. In the case of Duncan et al., student *i* binge drank in high school or did not, and *i*'s roommate either binge drank in high school or did not, implying that there are four possible categories. The outcome investigated (i.e., the left-hand-side variable) is binge drinking episodes per month *in college*. In other words, Duncan et al. run the following regression:

$$\begin{aligned}\mathrm{Y_i} = \alpha + \lambda 1 * (\mathrm{D_i} = 0 * \mathrm{D_{-i}} = 0) + \lambda 2 * (\mathrm{D_i} = 1 * \mathrm{D_{-i}} = 1) + \lambda 3 \\ *(\mathrm{D_i} = 0 * \mathrm{D_{-i}} = 1) + \varepsilon_i\end{aligned} \tag{4.2}$$

Here $Y_i$is student *i*'s number of binge drinking episodes per month in college and D_i and $D\text{-}_i$ are dummies for whether *i* and *i*'s roommate binge drank in high school. After running the regression, it is then a simple matter to hold student *i*'s type constant and test whether student *i* has more drinking episodes in college with versus without a roommate who drank in high school. For example, testing whether $\lambda 2 = 0$ asks whether students who drank in high school have more episodes with a roommate who drank in high school versus without such a roommate.

Hoxby and Weingarth (2005) use a similar approach in testing for nonlinear peer effects among third through eighth grade students, but they have many more possible categories of student type and peer type. They divide students into deciles of past test score performance. They then interact student *i*'s decile of previous score with the percent of *i*'s peers (classmates) falling into each of the 10 deciles. This generates 100 interaction terms. The coefficients on these interaction terms allow the authors to test a wide variety of hypotheses about peer effects. For instance, one can ask whether high ability students benefit from being in a class with a higher proportion of high ability students. Similarly one can ask whether low ability (as measured by past test scores) students benefit the most from having classmates in the lowest, middle, or upper part of the test score distribution. And one can ask whether high ability classmates provide more externalities for students who are themselves at the lower, middle, or upper part of the test score distribution.

Hoxby and Weingarth provide a nice categorization of different possible theories of peer effects, as shown in Table 4.1.

A model is categorized as having homogenous effects if magnitude of the peer effect is constant for all student types *i*. Categorization is from Hoxby and Weingarth (2005) and Lazear (2001).

This provides a summary of possible ways in which peer effects in a classroom might work. Among the most discussed models are the bad apple model and the boutique model. In the bad apple model (Lazear (2001)), the most relevant peer effects are those provided by the least academically able or least disciplined student in the classroom. This student provides large negative externalities in several possible ways: The bad apple peer may cause so much commotion in the classroom as to distract the teacher and students from productive tasks. Or he may encourage additional raucous or disruptive behavior among other students. Or the bad apple may not be a discipline problem but he may simply have low ability and require extra teaching attention, thereby detracting from the experience of the other students.

Table 4.1 Possible models of peer effects

Model	Homogenous effects?	Description
Linear-in-means	Yes	Only the mean of peers background or outcomes matters
Bad apple	Yes	One disruptive student harms everyone
Shining light	Yes	One excellent student provides great example for all
Invidious Comparison	No	Outcomes are harmed by the presence of better achieving peers
Boutique/ tracking	No	Students perform best when surrounded by others like themselves
Focus	Yes	Classroom homogeneity is good, regardless of student *i*'s ability relative to the homogenous classmates
Rainbow	Yes	Classroom heterogeneity is good for everyone
Single crossing	No	Positive effects from high ability classmate is weakly monotonically increasing in own ability

In the boutique model, students benefit from being around other students with a similar ability. One possibility is that a classroom with more homogeneity enables the teacher to customize the material and the pace of learning to that particular group of students. Another possible mechanism for the boutique model is that students can best learn from each other when the students are of similar ability or are working on similar material. The boutique model is perhaps the main justification behind tracking students into classroom by ability. Note that in the boutique model, the less able students are helped more by the presence of peers like themselves than they are helped by the presence of high ability peers.

Some of the other models are certainly possible from a theoretical point of view but may be less important from an empirical point of view. The rainbow model suggests that diversity of ability is good for all students. This notion seems contrary to the experience of many teachers and contrary to some (but not all) recent evidence on tracking. Furthermore the rainbow model can not explain why many if not most high schools in the U.S. use some form of tracking. The shining light model is interesting and is the opposite of the bad apple model. But it is somewhat more difficult to think of ways in which a great student could raise her classmates' achievement than it is to think of ways in which a terrible student could harm an entire classroom (Lazear (2001)). The invidious comparison model suggests that students are harmed by the presence of better students in the same classroom.

3. IDENTIFICATION OF PEER EFFECTS

As detailed in Manski (1993) and Brock and Durlauf (2001), the fundamental challenge for the peer effects literature is identification. One can imagine at least three reasons why running an OLS regression for Equation (4.1) would be problematic. First, since student *i*'s outcome (Y_i) affects his peers' mean outcome ($\overline{Y}_{-i}$) and vice versa, $\beta 1$ is subject to endogeneity bias. Manski labeled this the reflection problem. Second, in most settings peers self select into peer groups or classrooms in a manner that is unobserved to the econometrician. Frequently there is positive selection in which similar people join or are assigned to the same group. This positive selection could cause substantial upward bias in the estimated magnitude of peer effects $\beta 1$ and $\gamma 2$. Manski labeled the influence of selection the *correlated effect*.

Third, Equation (4.1) includes effects that stem both from peers' average outcome ($\overline{Y}_{-i}$) and peers' average background characteristics ($\overline{X}_{-i}$). Manski labeled the former the endogenous effects and the latter exogenous (or contextual) effects. Separate identification of $\beta 1$ and $\gamma 2$ is difficult since peer background itself affects peer outcome. Even if one has exogenous variation in peer background characteristics (as in many of the roommates papers, such as Sacerdote (2001) and Zimmerman (2003) or as in Hoxby (2000b)), that does not imply that both coefficients are separately identified. Note that endogenous effects have the potential for social multipliers since a small change for student *i* will affect the peer group which will then reflect back to student *I,* and so on.

The modern peer effects literature has managed to overcome some but not necessarily all of these challenges. As noted in the introduction, the most commonly used approaches are 1) to include student level and/or school level fixed effects in an effort to control for selection into peer groups, and 2) to rely on some form of exogenous shock to peer-group composition. That said, most papers have one source of exogeneity and do not separately identify the exogenous and endogenous peer effects.

Hoxby (2000b) was one of the first papers to look for some exogenous shock to peer group composition. The key source of variation is idiosyncratic changes in the gender mix across cohorts within a given elementary school. Increases in the fraction of girls in the cohort lead to increases in mean peer test scores. Other papers within primary and secondary education also rely on exogenous shocks to peer group composition or more directly on random or quasi-random assignment of students to classrooms. Vigdor and Nechyba (2007) make the case that the assignment of students to classrooms within their sample of North Carolina schools is fairly random. Boozer and Cacciola (2001) use experimental variation from Project Star as a source of variation in peer ability.[6] Duflo, Dupas, and Kremer (2008) created a randomized experiment in which some students are assigned to tracked (i.e., more homogenous) classrooms and others are not. Peer ability is generally measured by using peers' prior test scores.

A group of papers at the university level relies on random assignment of roommates, dormmates, or squadron members to generate random variation in peer groups. These papers include Sacerdote (2001), Zimmerman (2003), Carrell, Fullerton, and West (2008), Foster (2006), Stinebrickner and Stinebrickner (2006), Lyle (2007), and Siegfried and Gleason (2006). The central identification in these papers stems from the idea that university policies generate random variation in the makeup of peer groups.

Removing or controlling for the selection of students into peer groups is an important step in being able to identify peer effects. As noted above, this is not the same as being able to separately identify $\beta 1$ and $\gamma 2$. Most of the above papers estimate the reduced form effects of changes in peer groups and do not tackle the question of whether the peer effects identified stem from exogenous or endogenous effects. In other words, most authors do not attempt to estimate the structural parameters $\beta 1$ and. Some authors including Boozer and Cacciola (2001), Case and Katz (1991), and Gaviria and Raphael (2001) assume that peer background does not enter directly (i.e., $\gamma 2 = 0$ or no contextual effects). Under this assumption, one can potentially identify the magnitude of endogenous effects.

Bramoullé, Djebbari, and Fortin (2009) show that one can identify both $\beta 1$ and $\gamma 2$ if one assumes that an individual's outcome is affected by that person's friends' background *but not* the background of the friends of the person's friends. Thus the

[6] Project STAR (Student Teacher Achievement Ratio) was a large scale randomized experiment carried out in 79 schools in Tennessee. Students were randomly assigned to a large (22–25) student classroom, a small (13–17) student classroom, or a large classroom with the addition of a full-time teacher aide.

background of the friends' friends (those with whom the individual does not have direct contact) can serve as an instrument for the friends' endogenous outcome.

A second approach to dealing with selection into peer groups has been to control for characteristics of individual students and schools. Often this means including student fixed effects and school fixed effects and identifying peer effects using cohort to cohort variation within school. The papers using this approach include Betts and Zau (2004) and Lavy, Paserman, and Schlosser (2007). Hanushek, Kain, Markman, and Rivkin (2003) include school-by-grade fixed effects and hence use classroom-to-classroom level variation within a school and grade. Burke and Sass (2008) include teacher fixed effects too. The basic concept in these papers is that the student, school, or school-by-grade fixed effects remove selection effects and allow the researcher to identify peer effects from idiosyncratic variation in peer ability. Again, in most of these papers the ability of classmates is generally calculated using peers' prior test scores. In the case of Lavy, Paserman, and Schlosser (2007), peer background is the average of a dummy variable for whether a classmate skipped or repeated a grade.

3.1 Identification Using Excess Variance

There is also a different methodology which uses the variance in mean outcomes across groups to detect the presence of social interactions (peer effects). Glaeser, Sacerdote, and Scheinkman (1996) show that imitation within groups generates more variation across group means than would be expected if individuals were making independent decisions.[7]

Graham (2008) uses excess variance plus experimental variation to estimate the size of peer effects within the Project Star data. Like the majority of papers described above, Graham is interested in estimating the reduced form effect of classmates' incoming ability which he labels γ. Keeping the notation consistent with Equation (4.1), Graham's reduced form is:

$$Y_i = \alpha + \gamma * \overline{X}_{-i} + \varepsilon_i \tag{4.3}$$

where Yi is student i's test score and $\overline{X}_{-i}$ are i's classmates' incoming scores. His insight is that social interactions will generate more between classroom level variation in the outcome Y than would be predicted given individual level heterogeneity in student ability.

In most settings one would be concerned that sorting into classrooms or teacher effects would also generate excess variance in mean outcomes at the classroom level. However, in the Project Star data, students are randomly assigned to small versus large classrooms and Graham is able to use this fact to difference out the excess variance that comes from sorting or teacher effects. Specifically he shows that a consistent estimate of γ^2 equals

$$\frac{E(G_c^b|small) - E(G_c^b|big)}{E(G_c^w|small) - E(G_c^w|big)} \tag{4.4}$$

[7] The paper then proceeds to estimate the level of social interactions present in various forms of crime at the level of cities and police precincts.

where G_c^b and G_c^w are the between and within variance in outcomes for a classroom. In other words, an estimate of the social interaction parameter squared is the ratio of the between classroom to the within, where the between has been "inflated" by social interactions. And we first difference across the randomly assigned small versus big classroom category to account for nonsocial interaction factors which cause the between variation to be larger than the within.

Related to the Graham approach are the Glaeser and Scheinkman (2001) and Glaeser, Sacerdote, and Scheinkman (2003) papers which define the social multiplier as the ratio of the individual effect from an exogenous shock to the aggregate effect from the same exogenous shock. The intuition is that a small exogenous change at the individual level is magnified through the social interactions process to deliver the larger aggregate level coefficient. For a concrete example, think of the individual student level effect on achievement from giving some students extra teacher attention. Now consider a classroom level regression in which we calculate the effect on achievement from giving entire classrooms of students the additional teacher attention. If there are no social interactions (and no sorting into treatment status) the individual and aggregate coefficients should be the same and the social mutiplier is 1.0.

We suggest two related approaches for calculating the social multiplier. One is to simply take a ratio of an aggregate level coefficient to the corresponding individual coefficient. In the above example, the exogenous shift considered is an increase in teacher attention. At the individual level, one could regress test scores (*Yi*) on a dummy for receiving extra attention (*Xi*). This same regression could be run at the classroom level in which we regress classroom average test scores for classroom c, $\overline{Y}_c$ on the average number of students in *c* who received the treatment, that is, $\overline{X}_c$. The ratio of the classroom level coefficient to the individual level coefficient is by definition the social multiplier. Our second approach (which works well when there are many right-hand-side variables) is to use coefficients from an individual level regression to predict aggregate level outcomes. We then regress actual aggregate level outcomes on these predicted values and the coefficient from this second regression is the social multiplier.

Recovering the social interactions parameter from the social multiplier: Suppose we allow β to be the social interactions parameter, that is, the effect of average group level actions $\overline{Y}_{-i}$ on own action (*Yi*). If there is no sorting into groups and we take the aggregate coefficient for a sufficiently large group size *N*, then the social multiplier equals 1/(1- β). When there is sorting into groups based on an observable *X*, then we define $\sigma = \frac{Var(\overline{X})}{Var(X_i)}$ which is the share of total variation in *X* explained by variation at the group level. We show that as the group size gets large, the social multiplier converges to 1/[(1-β)*(1+σβ)]. Thus in either formulation, β can be calculated once the size of the social multiplier is known.

The social multiplier approach is useful for three reasons. First, it is easy to calculate and need not impose a specific functional form for peer effects. Second, for some research questions it delivers the parameter of direct interest to researchers or policy makers, namely if policy can exogenously induce one additional person to take action A, how many total people will take action A in equilibrium? Third, under some strong assumptions the social multiplier approach can take into account sorting into groups or locations.

4. EMPIRICAL RESULTS ON PEER EFFECTS IN PRIMARY AND SECONDARY EDUCATION

A major focus of the literature has been on peer effects in test scores for students in primary and secondary school. Table 4.2 shows estimated peer effects from a number of studies. The estimates encompass a large range. There are, however, two consistent themes: First studies using gender variation find larger effects. These studies generally conclude that increases in "percent female" help peer achievement through more channels than simply raising average classroom test scores. Second, there appear to be important nonlinearities. Several but not all studies find that reductions in peer heterogeneity improve outcomes and that students at the high end of the ability distribution experience the largest peer effects from high ability peers.

Hoxby (2000b) relies on random variation in the gender and racial makeup of peers to provide estimates of peer effects. She uses data from students in all Texas elementary schools in grades 3–6. Her outcome measure is performance on the Texas Assessment of Academic Skills (TAAS). Her strategy relies on the fact that within a school and grade level, cohort level variation in gender and racial composition is an unexpected shock to peer achievement. Girls on average score about a half a standard deviation higher than boys on the TAAS reading test, and hence changes in class composition do represent a significant shock to peer achievement.

Hoxby finds that a 10 percentage point rise in cohort percentage female is associated with boys reading scores being 0.04 to 0.08 test points higher. The positive effects of percent female on girls reading scores are similar in magnitude. Translating this into an effect of peer scores on own scores, she finds that an increase in the peer average reading score of 1.0 raises own reading score by 0.3 to 0.5 points.

When she performs a similar analysis (using gender variation) for math scores she finds a much larger peer effect coefficient of a 1.7 to 6.8 increase in own math score for every 1.0 increase in peer (cohort) average score. Because this figure is so large, she concludes that the effects of percent female likely work through additional channels beyond a simple increase in peer average test scores.

Hoxby also considers the effects of cohort level shocks to racial composition. She finds that peer effects are largest intra-race, meaning that altering the percentage black has the largest peer effect on black students.

Table 4.2 Peer effects in test scores for primary and secondary schools linear in means models

Paper	Sample	Identification	Effect of a 1.0 point move in average peer score**
Hoxby (2000b)	Texas Schools Project	Identified using within-school cohort-to-cohort variation in gender and racial makeup	using gender variation
	Grades 3–6		Reading: 0.3 to 0.5
			Math: 1.7 to 6.8
			Using racial variation
			(intra-group)
			Reading 0.68
			Math: 0.40
Lavy and Schlosser (2007)	Israeli primary, middle, high school students	Within-school variation in percent female	Matriculation exam score for males: 1.06; for females: 0.84
Whitmore (2005)	Tennessee elementary students in Project STAR	Random assignment of students to classrooms	0.60
Boozer and Cacciola (2001)	Tennessee elementary students in Project STAR	Instrument for peer mean using experimentally created variation	1st grade: 0.30
			2nd grade: 0.86
			3rd grade: 0.92
Angrist and Lang (2004)	Brookline students Iowa Test of Basic Skills. Grades 3,5,7	Effects of Metco students in Brookline, MA on non-Metco students	On all non-Metco[1]
			core IATB: 0.21
			On black non-Metco
			core IATB: 1.375

Continued

Table 4.2 Peer effects in test scores for primary and secondary schools linear in means models—cont'd

Paper	Sample	Identification	Effect of a 1.0 point move in average peer score**
Vigdor and Nechyba (2007)	North Carolina 5th graders	Limit sample to schools that appear to use quasi-random assignment; include school and year fixed effects and teacher effects	Reading: −0.10
			Math: −0.12
Betts and Zau (2004)	San Diego public school students grades 2–11	Estimate effects on gains; include student fixed effects	Reading: 1.40
			Math 1.9
Burke and Sass (2008)	Florida students grades 3–10	Student and teacher fixed effects	Reading: 0.014 to 0.068
			Math: 0.04
Hanushek, Kain, Markman, and Rivkin (2003)	Texas Schools Project	Student fixed effects	Math: 0.17
Hoxby and Weingarth (2005)	Wake County North Carolina	Exogenous reassignments due to school desegregation	0.24[2]
Ammermueller and Pischke (2006)	Students in Germany, France, Iceland, the Netherlands, Norway, and Sweden	School fixed effects	0.11
Carman and Zhang (2008)	Chinese elementary school students	Randomly assigned classrooms; teacher effects and student effects	Math: 0.40
			English: −0.03
			Chinese: 0.26
Lefgren (2004)	Chicago public schools grades 3 and 6	Uses school tracking policies as an instrument for peer ability	Math grade 6: 0.032
			Reading grade 6: 0.027

**Most papers have standardized the test scores to be mean zero variance 1. This is not required, though, to compare coefficients across papers. The effects of racial composition from Hoxby (2000) are the effects on black students of changing the cohort's percentage black. Intra-race effects are larger than cross race effects. 1. Angrist and Lang's results are not statistically significant. These effects are translated by me into a peer effect coefficient using the calculation described in the text. 2. This is from Hoxby and Weingarth's baseline linear-in-means specification.

Lavy and Schlosser (2007) also find large effects from percent girls within a classroom and they also interpret these effects as working through more than simply increasing peer average test scores. The authors report effects on a wide range of outcomes. Table 4.2 is limited to reporting their effects on average scores on matriculation exams. For females the effect of percent female on average matriculation score has an effect of 5.3. Since females on average score about 6.3 points better, this implies a peer coefficient ($\beta 1$) of $5.3/6.3 = 0.84$. The corresponding calculation for males yields an estimated $\beta 1$ of 1.06.

Lavy and Schlosser's additional results suggest that much of the effect of percent female is working through reductions in classroom violence and disruption and through improvements in inter-student and student–teacher relationships.

Kramarz, Machin, and Ouazad (2008) have the complete census (National Register) of children in English public schools and identify peer effects using movers. For second graders the authors find that a 10% increase in the percentage of boys reduces test scores by 0.004 standard deviations.

Whitmore (2005) exploits experimental variation in Project Star to examine separately the effects from percent female and the effects from having higher achieving peers. She finds that each 1.0 randomly generated increase in peer percentile score raises a student's own percentile score by 0.6. And having a predominantly female class has an independent positive effect of 1.3 percentile points.

Boozer and Cacciola (2001) use the random assignment of peers in Project Star and argue that much of Project STAR's class size effect is working through peer effects. Their insight is that entry and exit from the classrooms in the experiment caused some peers to be treated longer than others and that this generates experimental variation in peer quality. The authors find coefficients on peers' mean test score of 0.30, 0.86, and 0.92 for the first, second, and third graders respectively.

Angrist and Lang (2004) use Boston's "Metco" busing program as an exogenous source of variation in peer ability. Their sample includes 443 Boston area schools, 141 of which receive Metco students. Their outcome measures are MCAS scores and Iowa Test of Basic Skills scores. Using all schools in their sample, they find that percentage Metco in a suburban school has no statistically significant effect on the non-bused (i.e., local) students in the school.

When the authors examine Brookline schools specifically and performance on the Iowa Test of Basic Skills, they find peer effects from Metco students which are negative in the point estimates though the estimates are not generally statistically significant. A 10% increase in percent Metco lowers the percentile ranking (for non-Metco students) on the IATB by a statistically insignificant 0.5 percentile points. This same change in percent Metco lowers peer average test scores by 2.4 percentile points. Together these numbers imply a peer effect coefficient of 0.21. Limiting the sample to just black non-Metco students yields a larger implied peer coefficient of $-3.3/-2.4 = 1.375$.

In a similar spirit, Imberman, Kugler, and Sacerdote (2009) examine peer effects of Hurricane Katrina evacuees on nonevacuee students in Houston and Louisiana. They find modest peer effects which are greatest for students at the lowest quintile of the test score distribution. As noted above, when they allow for nonlinearities, their estimated peer effects are much larger. If 10% of the school is composed of evacuees from the top quartile of the English Language Arts (ELA) test score distribution, "native" students from the top quartile of the test score distribution have an ELA test score gain of 0.2 standard deviations. These same high achieving native students experience a drop of 0.3 standard deviations in their ELA score in response to a 10% inflow of low achieving (bottom quartile) evacuees.

In addition to exploiting experiments and natural experiments, another method of identifying peer effects is to calculate the effects of peer test scores while including school, school by year, or student level fixed effects to control for the sorting of students into schools. Typically peer mean ability is measured as mean performance on a prior year's standardized test.

Betts and Zau (2004) use data from the San Diego Unified School district. They use test score gains as their dependent variable and they employ student level fixed effects to control for positive selection (tracking) of students into classrooms. They find a coefficient on the peer mean reading score of 1.4 and a coefficient on peer math score of 1.9. They also find some evidence of nonlinearities in which the average student is hurt more by low achieving peers than she is helped by high achieving peers.

Hanushek, Kain, Markman, and Rivkin (2003) utilize data from the Texas Schools Project. They use student gains in the TAAS as their dependent variable and they include student fixed effects. They find a peer effect coefficient ($\beta 1$) of 0.17 for math scores. Furthermore they find that the peer effect is similar across quartiles of a student's initial position in the test score distribution, though students in the highest quartile do show a smaller peer effect.

Burke and Sass (2008) follow a similar approach and include both student fixed effects and teacher effects. For math scores, they estimate that a 1.0 increase in peer mean achievement raises own achievement by 0.04. For reading, the effect varies from 0.014 to 0.068. But, when the authors allow the effects to vary by student type, the estimated effects are much larger. For example, elementary students in the lowest third of past performance experience a gain of 0.82 points for every 1.0 gain in peer achievement. Peer effects are somewhat smaller for students in the middle third of the distribution and smaller still for students in the highest third. As the authors note, these results suggest that tracking does not maximize total output of test scores, but rather high ability students should be spread among classrooms.

In contrast to some of the above studies, Vigdor and Nechyba's (2007) results call into question the methodology of using school fixed effects to identify peer coefficients. These authors have data on all North Carolina public school students during 1994/1995 through 2000/2001. Within that set, they include school and year fixed effects.

In their baseline "naïve" school fixed effects approach, Vigdor and Nechyba find that a 1.0 standard deviation increase in peer mean reading score yields a 0.05 to 0.07 standard deviation increase in a student's own reading score. The comparable effect for math is 0.06 to 0.08. Of equal interest, the authors find strong support for the hypothesis that increasing dispersion of peers mean scores *raises* own test score. However both of these results flip signs (and are negative and statistically significant) when the authors include teacher fixed effects. Furthermore, in a falsification exercise Vigdor and Nechyba find that 5th grade peers appear to improve 4th grade test outcomes in their baseline specification. These results lead the authors to conclude that simple measures of peer effects may actually be driven by teacher effects or by selection into classrooms.

Several additional studies estimate linear-in-means models outside the U.S. Ammermueller and Pischke (2006) use data encompassing six European countries and find an average coefficient on peer achievement (β1) of 0.11. With a sample of Chinese elementary school students assigned randomly to classrooms, Carman and Zhang (2008) find coefficients of 0.4 for math scores, −0.03 for English scores, and 0.26 for Chinese scores. McEwan (2003) finds large effects from peers' background characteristics in a sample of 8th graders in Chile. Both Gibbons and Telhaj (2008) and Lavy, Silva, and Weinhardt (2009) find no evidence of linear-in-means effects for secondary school students in the U.K. Both of these papers use the full set of U.K. students at age 14 for several recent cohorts. They use Key Stage 2 national test scores (age 11) to measure peer inputs and Key Stage 3 national test scores (age 14) to measure outcomes.

4.1 More on Nonlinear Effects and Tracking

Hoxby and Weingarth (2005) proceed to a more general estimation of nonlinear effects. The authors rely on Wake County North Carolina's school reassignment policies which sought to even out disparities in average student backgrounds at each school. The authors instrument for actual peer groups using the peer groups that would be have been generated due to the reassignment rules. They allow the magnitude of peer effects to vary both by a student's *own position* in the test score distribution and by which part of the peer test score distribution is being increased. Specifically they interact own test score decile with the percentage of peers in each test score decile.

Hoxby and Weingarth find support for the boutique model of peer effects. Students in deciles 9 and 10 of the test score distribution benefit strongly from adding peers in the highest deciles. Students in the bottom decile benefit most from adding peers in deciles 2 and 3. The authors also find some evidence for the focus model, which is to say that students can be harmed by heterogeneity in their peers even when additional hetereogeneity might be giving the student additional peers more like herself.

Cooley (2009) provides further evidence of large nonlinearities in peer effects. She finds that the magnitude of peer effects experienced by student *i* differs both by student *i*'s race and by student *i*'s achievement and by peers' level of achievement. And she finds that high achieving students benefit the most from high achieving peers. Gibbons and Telhaj (2008) find that test scores for low achieving students in the U.K. are harmed by the presence of high achieving students while upper-middle achieving students benefit from the presence of high achieving students.

Lavy, Silva, and Weinhardt (2009) reach slightly different conclusions about nonlinear peer effects. These authors find that all students are harmed by the presence of the lowest achieving peers (those in the bottom 5%). However, only high achieving girls are helped by the presence of other high achieving (top 5%) peers.

Many of the results on nonlinearities are consistent with a world in which tracking students into classrooms by ability raises total student output relative to a set of untracked classrooms. This seems quite plausible, particularly given that tracking has been a feature of school systems for so long despite its potential to appear anti-egalitarian. This pro-tracking result is somewhat at odds with what Burke and Sass (2008) found (namely that low ability students benefit the most from high average peer ability) and Vigdor and Nechyba's (2007) result that classroom heterogeneity raises scores.

Roughly half of the research addressing tracking specifically finds positive effects from the policy. Using an experiment in Kenya, Duflo, Dupas, and Kremer (2008) find that students in tracked classrooms experienced test score gains of 0.14 standard deviations relative to students in untracked classrooms. And the effect persists for at least one year following the elimination of the program. Lavy, Paserman, and Schlosser (2007) find that high achieving students benefit from the presence of other high achieving students in the classroom while the high achieving students do not help average students. On the other hand, Betts and Shkolnick (2000) find little support for benefits from tracking. Lefgren (2004) reaches the same conclusion using data on 3rd and 6th graders in the Chicago Public Schools. He uses tracking status as an instrument for peer group ability and he finds very small linear-in-means peer effects, and hence few benefits from tracking.

Peers may also influence the aspirations of students. Jonsson and Mood (2008) examine Swedish secondary school students and find that having very high achieving peers depresses the desire to attend university for average students.

4.2 Effects of Racial Composition

Peer effects may of course stem from peer characteristics other than measured achievement. The evidence suggests that peer racial composition is strongly correlated with own achievement. For example there is a burgeoning literature on "acting white" that posits that some black students may underachieve in order to fit in with their peers (Austen-Smith and Fryer (2005), Fordham and Ogbu (1986), Ogbu (2003)).[8] Fryer

[8] Cook and Ludwig (1997) do not find that average attitudes toward academic success differ between black and white students.

and Torelli (2010) find that a student's popularity is negatively associated with her academic grades for the highest achieving black students. They also find that the acting white effect does not exist in schools with a high proportion black, perhaps because the market for social interactions with other high achieving black students is thick in such schools.

Using data from the Texas Schools Project, Hanushek, Kain, and Rivkin (2009) find that black students' test scores are strongly decreasing in the fraction black in the school. To identify the effects of racial composition, they rely on variation in the fraction black within a school over time. A 10 percentage point increase in the fraction black is associated with own test scores that are roughly 0.02 standard deviations lower. The fraction black also affects test scores for white students but the coefficients are half as large. This makes sense if we believe that many more peer interactions take place within race than across race.[9] The authors point out that differences in racial makeup across Texas schools can account for 10–20% of the black-white test score gap. Hanushek and Rivken (2009) go on to show that the effects of racial composition are highly nonlinear. Black students in the top quartile of the achievement distribution (based on prior years' scores) are affected much more negatively by the school fraction black than are black students in the lower half of the test score distribution.

There are numerous channels through which peer effects from racial composition may occur. It is possible that teachers lower their expectations or the level at which they teach as the fraction black in a school rises. Or maybe there is an acting white effect for black students which increases as the proportion black rises.

4.3 Effects Working through Classroom Disruption

Lazear (2001) suggests that the most significant effects are negative ones emanating from disruptive peers and, as noted above, Lavy and Schlosser (2007) agree that disruption (or lack thereof) is a key mechanism. Perhaps the most innovative paper on this topic is Figlio (2005) which finds that the presence of boys with female sounding names increases classroom disruption and decreases test scores for students in that classroom. Similarly, Carrell and Hoekstra (2008) show that the classroom presence of children exposed to domestic violence raises classroom discipline problems and lowers math and reading test scores. Adding an additional troubled boy to a classroom raises the probability that another boy commits a disciplinary infraction by 17% and lowers test scores by two percentile points.

[9] Marmaros and Sacerdote (2006) and Mayer and Puller (2008) both measure the relative frequency of within- versus cross-race interactions.

5. GOING BEYOND TEST SCORES

The above studies document the existence of peer effects in test scores, though with considerable disagreement as to the magnitudes. Effects on nontest score outcomes for youth are quite possibly larger and the existence of such effects less controversial. Case and Katz (1991) instrument for peer average actions using peer family background and find large peer effects in drug use, gang membership, and criminal activity. For instance, when own drug use is the dependent variable, the effect of peer average drug use is 0.32.

Gaviria and Raphael (2001) perform a similar analysis using students in the same school and grade in the National Education Longitudinal Survey (NELS). They find strong peer effects in drug use, alcohol drinking, cigarette smoking, church going, and the likelihood of dropping out of high school. Their coefficient on peer group average drug use is 0.25. Kooreman (2003) finds a very similar coefficient when looking at the effect of classmate alcohol expenditure on own alcohol expenditure in a sample of Dutch students.[10]

Evans, Oates, and Schwab (1992) consider peer effects in teen pregnancy and dropping out of school. They find that naïve estimates of peer effects are overstated and that once they control for selection into peer groups their estimated effects disappear. Similarly Krauth (2005) finds that estimated peer effects on smoking are reduced by a factor of three once he controls for selection into friendship groups. His preferred estimator suggests that having an additional friend who smokes leads to a 5 percentage point increase in the prevalence of own smoking. Mihaly (2008) estimates that having one additional friend who smokes is associated with a 9 percentage point increase in own prevalence of smoking.

Eisenberg (2004) uses the Adolescent Health Survey and considers the experiment of a substance-using friend moving away (relocating). He finds that the moving away of a friend who uses marijuana leads to a 12% reduction in the probability of own marijuana use.

Argys and Rees (2008) look at the effects of having older peers in the same grade. Using NLSY97 data they examine students in grades 6–12 and find that having older peers increases the use of alcohol, marijuana, and cigarettes. Controlling for own age, being in the younger half of the peer group raises the likelihood of drinking by 3.5 percentage points.

Bobonis and Finan (2009) are able to use experimental variation from Mexico's PROGRESA program to measure the degree to which peer participation in school raises own participation. A 10 percentage point increase in peer participation raises own participation rates by 5 percentage points.

[10] Kooreman also finds large peer effects in time allocated to certain activities including studying and part-time jobs.

Kremer, Miguel, and Thornton (2009) is another paper that identifies peer effects by taking advantage of experimental variation in a developing country context. The experiment discussed in the paper gave girls financial incentives to perform well on exams. But the experiment also had large spillover effects on the boys in the same schools despite the boys not being eligible for the cash rewards. In the experimental treatment group, girls' scores rose on average by 0.29 standard deviations while (the ineligible) boys' scores rose by 0.16 standard deviations.

Some of the most interesting and convincing evidence on peer effects in social outcomes comes from the Moving to Opportunity (MTO) Experiment (Katz, Kling, and Liebman (2001) and Kling, Ludwig, and Katz (2005)). MTO randomly offered some low-income families an incentive to move to a census tract with a lower poverty rate than the initial census tract. Results from the experiment show that girls in families offered such a voucher showed decreased arrests for violent and property crimes while results for boys were more mixed.

6. EFFECTS IN POST-SECONDARY EDUCATION

Post-secondary education boasts its own literature on peer effects though the empirical approaches and outcomes examined differ a bit from the literature on younger students. A host of papers have utilized the random assignment of students into housing units in order to examine the effects of roommates, dormmates, and squadron members on own outcomes.

Sacerdote (2001) examines the effects of roommates and dormmates on college GPA and the likelihood of joining a fraternity. Roommate academic ability has a modest impact on own academic performance. For example, assigning a student a roommate who was in the top 25% of incoming admissions scores will raise the student's freshman year GPA by 0.06. Several other studies find either no effect from roommates on academic outcomes or modest effects. Foster (2006) finds no effect from randomly assigned roommates at the University of Maryland. Zimmerman (2003) finds that roommate verbal SAT matters more than math SAT. He finds that students are harmed somewhat by being assigned a roommate in the bottom 15% of the distribution. Hoel, Parker, and Rivenburg (2005) find strong nonlinear effects from roommates and no detectable effects from peers in the same classroom.[11] Stinebrickner and Stinebrickner (2006) shows that roommate ACT is less important than roommate high school GPA and that a peer effect on hours spent studying in college may be the mediating influence. Fletcher and Tienda (2008) find that college students benefit from having more peers who attended their same high school, presumably because such peers serve as a support network.

[11] Martins and Walker (2006) and Parker, Grant, Crouter, and Rivenburg (2008) also find no evidence of classroom peers on own grades.

Several other papers look for peer effects within squadrons at the U.S. Military Academy (Lyle (2007)) and U.S. Air Force Academy (Carrell, Fullerton, and West (2008)). These contexts are particularly interesting because the Academies enforce a great deal of interaction within squadrons. Lyle finds that there are peer effects in first year GPA, but that common shocks in the form of the upperclassmen in the squadron may account for half or more of the estimated "peer effect." Carrell, Fullerton, and West find large peer effects on first year GPA at the Air Force Academy and they attribute this finding to the unique setting in which the true peer group is well-measured.

Brunello, De Paola and Scoppa (2010) use data from a university in Italy and examine how course grades are influenced by peers enrolled in the same course. They find that a one standard deviation in classmate ability is associated with a 0.08 standard deviation increase in own grades.

A variety of the papers move beyond college GPA as the outcome of interest and find some very interesting and statistically significant effects of peers. Duncan, Boisjoly, Kremer, Levy, and Eccles (2005) use randomly assigned roommates to examine peer effects in drinking, marijuana use, and sexual activity. Among male students who binge drank in high school, assigning them a roommate who also binge drank in high school leads to a fourfold increase in the number of binge drinking episodes per month. There is no evidence of a comparable effect for females. The peer effects on number of sexual partners are smaller and not robust to changes in specification, and the peer effects on marijuana use are small and statistically insignificant. DeSimone (2007) finds that after controlling for selection into fraternity membership, being a fraternity member raises the frequency of binge drinking by 15 to 20 percentage points. And Wilson (2007) finds large peer effects in smoking.

In subsequent work, Duncan, Boisjoly, Kremer, Levy, and Eccles investigate whether having a minority roommate or a high income roommate affects student attitudes one year after initial room assignment. White students assigned a black roommate report support for affirmative action that is one half to two thirds of a standard deviation higher (on a four-point scale) than white students assigned nonblack roommates. More generally, students assigned minority roommates are more likely to report that they are comfortable interacting with people of a different race or ethnicity. Students assigned a high income roommate are one-third of a standard deviation less likely to support the statement that "wealthy people should pay more taxes."

Carrell, Malmstrom, and West (2008) examine peer effects in academic cheating at West Point. They find that adding a student who cheated in high school to the college class results in an additional 0.33 to 0.47 cheaters in the college cohort.

Finally, Sacerdote (2001) finds large peer effects in whether or not students join fraternities or sororities and large effects in which specific Greek organization that they join. If a roommate joins a fraternity, a student is 8% more likely to do so. Moving the student from a dorm in which no one joins to a dorm in which everyone joins raises the likelihood of the student joining a fraternity by 32%.

Perhaps most interesting from the perspective of labor economists is the question of whether peers are important in the career or job selection process. In Marmaros and Sacerdote (2002), we define an indicator for whether or not a student takes a high paying job, defined as finance, consulting or law, as her first job. We regress the student's outcome on the average outcome for the student's randomly assigned first-year dormmates and we find a statistically significant coefficient of 0.24. We also find that students rely heavily on their peers and their peers' parents in the job search process. Arcidiacono and Nicholson (2005) examine peer effects in specialty choice among medical students and they find little evidence of peer effects. De Giorgi, Pellizzari, and Redaelli (2007) find that a student's peers at Bocconi have a significant impact on choice of major. This paper uses the novel identification strategy of relying on the background of an individual's peers' peers as an instrument for the peers outcome (major choice).

Overall the literature on peer effects at the university level is fairly consistent. First, most authors find small peer effects in GPA from roommates. One key exception is the Air Force Academy study which is from a unique environment. Measured peer effects in GPA would likely be larger at other institutions if we knew the true peer group rather than limiting ourselves to roommates. There are much larger effects on social outcomes like drinking, cheating, and fraternity joining. And there is some evidence that peers are important for career choice. One next logical step may be the designing of experiments to see whether the peer effects measured in observational data can be exploited by university administrators to maximize GPA or some other objective. Carrell, Fullerton, and West (2009) find some nonlinearities in the peer effects at Air Force and they are currently running an experiment to see if the "optimal" allocation of cadets to squadrons improves GPA and physical fitness scores.

Another strand of the literature uses detailed data on interactions among university students to look for patterns of social interaction and to learn about the determinants of who is friends with whom. Marmaros and Sacerdote (2006) do this using email data and Mayer and Puller (2008) use Facebook.com data. The conclusions from the two studies are remarkably consistent. There is very strong same-race attraction in the determination of friendships, with black students being 10 to 20 times more likely to interact with another black students than a nonblack student. And proximity matters a great deal. Students who share a first-year dorm are four times more likely to interact than students who do not.

7. CONCLUSIONS

Recent years have brought a flurry of attention to the modeling and measurement of peer effects in education at the primary, secondary, and university levels. Within the university literature, there is a fair amount of agreement that peer effects from roommates and dormmates in determining GPA likely exist but are modest in size. In contrast, the peer effects in determining certain social outcomes (drinking, fraternity joining, political attitudes) are a fair bit larger and potentially peer effects are a major determinant of such outcomes.

The jury is still out on the exact size of peer effects in primary and secondary school test scores. The studies detailed in Table 4.2 encompass a fairly large range of estimates. However, many researchers agree that the linear-in-means model (the focus of Table 4.2) is probably not the most interesting model anyway. The more interesting question is whether there are nonlinearities that make policies like tracking of students into classrooms a really good or a really bad idea. Hoxby and Weingarth (2005) is the benchmark study in this regard, though many of the other studies discussed here also test for nonlinearities and in some cases find substantial nonlinear effects. For instance Burke and Sass (2008) find that low achieving students benefit more from high ability peers than do high achieving students, and Vigdor and Nechyba (2007) find that classroom heterogeneity is good for test scores.

There is broad agreement that increasing the number of girls in the classroom is associated with less disruption and higher test scores. And the effects are big enough that the effects likely work through more than just increases in peer mean scores. The result that more boys translates to more violence seems to fit with the personal experience of many authors, though fortunately does not point to any obvious policy prescription.

Most parents and students behave as if peer effects matter a great deal and the findings of the literature are consistent with this. As Winston and Zimmerman (2003) note, students are both consumers of the educational product and part of the production function. Tracking by ability is in such widespread use that it would not be surprising if future studies find more evidence that is consistent with the boutique and focus models, which imply benefits from tracking.

While directions for future research are not entirely obvious, the summary presented here may give us some hints. Additional data with exogenous assignment of students to classrooms would help clarify the size and nonlinear nature of peer effects in elementary and secondary school. Actual experiments will allow us to learn whether peer effects in observational data can truly be exploited by policy makers. And more work that identifies the true peer group, as opposed to the peer group we can measure may cause us to revise upward our estimates of the importance of peer effects.

There are several major questions that researchers and policy makers should address in the coming years. First, now that we have measured the existence and importance of peer effects for a variety of outcomes, how large are these peer effects relative to the influences of teacher quality, school quality, and home environment? Second, can measured peer effects actually be exploited by policy makers in order to increase total learning or to decrease criminal behavior or drug use? In researching peer effects have we been describing and studying an important policy tool or rather is this a part of human behavior that is worth understanding but not particularly relevant for policy? And following the Coleman Report's (1966) prescient writing, how much can we actually benefit disadvantaged students by changing the peer group with whom these students interact?

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