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Peer Effects in Higher Education

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I. Introduction

> An important part of a child's school environment consists not of the physical facilities of the school, the curriculum, and the teachers, but of his fellow-students. A child's fellow-students provide challenges to achievement and distractions from achievement; they provide the opportunities to learn outside the classroom, through association and casual discussions. Indeed, when parents and educators think of 'a good school' in a community, they most often measure it by the kind of student body it contains: college-bound and high achieving. Parents strive to send their children to such 'good schools,' recognizing that whatever the quality of the staff, curriculum, and facilities, the level of instruction must be geared to the student body itself.
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> The Coleman Report, 1966

"Peer effects" describe the result of an interaction between two or more people in which the characteristics or behavior of one affects the behavior of the other – one student's interest in academic issues, for instance, affects the curiosity and learning behavior of another. And "peer" should be taken seriously to eliminate influences of parents or professors – peers are equals.

While interest in peer effects clearly goes back to decades of parental warnings, "Be careful not to fall in with a bad crowd," the attention of scholars, policy makers, and especially economists, began in earnest with the Coleman Report in 1966 (Coleman, Ernest Q. Campbell et al. 1966). It reported on a study mandated by the Civil Rights Act of 1964 to identify the sources of educational achievement in order, in turn, to redress inequalities in educational opportunity. The Coleman study found that peer effects – the qualities of classmates – were more important in determining a student's achievement than teachers and staff and that they, in turn, were more important than a school's economic resources. That finding, of course, was relevant to far-reaching policies in public education including bussing,[1] sorting or tracking students by ability, magnet schools, and most recently the debates on vouchers and even classroom "inclusion" of disabled students. And the finding of relative impotence of resources – especially in class size – has been challenged ever since (Hanushek 1986; Lazear 1999).

But while the Coleman Report established the importance of peer effects in education, it also set their context as that of public, K-12, education and it's in that context that most, by far, of the thought, analysis, and investigation of peer effects has taken place. Yet in the past few years, it has become increasingly evident that peer effects may play a central role, too, in economic structure and the behavior of college students, universities and higher education and while that different context raises some of the same and familiar questions, analytically and empirically, it gets rid of some of them and raises new ones.

So in this chapter, Section II will describe the background of the study of peer effects with, inevitably, emphasis on primary and secondary schools. Section III will turn to higher education and the role of peer effects in that context. Section IV will address the very different empirical challenges that higher education presents for their investigation and summarize the emerging empirical evidence. Section V describes our empirical strategy for estimating peer effects using the Mellon Foundation's *College and Beyond* data. Section VI describes that data and Section VII, our empirical findings.

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[1] The Coleman study was prominently cited in the Supreme Court decision on bussing (1971).

Finally, Section VIII concludes with an agenda for investigation of peer effects in colleges and universities and their role in these markets.

II. The Background – Peer Effects in K-12 Education

The Coleman Report not only concluded that peer effects existed and were significant in shaping educational attainment – so students were seriously advantaged or disadvantaged by their fellow-classmates – but it asserted, too, that those effects were non-linear – that the weak student benefited more from association with strong classmates than those strong students lost in associating with weaker classmates. As Hoxby noted, their existence allowed a distributional argument for mixing of abilities – to tip the balance of who gained and who lost in favor of the currently disadvantaged – while their non-linearity added an argument of efficiency – society in a world of (the right) non-linear peer effects would be better off in aggregate achievement if there were mixing of students of different abilities (Hoxby 2000). After Coleman, peer effects would play a powerful role in social policy.

So it was inevitable that the quality of the statistics and reasoning supporting those conclusions would come under close examination, and it has. Three difficulties were identified: Summers and Wolfe (Summers and Wolfe 1977) replaced student achievement with changes in achievement in order to separate "value added" from levels of ability; selection was seen to be a serious contaminant of the peer effect evidence in cross section data like that used by Coleman (and most subsequent scholars) – are student behaviors similar because of peer influence or because similar people want to be together? -- and, closely related in the American setting where residential location and school are determined jointly, it's hard to de-link them to know whether any observed peer effects are working in the classroom or in the neighborhood or both.[2]

While the standard approach since Coleman has been a sort of educational production function (Summers and Wolfe 1977; Hanushek 1986; Robertson and Symons 1996), it's useful to frame the issues even more simply as the "elementary particle" of peer effects in which individual 1's behavior, B_1, is affected by individual 2's characteristics, C_2, or behavior, B_2, along with a vector of all other relevant circumstances and characteristics, X_1,

$$(1) \qquad B_1 = f(C_2, B_2, X_1).$$

Then peer effects exist if the partial derivative of B_1 with respect to B_2 or C_2 is non-zero. Peer effects are non-linear if the second partial derivative with respect to either variable is non-zero. Equation (1) provides a convenient way to frame the issues.

Hoxby separated these two partials to differentiate peer effects that rest on characteristics – race and gender – from those that rest on behavior – test performance (Hoxby 2000). Manski noted that peer group effects arising from peers' behavior can be

[2] These are different: "Is there a peer effect in classrooms, or is it happening in the neighborhood?" And "It is happening in the classroom, but because of selection of students into the neighborhood, hence school and class, you can't use cross section data to establish the peer effect."

amplified by feedback – as group behavior affects individual behavior which, in turn, affects other individual behavior in the group, which… – while peer effects resting on peers' characteristics stop there (Manski 1993). He described one as "endogenous" (to the group) and the other exogeneous.

Selection effects appear when B_2 is a function of 1's preferences,

$$(2) \quad B_2 = u_1(B_1)$$

so $\partial B_1/\partial B_2$ becomes $\partial B_1/\partial u_1(B_{1..})$

The linkage of residence and school presents a slightly different problem in that while classroom behavior, B_1, can be identified, classroom and neighborhood influences, B_2^s and B_2^n, cannot be separated. If, for example,

$$(3) \quad B_2 = B^s{}_2 + B^n{}_2 \,,$$

then $\partial B_1/\partial B_2$ doesn't tell about what's going on in the classroom, $\partial B_1/\partial B^s{}_2$. So when it's classroom peer effects that are under scrutiny – as amenable to public policy – they are again unobservable.[3]

[3] See Evans (Evans, Oates et al. 1992) (Jencks and Meyer 1989) on neighborhood effects in, e.g., pregnancies where this separation is not at issue.

Selection bias has been handled in familiar ways. It's frequently ignored, as in Coleman's original work. It's treated through an explicit effort to identify the relevant determinants of selection (2) so they can be statistically accounted for (Heckman 1979). It's avoided by a natural experiment that generates (hopefully) exogeneous variation in the relevant variables, thus protecting the estimates from any bias induced by choice and selection. This is what Hoxby did with Texas data where the variations in peer characteristics and test performance among 3rd through 6th graders were generated by yearly cohort variations within the schools and classes among which parents might well select to increase peer quality (1999). She assumed that even eager parents would lack information to act on cohort differences as they'd quite likely act on more persistent differences between schools or between classes within schools. Finally, selection bias can sometimes be dealt with by a formal experiment where random assignment of peers is imposed as in the psych lab and college roommate data reported below.

The linkage of residence and classroom as a reason for distortion of classroom peer effects was addressed by Zimmer and Toma who used data from five countries that have very different institutional relationships between residence-neighborhood and school assignment. So the US pattern of joint determination could be compared with French, Canadian, New Zealand, and Belgian patterns that support private school choice apart from residential location (Zimmer and Toma 2000) and their conclusion was that peer effects were quite evident without residential influences.

All this is the now-traditional study of peer effects in K-12, deriving from Coleman's charge: how do peers affect learning outcomes and how do they potentially obscure evidence of the effects of other things on learning? The purpose is to identify what can be influenced by public policy in the hope of reducing educational disparities.

But with school voucher plans came a new and different role for peer effects in a market-driven K-12 education. Voucher programs introduced, of course, student choice among schools but also, at least to a limited degree, school choice among students – selection – and along with that came individual prices, the opportunity for price discrimination among students, and, frequently, commercial incentives and market competition driving the system to market clearing prices under individual schools' profit constraints. This was the familiar stuff of equilibrium modeling of firms, customers, and industries (Caucutt 1998; Epple and Romano 1998). So the role of economic analysis of peer effects shifted from empirical questions of their significance and shape – as determinants of educational achievement – to theoretical exploration of the equilibrium outcome of a market system of voucher-based school (and student) choice where schools select students to exploit peer effects to improve their product quality. The question was no longer, "Do peer effects exist?" but "If they exist, what are the implications of a market-based voucher system of school and student choice?"

Finally, in the most inclusive modeling of the role of K-12 peer effects, de Batrolome combined them with school choice, voucher payments, educational price discrimination, residential choice, and residential pricing, to ask about the social optimality of market behavior when school choice and Tiebout-type residential selection are both at work (de Bartolome 1990). Do voucher systems, in disconnecting residence and school peer quality, induce socially desirable adjustments in housing demand and hence prices? He concluded that either peer effects with vouchers or Teibout neighborhood effects could, alone, produce optimal allocations, but putting them together produces the second-best outcome that includes ability-abandoned schools in poor neighborhoods. This, again, is in the new tradition of modeling rather than the old tradition of finding, empirically, the determinants of educational achievement.

So, in sum, the K-12 empirical literature has firmly established the significance of peer effects, *per se*, on educational attainment and led to the deeper questions of how peers affect each others' learning – Lazear's studies of class size, disruption, discipline and achievement, for an important instance (1999). On non-linearity, there is a good deal of evidence that Coleman was right and weak students do gain more from mixing than strong students lose, but the results are not conclusive and Lazear's work suggests that there's enough going on in classrooms that simple generalizations on linearity may not emerge for some time. Most recently, voucher models have introduced strong commercial incentives to capitalize on differences in peer quality through price discrimination.

III. Peer Effects in Higher Education

In the economics of colleges and universities, peer effects have been largely ignored (or taken for granted) until recently. Two things have changed. The data have increasingly revealed higher education to be an industry with an economically odd structure and behavior of firms but one that would make economic sense – if unorthodox economic sense – if peer effects were significant (Rothschild and White 1995; Winston 1995, 1999). And more recently, the peer effect model developed by Epple and Romano to describe voucher systems in K-12 has been applied to higher education, producing a plausible fit with the data (Epple, Romano et al. 2001).

The anomalies that make higher education resistant to familiar economic logic and intuition – to the discomfort of policy makers and university Trustees – are quite fundamental (Winston 1995, 1997, 1997): Colleges turn away a majority of the potential customers who are willing and able to buy their product, if they can; they lower price to attract a student, replacing one who'd pay a higher price; they judge institutional quality by how many customers they can turn away and they manipulate admission policies to increase that number;[4] they require elaborate application procedures before one is allowed to make a purchase; they often make their customers live together in order to maximize their contacts; they practice extensive price discrimination, not always to increase sales revenues. All this is strange behavior for firms producing and selling services; all this is quite rational if these services are produced in part by the interactions of their customers . But until recently (Zimmerman 1999; Stinebrickner and

[4] Most recently, early decision – see Fallows (2001) or Avery-Zeckhauser (2001).

Stinebrickner. 2000; Goethals 2001; Sacerdote 2001) there has been virtually no evidence that these peer effects actually exist – some have extrapolated from K-12 evidence(McPherson and Schapiro 1990), but more generally, the subject has been ignored or assumed to be obvious (Kilitgaard 1985)[5].

It's useful to review what the data reveal about industry structure and firm behavior in higher education and the rationalizing role peer effects can play (Winston 1999). Two theoretical contributions frame those data: Rothschild-White's demonstration of what peer effects in production would do to a familiar market equilibrium (Rothschild and White 1995) and Hansmann's examination of the economic rationale of non-profit firms (Hansmann 1980).

If the production function for higher education includes peer quality as an input, then, Rothschild-White showed, the otherwise simple product sales transaction is, in fact, two transactions that exchange output (educational services) and input (peer quality) simultaneously, revealing only a single net price that reflects the difference between the two underlying prices. The student (college) is, at one and the same time, the buyer (seller) of educational services and the seller (buyer) of peer quality. So, in contrast to conventional product sales, the firm <u>cares</u> who it sells to as different student-customers are suppliers of different amounts of peer quality. As buyers, students are all the same; as input-suppliers they differ. So schools select their customers, if they can.

[5] See Pascarella and Terenzini for a survey of the early literature (1991)

Higher education markets are further complicated by the dual nature of colleges and universities – the fact that they are only partly ordinary firms, generating revenues by making and selling a product for a price. They're also charities, deriving revenues from donors' contributions in support of their charitable activities – what Hansmann called "donative-commercial non-profit enterprises" (Hansmann 1980). To give that some concreteness, in 1995-6, public and private donations, past and present,[6] accounted for three-quarters of the total flow of economic resources into US higher education while commercial sales (tuition) revenues made up only the remaining one quarter (Winston, Carbone et al. 2001). It is these donative resources that support the student subsidies that schools use, in turn, to pay for student peer quality. They averaged $8,400 per student in 1995-6 as an education costing $12,400 to produce was sold at an average net price of $4,000 (Winston 1999).

But – the final complicating fact – those charitable donations are quite unevenly distributed among colleges and universities so national averages hide large differences in institutions' donative wealth and hence their ability to offer student subsidies and attract peer quality. Those differences create the hierarchy that's apparent in Table 1. and the skewed distribution of Figure 1. Public and private sectors differ markedly, not in how much they subsidize their students (which is surprisingly similar), but in how they do it; public sector schools sell a modestly expensive education for a very low net price while private schools, on average, sell a much more costly education for a much higher price.

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[6] Ie, to account for current collegiate wealth (financial and physical) that's derived from past donations and generates current resource flows (revenues and capital services) (Winston 1993).

This is the economic environment in which colleges compete to sell their product and buy the peer quality with which to produce it. It is an environment in which differences among schools in their command over donative resources – with which to bid for student quality by providing educational services and discount prices – are pronounced and go far to determine which schools get how much of it.

Peer quality is scarce. The cumulative distribution of combined SAT scores in Figure 2 describes the pattern of SAT performance over the one million high school seniors who took the test in 1993.[7] Evidence of the success of institutional wealth in securing peer quality via student subsidies is reflected in the correlation between schools' subsidies and their average SATs that is evident in Table 2. These are crude measures of donative wealth and peer quality, but over the 877 schools for which we have the data reported in Table 2, the relationship is strong – a simple bivariate regression of student subsidy on schools' average SATs is highly significant.

Finally, peer effects explain the seductive and expanding role of merit aid as schools offer price discounts (higher wages) to individuals who bring them above-average peer quality, justifying the replacement of a lower quality student who would pay a higher price.

It should be said that, as implied by Figure 2 and Table 2, the scarcity of peer quality among students means that peer effects motivate college behavior more powerfully at the top of the hierarchy; schools that are increasingly unable to compete for

[7] This will be replace by current SATs when data arrive from College Board.

talented students are under pressure to attract paying customers of any sort and competition for peer quality inputs is gradually replaced by a more conventional competition for customers – competition becomes more typically commercial and more amenable to conventional analysis. Merit aid at one level lures higher quality students; merit aid at another level lures warm, tuition-paying bodies - the dual transactions shift from emphasis on the input market to emphasis on the product market.[8]

IV. The Empirical Challenge of Detecting Peer Effects in Higher Education

But do peer effects actually exist?[9] If they do, many of the economic anomalies in industry structure and firm behavior disappear and a plausible model and reasonable economic understanding will match the data. But finding peer effects in higher education is difficult. There's a great degree of selectivity – by both students and schools – and differences in peer quality are reinforced by differences in resources across the hierarchy so choice yields stratification of students and of institutional resources. What's more, other explanations might motivate the same highly selective structure – filtering or signaling, students (or parents) getting enhanced personal utility from selection and exclusivity, per se, the satisfactions of association with the rich and famous, a marketing strategy in which schools want to emphasize that "those who have choices chose us," a self-reinforcing seeking of prestige (Arrow 1973; Basu 1989; James 1990; Becker 1991;

[8] Of course, with market clearing wages and no explicit donative revenues, both Rothschild-White and Epple-Romano-Sieg could incorporate only merit pricing though the primary method of allocating peer quality between schools is the uniform sticker prices that embody schools' general subsidies – given to all students by virtue of a sticker price set well below production cost.

[9] The story just told would be the same if, in fact, there were no peer effects in educational production but it was widely believed -- by schools, students, and parents -- that there were.

Clotfelter 1996) – but these explanations produce no socially useful outcome[10] while peer effects that enhance learning do. Peer effects, what's more, may explain some of the educational technologies observed in higher education like residential colleges or organized study groups that increase peer interactions and hence performance (Alexander, R. et al. 1974; Fraser, Beamn et al. 1977).

Recent research has been supportive of the claim that peer effects exist in higher education (c.f. Zimmerman (1999), Sacerdote (2001), Stinebrickner & Stinebrickner (2001) and Goethals (2000)).[11] All of these studies have examined what we called "the elementary particle" of peer effects in equation (1) above – the influence the characteristics or behavior of one student has on the behavior of another. The peer characteristics observed were, for the most part, variants on academic ablity – SAT scores or more nuanced evaluations of academic promise generated in the admission process – while the influenced behavior was largely grades or performance on a written test. These characteristics were broadened to include gender and income and behaviors were broadened to include dropout behavior and fraternity membership.

Sacerdote (2001), using data from Dartmouth and a roommate-based strategy similar to that employed by Zimmerman (1999), found evidence of a peer impact of a student on his roommate's grade point average as well as on his participation in

[10] Arrow is explicit: "Higher education, in this (filtering) model, contributes in no way to superior economic performance; it increases neither cognition nor socialization. Instead, higher education serves as a screening device, in that it sorts out individuals of differing abilities, thereby conveying information to the purchaser of labor… But even if (it) does have a positive informational value, it by no means follows that it is socially worthwhile" (1973, p.199).

[11] See Zimmerman (1999) for a review of the earlier literature.

fraternities. Sacerdote's results suggest a non-linear relationship with both weaker and stronger students performing better when their roommate was in the top 25% of the academic index distribution. Zimmerman (1999) found that middling students performed worse if their roommates were in the bottom third of the SAT distribution. In addition, Sacerdote found no evidence of peer effects in choice of college major.

Stinebrickner and Stinebrickner (2001) employed a unique dataset from Berea College. Like Zimmerman (1999) and Sacerdote (2001) they used the random assignment of roommates to identify the peer effect. Berea College targets low income students and so provides a useful complement to the highly selective schools used in the other studies. There, Stinebrickner and Stinebrickner found no evidence that either first semester grades or retention are associated with roommates' ACT scores. They did, however, find evidence that roommate income had a positive impact on both grades and retention, holding ACT scores constant, but only for women.

Goethals (2000) employed a unique and innovative experimental framework to measure peer effects. The study explored whether "students would perform better writing about newspaper articles they read and discussed in academically homogenous or heterogeneous groups of three." Interestingly, he found that student's performance was not linked to their own academic rating, but was affected by whether they were placed in an academically homogenous or heterogeneous group. He found that groups composed of students who all had a low academic rating and groups composed of students who all had a high academic rating perform similarly – with both groups of these types out performing groups in which some students had high ratings and some low ratings.[12] These results were stronger for men than women.

In sum, there is a growing – though still small – body of evidence suggesting that peer effects exist in higher education. The evidence is not clear on the nature of any non-linearities or interactions based on gender. It also suggests that non-academic peer characteristics may also be important. The evidence is, however, still sparse and in the next section we offer additional roommate based evidence on the existence of peer effects using new data drawn from the Mellon Foundation's *College and Beyond* survey.

V. Empirical Strategy: New Evidence

To estimate academic peer effects from the College and Beyond data in terms of equation (1) above, we follow the now traditional path of relating the cumulative grade point average of a student (B_1) to his own SAT scores (in X_1) and to the SAT scores of his first year roommate (C_2). More formally, we estimate regression models specified as:

$$GPA_i = \alpha + \beta_1 SAT_i + \beta_2 SAT_i^{RM} + \beta_3 X_i + \varepsilon_{ic} \tag{4}$$

where *GPA* is the student's Grade Point Average measured cumulatively to graduation,[13] *SAT* is the student's own SAT score (sometimes entered separately for math and verbal

[12] Should these results hold up on further study, they have clear implications for sorting, stratification, and hierarchy among colleges.

scores), SAT^{RM} is the student's freshman roommate's SAT score (sometimes entered separately for math and verbal scores), and X is a vector of other characteristics (such as race, gender) of the student.[14] If students are randomly assigned their roommates, then the estimated peer effect (β_2) will be unbiased. More generally, the estimate will be unbiased if it is plausible that the error term is uncorrelated with the explanatory variables.

In addition, we estimate models that allow for non-linearities in the peer effect. In particular, we allow the peer effect to vary based on whether the student or his roommate is in the lowest 15 percent, the middle 70 percent, or the top 15 percent of the SAT distribution. Some models also disaggregate the SAT score into the associated math and verbal scores. Formally, we estimate:

$$(5) \qquad GPA_{ij} = \alpha + \beta_1 SAT_i + \sum_{g=1}^{3} \beta_g SAT_{ig}^{DRM} + \beta_3 X_i + \varepsilon_{ic}; j = 1,2,3$$

where SAT_{ig}^{DRM} are dummy variables for each SAT score range (indexed by g) and β_g is the peer effect associated with that range.

VI. Data

[13] Grade performance for the first year, alone, was not available, but analysis of the Williams' data where both cumulative and freshman year GPA could be used showed that they yielded the same results (Zimmerman 1999).

[14] An appealing alternative strategy would be to include the roommate's GPA in the regression. Such a variable might better measure actual rather than potential performance. The problem with including such a variable is that it is simultaneously determined within the roommate context. Using such a measure would introduce simultaneous equation bias.

The C&B data used in this study were created and made available to us by the Andrew Mellon foundation. The C&B data contain both institutional and survey data for over 90,000 students enrolled in thirty-four mostly-selective colleges and universities in the United States for the entering classes of 1951, 1976, and 1989. The present study uses data from three of the schools in the C&B population for the entering class of 1989 – for the Class of '93. Institutional data in College and Beyond provide information on the students' grades, major, race, gender, etc. These data were combined with housing information extracted from college phonebooks to form a unique data set that allowed us to identify college roommates.

The schools selected for our sub-sample were chosen because a) they house their first year students together and b) the assignment mechanism of students to rooms (as indicated by their housing descriptions on the World Wide Web and conversations with their housing offices) seems roughly random. It was necessary to use schools that group first year students together because the C&B data do not provide information on other classes. If, for example, a school allowed first and second year students to live together we would have no information on the second year students, given C&B's restriction to the three cohorts. Further, it is necessary for the allocation to be approximately random since selection bias can be serious when students are allowed to choose their roommates or if the housing office groups students in such a way that under- or over-performers are more likely to be housed together. In this case the requirement that the error term be uncorrelated with the explanatory variables would be violated. In Zimmerman's earlier study of Williams freshmen (1999) he was able to utilize data from the housing application forms to conduct some relatively simple analyses to check whether the assumption of random assignment was plausible, and it was. The schools in this sample employed a similar protocol to that used by Williams in using housing forms indicating sleep preferences, smoking behavior, etc. in assigning students to rooms/roommates – though the underlying housing form data was not obtained.[15]

VII. Empirical Results

Table 3 provides summary statistics for the sample. The number of observations for the samples from the three schools ranged from 1,458 to 2,116. SAT scores ranged from a low of 360 on the verbal test and 420 on the math test to a maximum of 800 on both tests. The average combined SAT score ranged from 1344 to 1409. These scores are high, putting the average student in the top 10 percent of the population of test takers. Each school had between 7% and 9% African American students and 2%-5% Hispanic students.

Table 4 presents estimates of equation (4). The results for each school are reported in a separate column where a student's cumulative grade point average is regressed on his own SAT score (divided by 100), race, gender, major, class cohort, and roommate's SAT score. The model includes controls for a student's major (which is selected in junior year) to provide some control for grade differentials arising from students' taking different courses.

[15] See (Zimmerman forthcoming) for a mathematical model that illustrates the possibility of bias in the estimated peer effects flowing from the use of housing forms in assigning students to rooms.

The effect of a student's own SAT score is large and statistically significant, with each 100 point increase resulting in between a .116 and a .132 increase in grade point average. After controlling for SAT scores, black and Hispanic students score between a quarter and a third of a grade point below white students. Female students score between .082 and .127 grade points higher than male students. Finally, roommate's SAT score is found to have a positive and statistically significant effect only for School #2 – where a 100 point increase in a student's roommate's combined SAT score translates into a .02 increase in the student's own grade point average. This effect is about 17 percent as large as that of a 100 point increment in the student's own SAT score.[16]

Tables 5, 7, and 9 report estimates of equation (5) allowing the peer effect to depend on the student's own position in the SAT distribution. Panel A allows us to see whether weak, average, or strong students (as measured by their SAT scores) are more, or less, affected by roommates. The results in these panels suggest that strong students at all three schools are unaffected by the SAT scores of their roommates. Students in the bottom 15% of the SAT distribution benefit from higher SAT scoring roommates at School #1 – though not at schools 2 and 3. Students in the middle 70 percent of the distribution are unaffected by the SAT scores of their roommates at Schools 1 and 3 – though they benefit from higher scoring roommates at School #2. Students in the middle 70 percent of the SAT distribution at School #2 experience, on average, a .02 increase in their cumulative GPA when their roommates' SAT scores increase by 100 points.

[16] It is worth noting here that models allowing for differential effects for math and verbal SAT scores were also estimated, but standard F-tests indicated no measurable difference in their impact. Accordingly, only models using combined SAT scores are reported.

Panel B allows the peer effect to be nonlinear. That is, it allows us to see whether weak, average, or strong students (as measured by their SAT scores) are more, or less, affected by having roommates who are weak, average, or strong in terms of their combined SAT scores. For this model, no peer effects are found at School #1. At School #2 neither the strongest nor the weakest students are affected by the SAT scores of their roommates. Students in the middle 70% of the SAT distribution, however, perform somewhat worse when their roommates are in the bottom 15% of the SAT distribution. The estimates suggest that a student in this part of the SAT distribution, with a bottom 15% roommate, would, on average, have a cumulative GPA that is lower by .086 points than that of a similar student whose roommate was in the top 15% of the SAT distribution. Similar results are found at School #3 where, in addition, there is evidence that the strongest students perform better when their roommates are academically stronger. It is worth noting that these results are robust to moderate variations in the percentile cutoffs used to define the groups.

Tables 6, 8, and 10 report estimates of equation (5) separately for men and women. Perhaps due to smaller sample sizes, peer effects are not statistically significant for most groups. The one exception is found at School #2, where male students in the middle of the SAT distribution are found to perform worse when their roommate is in the lowest 15% of the SAT distribution.

To put the myriad results in context it is useful to summarize the existing research more succinctly. The research to date, including the evidence reported in this paper, on the effect of peer academic characteristics on a "grade type" outcome, is summarized in the Table 11.

These studies differ in a variety of ways: the selectivity of the school surveyed, the measurement and detection of non-linearities, the outcome considered, the existence of differences by gender, and so on. The evidence found thus far suggests that the "elementary particle" of peer effects – namely their existence at the most basic level – has been confirmed in each of the studies. Sacerdote (2001) finds grades are higher when students have unusually academically strong roommates. Zimmerman (1999, 2001) finds that weak peers might reduce the grades of middling students. Stinebrickner finds that peer ACT scores are insignificant after controlling for roommate family income which is significant. Goethals finds that homogeneity *per se* matters – students perform better when grouped with others of like ability.

VIII. Conclusion and Agenda

Evidence on peer effects in colleges and universities now exists at the most basic level for six colleges and universities – some 12,000 students – with interactions measured for randomly assigned roommates and participants in psych lab experiments. It's clear that peer effects exist – that students' characteristics and behavior do, indeed, influence other students' behavior with conventionally measured academic characteristics (like SAT) influencing conventionally measured academic performance (like GPA).

New evidence presented in this chapter add to our confidence that peer effects exist. So the broad question "Can peer effects in educational production help explain the unusual economic structure and behavior of higher education?" is answered, "Yes." The models, of Winston (1999) and Epple-Romano-Sieg (2001), data-driven and formal respectively, fit both the data and the peer effect evidence.[17]

But beyond that key question, the facts become less clear and the agenda for investigation of peer effects becomes larger. So there are often different results by gender, even in these data that rest on individual interactions, rather than on those between groups. On non-linearities – whether peer influences operate equally and symmetrically across characteristics – the evidence is puzzling with homogeneous groupings – strong or weak – sometimes performing significantly better than those with peers of different abilities and students of middling ability apparently more susceptible to peer influence than those at either ability extreme (keeping in mind that the student populations reported on here, represent very narrow ability ranges). And since our data are based on pairwise interactions, analysis might well be extended to those interactions that are electronically mediated to see if "distance learning" environment generates any evidence of peer effects.

[17] Note that the peer effects that have been found are almost all in the "right" direction: there's no evidence of a "teaching effect" in which strong students gain from association with weaker students whom they can teach (as implied by Zajonc's analysis of older siblings (Zajonc 1976)) nor is there strong evidence of an "intimidation effect," though that might help explain Goethals' finding that weak students do better when grouped with other weak students.

The range of peer characteristics and behaviors should be extended, too. The work reported here sticks, by and large, to the most measurable and obvious aspects of education – academic ability and performance – with occasional departure into fraternity membership, family income and dropout behavior. But while these are clearly the right place to start, they capture a small part of the behaviors influenced by higher education and of interest to colleges in their selection of student peer quality. Bowles, Gintis and Osborn (Bowles, Gintis et al. 2001) point out that a small part of the variance in wages attributable to education is explained by the cognitive skills we measure with tests and GPAs – the rest, they argue, is due to behaviors learned in part through education that escape cognitive measurement but do influence job performance, nonetheless, lie reliability, attitude, discipline, fatalism, and impatience. To the extent that these characteristics and behaviors can be identified and measured, they need to be included in studies of peer effects in higher education. On the basis of evidence that a student's impatience (his time-discount behavior) influences his own academic performance (students with lower discount rates do get better grades, holding SATs constant (Kirby, Winston et al. 2002)) we tried, in a very small sample, to find peer influence such that one roommate's discount rate affected the other's academic performance, but while the sign of the relationship was right, it was decidedly insignificant.

So we conclude that evidence on the existence of peer effects in higher education is strong, supporting an understanding of its economic structure that relies on them, but that there remains a rich set of questions on how and how broadly peer effects operate among students in colleges.[18] Some of those characteristics and behaviors will play a role in college and student selection, some won't or can't.

[18] Nor has our discussion even touched on negative peer effects like binge drinking and date rape.

Table 1
The Distribution of Average Cost, Price and Student Subsidies
1996

	Subsidy per Student	Average Educational Cost	Average Net Tuition
All Colleges and Universities	$8,423	$12,413	$3,989
Public	$8,590	$9,896	$1,305
Private	$8,253	$14,986	$6,734
Schools ranked by Student Subsidies:			
Decile 1	$20,991	$27,054	$6,063
Decile 2	$11,865	$15,801	$3,936
Decile 3	$10,009	$13,310	$3,301
Decile 4	$8,752	$11,831	$3,080
Decile 5	$7,855	$10,565	$2,710
Decile 6	$7,020	$9,820	$2,799
Decile 7	$6,250	$9,464	$3,214
Decile 8	$5,447	$8,848	$3,401
Decile 9	$4,262	$9,297	$5,035
Decile 10	$1,736	$8,084	$6,348

Source: Based on US Department of Education IPEDS data. Includes 2791 institutions, of which 1411 are public and 1380 are private. All dollar amounts are per FTE student averaged over institutions. See Winston (2000) and Winston-Yen (1995) for details on the derivation of these data from the IPEDS Finance Survey (Medical schools are omitted here).

Table 2
The Distribution of Average Combined SATs by Student Subsidy

	School		% Schools
	Student Subsidy	Average SAT	requiring SAT for Admission
Decile 1	$20,991	1095	81%
Decile 2	$11,865	1026	74%
Decile 3	$10,009	1003	75%
Decile 4	$8,752	1004	68%
Decile 5	$7,855	991	61%
Decile 6	$7,020	984	58%
Decile 7	$6,250	997	57%
Decile 8	$5,447	982	59%
Decile 9	$4,262	990	64%
Decile 10	$1,736	938	42%

Source: Institutional SAT data from 1996-97 IPEDS institutional characteristics data.

Table 3: Descriptive Statistics

Class of '93

School #1	Mean	Standard Deviation	Minimum	Maximum
Sample Size	1863	0	1863	1863
Own SAT Score – Verbal	714	66	420	800
Own SAT Score – Math	695	69	480	800
Own SAT Score – Combined	1409	112	1090	1600
Black	.079	.270	0	1
Hispanic	.052	.223	0	1
Native American	.004	.069	0	1
Asian	.151	.358	0	1
Not a Citizen of the United States	.03	.169	0	1
Female	.432	.495	0	1
School #2	.430	.494	0	1
Sample Size	2116	0	2116	2116
Own SAT Score – Verbal	668	68	360	800
Own SAT Score – Math	676	68	450	800
Own SAT Score – Combined	1344	110	950	1600
Black	.086	.282	0	1
Hispanic	.044	.206	0	1
Native American	N/A	N/A	N/A	N/A
Asian	.160	.367	0	1
Not a Citizen of the United States	.095	.292	0	1
Female	.430	.494	0	1
School #3	Mean	Standard Deviation	Minimum	Maximum
Sample Size	1458	0	1458	1458
Own SAT Score – Verbal	687	61	450	800
Own SAT Score – Math	681	68	420	800
Own SAT Score – Combined	1368	106	880	1600
Black	.072	.258	0	1
Hispanic	.022	.148	0	1
Native American	.001	.036	0	1
Asian	.079	.270	0	1
Not a Citizen of the United States	.03	.148	0	1
Female	.466	.499	0	1

Table 4: Your Grades and Your Roommate's SAT Scores

	Cumulative GPA (School#1)	Cumulative GPA (School #2)	Cumulative GPA (School#3)
Own SAT Score/100	**0.131**	**.116**	**.132**
	(0.01)	**(.013)**	**(.012)**
Black	-.264	-.306	-.380
	(.068)	(.060)	(.054)
Hispanic	-.172	-.080	.005
	(.085)	(.055)	(.046)
Native American	-.268	N/A	.145
	(.157)		(.071)
Not a Citizen of the United States	N/A	-.047	N/A
		(.065)	
Asian	-.011	-.071	-.033
	(.031)	(.031)	(.042)
Female	.127	.082	.112
	(.028)	(.024)	(.024)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score/100	0.013	**0.020**	.013
	(0.007)	**(0.008)**	(.009)
Sample Size	1863	2116	1458
R- Squared	.303	0.215	0.2475

Note: Standard Errors are corrected for correlation within roommate cluster. Bolded peer and own SAT coefficients are significant at the 5% level.

Table 5: Your Grades and Your Roommate's SAT Scores by SAT Group – School #1 Class of '93 (Dependent Variable is Cumulative GPA)

	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
A. Linearity in Roommates Scores			
Own SAT Score – Verbal/100	.065	**.223**	.036
	(.087)	**(.029)**	(.124)
Own SAT Score – Math/100	.024	**.172**	.124
	(.127)	**(.033)**	(.148)
Black	-.174	-.297	-.758
	(.186)	(.079)	(.165)
Hispanic	.0402	-.311	-.024
	(.086)	(.142)	(.116)
Native American	-.045	-.356	(dropped)
	(.160)	(.251)	
Not a Citizen of the United States	N/A	N/A	N/A
Asian	.226	-.004	-.040
	(.230)	(.039)	(.052)
Female	.233	.138	.012
	(.110)	(.032)	(.056)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score/100	.032	.011	-.009
	(.010)	(.008)	(.014)
Sample Size	269	1281	313
R- Squared	. 0.288	0.295	0.154

	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
B. Non-linearity in Roommates Scores			
Own SAT Score – Verbal/100	.060	**.223**	.021
	(.089)	**(.02856)**	.125
Own SAT Score – Math/100	.021	**.172**	.100
	(.128)	**(.033)**	(.151)
Black	-.175	-.297	-.805
	(.183)	(.079)	(.163)
Hispanic	.043	-.312	-.022
	(.086)	(.141)	(.114)
Native American	-.075	-.352	(dropped)
	(.169	(.251)	
Not a Citizen of the United States	N/A	N/A	N/A
Asian	.233	-.004	-.039
	(.231)	(.039)	(.051)
Female	.220	.137	.022
	(.110)	(.032)	(.055)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score – Lowest 15%	-.156	-.044	-.002
	(.086)	(.032)	(.050)
Roommates SAT Score – Middle 70%	-.131	-.023	-.038
	(.085)	(.025)	(.043)
Sample Size	269	1281	313
R- Squared	0.295	0.295	0.154

Note: Standard Errors are corrected for correlation within roommate cluster. Bolded peer and own SAT coefficients are significant at the 5% level.

Table 6: Your Grades and Your Roommate's SAT Scores by SAT Group and Gender – School #1 Class of '93 (Dependent Variable is Cumulative GPA)

	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
A. Men			
Own SAT Score – Verbal/100	.048 (.108)	**.266** **(.034)**	-.006 (.172)
Own SAT Score – Math/100	.113 (.122)	**.163** **(.043)**	-.002 (.002)
Black	.041 (.124)	-.438 (.132)	-.817 (.206)
Hispanic	.067 (.096)	-.128 (.134)	.006 (.091)
Native American	(dropped)	-.717 (.254)	(dropped)
Not a Citizen of the United States	N/A	N/A	N/A
Asian	.926 (.220)	.039 (.056)	-.075 (.112)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score – Lowest 15%	-.167 (.117)	-.054 (.046)	.078 (.060)
Roommates SAT Score – Middle 70%	-.108 (.088)	-.042 (.035)	-.022 (.033)
Sample Size	137	739	187
R- Squared	0.637	0.323	0.309
	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
B. Women			
Own SAT Score – Verbal/100	.117 (.166)	**.187** **(.057)**	-.101 (.182)
Own SAT Score – Math/100	-.062 (.200)	**.192** **(.046)**	.095 (.227)
Black	-.436 (.347)	-.228 (.085)	(dropped)
Hispanic	-.057 (.161)	-.474 (.251)	(dropped)
Native American	-.242 (.185)	-.064 (.130)	(dropped)
Not a Citizen of the United States	N/A	N/A	N/A
Asian	.105 (.149)	-.073 (.052)	-.040 (.086)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score – Lowest 15%	-.104 (.124)	-.026 (.040)	-.020 (.084)
Roommates SAT Score – Middle 70%	-.143 (.124)	-.006 (.034)	.028 (.101)
Sample Size	132	543	128
R- Squared	0.279	0.325	0.441

Note: Standard Errors are corrected for correlation within roommate cluster.
Bolded peer and own SAT coefficients are significant at the 5% level.

Table 7: Your Grades and Your Roommate's SAT Scores by SAT Group – School #2 Class of '93 (Dependent Variable is Cumulative GPA)

	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
A. Linearity in Roommates Scores			
Own SAT Score – Verbal/100	.162	**.142**	-.109
	(.088)	**(.025)**	(.098)
Own SAT Score – Math/100	.077	**.166**	.063
	(.101)	**(.027)**	(.112)
Black	-.235	-.341	-.117
	(.079)	(.085)	(.160)
Hispanic	-.036	-.060	-.071
	(.127)	(.070)	(.095)
Native American	N/A	N/A	N/A
Not a Citizen of the United States	-.204	-.016	.026
	(.243)	(.079)	(.065)
Asian	.102	-.083	-.111
	(.145)	(.033)	(.081)
Female	.067	.099	-.109
	(.077)	(.026)	(.129)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score/100	.017	.020	.0438
	(.021)	(.009)	(.026)
Sample Size	280	1500	336
R- Squared	0.286	0.181	0.178

	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
B. Non-linearity in Roommates Scores			
Own SAT Score – Verbal/100	.167	**.143**	-.110
	(.088)	**(.025)**	(.098)
Own SAT Score – Math/100	.088	**.166**	.059
	(.100)	**(.027)**	(.111)
Black	-.238	-.340	-.086
	(.079)	(.085)	(.168)
Hispanic	-.035	-.050	-.055
	(.127)	(.069)	(.102)
Native American	N/A	N/A	N/A
Not a Citizen of the United States	-.174	-.009	-.109
	(.242)	(.078)	(.128)
Asian	.108	-.082	-.110
	(.142)	(.033)	(.081)
Female	.061	.102	.015
	(.077)	(.026)	(.064)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score – Lowest 15%	-.042	**-.086**	-.099
	(.088)	**(.034)**	(.102)
Roommates SAT Score – Middle 70%	-.066	-.022	-.079
	(.072)	(.023)	(.057)
Sample Size	282	1505	337
R- Squared	0.286	0.181	0.172

Note: Standard Errors are corrected for correlation within roommate cluster.
Bolded peer and own SAT coefficients are significant at the 5% level.

Table 8: Your Grades and Your Roommate's SAT Scores by SAT Group and Gender – School #2 Class of '93 (Dependent Variable is Cumulative GPA)

	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
A. Men			
Own SAT Score – Verbal/100	.230 (.166)	**.194** **(.034)**	-.164 (.114)
Own SAT Score – Math/100	.105 (.165)	**.212** **(.038)**	.038 (.127)
Black	-.239 (.187)	-.281 (.131)	(dropped)
Hispanic	-.134 (.233)	.055 (.077)	-.087 (.112)
Native American	N/A	N/A	N/A
Not a Citizen of the United States	-.068 (.377)	.027 (.093)	-.163 (.141)
Asian	.188 (.270)	-.053 (.048)	-.166 (.112)
Major Dummy Variables	YES	YES	YES
Roommates Verbal SAT Score – Lowest 15%	-.132 (.194)	**-.132** **(.056)**	-.092 (.121)
Roommates Verbal SAT Score – Middle 70%	-.093 (.109)	-.036 (.029)	-.082 (.068)
Sample Size	110	839	245
R- Squared	0.258	0.209	0.238

	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
B. Women			
Own SAT Score – Verbal/100	.126 (.094)	.074 (.041)	.093 (.179)
Own SAT Score – Math/100	.165 (.123)	**.118** **(.040)**	.119 (.269)
Black	-.226 (.083)	-.375 (.113)	-.477 (.166)
Hispanic	.046 (.124)	-.273 (.116)	(dropped)
Native American	N/A	N/A	N/A
Not a Citizen of the United States	-.358 (.403)	-.087 (.070)	(dropped)
Asian	.030 (.133)	-.102 (.048)	-.065 (.145)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score – Lowest 15%	.102 (.112)	-.014 (.043)	.139 (.129)
Roommates SAT Score – Middle 70%	.072 (.095)	.022 (.036)	-.018 (.080)
Sample Size	172	666	92
R- Squared	0.439	0.204	0.209

Note: Standard Errors are corrected for correlation within roommate cluster. Bolded peer and own SAT coefficients are significant at the 5% level.

Table 9: Your Grades and Your Roommate's SAT Scores by SAT Group – School #3 Class of '93 (Dependent Variable is Cumulative GPA)

	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
A. Linearity in Roommates Scores			
Own SAT Score – Verbal/100	**.214**	**.114**	**.183**
	(.061)	**(.032)**	**(.085)**
Own SAT Score – Math/100	**.146**	**.101**	**.236**
	(.065)	**(.031**	**(.106)**
Black	-.309	-.498	-.186
	(.082)	(.112)	(.076)
Hispanic	.028	-.021	.191
	(.086)	(.064)	(.131)
Native American	(dropped)	.120	(dropped)
		(.087)	
Not a Citizen of the United States	N/A	N/A	N/A
Asian	.310	-.097	.045
	(.164)	(.049)	(.090)
Female	.108	.088	.122
	(.078)	(.030)	(.068)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score/100	-.016	.019	.036
	(.025)	(.011)	(.026)
Sample Size	221	975	262
R- Squared	0.3560	(0.1151)	0.1215
	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
B. Non-linearity in Roommates Scores			
Own SAT Score – Verbal/100	**.207**	**.114**	**.186**
	(.056)	**(.032)**	**(.083)**
Own SAT Score – Math/100	**.148**	**.100**	**.238**
	(.065)	**(.031)**	**(.102)**
Black	-.303	-.498	-.145
	(.078)	(.111)	(.079)
Hispanic	.031	-.014	.193
	(.082)	(.059)	(.116)
Native American	(dropped)	.110	(dropped)
		(.085)	
Not a Citizen of the United States	N/A	N/A	N/A
Asian	.314	-.094	.058
	(.165)	(.049)	(.090)
Female	.110	.090	.139
	(.078)	(.030)	(.066)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score – Lowest 15%	.069	**-.092**	**-.175**
	(.096)	**(.041)**	**(.077)**
Roommates SAT Score – Middle 70%	.004	-.038	**-.127**
	(.081)	(.031)	**(.061)**
Sample Size	223	981	263
R- Squared	0.3585	0.1173	0.1377

Note: Standard Errors are corrected for correlation within roommate cluster. Shaded peer and own SAT coefficients are significant at the 5% level.

Table 10: Your Grades and Your Roommate's SAT Scores by SAT Group and Gender – School #3 Class of '93 (Dependent Variable is Cumulative GPA)

	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
A. Men			
Own SAT Score – Verbal/100	.079 (.073)	**.136** **(.048)**	.154 (.099)
Own SAT Score – Math/100	**.255** **(.105)**	**.174** **(.049)**	.176 (.134)
Black	-.261 (.151)	-.632 (.159)	-.077 (.077)
Hispanic	.006 (.124)	-.170 (.087)	.112 (.170)
Native American	(dropped)	.043 (.088)	(dropped)
Not a Citizen of the United States	N/A	N/A	N/A
Asian	.236 (.219)	-.158 (.071)	-.008 (.105)
Major Dummy Variables	YES	YES	YES
Roommates Verbal SAT Score – Lowest 15%	.161 (.120)	-.085 (.069)	-.107 (.093)
Roommates Verbal SAT Score – Middle 70%	.105 (.112)	-.063 (.045)	-.107 (.063)
Sample Size	104	464	204
R- Squared	0.4625	0.1634	0.1396

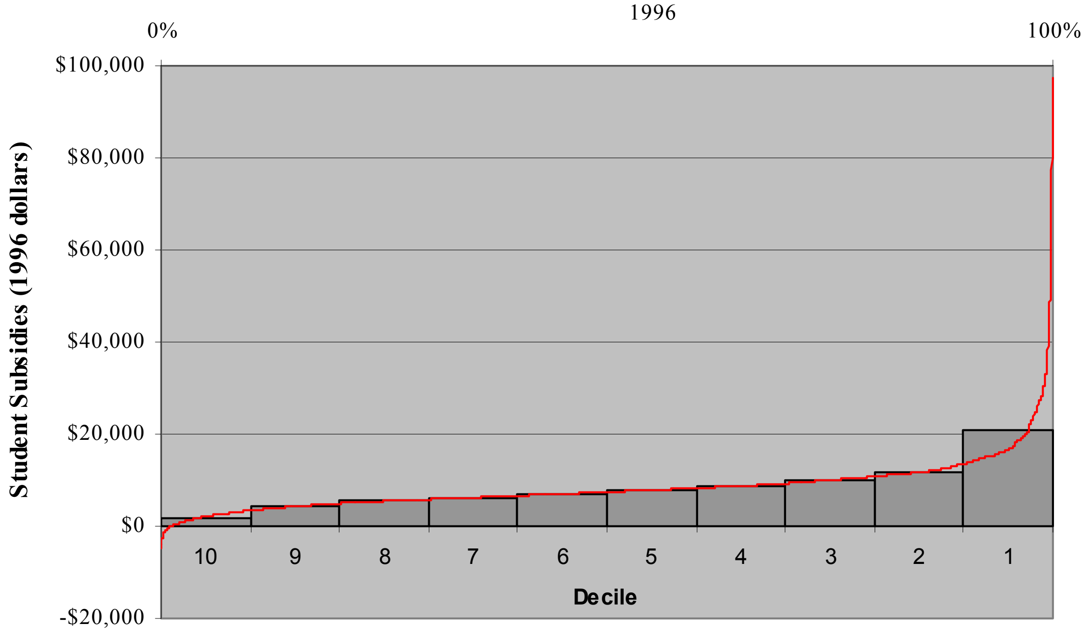
	Combined SAT Score (lowest 15%)	Combined SAT Score (middle 70%)	Combined SAT Score (top 15%)
B. Women			
Own SAT Score – Verbal/100	**.292** **(.081)**	**.110** **(.044)**	**.460** **(.127)**
Own SAT Score – Math/100	**.200** **(.098)**	.031 (.039)	**.350** **(.123)**
Black	-.192 (.107)	-.377 (.135)	-.335 (.055)
Hispanic	.0190 (.145)	.070 (.073)	.429 (.233)
Native American	(dropped)	(dropped)	(dropped)
Not a Citizen of the United States	N/A	N/A	N/A
Asian	.128 (.150)	-.050 (.072)	.212 (.084)
Major Dummy Variables	YES	YES	YES
Roommates SAT Score – Lowest 15%	.018 (.179)	-.059 (.048)	**-.266** **(.133)**
Roommates SAT Score – Middle 70%	-.124 (.114)	.003 (.039)	-.149 (.076)
Sample Size	119	517	59
R- Squared	0.4546	0.1172	0.6660

Note: Standard Errors are corrected for correlation within roommate cluster.
Shaded peer and own SAT coefficients are significant at the 5% level.

Table 11

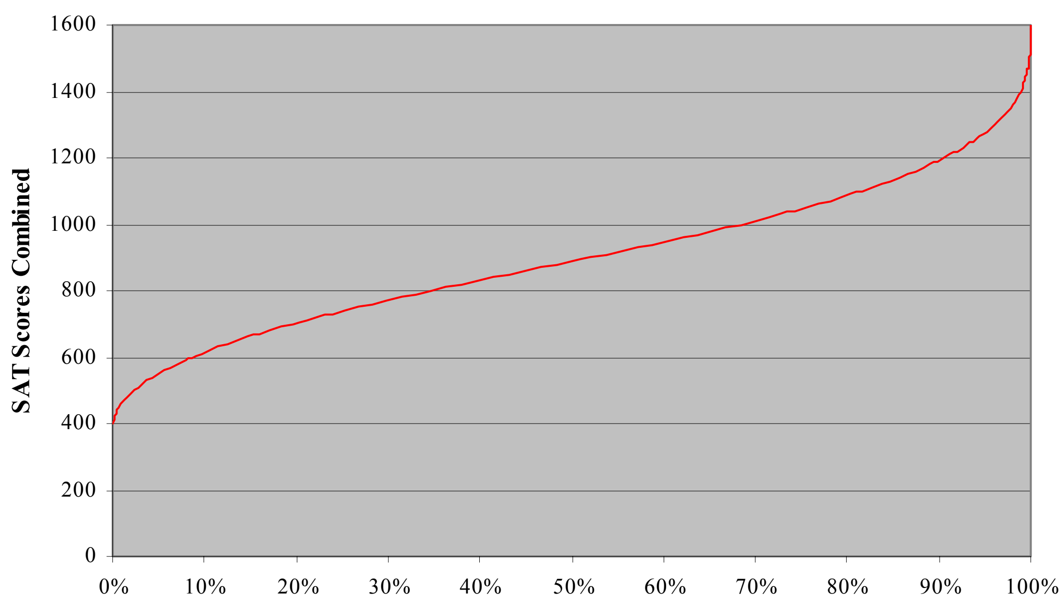
Academic Peer Effects			
Study	Peer Characteristic	Coefficient on grades*	Comments
Zimmerman (1999)	Roommate's Verbal SAT in bottom 15%	-.077 (.027)	Impact on middle 70% of SAT distribution, Williams College
Zimmerman (as reported in this chapter)	Roommate's Verbal SAT in bottom 15%	-.086 (.034)	Impact on middle 70% of SAT distribution, three schools from College and Beyond.
Sacerdote (2001)	Roommate in top 25% of Academic Rating Index	.060 (.028)	Dartmouth. Controls for housing questions. Also peer effect on fraternity membership but none on major.
Stinebrickner and Stinebrickner (2001)	ACT score	.001 (.004)	Controls for roommate's family income. Roommate income is significant with grades, rising .052 per $10,000 income, for women.
Goethals (2000)	Admissions office academic rating	N/A	Finds performance increases with group homogeneity in academic rating.
* Taken from Table 4 in Zimmerman (1999), Table 3 in Sacerdote (2000), Table 3 in Stinebrickner (undated) and Tables 5 to 10 above.			

Figure 1
The Distribution of Student Subsidies Among Schools
1996



Public and Private Colleges and Universities
(Decile Averages and Smoothed Percentiles)

Figure 2
The Cumulative Distribution of Combined SAT Scores
1993



High School Senior Test Takers

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