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The Effect of Classmate Characteristics on Individual Outcomes: Evidence from the Add Health

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Abstract

We use data from the National Longitudinal Study of Adolescent Health (Add Health) to examine the effects of classmate characteristics on economic and social outcomes of students. The unique structure of the Add Health allows us to estimate these effects using comparisons across cohorts within schools, and to examine a wider range of outcomes than other studies that have used this identification strategy. This strategy yields variation in cohort composition that is uncorrelated with student observables suggesting that our estimates are not biased by the selection of students into schools or grades based on classmate characteristics. We find that increases in the percent of classmates whose mother is college educated has significant, desirable effects on educational attainment and substance use. We find no evidence that in-school achievement, student attitudes, or behaviors serve as mechanisms for this effect. The percent of students from disadvantaged minority groups does not show any negative effects on the post-secondary outcomes we examine, but is associated with students reporting less caring student-teacher relationships and increased prevalence of some undesirable student behaviors during high school.

Journal of Economic Literature Classification: I21, I19, J13, J15

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I. Introduction

Social scientists have long been interested in determining whether the characteristics of one's schoolmates influence important economic and social outcomes. Policy developments over the last 10 to 15 years have heightened interest in this question. Changes in the law governing racial desegregation efforts and the growth of the school choice movement have led many local school districts to replace student assignment policies focused on promoting racial integration with policies designed to expand parents' discretion over what school their child attends. Several studies suggest that such policy changes may increase the isolation of minority students and the stratification of schools by measures such as parental education and academic achievement.[1] Whether or not such changes can be expected to exacerbate social and economic inequalities depends on how the student composition of a school influences individual outcomes.

The fundamental problem facing studies of schoolmate or peer effects is that individual children or their parents choose the students' peers. For primary education in the United States, the opportunities to exploit random assignment to investigate peer effects are limited.[2] In a recent innovation introduced by Hoxby (2000b), variation in student composition across cohorts within schools has been used to identify the effect of peers under the assumption that parents and their children do not sort across schools based on differences between the demographic composition of the child's cohort and the average composition of the school. Recent studies

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[1] Since 1990, school segregation has declined more slowly than neighborhood segregation, and the isolation of black students in many areas has increased (Clotfelter, 2004; Vigdor & Ludwig, 2007). Clotfelter, Ladd, and Vigdor (2006) provide evidence that federal court rulings during the period have contributed to these trends. Several studies, including but not limited to Bifulco, Ladd, and Ross (2009), Cullen, Jacob, and Levitt (2005), and Hastings, Kane, and Staiger (2006), indicate that students with college educated parents and high achievers are more likely than others to use expanded choice of schools to avoid concentrations of educationally disadvantaged students and to enroll in schools with other educationally advantaged students. Brunner, Imazeki, and Ross (In Press) find that voting patterns for a school choice program in California were consistent with increases in school segregation. As a result many different types of school choice programs can be expected to increase stratification of schools.

[2] A few studies have tried to exploit random assignment in Tennessee's project STAR to examine variation in gender, age, and ability composition, see Whitmore (2005), Cascio and Schanzenbach (2007), and Boozer and Cacciola (2001). In a developing country context, see Duflo, Dupas, and Kremer (2008).

applying this approach include Angrist and Lang (2004), Friesen and Krauth (2008), Gould, Lavy, and Passerman (2004), Hanushek, Kain, and Rivkin (2002), Hoxby (2000a, 2000b), Lavy and Schlosser (2007), Lavy, Passerman, and Schlosser (2008). However, because this approach requires data on multiple cohorts from the same schools, studies that use it have had to rely on state and local administrative data sets which provide information on only a small set of outcomes, usually limited to student test scores. As a result our knowledge of the effects of student composition on individual outcomes is still quite limited.

In this study, we use data from the National Longitudinal Study of Adolescent Health (Add Health) to extend this line of research on the effects of school composition. The Add Health is a longitudinal survey program that collects information on a wide range of individual outcomes beginning during the teenage years. The study samples students from multiple cohorts and conducts a limited survey of all students in each cohort from a nationally representative set of schools. These aspects of the Add-Health allow us to use comparisons across cohorts within schools by controlling for school fixed effects and trends to estimate the effect of classmate characteristics on a much wider range of outcomes than have previous studies.

Our analysis focuses on the effects of the percent minority and the percent with a college educated mother among the students in one's school cohort. Distinguishing the effects of school racial composition from social class composition is potentially important. Analysis by Reardon, Yun, and Kurlaender (2006) demonstrate that policies to promote integration by social class might not significantly reduce racial segregation, and vice versa, and so estimates of the distinct effects of racial and social class composition can inform choices about policy priorities.

Several diagnostic analyses support our use of across cohort variation in student composition to identify the effect of peers on student outcomes. First, we run simulations to examine how much within school variation in cohort composition would be expected if students were assigned randomly to school-specific cohorts, and find that the amount of variation across cohorts within schools that we observe in our sample is quite consistent with random assignment.[3] Second, as suggested by Lavy and Schlosser (2007), we conduct balancing tests which examine whether across cohort variation in peer composition can explain predetermined student attributes. The results of these tests imply that students have not sorted on their observables across cohorts within schools. Third, we test the robustness of our effect estimates by progressively adding different types of observable student covariates to our regression models. Following the intuition behind Altonji, Elder and Tabor (2005), the fact that our peer effect estimates do not change substantially when observable student attributes are added suggests that the potential bias from unobservables is small. Finally, we find that our results are robust to our attempts to control for non-linear changes across cohorts by dropping schools with large deviations from trends and by estimating models that drop either the 9th or the 12th grade cohorts from the sample.

Our primary analysis focuses on the influences of classmates on post-secondary outcomes.[4] Our results indicate that having a higher percentage of classmates with a college educated mother decreases the likelihood of dropping out of high school, increases the likelihood of attending college, and reduces the likelihood of using marijuana after high school. This pattern of effect estimates is unlikely to have arisen unless the percent of classmates with college educated moms has a real influence on individual outcomes. In addition, a higher share of minority classmates is associated with a lower likelihood of binge drinking after high school. This last finding, however, might be viewed as only suggestive evidence because it is only one

[3] We thank Joe Altonji for this suggestion.
[4] Throughout this paper we will use the term "classmates" to refer to the students in an individual's school specific cohort.

finding on peer racial composition out of seven hypothesis tests conducted and might reasonably have arisen by chance. Overall, these results suggest the composition of one's classmates has potentially important effects on individual outcomes.

We also investigate potential mechanisms through which classmates might influence post-secondary outcomes. Specifically, we estimate the impact of cohort composition on academic outcomes, perceptions of school, and behaviors during high school. The percent of classmates with a college educated mother does not show a significant effect on any of these outcomes. In light of these null findings on mechanism, one possible explanation for these peer effects is "contagion" where a student's educational and substance use choices are directly influenced by their classmates' choices. A necessary condition for contagion effects is that the peer attribute directly correlate with the behavior, and we find higher rates of high school graduation and college attendance for students whose mothers have a college degree and lower rates of binge drinking among African-American and Hispanic students. In contrast to the findings on the parent education of classmates, we find that having a higher percentage of black or Hispanic classmates decreases students' ratings of how much their teachers care about them, increases the amount students watch television each week, and increases the likelihood that students will get into physical fights, engage in unruly behavior, and smoke marijuana during high school. Although, these short-term influences on perceptions and behavior do not translate into effects on the post-secondary outcomes that we examine, they may nonetheless raise concerns among policymakers.

The paper is organized as follows. Section II briefly reviews the prior research on the effects of student composition. Section III describes the data we use. Section IV explains our identification strategy, discusses our approach to assessing statistical significance in the context of multiple hypothesis tests, and provides evidence on the validity of our identification strategy. Section V presents our estimates of the effect of classmates on post-secondary outcomes, and Section VI presents our exploration of possible mechanisms by which classmates might influence those outcomes. Section VII concludes.

II. Prior Research

Early empirical work on the effects of classmate characteristics focused on the effects of racially desegregated schools. These studies primarily examined outcomes related to academic achievement and racial attitudes and focused largely on the short-run effects of deliberately moving students to less racially segregated schools. Comprehensive reviews of this early research suggest that the results of desegregation were quite mixed, with some evidence of small, positive effects on the academic achievement of black students and little evidence of consistently positive effects on racial attitudes (Cook, 1984; Schofield, 1995). Much of this literature is based on comparisons of students who attended desegregated schools with students who remained in segregated schools, and has been criticized for failing to adequately control for unobserved differences between these two groups of students. Also, Hanushek, Kain, and Rivkin (2002) point out that desegregation efforts were often accompanied by conflict and resistance, and thus, estimates of the short run effects of desegregation might be contaminated by factors related to the desegregation process.

More recent research has focused on the relationship between student composition and outcomes rather than on the effects of specific desegregation efforts.[5] This more recent research has used two different approaches—(1) an instrumental variable approach that uses variation across schools or (2) a fixed effects approach using within-school, across cohort variation. The

[5] Some recent studies have examined the effects of desegregation. Using variation in the timing of court ordered desegregation, Guryan (2004) finds that desegregation plans in the 1970s decreased black dropout rates and Ludwig, Lutz & Weiner (2007) find that desegregation decreased homicide victimization rates for both blacks and whites.

first approach uses arguably exogenous variation across schools in student composition to identify effects and the other uses variation across cohorts within schools.[6] Any study that draws on variation in student composition across schools must address the fact that the composition of students in a school influences parents' decisions about whether or not to enroll their child. As a result, students in integrated schools are likely to differ from students in less integrated schools in difficult to observe ways, and these differences are likely to confound estimates of the effect of student composition. Vigdor and Nechyba (in press) illustrate the potential bias using data from North Carolina. They find evidence of strong peer effects using methods that compare students with high and low achieving classmates, but no evidence of peer effects using comparisons that exploit arguably exogenous changes in school composition associated with administrative redistricting.

To address the nonrandom choice of schools, several studies have used measures of racial composition or segregation from higher levels of aggregation to instrument for school racial composition. Rivkin (2000) uses district level variation in exposure to whites, and finds that racial composition has no effects on test scores, educational attainment or earnings.[7] Boozer, Krueger, and Wolken (1992) use variation across time and states in school racial composition, and find that high white enrollment shares are associated with higher educational and occupational attainment. Evans, Oates, and Schwab (1992) use metropolitan level measures of socioeconomic well-being as instruments, and find no relationship between the percent of economically disadvantaged schoolmates and either teenage pregnancy or drop-out rates. Cutler

[6] Most studies avoid examining variation in composition across classrooms due to concerns of non-random assignment of students into classrooms. See Vigdor and Nechyba (2004) and Zabel (2008) for examples of attempts to examine peer effects within the classroom.

[7] Rivkin's effect estimates control for the average academic achievement gains made by students in the school (value added), which of course is one of the mechanisms through which school peers can influence student outcomes.

and Glaeser (1997) and Card and Rothstein (2007) also draw on metropolitan level variation and find that residential segregation by race is associated with lower high school graduation rates, lower wages, and higher rates of single parenthood for blacks and a larger black-white test score gap, respectively.

Although these analyses do not require exogenous selection into schools, they are subject to potential biases related to unobserved differences in students across districts, metropolitan areas, or states. Another limitation of studies that use metropolitan level variation is that they often cannot distinguish between peer effects in schools and the effects of processes that occur in the broader urban environment. Card and Rothstein (2007), for instance, find that more segregated metropolitan areas have larger black-white test score gaps. However, after controlling for residential segregation, school segregation is unrelated to their measure of the test score gap, and they cannot conclusively distinguish the effects of school segregation from the effects of residential segregation.[8]

A second approach to estimating the effect of classmate characteristics exploits variation across cohorts within schools. These studies use data drawn from state or local administrative sources to estimate models that control for school-by-grade fixed effects. Such models arguably isolate idiosyncratic variation in student composition across cohorts within a school. Focusing on within school variation reduces concerns about nonrandom selection across schools and also helps to isolate the effects of student composition from any aspects of school quality that are constant across cohorts.

[8] A recent study by Friesen and Krauth (2007) makes efforts to address both of these limitations. Using data from Alberta, Canada, they examine the relationship between segregation across schools within a community and the variance in high school test scores. To control for the possibility that unobserved heterogeneity among students within a community causes higher levels segregation, they focus on the relationship between changes in the level of segregation and changes in test score variance between sixth and ninth grade, and use plausibly exogenous sources of variation in changes in the level of segregation. They find that increases in sorting by parent education level increases variance in test scores, but increases in ethnic and income segregation does not influence test score variance.

Hoxby (2000b) pioneered this approach using data from Texas, and finds that elementary students have lower test scores when they are in cohorts with a larger share of black students, and that the negative effects are larger for black students than other students.[9] Hanushek, Kain, and Rivkin (2002), using similar data and methods, find that the negative effects of percent black on test scores are significantly higher for high ability black students than either low ability black students or students from other ethnic groups. Angrist and Lang (2002) use data from the Boston area's Metco program, which allows minority students from Boston to attend schools in suburban districts. In contrast to the Texas studies, they find only small effects of an increased share of Metco students on the test scores of non-Metco students--effects that are limited to minority girls.[10] Other studies have used the cross cohort approach to examine the effect of other classmate characteristics. Using data from Israel, Lavy and Schlosser (2007) find significant effects of variation in gender composition on student test scores and Lavy, Schlosser, and Passerman (2007) find significant effects of peer ability. Friesen and Krauth (2008) find that the home language spoken by peers influences academic performance in British Columbia.

Our study employs this cross cohort approach, and thus plausibly addresses biases associated with self-selection into schools and provides estimates of the effects of classmate characteristics that are clearly distinguished from the effects of residential segregation and other metropolitan level processes. Previous studies that have used the cross-cohort approach have been limited to estimating effects on test scores. We, however, are able to estimate the effects of classmate characteristics on a number of outcomes including post high school outcomes like college attendance or idleness. In this way, we are able to combine a key strength of studies that

[9] Hoxby (2000a) uses a similar approach to examine the impact of class size on student performance.
[10] The Metco students who transferred into suburban schools were a select sample of Boston students, and their effects on group dynamics, may not be typical, which might account for differences between the Metco and Texas findings.

have used metropolitan level variation—the ability to examine longer term outcomes—with the methodological advantages of the cross-cohort approach. Our study is also the first to conduct a cohort style analysis on a nationally representative sample of students.

III. Data

The data for this study come from the restricted version of the National Longitudinal Study of Adolescent Health (Add Health). The Add Health is a school-based, longitudinal study of the health-related behaviors of adolescents and their outcomes in young adulthood. Beginning with an in-school questionnaire administered to a nationally representative sample of students in grades 7 through 12 in 1994-95 (Wave 1), the study follows up with a series of in-home interviews of students approximately one year (Wave 2) and then six years later (Wave 3). Other sources of data include questionnaires for parents, siblings, fellow students, and school administrators. By design, the Add Health survey included a sample stratified by region, urbanicity, school type, ethnic mix, and size.[11]

Over 20,000 individuals completed the full, Wave 1 survey. However, because we are interested primarily in post-high school outcomes, we drop the 6,000 students who were not in grades 9-12 (grades 10-12 for three year high schools) during Wave 1 and approximately 80 additional students who report still being in high school during Wave 3. The remaining students range from 20 to 24 years old at the time of the Wave 3 survey. In addition, we drop approximately 4,500 individuals who were not followed through Wave 3, and, because our identification strategy depends upon having multiple cohorts within schools, we drop 500 students who are in schools that do not have a 10th, 11th, and 12th grade. Finally, we drop approximately 150 students who did not identify themselves as either white, black, Hispanic, or Asian and 60 students in grades with fewer than 10 sample students. The sample restrictions

[11] See Udry 2003 for full description of the Add Health data set.

leave an analysis sample of approximately 9,500 students in 75 high schools, although the sample varies slightly by the outcome of interest.

Among these various sample edits, the approximately 4,500 individuals who were dropped because they were not followed through Wave 3 are of particular concern. If within-school variation in cohort composition is systematically related to the probability of non-response in Wave 3, then our estimates of the effects of cohort composition could be biased. To test this possibility, we regressed an indicator of whether or not a student was followed through Wave 3 on our cohort composition measures, the set of control variables described below, school fixed effects, and school specific trends. The results indicate that the cohort composition measures are not related to probability of non-response in Wave 3, and thus, dropping non-responders should not introduce any bias into our effect estimates.

We create our cohort-level variables by using the items from the in-school sample of Add Health at Wave 1. The in-school survey was administered to over 90,000 students and asked a limited amount of information, including race/ethnicity and maternal education, for (in principle) a census of students in each sampled school. This feature of the Add Health allows us to reduce the error in our aggregate measures of classmate characteristics and is crucial for obtaining precise estimates in models that are identified using across cohort variation

Table 1 provides descriptive statistics for the variables used in the core analyses.[12] The variables include those we use as outcome measures from wave 3 of the survey (high school graduation, college attendance, score on the Picture Vocabulary Test (PVT),[13] idleness, and post-

[12] Additional descriptives on indicators of academic success, perceptions of school, and behaviors during high school that are used in our exploration of mechanisms are presented below in Table 6.

[13] The Add Health Picture Vocabulary Test (AHPVT) is a computerized, abridged version of the Peabody Picture Vocabulary Test-Revised (PPVT-R). The AHPVT is a test of hearing vocabulary, designed for persons aged 2 1/2 to 40 years old who can see and hear reasonably well and who understand standard English to some degree. The test scores are standardized by age. Some psychologists interpret PVT scores as a measure of verbal IQ. Information on the test is provided online at http://www.cpc.unc.edu/projects/addhealth/files/w3cdbk/w3doc.zip.

high school smoking, binge drinking, and marijuana use), our key cohort composition variables, a set of baseline controls that include grade fixed effects and student attributes directly related to the cohort variables, an extended set of wave 1 controls that are unlikely to be influenced by school experiences plus the wave 1 PVT test score as a measure of verbal ability and an additional set of family variables, which are less clearly exogenous to student school experiences. Table 2 provides means and standard deviations for the outcome variables and our cohort composition variables for different racial groups and for groups defined by the level of mother's education. Black students, Hispanic students, and students with lower levels of parental education do worse on several outcome measures. Each has relatively high dropout rates, low rates of college attendance, low test score levels and high rates of idleness. Black and Hispanic students also attend schools with relatively high percentages of minority students, and Hispanic students and students with lower levels of parental education attend schools with low percentages of college educated mothers. White students, for their part, are more likely to report that they smoke, use marijuana and binge drink than are other groups.

IV. Analytic Methods

In this section, we describe the regression models that we use to estimate the effects of classmates on individual outcomes, and explain how classmate effects are identified in these models. Then we explain how we handle inferences in the context of testing multiple hypotheses. Finally, we present the results of balancing tests and other diagnostics designed to test the validity of our identification strategy.

a. *Identification Strategy*

To avoid issues of selection across schools and to isolate the effect of classmate characteristics from other aspects of school quality, our identification strategy relies on variation across cohorts within schools. To implement this strategy we estimate regressions of the following form:

$$y_{isc} = \alpha_c + \beta_s + \delta_s c + x_{isc}\phi + \varphi P_{sc}^{E} + \gamma P_{sc}^{M} + \varepsilon_{isc} \qquad (1)$$

y_{isc} is an outcome measure for individual i from school s and cohort c; α_c is a cohort or grade specific effect; β_s is a school fixed effect; $\delta_s c$ is a school-specific time or cohort trend where c takes the value of 0 for the oldest cohort and increases by 1 for each successive cohort;[14] x_{isc} is a vector of student level covariates; P_{sc}^{E} is the percent of students in school s and cohort c with a college educated mother; P_{sc}^{M} is the percent of students in school s and cohort c who are either black or Hispanic; and ε_{isc} is a random error term which might be correlated across observations from the same school.[15]

We examine several different outcome measures including whether or not the individual has dropped out of high school, has attended college, is idle (i.e. neither working nor attending school), uses cigarettes, uses marijuana or engages in binge drinking. Each of these variables is measured using Wave 3 of the Add Health and thus represent post-high school outcomes. We also examine the individual's post-high school PVT test score as a measure of verbal ability.

Students from different cohorts are in different grades during the initial wave of the Add Health, and thus we include a cohort specific effect, α_c, to control for these differences in grade level during the initial Wave 1 interview. Including school fixed effects, β_s, ensures the estimation of classmate effects is based on comparisons across cohorts within a school, and

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[14] All students are observed at the same points in time, so referring to these as school-specific cohort or grade trends is more accurate than referring to them as time trends. However, in studies that use administrative data variation across cohorts is often referred as variation within schools over time, and so we use the two terms interchangeably.
[15] Thus, for all our regressions we compute standard errors that are robust to any type of clustering within schools.

controls for unobserved differences in average student characteristics across schools as well as for aspects of school quality that are constant across cohorts within a school.

Although school fixed effects provide powerful controls for selection across schools, differences in peer characteristics across cohorts within a school might be systematically correlated with unobserved variables that affect achievement. Schools that show systematic trends in peer characteristics are of particular concern. For instance, parents might be able to discern when the share minority in a school is increasing over time, and as a result, students from older cohorts who select into the school might differ in systematic, but unobserved ways from students in younger cohorts. Similarly, the quality of teachers who can be attracted and retained to teach younger cohorts might differ from those who can be attracted and retained to teach the older cohorts. In either case, unobserved differences in student and teacher quality across cohorts within the same school could be correlated with differences in the share minority, and would confound estimates of the effect of share minority on outcomes. To address this concern we control for school specific linear trends, $\delta_s c$. As a result our effect estimates are based on the correlation between deviations from the school specific trend in cohort student composition and deviations from school specific trends in student outcomes.

The cohort fixed effects together with the school specific linear trends also help address another problem created by the structure of our data. Unlike school administrative data, we do not observe multiple cohorts passing through the same grade, but rather observe all cohorts at the same time in different grades. Therefore, we cannot explicitly control for school-grade fixed effects, and school specific, systematic changes in cohort variables across grades might be correlated with differences in outcomes across grades. For example, because minorities and those whose parents have less education are more likely to drop out sometime between grades 9 and 12, the cohorts that are in later grades during Wave 1 will have lower percentages of minority and higher percentages of students with college educated mothers than cohorts in earlier grades during Wave 1. Also, because the least motivated students are more likely to dropout as they age, students in the later grades during Wave 1 might be systematically different than students in the earlier grades on unobserved characteristics that influence outcomes. The average effect of any systematic, unobserved differences between older cohorts and younger cohorts that arise because of drop out decisions or other selection that occurs as cohorts move through grades will be controlled for by the cohort or grade fixed effects. Because the effects of dropouts on cohort composition and on unobserved student characteristics are likely to be larger in some schools than others, however, cohort fixed effects may not be sufficient to eliminate potential biases. If we assume, however, that the school specific effects of dropouts or other grade specific effects on cohort composition and on unobserved student characteristics are approximately linear in grades in most schools, then school specific trends will break any correlation between the two variables, and thus minimize any potential biases. While this limitation of our data requires more assumptions than traditional applications of the cohort approach, it does not impact the validity of our diagnostics for instrument exogeneity, and therefore simply requires us to lean more heavily on those diagnostics.

Deviations from school trends in student composition, which are difficult for parents and students to predict, are unlikely to influence their decision to attend a school, and thus, such deviations from trend can be expected to be uncorrelated with student characteristics that influence outcomes. Nonetheless, race and parents' education are likely to be correlated with several other factors that influence outcomes. Thus, even if deviations from school trends in cohort composition are truly random, the students in cohorts with higher than predicted percentages of minority students or college educated parents will differ from students in other cohorts in systematic and potentially important ways. Including individual controls for race and parent education will prevent these systematic differences from confounding our effect estimates. Also, even if deviations from school trends in student composition do not influence a student's initial decision to attend a school, students might systematically opt out of a school that they find unsatisfactory after their initial experience, potentially introducing a source of omitted variable bias into our school trend model. Thus, we include a full set of controls for individual student characteristics measured during Wave 1, x_{isc}. As discussed earlier, these tests also provide some indication of the general validity of our identification strategy.

Our baseline model only includes controls for the individual student variables directly related to the school cohort variables: race/ethnicity[16] and years of education for the parent who responded to the parent survey.[17] A second set of models include additional controls for clearly exogenous student characteristics including gender, age, whether or not the responding parent reports being born in the U.S., number of years the family has lived in the U.S.,[18] a dummy variable indicating whether the parent information was reported or imputed, plus the student's PVT test score during Wave 1 which, while potentially influenced by cohort composition, is our best available proxy for a student's underlying cognitive ability.[19] A third set of models adds an

[16] We include mutually exclusive and exhaustive categories of race and ethnicity, including non-Hispanic white, non-Hispanic black, Hispanic, and Asian. Students who reported being multi-race were designated as black if the races were white and black, and designated as Asian if the races were Asian and white.

[17] Years of education of survey respondent is used since it is our most error free measure of parental education levels. In principle, we might have included dummy variables for mother's educational attainment paralleling the construction of the mother's education cohort variable. Models controlling for those variables instead of parental education yield results that are very similar to the estimates presented in the paper.

[18] Reported by the parent. The variable is set equal to the age of the parent if the parent was born in the U.S.

[19] With the exception of the model for Wave 3 test scores (where estimates are insignificant anyway), the estimates on cohort variables are nearly identical whether or not the set of controls for student attributes includes Wave 1 test scores. While including test score has no substantive effects on our estimates, in principle, including this variable changes the interpretation of our estimates slightly. The baseline models can be interpreted as estimating the total effect of changes in classmate characteristics that operate through dynamics that vary across cohorts within the

extended set of family background variables including log of family income, a single parent family indicator, an indicator of whether or not a student lives with both biological parents, the number of older siblings, and indicators of whether the student reports having discussed school or grades with a parent in the last month, whether one of the student's parents report being a member of a parent/teacher organization, and whether the responding parent reports that he/she or the student's other biological parent has alcoholism. All these variables are measured during Wave 1. These variables provide powerful protection against any potential omitted variables bias. Many of them, however, might be influenced by a student's experiences in school and by the student's behavior, and thus we do not include them in our baseline models.

Our variables of interest are measures of student composition for each cohort within each school. We focus on the percent disadvantaged minority, which is the percent black plus the percent Hispanic, in the school specific cohort, P_{sc}^{M}, and the percent of students in the cohort who have a college educated mother, P_{sc}^{E}. The racial composition of schools has been a leading policy concern dating back to the Supreme Court's landmark ruling in Brown v. Board of Education (1954), and much of the literature on the effects of student composition has focused on racial composition.[20] It is also important, however, to focus on segregation by other family background characteristics, and particularly parental education. Evidence from a wide range of school choice programs indicates that students whose parents have higher levels of education are more likely than other parents to use expanded schooling options to avoid schools with concentrations of disadvantaged students and to attend schools with higher levels of achievement. Thus, the growth of student assignment policies that emphasize parental choice is

school. The models that include the Wave 1 test score give us estimates of the effects of classmate characteristics that operate through dynamics that vary across cohorts within a school and independently of any effects on cognitive development through Wave 1.

[20] We also ran models that use percent black rather than percent black or Hispanic. The estimated effects of percent black were similar to the estimated effects of percent black or Hispanic, but the latter are more precise.

likely to increase stratification of schools by levels of parental education. Also, it is important to distinguish the effects of racial composition from class composition because policies to decrease segregation by class will not necessarily decrease segregation by race, and vice-versa.

Some of the mechanisms through which student composition might influence individual outcomes are constant across cohorts within schools. For instance, a school's ability to garner resources is likely to be determined largely by the composition of the school as a whole and may not vary across cohorts within the school. Similarly, teacher expectations and motivation might be influenced as much by the composition of preceding cohorts as by the composition of the current cohort. By relying on within school variation in cohort composition, however, our estimates will miss any effect that the student composition of the school as a whole has on student outcomes. Thus, we will interpret our estimates as the effects of cohort composition that operate through the mechanisms of cohort specific group dynamics, holding other aspects of school quality constant. It is important to realize that this effect may be only part of the total effect that school composition has on student outcomes.

b. *Type I Error with Multiple Hypothesis Tests*

Studies that examine multiple outcomes must address the concern of type I error because an increase in the number of tests increases the likelihood of rejecting the null hypothesis for at least one of these tests using traditional inferential techniques. One approach to handling the increased likelihood of type I error is to correct the p-values using either a Bonferroni correction (Shaffer, 1995) or resampling approaches described in Westfall and Young (1993) to estimate the likelihood that a specific hypothesis would be rejected under the composite of all the relevant null hypotheses. As noted by Anderson (In Press), this approach has the advantage of identifying the specific hypotheses where the individual null can be rejected with statistical confidence.[21]

On the other hand, Ross et al. (2008) points out that p-value corrections of this sort are quite conservative because they often lead researchers to fail to reject composite nulls in the face of evidence that was very unlikely to have arisen by chance. In their case, they find evidence of discrimination in 7 of 12 measures of adverse treatment for the city of Chicago even though none of the results for those individual measures met the standards for statistical significance using a Bonferroni correction. Similarly, in our paper, none of our individual findings are statistically significant based on adjusted p-values even when using the somewhat less conservative resampling approaches proposed in Westfall and Young (1993, p. 62-68), but for one measure of peer composition we reject the null in 3 out of 7 tests, which would seem unlikely to have occurred by chance under the composite null of no peer effects. Given that the central purpose of our study is to examine the causal effect of peers on a broad set of post high school outcomes and that the Add Health is the only sample that can support such an analysis, we believe that it makes sense to proceed even if we cannot definitively identify the specific outcomes that drive our findings concerning the existence of peer effects.

Therefore, we adopt a strategy to estimate the likelihood p_m that the pattern of p-values that we obtain could have arisen by chance under the null hypothesis that peer composition has no effect on student outcomes.

$$p_m = \Pr[\underline{\hat{p}}^* \mid \underline{h} = 1] \tag{2}$$

where $\underline{\hat{p}}^*$ is the vector of estimated p-values for the likelihood of rejecting each individual null hypothesis when the null is true, and $\underline{h}$ is a vector of hypothesis tests where 1 represents that the

[21] See Anderson (In Press) and Kling, Liebman, and Katz (2007) for recent applications of this approach.

null is true. The logic of this approach is analogous to that employed in F-tests of multiple hypotheses in a multivariate regression context. Note that the probability operator is used as short hand and will be defined more precisely below.

To estimate the likelihood of obtaining a specific vector of p-values when peers have no influence on any individual outcomes we employ a resampling procedure described by Westfall and Young (1993, p. 214-215) and adapt a strategy used by Agresti (2003, p. 97-98) for calculating the exact test for independence in a general contingency table. Agresti's defines the likelihood of a type I error as the sum of the probability of all possible outcomes that occur with equal or less probability than the outcome observed in the data, and we use the resampling approach to estimate the fraction of possible outcomes that are less likely than the observed vector of p-values.

We begin by estimating a logit model for each of our outcomes, with the exception of test score which is not discrete and is resampled using a standard bootstrap technique (Westfall and Young, p. 122-123), on the school fixed effects and trends, cohort fixed effects, and a vector of individual characteristics used in our regressions, but excluding the cohort composition measures in order to obtain estimates under the null hypothesis that peer composition has no effect on individual outcomes. We then use the estimated parameters to predict the likelihood of each outcome for each student in our sample under this null. Next, we generate 10,000 simulation samples by drawing uniform (0,1) random variables for each outcome and each student in our sample, and setting the outcome variable for a particular student equal to one if the draw is less than the expected probability that we calculated for that student. Westfall and Young (1993) recommend this approach because it recognizes that the likelihood of each outcome and correlations between those outcomes varies across observations based on observable attributes.[22]

Next following the logic of Agresti (2003), the p-value patterns that arise from estimating our models using the 10,000 simulation samples must be ordered based on their likelihood of occurrence. Unlike with cell counts in contingency tables, however, p-values fall on a continuum so that the pattern of p-values arising for each simulation sample will be unique and any specific pattern of p-values has an infinitesimal a priori probability of occurring. Therefore, we order the samples by interpreting each estimated p-value as the likelihood of that particular parameter estimate or an estimate smaller in magnitude arising under the null and multiplying the p-values for all outcomes k in a simulation sample *j*, $\hat{p}_{kj}$, to capture the likelihood of that this combination of p-values arose in a simulation sample, $\rho_j = \prod_{k=1}^{K} \hat{p}_{kj}$. The likelihood of obtaining a specific vector of p-values under the null hypothesis that cohort composition does not influence individual outcomes is then computed as the fraction of simulations where ρ_j is less than the associated likelihood of obtaining the p-values that arise from the actual data, $p_m = \sum_{j \in \rho_j < \rho^*} 1/J$, where ρ^* is the product of the estimated p-values from the data and J is the number of simulated samples.[23]

[22] Even though our models are estimated by a linear probability model via OLS, we simulate our data under the null following Westfall and Young's (1993) recommendation to use a formal limited dependent variable specification. In doing so, we draw on their discussion of the fact that resampling approaches can be distorted by skewness and higher moments in the residuals (Westfall and Young, 1992, p. 56-60), which are clearly created by predicting residuals for discrete variables with a linear probability model. Nonetheless, our approach is likely conservative because the non-linear effects of observables that are created using the probit model to generate the simulation samples cannot be captured by the observables in our linear probability model.

[23] The alternative is to enumerate or categorize sets of p-values based on the number of p-values falling below some threshold, such as 0.10 or 0.05 in which case one can calculate the likelihood for each set of p-values for ranking directly using exact probability calculations based on the multiple hypergeometric distribution as described in Agresti (2003, p. 91-98), but this approach is quite awkward because in practice it should be repeated for many different p-values. This approach is analogous to what was done in Ross et al. (2008).

The product of p-values does not actually represent the a priori likelihood that this simulation draw arose, which would be zero since the p-values fall on a continuum. Rather, it is simply used as an index for ordering sets of p-values arising from the simulation samples, and then we calculate the fraction of simulation samples that represent relatively unlikely draws under this ordering when compared to the p-values based on our data. Of course, the likelihood of a set of events occurring only equals the product of the individual event probabilities under the assumption of a zero correlation between the random events. This assumption, however, seems reasonable given that the assumption is being applied to p-values estimated under the null hypothesis. Further, we can examine this correlation directly using the meta-sample of p-value vectors from the simulated data and find that the correlations between the p-values of different outcomes are always below 0.03 and that the vast majority of correlations are below 0.01.

c. *Evidence on Identification Strategy*

As Lavy and Schlosser (2007) point out in a similar analysis of gender composition effects, the success of our identification strategy rests on two things. First, in order to obtain precise estimates, we need sufficient variation in our cohort composition measures after controlling for school fixed effects and trends. Second, in order to make causal interpretations of our effect estimates plausible, deviations from school specific trends in student composition must be uncorrelated with differences in student characteristics across cohorts. In this section we investigate whether or not these conditions are met.

Table 3 examines the extent of variation in cohort composition that is left after removing school fixed effects and trends. As we would expect, most of the variation in our student composition measures is across schools rather than within schools. Removing school fixed effects and trends reduces the standard deviations in the percent of students with college educated mothers by nearly 80 percent and the standard deviation in percent black or Hispanic by more than 90 percent. Thus, our effect estimates are based on small, marginal changes in student composition, and cannot tell us about the effects of moving an individual student across schools with very different student compositions.

Table 3 does, however, suggest that we have sufficient variation to estimate the effects of small changes in cohort composition with reasonable precision. The precision of our estimates depends on our sample size and on the absolute magnitude of the variation we use. The variation in our data in the percent of mothers with college and the percent black or Hispanic after removing school fixed effects and trends is 20 to 80 percent greater than the within school variation in gender composition reported by Lavy and Schlosser (2007), which was enough variation for those authors to obtain statistically significant estimates of gender composition effects. It is fortunate that we have greater within school variation in our student composition measures than Lavy and Schlosser, because our data has roughly one-third as many schools and thus fewer school-specific-cohorts than they do, which reduces the precision of our estimates.

Our identification strategy assumes that variation in student composition across cohorts within a school is generated randomly. To test whether the amount of variation observed in Table 3 is consistent with random assignment of students across school specific cohorts we ran a series of simulations. In each simulation, we randomly match students in our sample to the school and grade specific slots in our sample, and use the resulting distribution of students across school specific cohorts to compute standard deviations for the cohort composition variables. Across 50 simulations of this kind, the average standard deviation for percent of students with college educated mothers in the same school and cohort was 0.029 and for percent of black or Hispanic the average standard deviation was 0.025, which are quite similar to the standard deviations of 0.031 and 0.025 reported in Table 3.[24] These results indicate that the amount of variation across cohorts within schools that we observe in our sample is quite consistent with random assignment of students.

Another informal test of our key identifying assumption can be conducted by checking whether deviations from school specific trends in our cohort composition measures are correlated with deviations from school specific trends for a variety of student background characteristics (balancing tests). If these deviations are uncorrelated, the analysis supports the premise that school trends capture any systematic selection (due to either sorting or attrition) on student observables. Further, if one uses the degree of selection on observables as a guide to the degree of selection on unobservables as suggested by Altonji, Elder and Tabor (2005), null results on the balancing tests would support the assumption that our model specification identifies variation in cohort composition unrelated to unobservables that determine student outcomes.[25]

We performed this check by regressing different student background characteristics on our measures of cohort composition controlling for cohort fixed effects, school fixed effects, school trends, the student's race and the education level of the student's mother. If deviations from school trends in parent education levels and student composition are truly idiosyncratic, then once we control for the student's own race and parent's education level, any correlation between deviations from school trends in the cohort composition variables and deviations from school trends in other student background characteristics should be removed.

[24] The standard deviation around the mean standard deviation for percent with college educated mothers was 0.002 and for percent minority was 0.001 placing the actual standard deviations well within the 95% confidence intervals.
[25] To the best of our knowledge, these balancing tests were first implemented by Lavy and Schlosser (2007). Similar logic has been used in recent studies of neighborhood effects by Grinblatt, Keloharju, and Ikaheimo (2008) and Bayer, Ross, and Topa (2008) that document no sorting on observables over space conditional on their models. Hoxby (2000b) addressed the potential of bias from student selection into cohorts by a strategy of dropping any schools that exhibit trends in racial composition, her "drop if more than random" approach.

Table 4 presents the results of these balancing tests. The results of 11 separate regressions and a total of 22 coefficient estimates are presented. Four of 22 coefficients are significantly different than zero at the 0.10 level, which is more than we would expect to result from chance, but none are significantly different from zero at the 0.05 level, which is less than we would expect to result from chance. Only one of the 11 F-tests for the joint significance of the two cohort variables is significantly different than zero at the 10 percent level. More formally, using the procedure described above, we find that the pattern of p-values for the 11 hypotheses or patterns less likely to occur had 0.464 and 0.603 likelihoods of arising by chance for the share minority and percent mom's with a college education cohort variables, respectively.[26] The balancing tests, then, provide general evidence that school specific trends are sufficient to isolate variation in cohort composition that is not systematically related to student observables, and thus, there is little reason to suspect that differences in unobserved student characteristics across cohorts within a school are biasing our effect estimates.

Despite our appeal to type I error, one might be concerned about the specific rejections of the null hypothesis in our balancing tests. Further, even if our results cannot be distinguished from results that arise by chance, the estimates may represent effect magnitudes that are fairly large and could potentially contribute to bias in our tests for peer effects. To address this concern we estimate models with and without the variables examined in the balancing test. The first specification includes only the covariates also used as controls in our balancing tests, the second specification adds a substantial number of controls including three of the controls that fail the balancing test at the 0.10 level, and the third specification includes all remaining covariates including one additional variable that failed the balancing test. If our identification strategy is working, adding these control variables should have no influence on the estimated coefficients

[26] If considering all 22 tests together, we obtain a composite p-value of 0.573.

for our cohort composition variables. Following the intuition behind Altonji, Elder and Tabor (2005), the impact of including observable student attributes on peer effect estimates likely provide a good indication of the potential bias from unobservables, and so if adding observable controls has little impact on estimates it is reasonable to presume that there is little bias from student unobservables.

V. Effects on Post Secondary Outcomes

Table 5 presents estimates of the effects of our cohort composition variables on seven different wave 3 or post secondary outcomes. All of the estimates presented in Table 5 are from regressions that include controls for cohort fixed effects, school fixed effects, and school trends. For each outcome, we present estimates from regressions that include the baseline set of student covariates listed above, the baseline set of covariates plus extended covariates that include the Wave 1 PVT test score, and the extended set of covariates plus a set of additional family background controls. The estimates on our cohort composition variables are quite stable across each specification of student covariates. The robustness of our estimates with respect to choice of student covariates provides additional support for the results of the balancing tests presented in Table 4.

As seen in Table 5, the percent of students in the cohort with a college educated mother shows significant effects on the decisions to drop out of high school, to attend college, and to use marijuana post-high school. With seven tests for the share students with college educated mothers, we might not expect to find even one rejection with a 5% type I error rate, yet we reject the null at this level of significance for three of the seven tests. Following our resampling approach for these seven tests, we find that our pattern of results or other less likely patterns for share college educated only had a 0.013 chance of occurring under the null hypothesis of no peer effects associated with mother's college education.[27]

Most would consider the direction of these significant effects desirable. The point estimates imply that a 1 percentage point increase in the percent of students whose parents are college educated is associated with a decrease in the likelihood of dropping out of about 0.3 percentage points, an increase in the likelihood of attending college of between 0.4 and 0.5 percentage points, and a decrease in the likelihood of using marijuana after high school between 0.4 and 0.5 percentage points.[28] The figures in Table 2 indicate that the percent of college educated mothers among the classmates of students whose own parents are college graduates is 11.1 percentage points higher than that of students whose own mothers have no college experience. Also, among individuals in our sample, those whose own parents are college educated are 12.3 percentage points less likely to drop out of high school than students whose mothers do not have any college. The effect estimates in Table 5 imply that reducing the gap in exposure to classmates with college educated mothers by half (5.5 percentage points), would decrease the gap in dropout rates nearly 14 percent (from 12.3 to 10.6 percentage points). Similar calculations indicate that reducing the gap in exposure to classmates with college educated mothers by half, would decrease the gap in college attendance between individuals whose own parents are college graduates and individuals whose own parents have no college by nearly 7 percent (from 34.7 to 32.2 percentage points).

[27] These results are based on model three with a full set of controls, but results for the three models are very similar. If we run the resampling approach for all 14 hypothesis tests together, we find a composite p-value of 0.026, which is nearly identical to what we get if we multiply the p-value of 0.013 by 2 in order to perform a Bonferroni correction for the fact that we conducted tests for two different cohort variables.

[28] We also estimated alternative versions of these regressions using the average years of mother's education in the cohort instead of percent of students with a college educated mother. For all of our findings, the point estimates on this variable were in the same direction and implied effects of a similar magnitude as the coefficient on percent with college educated mothers. However, the precision of estimates fell slightly for drop-out and college attendance.

A higher share of students who are black or Hispanic in a cohort is associated with a smaller likelihood of binge drinking after high school. A one percentage point increase in the percent minority is associated with a decrease in binge drinking after high school of nearly 0.6 percentage points. While suggestive, we cannot place the same confidence on this result as we placed on our results for share college educated mother. Only one out of seven findings is statistically significant, and the likelihood of this or less likely patterns arising under the null is 0.301. Just as noteworthy as the effect on binge drinking, the estimated effects of minority share on educational attainment, post-high school test scores, and idleness are small and statistically insignificant.

By definition, exceptionally large deviations from school trends are unlikely to arise by chance, and one might suspect that non-random events and non-linear trends that are associated with large deviations in cohort compositions within a school could cause contemporaneous changes in the unobserved characteristics of students in the school. For instance, if the district a school is located in adopts a school choice program sometime between when the twelfth graders and the ninth graders in our sample entered high school, that could simultaneously cause differences in student composition and unobserved student "quality" across cohorts within a school. To test the sensitivity of our results to the inclusion of cases with large deviations of individual cohort compositions from school trends, we identified cases of large deviations, dropped them from our sample, and reestimated our regressions. The results from these alternative regressions were very similar to the results in Table 5.[29] In addition, given the design of the Add Health, one might worry that our trends might be incapable of controlling for non-

[29] Specifically, we regressed the student's cohort composition measures on a set of school fixed effects and trends, and if the residual from this regression for a particular observation was more than three times the standard deviation of all such residuals, we dropped that observation. Generally, the significant effect estimates became slightly larger and slightly less precise when cases of large deviations were dropped. In no cases, did the results of inference tests change.

linearities in compositional change across grades at the time of survey (as opposed to across cohorts of students). One might especially worry about bias from a peek in drop-out rates in 12th grade or high retention rates in 9th grade. We re-estimated all models dropping either the 9th or 12th grade cohorts and results are very similar.

VI. Exploration of Mechanisms

Peers can influence individual choices and outcomes through a variety of intermediate channels or mechanisms (Hoxby, 2000; Lavy & Schlosser, 2007; Lavy, Passerman, & Schlosser, 2008). Unlike many studies that rely on administrative data,[30] the rich set of survey and other data collected during wave 1 of the Add Health allow us to empirically investigate several potential mechanisms through which cohort composition might influence individual choices. We group the potential mechanisms we are able to investigate into three categories: academic outcomes and expectations; perceptions of school; and behaviors during the high school years.

These student response variables can be viewed as mechanisms in two related, but distinct ways. First, they might be viewed as short-term individual outcomes that are potentially influenced by cohort composition and which, in turn, potentially influence post-secondary outcomes. Second, when aggregated across students within a cohort they can be viewed as indicators of school climate and behavioral norms, which in turn might influence student choices. For example, if students who report that teachers do not care (one of our perception variables) are more prevalent in a particular cohort than in another, that might reflect effective differences in teacher expectations across the cohorts, or if more students report being rowdy or unruly (a behavior variable), then all students in that cohort are exposed to a more undisciplined environment.

[30] Lavy & Schlosser (2007) and Lavy, Passerman, & Schlosser (2008) are exceptions.

Academic success may influence a student's attachment to school and expected returns to continued schooling. Thus, academic success would be expected to influence drop out and college attendance decisions. Also, as indicated above, much of the literature on school peers has examined effects on short term academic outcomes. To estimate the effects of cohort composition on academic outcomes we use four indicators of academic success during high school: grade point average and highest level math course,[31] score on the wave 1 PVT test, and the response to a question asking the student to rate, "how likely is it that you will go to college."

Perceptions of school might also influence a student's attachment to school and thereby dropout and college attendance decisions. We examine responses to a question that asks the student to rate how much he or she feels "that teachers care about you" and responses to three other questions that ask the student to assess how much he or she agrees or disagrees with the statements: "The teachers at your school treat students fairly;" "You feel safe in your school;" and "You feel close to other students at your school."

Finally, the peers one encounters in school can influence an individual students' behaviors during high school. Habits of behavior established during high school might, in turn, influence behavioral choices after school, and also one's ability to access educational opportunities and secure employment. We examine several self-reported indicators of behavior during high school including: how many hours per week the student watches television; whether or not the student got into a physical fight during the last year; whether or not the student acted rowdy or unruly in a public place during the last year; whether or not the student has ever been suspended from school; how often the student had trouble "getting along with teachers" during

[31] This variable was determined using student transcripts. The highest level match course takes on a value from 1 to 9 where higher numbers indicate a more advanced course. Specifically: 1 = Basic/Remedial Math; 2 =General/Applied Math; 3 = Pre-algebra; 4 = Algebra 1; 5 = Geometry; 6 = Algebra II; 7 =Advanced Math (Algebra III, Finite Math, Statistics); 8 = Pre-calculus (includes Trigonometry), and 9 = Calculus

the school year; and whether or not the student smokes cigarettes, uses marijuana, and has engaged in binge drinking. Summary statistics on each indicator are presented in Table 6.

To assess the potential importance of these mechanism variables, we follow Lavy & Schlosser (2007) and Lavy, Passerman, & Schlosser (2008) and estimate regression models identical to equation (1), except that we use as dependent variables our indicators of academic outcomes, perceptions of school, and behaviors. Estimates from these models tell us whether or not a particular short-term outcome or particular aspect of school climate is influenced by differences in student composition across cohorts. Results from models that include all of the individual student controls listed in Table 1—baseline controls, extended controls, and additional family controls—are reported in Table 7.[32]

The percent of classmates who have college educated mothers does not appear to have statistically significant effects on any of the potential mechanism variables in Table 7. Further, when analyzing the likelihood of the p-values observed or less likely patterns arising using the resampling approach, we finding likelihoods of 0.500, 0.980, and 0.517 associated with share college educated mothers for the success, perception, and behavior variables, respectively. Thus, we find no evidence that this aspect of a student's peer environment influences post-secondary outcomes through indirect channels.

In contrast to the results for mothers' education, the percent of classmates who are black or Hispanic appears to have several significant effects on school environment and student behavior. Students in cohorts with high percentages of black or Hispanic students perceive that

[32] This specification is model 3 in Table 5. Estimates from models that include only baseline controls, model 1, or baseline controls plus extended controls, model 2, are substantially similar. The PVT test score variable is dropped from the right hand side in the regression that uses the PVT test score as a dependent variable.

their teachers are less caring,[33] watch considerably more television, and are more likely to engage in undesirable behaviors such as fighting, acting unruly, and smoking marijuana than students in cohorts with lower percentages of minority students. Our resampling approach implies that these results were unlikely to arise by chance with type 1 error rates of 0.025 and 0.005 percent for the perception and behavior variables, respectively.[34] The results in Table 5 suggest that these effects on school environment and behavior during high school do not translate into higher rates of dropping out, idleness or substance use, or into lower rates of college attendance in early adulthood. Nevertheless, these largely undesirable effects of having more black or Hispanic classmates might pose important concerns in the own right and might have longer term consequences for individual outcomes that we are not able to examine.

A reasonable question to ask in light of the null findings on mechanism for share college educated mothers[35] is whether other reasonable mechanisms exist to explain the peer relationships found above. Perhaps the most direct channel is through what sociologists have called the contagion effect (Crane, 1991). According to the contagion hypothesis, increased prevalence of a behavior in an individual's environment may increase the likelihood that the individual engages in the behavior. While a formal test of the contagion hypothesis is beyond the scope of this paper, we can at least examine whether contagion is a feasible explanation for the post secondary peer effects found in our study. A necessary condition for contagion to be an

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[33] The other perception variables also take the expected sign and are substantial in magnitude, but are imprecisely estimated.

[34] If we pool all tests associated with the percent minority cohort variable, these tests yield a composite p-value of 0.005. If we take the most conservative approach and pool the minority cohort tests with the share mom's college educated tests where the findings are universally negative (high p-values), we still have a composite p-value of 0.056. A more reasonable approach would be to multiply 0.005 by 2 as a Bonferroni correction for the fact that we ran tests for two different cohort variables yielding a p-value of 0.010 for the share minority variables as a set, or multiply 0.025 and 0.005 by 6 for 3 mechanisms and two cohort variables yielding p-values of 0.150 for perceptions and 0.030 for behavior on cohort share minority.

[35] As well as, the significant negative effects of share minority on in-school environment when share minority has a positive effect on binge drinking.

explanation for the effect of a peer attribute on outcomes is that students with this attribute must engage in the relevant behaviors at a higher rate than other students. For example, in our case, if students whose mothers have college degrees are less likely to drop out of high school, then students in cohorts with a large number of students with college educated mothers will be more likely to observe that choice and if there is contagion also be more likely to make that choice.

To assess whether the contagion hypothesis could plausibly explain the effects of classmates on post-secondary outcomes in Table 5, we examined the prevalence of specific outcomes among students with a college educated mother and among minority students relative to other students in the same school. Specifically, we regressed each of the seven outcomes in Table 5 on indicators of the race and mother's level of education for the individual student controlling for cohort fixed effects, school fixed effects, and school specific time trends.[36] The results are consistent with the contagion hypothesis. Compared with other students in the same school, students with a college educated mother are 9 percentage points less likely to drop out of high school and 21 percentage points more likely to attend college, and both of these differences are statistically significant. In contrast, students with college educated parents are less than 5 percentage points more likely to be idle, less than 4 percentage points less likely to smoke, and only 6 percentage points more likely to binge drink. Thus, the differences between students with college educated mothers and other students are considerably more marked for dropping out and college attendance than for the other outcomes we examine, and dropping out and college attendance is precisely where we see cohort effects.

[36] Race and mother's education are controlled for using four dummy variables each following the decompositions presented in table 2. Estimated effect of minority is based on a weighted average of coefficients for African-American and Hispanic students and the effect of college educated mother's (the omitted category) is based on a weighted average of estimates associated with mother high school drop-out, high school graduate, and obtaining some college. These specifications are very close to model 1 presented in Table 5 and yield nearly identical estimates for the coefficients on cohort variables. Full regression results are available upon request.

Similarly, compared to white students in the same school, black and Hispanic students are 10 percent less likely to binge drink after high school, and that difference is statistically significant and considerably larger than any other difference that we observe between minority students and white students. For instance, black and Hispanic students are less than 2 percentage points more likely to dropout and between 3 and 4 percentage points less likely to attend college than white students from the same school. As in the case of mother's education, then, the percent black or Hispanic in the cohort shows effects on the outcome for which differences between minority and white students are largest.

It is worth noting that we do not find evidence of peer effects for all outcomes where we might expect to see contagion effects. Students with college educated mothers score significantly higher and both black and Hispanic students score significantly lower than other students from the same school on post-high school tests of cognitive ability. However, neither the percent of students with a college educated mother nor the percent minority in the cohort shows significant effects on test scores. This finding is not that surprising, however, given the relative immutability of cognitive ability by the time a student reaches high school (Heckman 2006, Cunha and Heckman 2007). Also, contagion effects are unlikely to explain the association between the percent of classmates with college educated mothers and the choice to smoke marijuana after high school, as the prevalence of marijuana use among students with college educated mothers is virtually the same as it is among other students from the same school.

VIII. Conclusions

Our analyses use data from the Add-Health to estimate the effects of classmate characteristics on a range of student choices and outcomes. The unique structure of the Add-Health allows us to estimate these effects using comparisons across cohorts within schools, and to examine a wider range of outcomes than other studies that have used this identification strategy.

We find evidence that classmate characteristics do matter for potentially important individual outcomes. Most importantly, we find that increases in the percent of students with a college educated mother in one's cohort have several desirable affects on individual outcomes. Specifically, higher levels of parent education among one's classmates is associated with lower rates of dropping out of high school, higher rates of college attendance and a reduced likelihood of using marijuana after high school. Analyses of potential mechanisms suggest that the percent of classmates with college educated parents has no discernible effect on academic success, on perceptions of school, or on behaviors during high school. However, students with college educated mothers are considerably less likely to drop out and considerably more likely to attend college than other students in the same school, which is necessary if direct contagion is to be a plausible explanation for these effects.

We do not find any evidence that the share of students from disadvantaged minority groups negatively affects post-secondary student outcomes, on average. An increase in the percent of black or Hispanic classmates is, however, associated with lower rates of binge drinking post-high school. As with drop-out and college attendance above, the estimated effect of minority classmates on binge drinking is consistent with the contagion hypothesis. Although the percent of students from disadvantage minority groups does not show any negative effects on the post secondary outcomes we examine, it is associated with students reporting less caring student-teacher relationships and increased prevalence of some undesirable student behaviors during high school. Although these negative short-term effects do not appear to influence the longer term outcomes we are able to examine, they might be of interest to policymakers in their own right or because they might have consequences for outcomes that we are not able to examine.

A few caveats on our findings are worth noting. First, our estimates only capture the effects of classmate characteristics that operate through the mechanisms that vary across cohorts within schools. Any effects that operate through the school's ability to attract resources are missed, as are any other effects of student composition that operate schoolwide such as the long-term effect of school demographic composition on school environment and teacher attitudes. Thus, our results might underestimate the total effect of school composition on individual outcomes. Second, our research design does not provide enough statistical power to examine whether the effects of school composition vary by individual student characteristics. This type of variation in effects is important for assessing the costs and benefits of policies that influence levels of student segregation across schools. Finally, we are only able to examine outcomes in early adulthood. Examination of longer term outcomes await future waves of the Add Health. Nonetheless, our results provide evidence that classmate characteristics play a role in determining individual outcomes, and argue that policies that influence the grouping of students into schools may, therefore, have important costs and benefits.

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Table 1: Sample descriptives

	N	Mean	Standard Deviation
Post Secondary Outcome Variables			
Drop Out of High School	9398	0.136	0.343
Attend College	9043	0.586	0.493
Post High School (PVT) Test Score	9051	101.11	17.21
Idleness Post High School	9052	0.130	0.336
High School Smoking	9350	0.312	0.463
Post High School Smoking	9361	0.338	0.473
High School Marijuana Use	9244	0.170	0.376
Post High School Marijuana Use	9371	0.211	0.408
High School Binge Drinking	9372	0.345	0.475
Post High School Binge Drinking	9356	0.517	0.500
Cohort Variables			
Percent black or Hispanic in cohort	9398	30.4	29.4
Percent with college educated mother in cohort	9398	28.8	14.0
Baseline Controls			
Black	9398	0.163	0.370
Hispanic	9398	0.119	0.323
Asian	9398	0.048	0.207
Parent Education	9398	13.62	2.27
Grade 10 Indicator	9398	0.255	0.436
Grade 11 Indicator	9398	0.239	0.426
Grade 12 Indicator	9398	0.256	0.436
Extended Controls			
Male	9398	0.505	0.500
Age	9398	16.95	1.25
Parent Age	9398	42.59	5.82
Parent Native Born	9398	0.872	0.302
Parent Years in US	9398	35.70	13.09
Parent Information Missing	9398	0.335	0.472
PVT Score (Wave 1)	8953	101.10	14.33
Additional Family Controls			
Log Family Income	9398	0.358	0.209
Single Parent	9398	0.264	0.405
Live with Both Parents	9398	0.573	0.456
Number Older Siblings	9385	0.834	1.179
Talk about School with Parents	9398	0.638	0.464
Parent Involvement	9398	0.310	0.426
Parent Alcoholic	9398	0.149	0.328

Post secondary variables are measured using wave 3 of the Add Health, and all other variables are measured using wave 1. Cohort variables are calculated for each grade surveyed in each high school using the full in-school wave 1 sample. Percent black or Hispanic is based on student report that their race is African-American and/or their ethnicity is Hispanic, and percent mothers with college is based on student report that mother had completed at least a four year college degree.

Table 2: Student outcomes and cohort composition, by race and mother's educational attainment

Outcomes	White Students	Black Students	Hispanic Students	Asian Students
Drop Out of High School	0.121 (0.326)	0.159 (0.366)	0.206 (0.404)	0.097 (0.296)
Attend College	0.607 (0.488)	0.519 (0.500)	0.489 (0.500)	0.764 (0.425)
Post High School Test Score	0.344 (0.682)	-0.399 (1.024)	-0.184 (1.103)	-0.028 (1.003)
Idleness Post High School	0.116 (0.320)	0.194 (0.395)	0.140 (0.347)	0.100 (0.300)
Post High School Smoking	0.390 (0.488)	0.204 (0.403)	0.260 (0.439)	0.254 (0.435)
Post High School Marijuana Use	0.229 (0.420)	0.181 (0.385)	0.184 (0.387)	0.131 (0.338)
Post High School Binge Drinking	0.599 (0.490)	0.253 (0.435)	0.460 (0.498)	0.390 (0.488)
Percent black or Hispanic in cohort	17.0 (17.9)	62.6 (29.1)	57.9 (29.7)	41.7 (23.0)
Percent with college educated mother in cohort	28.5 (13.8)	29.6 (14.0)	26.4 (13.1)	37.2 (14.9)
Sample Size	4920	1921	1701	856

	High School Drop-Out	High School Graduate	Some College	College Graduate
Drop Out of High School	0.250 (0.433)	0.143 (0.350)	0.127 (0.264)	0.057 (0.232)
Attend College	0.356 (0.479)	0.518 (0.500)	0.620 (0.485)	0.808 (0.394)
Post High School Test Score	-0.297 (1.026)	0.111 (0.793)	0.240 (0.835)	0.398 (0.792)
Idleness Post High School	0.185 (0.388)	0.139 (0.346)	0.126 (0.332)	0.085 (0.278)
Post High School Smoking	0.295 (0.456)	0.376 (0.484)	0.347 (0.476)	0.303 (0.460)
Post High School Marijuana Use	0.147 (0.354)	0.214 (0.410)	0.229 (0.420)	0.235 (0.424)
Post High School Binge Drinking	0.377 (0.485)	0.528 (0.499)	0.528 (0.499)	0.583 (0.493)
Percent black or Hispanic in cohort	44.0 (32.9)	27.2 (28.0)	28.1 (27.7)	28.2 (28.1)
Percent with college educated mother in cohort	23.4 (10.5)	26.1 (11.8)	29.1 (12.3)	36.3 (17.2)
Sample Size	1657	3083	2189	2469

Categories are mutually exclusive with Hispanic students classified as any student who reports race and ethnicity as being Hispanic, and black students classified as any non-Hispanic student reporting race as African-American. Means and standard deviations in parentheses.

Table 3: Variation in cohort composition measures after removing school fixed effects and trends.

	Raw cohort variables				
Full Sample	N	Mean	Std Dev	Min	Max
Percent mothers with college	9384	0.302	0.139	0.000	0.877
Percent black or Hispanic	9398	0.377	0.312	0.000	1.000
	Residuals after removing school fixed effects and trends				
Full Sample	N	Mean	Std Dev	Min	Max
Percent mothers with college	9384	0.000	0.026	-0.159	0.143
Percent black or Hispanic	9398	0.000	0.030	-0.203	0.122

The residuals are calculated based on a simple linear model including school fixed effects and school dummies interacted with a time trend.

Table 4: Balancing tests for cohort composition measures

Dependent Variable	% black or Hispanic	% with college educated mother	F-statistic
Male	-0.431* (0.231)	-0.151 (0.200)	1.980
Age (in years)	-0.120 (0.312)	-0.076 (0.273)	0.113
Parent's age (in years)	-3.918 (4.012)	3.908* (2.044)	2.025
Parent born in the U.S.	-0.174* (0.101)	-0.074 (0.085)	1.856
Missing parent information	0.260 (0.257)	-0.105 (0.262)	0.675
PVT test score	9.923 (6.652)	0.668 (5.497)	1.123
Log of family income	0.095 (0.094)	0.139 (0.096)	1.655
Single parent family	0.396 (0.270)	0.007 (0.219)	1.117
Live w/both biological parents	-0.218 (0.247)	0.442* (0.259)	2.396*
Number of older siblings	-0.251 (0.638)	-0.167 (0.398)	0.193
Parent alcoholism reported	0.039 (0.120)	-0.113 (0.178)	0.322

The figures in each row are coefficients from regressions that include in addition to the cohort composition measures controls for cohort fixed effects, school fixed effects, school trends, the student's race, and the student's parent's years of education. All variables are measured using Wave 1 of the Add Health. Figures in parentheses are standard errors robust to clustering at school level. The F-statistics is for the joint effect of percent black or Hispanic and percent with college educated mothers. * designates significantly different from zero at 0.10 or an F-Statistics greater than 2.303.

Table 5: Estimated impacts of cohort composition on post-secondary student outcomes

Cohort Variables	Baseline Controls	Baseline + Extended Controls	Baseline + Extended Controls+ Additional Family	Baseline Controls	Baseline + Extended Controls	Baseline + Extended Controls+ Additional Family
	Drop Out of High School			*Attend College*		
% College Educated Mother	-0.327**	-0.312***	-0.299***	0.515**	0.504**	0.439**
	(0.131)	(0.106)	(0.112)	(0.223)	(0.210)	(0.189)
% black + % Hispanic	0.080	0.104	0.064	0.034	0.027	0.060
	(0.188)	(0.169)	(0.166)	(0.296)	(0.273)	(0.267)
	Post High School Test Score			*Idleness Post High School*		
% College Educated Mother	0.239	0.210	0.232	0.039	0.020	0.042
	(0.322)	(0.249)	(0.257)	(0.130)	(0.136)	(0.137)
% black + % Hispanic	0.591*	0.342	0.327	0.085	0.123	0.118
	(0.334)	(0.229)	(0.222)	(0.160)	(0.147)	(0.150)
	Post High School Smoking			*Post High School Marijuana Use*		
% College Educated Mother	0.126	0.215	0.248	-0.474***	-0.435**	-0.422**
	(0.190)	(0.196)	(0.197)	(0.166)	(0.172)	(0.169)
% black + % Hispanic	0.311	0.295	0.269	0.267	0.254	0.218
	(0.250)	(0.237)	(0.237)	(0.195)	(0.205)	(0.204)
	Post High School Binge Drinking					
% College Educated Mother	-0.205	-0.096	-0.116			
	(0.213)	(0.208)	(0.209)			
% black + % Hispanic	-0.452	-0.578*	-0.599**			
	(0.276)	(0.291)	(0.294)			

All regressions include controls for cohort fixed effects, school fixed effects, and school trends as well as the individual student covariates related to the cohort variables. The dependent variables are shown in italics above the parameter estimates, and all dependent variables are measured using wave 3 of the Add Health. Figures in parentheses are standard errors robust to clustering at the school level. * designates significantly different from zero at 0.10, ** significantly different than zero at 0.05 level, and *** significantly different from zero at 0.01 level.

Table 6: Sample descriptives for potential mechanism variables

	N	Mean	Standard Deviation	Min.	Max.
High School Academic Outcomes					
Grade point average	9296	2.74	0.77	1	4
Highest math course	7683	6.14	1.92	1	9
PVT score (wave 1)	8953	101.00	17.20	9	123
College expectations	9363	4.15	1.17	1	5
Perceptions of School					
Teachers care	9332	3.47	0.97	1	5
Teachers treat students fairly	9393	2.44	1.05	0	4
Feel safe in school	9394	2.71	1.04	0	4
Feel close to peers	9390	2.67	1.01	0	4
Behaviors During High School					
Television hours/week	9378	14.89	14.08	0	99
Got in physical fight	9346	0.29	0.45	0	1
Acted unruly in public place	9346	0.49	0.50	0	1
Ever suspended from school	9393	0.19	0.39	0	1
Trouble getting along with teachers	9395	0.80	0.92	0	4
Smoke cigarettes	9348	0.28	0.45	0	1
Smoke marijuana	9244	0.16	0.37	0	1
Binge drink	9372	0.31	0.46	0	1

Variables measured using wave 1 of the Add Health

Table 7: Estimated impacts of cohort composition on high school academic outcomes, perceptions of school, and behaviors

Dependent Variables	% College Educated Mother		% Black or Hispanic	
	Coeff.	(S.E.)	Coeff.	(S.E.)
High School Academic Outcomes				
Grade Point Average	0.507	(0.408)	0.231	(0.511)
College Expectations	0.555	(0.663)	0.174	(0.646)
Highest Math Course	-0.883	(1.064)	1.422	(1.190)
PVT Score (Wave 1)	0.141	(0.316)	0.603*	(0.342)
Perceptions of School				
Teacher Cares	0.098	(0.424)	-1.175**	(0.417)
Teachers are Fair	-0.377	(0.693)	-0.622	(0.525)
School is Safe	0.154	(0.412)	-0.380	(0.483)
Feel Close to Peers	0.153	(0.502)	-0.582	(0.572)
Behaviors During High School				
Television Hours/Week	-5.356	(5.183)	18.268**	(7.729)
Fight	-0.100	(0.213)	0.512**	(0.240)
Unruly Behavior	-0.061	(0.277)	0.695***	(0.229)
Suspended	-0.160	(0.201)	0.172	(0.224)
Trouble with Teachers	-0.357	(0.606)	1.080	(0.669)
Smoke Cigarettes	-0.173	(0.268)	-0.047	(0.249)
Smoke Marijuana	-0.170	(0.161)	0.400**	(0.183)
Binge Drink	0.006	(0.199)	0.165	(0.164)

Each row presents results from a separate regression. Left most column indicates the dependent variable. Each regressions includes regression includes controls for cohort fixed effects, school fixed effects, and school trends as well as baseline, extended and additional family controls. All variables are measured using wave 1 of the Add Health. Figures in parentheses are standard errors robust to clustering at the school level. * designates significantly different from zero at 0.10, ** significantly different than zero at 0.05 level, and *** significantly different from zero at 0.01 level.