PEER GROUP EFFECTS ON STUDENT OUTCOMES:

EVIDENCE FROM RANDOMIZED LOTTERIES

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To my husband, Dr. Haitao Hu, unconditional love and support

To my children Kevin and Emma Hu, unending inspiration

And

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CHAPTER I

INTRODUCTION

This dissertation investigates the peer group effects on middle school students' academic achievement and behavioral outcomes in a mid-size urban district in the South. The primary objective of this project is to implement credible methodologies to circumvent the endogeneity problems in peer effect estimation and therefore obtain unbiased estimates of peer group effects on individual student outcomes.

Peer qualities and peer behaviors have long been recognized as among the most important determinants of student outcomes. As early as 1966, Coleman and his colleagues have demonstrated the importance of student composition on individual's achievement. In the well-known report *Equality of Educational Opportunity*, Coleman et al. (1966) write:

> *"(Finally), it appears that a pupil's achievement is strongly related to the educational backgrounds and aspirations of the other students in the school. ………" (p.22)*
>
> *"Attributes of other students account for far more variation in the achievement of minority group children than do any attributes of school facilities and slightly more than do attributes of staff"(p.302)*

Aware of the importance of peer impacts, both families and policy makers have included peer quality as a prominent element in educational decision making. For example, parents tend to seek for better companions for their children through residential choices and other school choice options. Many controversial education policies, such as vouchers, school desegregation, and ability tracking, intend to improve student performance through changing the composition of peers.

However, identifying peer effects is a very difficult task. The most problematic issue is that families and students usually choose schools and peer groups where they share similar attributes with other members. Therefore, measures of peer characteristics may just signal other unobservable individual factors that also affect the outcomes, such as student willingness to work and parental ambition and resources. This endogenous choice leads to a selection bias problem. Another problem is that an individual's outcome and that of his peers are formed simultaneously --- a student's achievement is impacted by the achievement of his classmates and vice versa. This creates a standard simultaneity bias problem, also termed as *reflection* problem by Manski (1993). In addition, inference about peer effects is particularly vulnerable to a general misspecification problem --- omitted variable bias, due to the fact that both individuals and peers are subject to a common environment.

Studies attempting to measure peer group effects are susceptible to the endogeneity biases arising from self-selectivity, simultaneity, and omitted variables correlated with peer characteristics. Interestingly, in spite of the fact that theoretical articles on social interaction or peer effects have concentrated most attention on the reflection problems (or simultaneity bias, e.g. Manski, 1993; Moffit, 2002), selection bias is the most pervasive methodological issue discussed in empirical studies. The majority of empirical research on peer effects circumvents simultaneity bias by examining only peer demographic characteristics (such as race or gender composition) or using lagged values for peer behaviors or outcomes. Meanwhile, most peer effect studies have focused on reducing or eliminating selection bias by implementing a variety of creative techniques, such as instrumental variables (IV), fixed effect models (FE), and randomization experiments (RA).

In the most recent two decades, efforts to identify peer effects on student outcomes span social science. However, thus far, they have not reached a consensus. For example, Evans, Shwab, and Oats (1992), using instrumental variable methods, find no significant school peer effects on teenager behaviors. Hanushek et al. (2003) estimate moderate peer effects on student achievement in Texas schools. They tackle across-school selections by implementing fixed effect strategies, and eliminate simultaneity problems by using lagged measures of peer achievement. Two studies by Sacerdote (2001) and Zimmerman (2003) use randomly assigned college roommate data and find a significant association between roommate academic attainment and individual achievement.

Among all the methods to reduce selection bias, randomization is the most credible one. In a randomized experiment with participants arbitrarily assigned to a treatment group (for example, a choice school) or a control group (a neighborhood school), differences between the individuals in the two groups arise solely by chance, which ensures the endogeneity of peer group formation. Peer group effects therefore can be ascertained from examining how the peer composition differences between the treatment group and the control group influence individual outcomes.

In recent years, the administration of school choice programs often provides good opportunities of studying peer effects with randomization approach. In many school choice programs, the admissions are conducted through lottery when the choice schools are oversubscribed --- the unsuccessful lottery participants who enroll in the neighborhood school can then serve as the natural control group for the purpose of measuring the peer group effects on student outcomes. This approach is used in the study by Cullen and Jacob (2007), who find no evidences that lottery winners to higher quality schools (measured by average peer achievement) are better off in academic achievement than those who lost the opportunity to go to the selective schools.

In this study, I will exploit randomization via magnet school admission lotteries to examine the peer group effects on student outcomes at both school and classroom levels. The district studied operates magnet programs at three levels---elementary, middle, and high school. This study will focus the peer effect investigation on middle school students (from grade 5 to grade 8) because the state end-of-year assessments have been most consistent from grade 3 to 8. The district conducts separate lotteries for each magnet school to determine admission. Students who apply for the district-operated magnet schools are randomly assigned to either a choice school or a neighborhood school; conditional on the attendance zone, students are also randomly assigned to the peers in either the choice school or the neighborhood school. Randomized lotteries therefore bring an exogenous source of variation in peer characteristics and will be exploited to overcome the critical issue of selection bias in identifying peer effects.

Under an ideal situation when there is only one magnet school and one neighborhood school and all participants fully comply with the lottery assignment, peer effects can be estimated directly from the average differences between the treatment group (magnet school) and the control group (neighborhood school). However, similar to many other social experiments, the lottery-induced admission process in the district studied also has a lot of complications. First, the lottery school enrollment process is voluntary and participants do not fully comply with the lottery assignment. For example, lottery winners may not enroll in the choice school; lottery losers seek for other options like private school. With the existence of non-compliance, the lottery-induced admission can no longer be considered as a pure randomized experiment. Second, the district operates more than one magnet programs at middle school level. Although a student can only enroll in one magnet school, a lot of students applied for multiple lotteries. Third, in the years of investigation, there are significant student attritions among the lottery participants. Particularly, the attrition rate is higher among lottery losers than lottery winners. Forth, peer effects may be confounded with student's heterogeneous responses to magnet school treatment effects, or may be proxies for some unobserved school factors, such as the quality of teachers. For example, if less effective teachers tend to be assigned to schools (classrooms) with high proportion of low SES students, peer effects are likely to signal teacher quality.

Due to all the complications, even the lottery-induced randomization can not ensure the exogeneity of the peer compositions. Therefore, instead of simply comparing the outcomes of winners and losers with different peers, this study exploits the randomized admission lotteries to form an instrument variable for the regressor of interest --- the actual peers, and estimates the causal relationship between individual outcomes and the peer groups from the instrumented (exogenous) peer variables. The model controls for a large number of individual and school characteristics to improve the precision of the regression models and eliminate the biases from attrition. In order to test if peer effects are just signals for teacher impacts, I will also include teacher fixed effects in the analyses. In addition, the models will control for the interaction between treatment indicators with observable individual characteristics to examine if peer effects are confounded with heterogeneous responses to treatment effects. Like many other empirical studies in peer effects, this study will circumvent the simultaneity problem by using pre-determined peer characteristics (such as race, gender, and social economic status in peer composition) and lagged measures of peer outcomes.

This dissertation examines the impacts of peer groups on both student academic achievement (measured by math and reading test scores) and student behavioral outcomes (measured by student absence rate and disciplinary infractions). The investigation of peer effects is conducted at both school and classroom levels. Specifically, the dissertation will answer the following research questions:

1. What is the impact from average peer characteristics on individual student outcomes at both school and classroom levels?
2. What is the impact from the dispersion of peer characteristics on student outcomes at both levels?
3. To whom do peer effects matter the most --- which subgroup of student population are more significantly impact by the peer characteristics?

The rest of the dissertation will be organized in the following manners: Chapter II provides a review of relevant literature. The theoretical research review presents the canonical model for peer effect estimation and explains the three major methodological challenges in identifying peer effects; and the empirical research review examines the existing evidences from some selective studies in peer effects, with a focus on the strengths and weaknesses of the methodologies used in these studies. Chapter III describes the district under study and its magnet programs, presents the data sources, and most importantly, explains the analytical strategies. The discussion of the methodology includes peer effect identification strategies at both school and classroom levels. It will start with the basic models under the strict assumption of a pure randomization, followed by the 2-stage least square (2SLS) IV models with the relaxation of that assumption. Chapter IV presents the results of school level peer group effects on student academic achievement. The findings will include the average peer effects, the impacts from dispersion of peer characteristics, and the heterogeneous peer effects on students in different subgroups. Chapter V reports the findings of classroom peer effects on student academic achievement, and compares the results with those from school level analyses. Chapter VI provides the findings of peer influences on student behavioral outcomes at both school and classroom levels. Finally, chapter VII summarizes the findings, discusses the methodological contributions and political implications of this study.

CHAPTER II

REVIEW OF THE LITERATURE

This study exploits randomization through admission lotteries to examine the relationship between peer composition and student outcomes. As such, the review of literature focuses on previous estimates of peer group effects. The first part of this chapter reviews theoretical studies on social interaction and peer effects in schools. It begins by defining peer group effects, describing the multiple channels through which peers affect student outcomes and presenting the methodological challenges in identifying peer effects. The second part of this chapter provides an overview of the existing evidences on the nature and quantitative importance of the association between peer group composition and student outcomes in academic and behavior. The review of empirical studies will focus on the methodological strengths and weaknesses.

Conceptual Framework

Peer effects, neighborhood effects, and other non-market social influences are generally termed as ‘social interactions’--- the impact on one individual of the attributes or actions of other group members (Moffit, 2001). Peer effects in education usually include the impact of social interactions between individual student and other students in the same school or classroom, rather than the interactions between the student and families or teachers.

The mechanisms through which peer groups affect individual's academic achievement are complex. Peers not only influence individuals directly through student teaching, role modeling, or classroom disruption; they also impact individual students indirectly through the perceptions of teachers and administrators on the peer groups. For example, if a teacher thinks one particular socioeconomic group is academically weak, she may lower her expectation and slow down her curriculum in a classroom with a high proportion of students from that group, which therefore may negatively affect an individual student's performance, regardless of that student's own SES status.

In an influential article on the topic of social interaction effects, Manski (1993) proposes that the relationship between one individual's behavior and other group member's behavior comes from three distinct effects. Here, let's apply the concepts to peer effects in education:

a. Endogenous effects (or simultaneous effects)—a person's behavior varies with the mean behavior of the peer group. For example, the propensity of a student graduating from high school will be impacted by the proportion of students graduating from high school in the same school.

b. Exogenous effects (or contextual effects)—a person's action varies with the exogenous characteristics (pre-determined characteristics) of the peer group. For example, the propensity of a student graduating from high school will be affected by the average level of mother's education of other students in the school.

c. Correlated effects—persons in same group tend to behave similarly because they are subject to a common institutional environment or they share the similar characteristics. Literature often terms the shared institutional settings as 'common shocks'---for example, that all students in the same classroom do well academically may reflect nothing but the high quality of the teacher. The other part of the correlated effects, 'the shared characteristics', draws a lot of interest from empirical studies. It is called 'selection problems', which arises when individuals tend to self select into a group with members sharing similar attributes. For example, families that are very supportive of children's education are more likely to sort themselves across schools in order to seek for better peers.

Accordingly, research studying peer effects typically models the behavior (or outcomes) of an individual (e.g., educational outcomes, criminal behavior, or teen childbearing), as a function of the average behavior of his/her social group, the individual's own characteristics, and also the characteristics of the group:

$$y_{ij} = \alpha + \beta\overline{y}_{-ij} + \gamma\overline{X}_{-ij} + \delta X_{ij} + \eta Z_j + \mu_{ij} \qquad (2.1)$$

where y_{ij} represents individual behavior, like test scores, for individual i in school j; $\overline{y}_{-ij}$ is the average test scores for peers of student i in school j; $\overline{X}_{-ij}$ are a vector of mean pre-existing peer characteristics of student i in school j; X_{ij} are a vector of individual or family characteristics of student i in school j, including gender, race, and social economic status; Z_j are school level characteristics, such as teacher quality and school policies etc.; μ_{ij} is an individual error term. In the language of Manski (1993), the coefficients β reflects the endogenous effects, the coefficient γ reflects the exogenous or contextual effects, and the coefficient of η then reflects the correlated effects.

However, Manski (1993, 1999, and 2000) demonstrates that without severe restrictions, the standard single equation approach like model (2.1) is unable to separately identify the causal peer group effects from other influences. The key issue, according to Moffit (2001), is that peer effects are ***endogenous***. The endogeneity, as Moffit further explains, arises from three problems:

- Simultaneity problem: simultaneity bias is also called reflection problem by Manski (1993, 1999, 2000), which arises from the endogenous effects wherein one person's actions affect other group members' actions and vice versa. As a simple illustration, in the linear-in-mean model discussed in above, while we assume that the average achievement of peers affects individual achievement through the coefficient β, individual i also influences the average achievement of the peers if a symmetric equation holds for every students in the group. As a result, the individual error term (μ_{ij}) is mechanically correlated with the peer effect variable $\bar{y}_{-ij}$, which leads to an inconsistent estimation of peer parameters. Due to this simultaneous nature, it is extraordinary difficult to identify the causal effects of peer interactions using contemporaneous peer behavior or outcome measures without severe restrictions.
- Omitted unobserved factors or measurement error: Omitted variables problem or measurement error occurs when a determinant of the student's outcome is omitted or measured poorly in the model. Omitted variables bias is a common misspecification to all types of regression models---it is virtually never possible to include all relevant factors in a model. However, due to the correlation effects, omitted variable bias is particularly damaging to the inferences of peer

effects (Hanushek et al., 2003). For example, students in a same school are subject to similar environment and experiences. Therefore both individual's and peers' achievement will tend to be affected by the common omitted factors, which may induce a correlation between the peer variables and the random error terms for all students. It will lead the false attribution of common behavior among students in the same school to peer influences, whereas, in truth, the students have similar behavior just because they are subject to a common (unobserved) environment, such as high-quality teachers.

- Endogenous membership problems: it is usually called selection bias or group endogeneity in the literature; and it is the most pervasive methodological issue discussed in empirical studies. The peer group itself is often the matter of individual choice---families and children usually choose being in a neighborhood or school where they share similar attributes with other members. Within a school, student placement across classes is also influenced by school policies as well as parental involvement. Under this circumstance, measures of peer characteristics may proxy for other unobservable factors that also affect the outcomes, such as student willingness to work, or parental ambition and resources. However, those family factors are usually unobservable to researchers. A standard approach that ignores the endogenous parental choices might erroneously attribute the entire increment in students' performance to the superior peer group.

Given the multiple mechanisms through which peer group impacts student outcomes, one would predict a strong relationship between peer qualities and student achievement.

However, these sources of endogeneity biases make it difficult to identify peer effects. As Rivkin (2001) argues, regardless of the number of included covariates, single equation methods almost certainly do not identify true peer group effects. Therefore, in order to overcome the endogeneity problems, empirical studies on peer effects have attempted to search for alternative techniques.

Previous Research

Coleman Report (Coleman et al. 1966) is one of the earliest studies on peer group effects in education. In particular, Coleman and colleagues indicate that black students performed better academically if they were in schools with higher fraction of white students. Winkler's study (1975) finds that both white and black student's scholastic achievement is positively related to peer social economic composition; and especially, transferring from a predominantly black school to a school with lower black population adversely affects the achievement of black students. Two studies in the 70s by Summers and Wolf (1977), and Henderson, Mieszkowski and Sauvageau (1978), have shown that students achieve higher if they are placed with high performing peers. However, the early studies take few steps toward addressing the endogeneity problems.

In the past two decades, a growing literature has adopted a variety of innovative techniques to circumvent the methodological challenges in estimating peer effects. Despite the differences among all the methods used in recent studies, the key to overcoming the endogeneity problem is to find exogenous sources of variation in peer composition. The following review will focus on the strengths and weaknesses of identification strategies in some selected studies.

Because this study examines peer group effects on both student academic achievement and behavioral outcomes, the literature review also includes previous peer effect studies on both outcomes.

Peer effect on student academic achievement

Relying on longitudinal panel data from Texas, Hoxby (2001) estimates substantial peer effects on student achievement by comparing the idiosyncratic variations in *adjacent* cohorts' race and gender composition within a grade within a school. The author argues that the identification strategies are credibly free of selection biases because the between-cohort peer variations are beyond the easy management of parents and schools. However, Hanushek et al (2009) examined the same data set and pointed out that the between-cohort peer composition changes actually come from frequent student transfers rather than birth or biological rate differences. Student transfers, however, are related to some unobserved family factors that also impact student achievement. If it is the transfers that cause the variations in peer characteristics, Hoxby's method can not eliminate the endogeneity of family selection. Another study by Lavy and Schlosser (2007) uses very similar strategies to Hoxby's to examine classroom level peer impacts, and find that a high proportion of female classmates improve both boys' and girls' academic performance. Both studies avoid simultaneity bias by only examining predetermined peer characteristics, such as peer race and gender.

Hanushek and colleagues (2003) also investigate school level peer effects using the same set of Texas data as Hoxby; but they implement different techniques to address the endogeneity problem. Their study eliminates the across-school sorting problems by using fixed effect (FE) methods, and circumvents the reflection problems using lagged peer achievement measures. As argued by the authors, fixed student effects account for all systematic and unobservable time-invariant individual and family factors that may influence the residential choice as well as student achievement, such as individual ability and parental motivations; fixed school effects are correlated with peer composition through school and neighborhood choices. The paper finds moderate effects of average peer achievement on student learning, but no impacts from average peer economic status or the dispersion of peer achievement. Fixed effects are also used in other school level peer effect studies by Hanushek et al. (2009), McEwan (2003) and Ammermueller and Pischke (2006).

Fixed effects are widely used in studies investigating classroom peer effects to overcome the self-sorting problems. For example, Burk and Sass (2004) measure the peer influences on mathematic achievement within specific math classrooms for middle school students in Florida. Both student and teacher fixed effects, as well as school/grade and year fixed effects are included in the regression. Based on the findings that adding teacher fixed effects purges away the peer influences, the authors argue that the apparent peer impacts found in other studies may just reflect the endogenous matching between teachers and students within a school. Other classroom peer studies using fixed effect method include Betts and Zau (2004), Vigdor and Nechyba (2004), Stiefel, Schwartz, and Zabel(2004), and Sund(2009). Using fixed effects is expected to remove the spurious correlations between the time–invariant unobservables and the peer measures. However, despite its popularity, fixed effect models are not able to overcome the endogeneity that results from time varying factors, such as the year-to-year shocks.

Some studies examine how student performance is impacted by externally induced changes in peer composition. For example, Angrist and Lang (2002) find that classroom composition changes brought by Boston METCO program only moderately impacted minority students' achievement in reading and language. The METCO program transferred and randomly placed inner city students to some suburban schools and therefore introduced plausible exogenous sources of variation in peer composition. Similarly, Imberman and Kuglar (2008) estimate how the influx of hurricane Katrina evacuee students impact the performance and behavior of non-evacuee (native) students.

Another conventional approach to deal with selection bias problem is by the implementation of instrumental variables (IV). For example, in order to address the non-random classroom assignment problem, Lefgren (2004) instruments the covariates of peers with the interaction between student's initial ability with school tracking policy. The author also uses lagged peer achievement measures to overcome simultaneity bias. This study suggests modest peer effects--- moving from a 10^{th} percentile classroom to a 90^{th} percentile classroom would only increase the achievement gains by between 0.03 and 0.05 grade equivalents.

Several other empirical studies on school peer effects have also used IV method to address the endogeneity problems caused by simultaneity and self selection. For example, Case and Katz (1992), and Gaviria and Raphael (1999) instrument the average peer behaviors using the average background characteristics of the peers to solve the simultaneity problems; Boozer and Cacciola (2001) use the fraction of students previously randomly exposed to small class treatment as the IV for the contemporaneous peer group measures; Kang (2007) examines the classroom peer effects in South Korea by implementing an IV model that uses the mean and standard deviation of peer science scores as the instruments for the variable of interest---average classmate math test scores. The study by Evans *et al.* (1992) is one of the early studies using IV method to address group endogeneity (self-selection) problems, wherein a set of metropolitan area social economic indexes are used as instruments for the peer variable ‘proportion of economically disadvantaged students at a school’. The study finds significant peer effects from the simple OLS model, but no impacts from the IV model. However, Rivkin (2001) questions the validity of instruments used in Evans *et al.* He examines the same research question using similar set of instruments but different data set. His findings suggest that using aggregated metropolitan area characteristics as instruments actually increases the magnitude of group selection biases. Another peer effect study by Fertig’s (2003) tackles the potential endogeneity arising from both selectivity and simultaneity by utilizing two sets of instrumental variables: the first set of IVs indicating school policy in selecting students upon entry and whether it is a private school; the second set of IVs including measures of parental caring behavior. Fertig’s study finds that individual student reading achievement is negatively impacted by the achievement heterogeneity in school peer composition.

A new stream of empirical literature focuses on special cases where individuals are randomly assigned across groups. Among all the methods intending to reduce selection bias, randomization is the most credible one--- it ensures that peer group formation is totally exogenous. Two frequently cited studies are conducted by Sacerdote (2001) and Zimmerman (2003), who find significant association between roommate academic attainment and individual achievement using randomly assigned roommate data at

Dartmouth University and Williams College respectively. However, due to the limited experimental data in social sciences, randomization methods are only applicable to a few special cases, such as college freshman roommates, or government assisted housing programs (e.g., in Katz, Kling, and Liebman, 2001), where a central authority conducts the group assignment.

The random assignment (RA) approach is rarely seen in research on pre-collegial peer effects. One exception is the study by Boozer and Cacciola (2001) relying on the random assigned classroom data in Tennessee STAR program to investigate the impact of average classmate achievement on student own performance. Unlike most other empirical studies, this study examines the endogenous peer effects --- effects from average contemporaneous classmate achievement. Since randomization eliminates selection bias, the authors use instrument variable methods to tackle the simultaneity problems: the fraction of classmates previously exposed to small-class treatment is formed as instrument. A possible flaw of this study is that the authors did not address issues that may affect the purity of the randomization, such as selective attrition and student mobility between class types. Two other studies using random assignment approach to examine peer effects on student outcomes focus on classrooms in other countries. (e.g., the South Korea study by Kang, 2007; and the Kenya by Duflo, Dupas, and Kremer, 2008)

Vigdor and Nechyba (2008) recently present a new method attempting to disentangle the true peer effects and the effects from selection. Based on the observed peer characteristics, they predict the probability of random assignment of students across classrooms, which then enter the model as a predictor of selection effects by interacting with the peer variables. Similar to many other studies, reflection problems are eliminated by using previous peer achievement measures. Their results suggest that a great portion of peer effects from OLS estimation actually reflect selection.

Peer effect on behavioral outcomes

Researchers in education have been interested in how peer composition impacts both individual's scholastic and non-scholastic outcomes. Due to the limitation in data access to individual behavioral outcomes, many empirical studies have to rely on survey data to examine peer influences on student conducts. For example, Evans, Oats and Schwab (1992) use National Longitudinal Survey of Youth (NLSY) data and find no significant correlation between percentage of economically disadvantaged school population and student behaviors such as teenager pregnancy and high school drop out. Argys (2008) uses the same data set and finds that female students are more likely to use substances if they are accompanied by older peers. Two other studies by Gaviria and Raphael (2001, using National Educational Longitudinal Study (NELS) data) and by Bifulco and Fletcher (2008, using National Longitudinal Study of Adolescent Health (NLSAH) data) also investigate peer effects on issues like high school drop out, church attendance, college attendance, and substance uses. Behavioral outcomes (such as alcohol use, participation in fraternities, and major choices etc) are also widely examined in peer effect studies at college level (e.g., Lyle, 2007; Kremer & Levy, 2001; and Sacerdote , 2000), wherein the outcome variables are usually derived from individual responses to research surveys.

Existing literature provides little knowledge on peer impacts on student conducts at elementary and middle school levels. The major explanation for lack of research on this issue is data limitation. Many popular approaches used in identifying peer effects, including fixed effect models (used in studies by Hanushek et.al., 2003, 2009; Betts and Zau, 2004) and idiosyncratic between-cohort peer variations (used by Hoxby, 2000; and Lavy and Schlosser, 2007), require the use of longitudinal panel data; so studies using these methods have had to rely on state or local administrative data sets, which usually just provide a small set of student outcomes, mostly limited to test scores. Therefore, most studies on school peer effects have only focused on academic outcomes. One exception is the study by David Figlio (2005), which investigates how disruptive classmates impact student achievement and behavior. The behavioral outcome in Figlio's study is represented by whether a student is suspended at least once for more than 5 days. However, the validity of the instrumental variable (proportion of male students with female names) used in this paper is questionable.

Like most empirical studies, this paper also concentrates on identifying contextual effects measured by pre-determined peer characteristics and lagged peer outcomes, which avoids the simultaneity biases arising from endogenous peer effects. The identifying strategies then focus on tackling the selection bias problems. Specifically, this study will combine two approaches used in previous studies --- instrumental variables (IV) and Randomization (RA). As mentioned before, two sets of dependent variables will be examined: student academic achievement in both math and reading, and student behavioral outcomes in discipline and attendance. Next chapter provides detailed discussion on these two methods and their implementations in this study.

CHAPTER III

INSTITUTIONAL BACKGROUND, DATA AND METHODOLOGY

The identification of peer group effects in this dissertation relies on the randomization through magnet school admission lotteries. This chapter starts with the introduction of the background of the district under study and its operation of magnet programs. It then lists all the data sources. Finally, this chapter introduces the analytical strategies and specifies the regression models to estimate peer effects at both school and classroom levels.

Institutional Background

This study focuses on peer group effects on middle school students in a mid-size Southern urban district. In the school year 2003-2004, the district serves approximately 80,000 students from kindergarten to 12th grade in 129 schools, with half of the student population eligible for the federal free or reduced price lunch program. Similar to other urban school systems in the nation, the district is racially mixed, serving 41% White students, 47% Black students, and 9% Hispanic students. About 6% students are categorized as English Language Learners (ELL). Middle schools in this district are structured from grade 5 to 8, which is one grade earlier than many other districts in the nation. During 2003-2004 school year, there are approximately 24,500 students in 52 middle schools. The demographic characteristics of middle school students are almost identical to the whole district population.

The district operates magnet schools at all three levels—elementary, middle, and high schools. There are two types of magnet programs: selective academic magnet (applicants must meet the grade/test score requirement), and non-academic magnet. At the middle school level, there is one academic magnet serving grades 5 to 8. While there is another academic magnet serving grades 7 to 12, it is not considered a middle school magnet in this study because the lottery to this school happens 2 years later than the other magnet programs starting from 5th grade. The complications caused by the second academic magnet school will be addressed later in this chapter.

Students are admitted to a magnet school through four channels: (1) lottery; (2) sibling preference; (3) geographic priority zones; (4) promotion from a feeder magnet. In practice, all students eligible for the latter 3 categories are admitted to the magnet school without going through the lottery. Since the identification strategy relies on lottery outcomes, the investigation of peer effects in this study will limit to the sample of students who participated in the admission lotteries to the magnet middle schools. Students who did not participate in magnet school lotteries are included in the calculations of peer variables, but are dropped from the regression analysis.

Middle school lotteries are held in the spring of the fourth grade for the following academic year. The district conducts separate lotteries for each magnet school. Students can enter multiple lotteries. Students who are accepted outright on lottery day must decide whether to accept any of the positions offered them --- if they accept a position in one school, they go to the bottom of the wait list for any other magnets. Those who lost the lottery on the lottery day are placed on wait lists and will be accepted off the list as positions become open.

The district offers lottery data starting from the spring of 1997, but the achievement data are available from school year 1998-99 through 2006-07. Because student prior achievement (measured by 4th grade test scores) is an essential covariate in the analyses, this study includes the 5 cohorts of students entering 5th grade between fall of 1999 and fall of 2003.

Table 3.1 lists the middle school level magnet programs for all 5 cohorts. During the years of the investigation, the number of magnet programs at middle school level has increased from 3 in year 1999 to 6 in year 2003. The fifth non-academic magnet, Central, was added in 2003, and filled most of its places in that year through geographic priorities; therefore, it is not treated as a magnet school since few observations from this school contribute to either control group or treatment group.

Table 3.1 Number of Magnet Programs at Middle School Level[1]

Lottery Year	Magnet Schools[2]	Grades Observed
1999	Academic, North, East	5,6,7,8
2000	Academic, North, East, South	5,6,7,8
2001	Academic, North, East, South	5,6,7,8
2002	Academic, North, East, South, West	5,6,7,8
2003	Academic, North, East, South, West, Central	5,6,7,8

1. The second academic magnet school is not listed in this table, but it operates since the first lottery year in the sample.
2. All school names are pseudonyms.

Because our student level data include year 1999/2000 through year 2006/07, all 5 cohorts will be followed through all middle school grades as long as they stay in the district. As shown in Table 3.2, for all five cohorts, there are total 85872 student observations in all middle schools in the district.[1] The observations of lottery participants count for 14-15% of all middle school observations for each cohort; the total number of participant observations is 12314, which makes up the analysis sample in this study.

Table 3.2 Number of Student Observations in Middle Schools by Cohorts

	Cohot1	**Cohort2**	**Cohort3**	**Cohot4**	**Cohort5**
Enrollment					
All Schools	14844	17504	17991	17406	18127
Magnet School					
Academic	516	524	571	621	596
North	425	514	408	312	282
South		555	586	589	532
East	302	225	290	326	371
West				250	353
Total	1243	1818	1855	2098	2134
Lottery Participation					
Total Participants	2087	2449	2820	2499	2459
Academic	1261	1272	1657	1318	1608
North	860	970	997	927	528
South		1226	1292	1155	903
East	793	707	1021	922	705
West				520	461
Composite Non-Academic	1246	1821	2020	1831	1530

Note: Counts only middle school students (5th to 8th graders) who were also enrolled in the district as 4th graders and had non-missing math test scores in 4th grade.

From lottery year 1999 to lottery year 2003, there were approximately 5000 applications to all middle school magnet programs, among which nearly half applied for the academic magnet. Table 3.3 describes the lottery outcomes and 5th grade enrollment patterns for each magnet school. There are two types of lottery winners defined: those

[1] Because prior achievement is desirable to be included in the calculation of peer characteristics and in the regression, the counts of student observations are limited to students who were also enrolled in the same district as 4th graders and had non-missing math scores. The actual numbers of student observations are larger for all cohorts.

admitted outright on the lottery day and those whose place on the wait list was reached by the start of the school year[23].

As shown in Table 3.3, for the 2306 lottery participants for the academic magnet, the probability of admission is less than 50 percent. Of the 1201 students not admitted by the academic school at the beginning of 5th grade, 727 (60%) either did not apply or failed to win any other magnet programs. Although 40% of the students who lost the academic magnet lottery were admitted to other non-academic magnet programs, many of them did not comply with the lottery assignment: only 285 of them chose to attend a non-academic magnet in 5th grade, compared with 826 enrolled in regular schools. About 19% (437) of lottery participants did not have test records in grade 5, of whom the majority no longer attended a school in the district.[4]

[2] Delayed winner is defined based on student original position in the wait list. If a student lost the lottery outright on the lottery day, but his number in the wait list is reached by the start of the school year, the student is defined as a delayed winner. The definition of delayed winner is based on the original wait list because the number on the list is solely determined by the lottery and not by subsequent decisions of students and parents.
[3] Accordingly, there are two lottery outcome indicators are defined: outright_win$_{Mj}$=1 if a student is an outright winner of magnet school *j*, 0 otherwise; delayed_win$_{Mj}$=1 if a student is a delayed winner of magnet school *j*, 0 otherwise.
[4] 337 academic magnet lottery participants never enrolled in the district as 5th graders. Another 100 students enrolled in district schools but were not present for testing --- the majority of these students had probably left the system prior to the test date, as 65% of them were never enrolled in 6th grade.

Table 3.3 Magnet School Lotteries and Enrollment

	Academic	North	South	East	West	Composite Non-Academic
		Non-Academic				
Lottery Participants[1]	2306	1307	1384	1260	289	2564
Winners						
Outright	884	709	492	511	50	1430[4]
Delayed[2]	221	308	374	326	46	890
Losers						
This Lottery	1201	290	518	423	193	397
All Lotteries	727	54	111	73	18	225
Grade 5 Enrollment[3]						
This Magnet	758	391	384	243	30	1048
Other Magnets	285	325	413	430	143	337
Non-Magnets	826	410	408	406	90	824
Left System/Untested						
5th Grade	437	178	172	180	26	347
6th Grade[5]	149	122	99	93	19	196

1. Lottery participants count only the students who were enrolled in the district as 4th graders and had non-missing 4th grade test scores when lottery was conducted.
2. Delayed winners in this table count only the non-outright winners who received notice before the 12th day of school year in 5th grade.
3. Counts only students tested in mathematics as 5th graders as well as 4th graders.
4. Win at least one non-academic magnet lottery
5. Students who were in the district as 5th graders but left the system (or untested) in 6th grade

There were more than 2500 entrants in one or more non-academic magnet lotteries. Numbers for the West magnet school are very low because this school did not become a magnet school until the school year 2002/03 and most of its places were actually taken by students promoted from a feeder school. As we can tell from the table, many students applied for more than one magnet programs, so the majority of the participants won the admission opportunity in at least one non-academic magnet, either outright or through delayed notices. 397 (16%) students did not win a place among all non-academic programs; but because many of the students also applied for the academic magnet, there are only 225 students who were not admitted by any magnet program, representing 8% of all non-magnet lottery participants. Regardless of the high admission rate, less than 50% of participants attended a treatment group school as 5th graders, while 846 were enrolled in non-magnet schools. About 13% students left the system or were not tested in 5th grade.

As Table 3.3 shows, there are many complications in this district's lottery based admission process: non-compliance, multiple choices, and attrition. All these complications will threaten the purity of randomization; and therefore, Ordinary Least Square (OLS) method is not able to obtain unbiased estimates of peer effects. Later discussion in this chapter will present the solutions to address the problems caused by these complications.

Data Source

Data for this study are collected from the district's administrative data system. All the datasets are student level data, including a rich set of information on individual students, such as academic achievement, demographic background, course enrollment, lottery participation, disciplinary infractions, and absence records etc.

(1) Achievement Data. Student academic achievement information comes from the state annual testing program that includes virtually all students beginning in 3rd grade and continuing through 8th grade. The tests cover five subjects: reading, mathematics, language arts, social studies, and science. The assessments adopt the Terra Nova series of achievement tests constructed and calibrated annually by CTB/McGraw-Hill, with additional items reflecting special content of the state K-12 curriculum. Achievement

data from 4th grade will serve as the prior achievement benchmark for individual students and will be aggregated at classroom and school levels to calculate academic ability measures for peers. Student test scores in grades 5 to 8 are used to form the dependent variables reflecting student academic outcomes.[5] Similar to many other empirical studies, this project focuses on achievement in math and reading.

(2) Course Data. Every year, the district provides a detailed course file, including information of course code, course id, course title, course period, and instructor name, etc. The course file reveals the specific class placement for each student in every subject, thereby identifying the classroom peers. In addition, the course file also enables me to match a student with his instructor for a specific class, which will be used to estimate teacher fixed effects to test if peer effects actually represent the impacts of teachers.

(3) Lottery Data. The district provides lottery records of all admission lotteries conducted for the magnet schools that have oversubscribed applications. The data include information such as lottery application, lottery results, and wait list number etc, which are going to be used to create variables indicating lottery participation and lottery outcomes. Although the data are available from lottery year 1997[6] through 2003, only lottery participants from lottery year 1999 to 2003 are included in the sample.

(4) Student Background Data. Another student level data file contains student background characteristics, such as student gender, race, whether a student is in special education program, whether a student is an English Language Learner, whether the

[5] The achievement data provide scale scores in all subjects. However, we received two sets of achievement data separately: one for school year 1999 to 2004 (received in 2005), and the other for school year 2005-2007 (received in early 2009). The scores in early year data and late year data are differently scaled --- the average scores in later years are lower by 130-150 points for both math and reading. In order to address the inconsistence in test scores, I transformed student test scores (from 4th grade to 8th grades) to standardized scores with a mean of 0 and a standard deviation of 1 in each grade and each year.

[6] In our sample, it means the lotteries conducted in the spring of 1997 for magnet schools starting from 5th grade in the fall of 1997.

student is eligible for federal free or reduced price lunch program, and student prior achievement. All the background variables will be included in the regression as explanatory variables to improve the model precision and deal with the possible biases arising from sample attrition; they are also going to be aggregated at classroom and school levels to construct peer characteristics.

(5) Discipline and Attendance Data. The district administrative data also provide student attendance and mobility records, as well as disciplinary consequences reflecting the frequency and severity of student misconduct incidents. Two variables are derived from the attendance and disciplinary records: annual attendance rate, and annual number of suspensions. The contemporary values (values in grades 5 to 8) of these two variables are used to form the dependent variables representing student behavioral outcomes. The lagged values (4^{th} grade) of these two behavioral variables will be introduced as explanatory variables and will be used to construct peer variables at both classroom and school levels.

All of the datasets are linked by a unique student identification number and variables are created to estimate peer group impacts on individual students in middle schools of this Southern city district. Middle schools are chosen for two major reasons. First, the achievement data from the state standard test are only available for students in grades 3-8. Because most elementary schools in the district are structured from Kindergarten to 4^{th} grade, the elementary sample is much smaller than the middle school sample. Moreover, focusing on students from 5^{th} to 8^{th} grade, I can use student's elementary school (4^{th} grade) records to establish the achievement benchmark. Second, this project is going to investigate the peer effects at both school and classroom level. In this district, the majority of middle school students (especially when students reach 7th and 8th grade) typically rotate through classrooms for different subjects. The classroom level investigation then will estimate subject-specific peer effects, which is different from most previous studies that examine general class peer effects using data from elementary schools.

Analytical Strategy

In this study, the identification strategy combines both the instrumental variable method and the method of randomization. In particular, the lottery outcomes will be exploited to construct the counterfactual peer variables and the instrumental variables. This part will first presents a review of the two econometrical methods --- instrumental variables (IV) and randomization (RA). It will then discuss the analytical models to answer the three research questions.

Overview of the methods

1. Instrumental Variables

Instrumental variables (IV) method is a frequently used estimation technique in empirical economic studies (probably second only to Ordinary Least Squares (OLS) according to Wooldridge (2001)). It was originally proposed by Reiersol (1941), and further developed by Durbin (1954) and Sargan (1958).

The motivation of using instrumental variables method comes from the fact that OLS yields consistent estimates only when the error terms are asymptotically orthogonal to the regressors. For example, in a simple linear regression model:

$$y = \beta_0 + \beta_1 x + u \qquad (3.1)$$

If $\hat{\beta}_1$ is to be consistent for β_1, the condition $COV(x,u) = 0$ must hold[7]. In other words, OLS obtains consistent estimates only if all regressors are exogenous. However, in applied econometrics, endogeneity that causes $COV(x,u) \neq 0$ arises from many sources, such as omitted variables, self-selection, measurement errors, and simultaneity etc. For example, in peer effect estimation, one regression equation can often have multiple sources of endogeneity.

The method of instrumental variables provides a general solution to the problems of endogenous regressors. Valid instrumental variables must satisfy two basic conditions. First, the instruments must be mean-independent of the error terms. For example, if we find an observable variable z to be used as instrumental variable for the endogenous variable x in equation (3.1), it must be uncorrelated with u : $COV(z,u) = 0$. In other words, the instrumental variable should not be correlated with any unobserved factors that influence the outcome. The second condition requires that the regressor x depends on z . In the simple case like equation (3.1), this means $COV(z,x) \neq 0$ --- the instrumental variable and the regressor of interest must be correlated. In summary, an instrumental variable is a variable that does not belong in the model in its own right; it must be correlated with the regressor of interest x and only contributes to the outcome y through x . When variable z satisfies the two conditions, the parameter β_1 can be identified from the following:

$$\beta_1 = COV(z,y) / COV(z,x) \qquad (3.2a)$$

[7] Another condition is E(u) = 0, but with the existence of the intercept β_0, this assumption is for free.

Given a sample of data on x, y and $z,$ it is simple to obtain the IV estimation of β_1[8]:

$$\hat{\beta}_{1IV} = \sum_{i=1}^{n}(z_i - \bar{z})(y_i - \bar{y}) \Big/ \sum_{i=1}^{n} (z_i - \bar{z})(x_i - \bar{x}) \qquad (3.2b)$$

IV methods have been used in a number of educational studies to solve the endogeneity problems. For example, Evans and Schwab (1995) use student's affiliation with Catholic Church as IVs to investigate the effectiveness of attending Catholic schools; Neal (1997) examines the same question using geographic proximity to a Catholic school as IVs; Hoxby(2000) uses the natural boundaries created by rivers as IVs for the concentration of public schools within a school district to estimate the impact of public school competition on student achievement.

Empirical studies using IV approach all face the challenges to justify the validity of the chosen instruments. A valid instrument must satisfy the aforementioned two conditions. In general, it is likely that the instruments can meet the second requirement of being correlated with the regressor of interest $COV(z,x) \neq 0$; it is often hard to prove if the instruments meet the first condition of not being correlated with the error term $COV(z,u) = 0$. For example, in Evans and Shwab's influential Catholic school paper, the authors argue for the credibility of using Catholic church affiliation as instrumental variables from two perspectives: first, being a Catholic strongly affects Catholic school attendance; second, Catholic families are very close to the national average on a variety of social-economic indicators --- therefore, the students from Catholic families do not differ from other students in any way impacting outcomes than attending Catholic

[8] In practice, instrument variable estimator is usually estimated from the two-stage least square (2SLS) models: (1) at the first stage: obtain the fitted values $\hat{x}$ (the endogenous regressor) from the regression of x on the instrument variable z and other exogenous regressors in the model; (2) at the second stage, run the OLS regression of y on $\hat{x}$ and other exogenous regressors.

schools. However, as Tyler(1994), Neal (1997) and Altonji et al. (2005) note, being Catholic may still be well correlated with some less easily measurable neighborhood or family characteristics that also influence individual outcomes.[9]

A valid instrument can be found in the context of program evaluation, such as job training program, or school choice program, wherein a social experiment is usually conducted to randomly assign individuals to be a control group or a treatment group. Randomization determined eligibility meets the two requirements of a convincing instrument: first, individuals are *randomly* selected to treatment or control groups, so the eligibility is exogenous; second, in many experiments, being treated is solely determined by the randomized assignment, therefore the eligibility is strongly correlated with the regressor of interest --- the treatment.

2. Randomization

In program evaluation or peer effect studies, the counterfactual question asked is what would have happened to individual's outcomes if he/she had been placed in another situation. For example, what would the student math performance be if he attends a choice school instead of a neighborhood school, or if he stays with a group of high math performing students instead of with low math performing students? Clearly, the sample counterparts for the missing counterfactuals are not observable in ordinary data --- the same person can not be placed in both states in the same time. Therefore, the best way to identify the causal relationship would be comparing the outcomes of individuals with nearly identical characteristics (both observed and unobserved) being assigned to

[9] In Neal's Catholic school study, the author uses the geographic proximity to Catholic schools as the instruments for Catholic school attendance. Altonji, Elder, and Taber(2005) examines the validity of three sets instruments used to address the self-selection problems in attending Catholic schools: religious affiliation, geographic proximity, and the interaction between religion and proximity. However, their findings suggest that none of the three instruments are valid in identifying the Catholic school effects.

different situations. Randomization makes this realistic. Individuals are randomly assigned to a treatment group (for example, a choice school) or to a control group (a neighborhood school); therefore, the control group can be used to estimate the average outcomes corresponding to the counterfactual state that would happen to the individuals in the treatment group had they not received the treatment.

For example, suppose there is a school choice program with one choice school (treatment group) and one regular public school (control group). Let's assume that a lottery is the only way through which a student will be enrolled in the choice school. Let p_i be an indicator variable for lottery participation. For all the lottery participants ($p_i = 1$), let d_i denote the treatment: $d_i = 1$ indicates that student i is enrolled in the choice school; $d_i = 0$, otherwise. Let r_i denote the eligibility of the treatment: $r_i = 1$ indicates that student i won the lottery and was offered the opportunity to the choice school; $r_i = 0$ indicates that student i lost the lottery.

In theory, selection bias does not arise in a randomization-induced lottery program because winners and losers are identical on average in both observable and unobservable characteristics. Therefore, for all the lottery participants $(p_i = 1)$, the causal effect of being offered the opportunity of treatment (in this case, being accepted into the choice school) can be directly estimated from a simple OLS model:

$$y_i = \delta r_i + \eta_i \qquad (3.3)$$

In equation (3.3), r_i is determined by the randomized lottery and is orthogonal to the residual η_i. The parameter δ measures the average difference between the outcome of lottery winners and lottery losers, also known as Intention to Treat effect (ITT). Although equation (3.3) yields unbiased estimation of ITT effect, many empirical studies still add a set of additional individual characteristics (denoted by X_i, including demographic characteristics and family background etc; for examples, see Cullen et al., (2006, 2007), Katz et al (2002)) into the equation to improve the model precision:

$$y_i = \delta r_i + \lambda X_i + \eta_i \qquad (3.4)$$

Because these individual characteristics also impact the outcome variable y_i, the inclusion of X_i means that the influence from these variables does not enter the error term η_i. Therefore, including the individual characteristics improves the precision of the estimates on ITT effect by reducing the residual variation.

The intention-to-treat (ITT) estimate measures the impact of being offered treatment. It is argued that ITT is of the direct interest of policy makers because it is the only policy variable under control by public officials. For example, policy makers can offer vouchers for students to attend private schools, but they can not force families who received the vouchers to actually use them. However, from the perspective of program evaluation, it is still desirable to estimate the actual impact of treatment (such as choice schools, training programs) itself, rather than just being offered the chance to get treatment. The average effect of treatment on the treated (ETT) can be measured from the equation:

$$y_i = \delta d_i + \lambda X_i + \eta_i \qquad (3.5)$$

Wherein $d_i = 1$ means that student i actually enrolled in the choice school, and parameter δ estimates ETT. In an ideal situation where student enrollment is solely determined by lottery and all participants comply with the lottery assignments, d_i equals r_i in equation (3.4) --- ITT and ETT coincide.

However, in many social experiments, participants usually do not fully comply with the lottery assignments: winners may choose not enrolling in the choice school, and losers may seek for other choices rather than the neighborhood school. In some school choice programs that offer multiple opportunities (such as the Milwaukee voucher program and the magnet programs in this study), students may participate in several lotteries at the same time. Families that win multiple lotteries have to make one choice among all the choice schools that accept their child; and lottery loser families may also choose to relocate to another neighborhood or seek for other choices such as private schools. Under all these circumstances, the differences between the winners and losers are no longer purely determined by the lottery. Rather, they may be correlated with other unobserved factors influencing student outcomes. Therefore, in equation (3.5), d_i is no longer exogenous and OLS approach can not yield an unbiased estimate of parameter δ.

This problem can be solved by using lottery assignments (r_i) as instruments for the actual school where the student finally enrolled in (d_i). Lottery assignments meet the two required conditions of valid instrumental variables: because the assignments are randomly decided, they are orthogonal to the error term; because lottery is the only way through which the participants can enroll in the choice school, the assignments are highly correlated with the enrollment variable d_i. Therefore, ETT can be estimated from a two stage-least-square (2SLS) model using IV approach:

First, obtain the fitted values $\hat{d}_i$ from the regression:

$$d_i = \pi r_i + \varphi X_i + \varepsilon_i \text{ i} \qquad \text{(1}^{\text{st}}\text{ stage model, 3.6a)}$$

Second, obtain the IV estimate of parameter δ (ETT) from the regression:

$$y_i = \delta\hat{d}_i + \lambda X_i + \eta_i \qquad \text{(2}^{\text{nd}}\text{ stage model, 3.6b)}$$

Randomization has been exploited by a number of studies to examine the impact of educational policy/reform on student achievement. For example, Cullen and colleagues (Cullen, Jacob, and Leavitt, 2003; Cullen and Jacob, 2007) estimate the ITT effects of Chicago public school choice program. Rouse (1998) and Howell and Peterson (2002) examine the impact of private school voucher plans using lottery assignments as instrument. Kruger (1999) also exploits randomized assignment as an instrumental variable to evaluate the class size reduction program in Tennessee STAR experiment.

Identification strategies in this study

As many other social experiments, the lottery-induced randomization in the understudy district also has lots of complications: first, the lottery school enrollment process is voluntary and participants do not fully comply with the lottery assignment; second, in many school choice programs, students can apply for multiple schools; third, in the years of our investigation, there are significant student attritions in both lottery winner and loser population; forth, student's responses to peer impact are hetergenous; finally, peer effect may signal some unobserved school factors, such as teacher qualities. Therefore, instead of simply comparing the outcomes of winners and losers with different peers, this study exploits the randomization through admission lotteries to form an instrument variable for the regressor of interest --- the actual peers, and estimates the causal relationship between individual outcomes and the peer groups from the instrumented (exogenous) peer variables. The model also controls for a large number of individual and school characteristics to improve the precision of the regression models and eliminate the biases from attrition. In addition, fixed teacher effect will be included to test if the peer variables signal for teacher quality.

The following sections will discuss the analytical models identifying peer group effects on student outcomes. The discussion will start with school level average peer effect estimation, followed by classroom average peer effect analysis, the estimates of effects from dispersion of peer characteristics, and the heterogeneity in peer group impacts.

1. School level analysis

I. Basic models and variables

a. Basic models

The basic idea of the identification strategy is to estimate peer effects from the differences between the actual peers of a magnet school student and his counterfactual peers in a neighborhood school he would have attended had he not been enrolled in the magnet school. Since one student can not attend both magnet school and neighborhood school simultaneously, the counterfactual peers are not observable. However, we can exploit the randomized admission lottery for the magnet school to construct the counterfactual peer values. In this district, the admission lotteries randomly assign participants to magnet schools and neighborhood schools; conditional on the attendance zone, the lottery also randomly assign participants to peers they will encounter. If all lottery participants fully comply with the lottery assignments, the peers for the lottery losers who enrolled in neighborhood schools can serve as the counterfactual peers for the magnet school students. Peer effects then can be estimated from the average differences in outcomes associated with the treatment group (magnet school) peers and control group (neighborhood school) peers.

To facilitate the explanation, let's make some assumptions first: (1) All participants are randomly assigned to treatment group (magnet school) and control group (neighborhood school), and fully comply with the lottery assignment; (2) The treatment group only includes one magnet school; (3) The indicator for lottery outcomes is binary: win or lose. The outcomes of individual student i in school j can be estimated from the model:

$$Y_{ij} = \mu(X_i) + \beta P_j + u_{ij} \qquad (3.7)$$

where X_i is a vector of observed student characteristics; P_j represents the peer characteristics in school j; u_{ij} is the error term for student *i* in school *j*[10].

Under the assumptions, student i would attend a neighborhood school (N) if he does not enroll in a magnet school (M). Therefore, the outcomes are (Y_{iM}, Y_{iN}), which can be obtained from:

$$Y_{iM} = \mu(X_i) + \beta P_M + u_{iM} \qquad (3.8a)$$

or

$$Y_{iN} = \mu(X_i) + \beta P_N + u_{iN} \qquad (3.8b)$$

Determined by the lottery, a student is assigned to either P_M (peers in the magnet school) or P_N (peers in the counterfactual neighborhood school where student i would attend if he had not been in the magnet school). Let $d_i = 1$ if student i enrolls in magnet school M (in

[10] There are two components included in the error term u_{ij} : the school effect on all students enrolled in school *j*, and the idiosyncratic error for student *i*.

treatment group); $d_i = 0$ if in neighborhood school N (in control group). Then we can combine equation (3.8a) and (3.8b) together:

$$Y_{ij} = \mu(X_i) + (u_{iM} - u_{iN})d_i + \beta[P_M d_i + P_N(1-d_i)] + u_{iN} \quad (3.9)$$

where $(u_{iM} - u_{iN})d_i$ represents the magnet school treatment effects and $\beta[P_M d_i + P_N(1-d_i)]$ represents the peer impacts on student outcomes.

Because N varies across students, the treatment indicator d_i is heterogeneous. Let's rearrange the model by defying $\delta = E(u_{iM} - u_{iN} \mid d_i = 1)$ as the mean expectation of treatment effects over the students who received treatment and $\eta_i = (u_{iM} - u_{iN}) - E(u_{iM} - u_{iN} \mid d_i = 1)$ as the heterogeneous responses to treatment:

$$Y_{ij} = \mu(X_i) + \delta d_i + \beta[P_M d_i + P_N(1-d_i)] + \eta_i d_i + u_{iN} \quad (3.10)$$

Now there are two components in the error term $\eta_i d_i$ and u_{iN}.

Although the lottery randomly assign student i to either P_M or P_N, P_N itself is a matter of parental choices, especially residential location decision. Thus, it may be related to other unobservable factors that also affect student outcome, such as family resources, and bias the estimation of the peer effect β. If we can control for the correlation of P_N and the error term u_{iN}, this source of endogenous bias can be eliminated. So let $\gamma P_N = E(u_{iN} \mid P_N)$ denote the correlation between P_N and u_{iN}, including γP_N into the equation:

$$Y_{ij} = \mu(X_i) + \delta d_i + \beta[P_M d_i + P_N(1-d_i)] + \gamma P_N + \eta_i d_i + v_{iN} \quad (3.11)$$

where $v_{iN} = u_{iN} - E(u_{iN} | P_N)$. With γP_N controlled in the model, the new error term v_{iN} is no longer correlated with the peer measures --- the coefficient β now does not pick up the endogenous bias from P_N.

Equation (3.11) includes two peer variables: the first term $[P_M d_i + P_N(1-d_i)]$ representing lottery-based variation in peers, and the second term P_N representing residence-based variation in peers. The coefficient on the former term β measures the causal relationship between peers and student outcomes. Note from equation (3.11) that the estimation of peer effect β depends on the treatment variable d_i. Under the assumptions, the value of d_i is randomly determined by the magnet school admission lottery; conditional on P_N, the lottery also randomly assigns students either to P_M or P_N. Therefore, given that P_N is included in the model, both the treatment term d_i and the peer term $[P_M d_i + P_N(1-d_i)]$ are exogenous --- OLS estimation of model (3.11) can yield unbiased estimation of peer effects[11].

However, also note that peer variable P_N represents the counterfactual non-magnet school peers with whom a student would stay if not enrolled in a magnet school. Therefore, P_N is not observable for students who attended the magnet school. The next step then is to predict the counterfactual peers P_N for magnet school students. The prediction is based on the sample of students who lost the lottery and attended regular public schools, using their information in vector X as well as their elementary schools. So a student in the magnet school get assigned an predicted value of $\hat{P}_N$, which is close to

[11] Equation (3.11) is considered as the basic model estimating school level peer effects. Other regressors included in the final regression model (not shown in the equation due to text limit) include the lottery participation indicators (including lottery year and lottery school combination), year and grade indicators.

the actual P_N of a counterpart student (a lottery loser attending a neighborhood school) who share similar background (such as demographic characteristics and elementary school) with this magnet school student[12].

b. Variables

The dependent variable Y_{ij} includes both student academic achievement as well as behavioral outcomes. Individual academic achievement as dependent variable is measured by the standardized test scores based on the distribution of original scale scores in each grade and each school year. The behavioral outcomes are measured by student's total numbers of suspension and the absence rate in each school year.

The vector X includes student demographic variables as well as previous test scores and behavioral measures: black, female, low income (measured by eligibility for the free and reduced-price lunch program), special education, ELL, 4th grade reading and mathematics test scores, and 4th grade disciplinary incidences and absence rate.

The regressors of interest cover a rich set of attributes of the peer groups. The specifications not only include the most commonly expressed peer characteristics such as race, gender, social economic status (SES)compositions and peer academic abilities, but also include less commonly examined characteristics such as percent special education student, percent ELL students, peer disciplinary infractions and attendance behaviors. The contextual (exogenous) peer characteristics, such as race, gender or SES compositions, are constructed by averaging over all individual students at each grade in every school. The endogenous peer characteristics, including both peer achievement and

[12] Detailed discussion on the prediction of P_N is provided in Appendix A, which includes the regression model that predicts P_N , and the descriptive statistics comparing the predicted value and the actual value of P_N for some lottery losers.

behavioral outcomes, are constructed as the aggregate mean of lagged values of the outcome variables (both academic achievement and behavioral records in 4th grade). As noted before, the main reason of using lagged peer outcomes is to avoid the mechanical peer correlation arising from reflection problems. Another reason of relying on peer outcomes in 4th grade is because these values are determined before the admission lotteries that assign participants to treatment and control groups. Therefore, it not only circumvents the simultaneity bias, but also reduces the possible biases from omitted variables that are correlated with both individual outcomes and the peer variables since these peer outcomes are predetermined before students enter middle schools. However, although lagged values of peer outcomes are expected to capture most of the relevant variation in current peer achievement and behavior, they are still not perfect proxies for the current values. As Hanushek et al. (2003) suggested, the estimated effects from the lagged peer behavioral variables are most likely to be downward biased given the fact that the current innovations to behavior form an important avenue through which peers affect outcomes; therefore, the estimated effect of lagged peer behaviors may just provide a lower bound estimate of current peer behaviors.

The regression models require multiple versions of peer variables for each student: P_M and P_N for school level peers. P_M will be calculated straightforwardly using students actually enrolled at M, but P_N is an estimated variable for magnet school students. In order to avoid introducing a difference between students whose P_N is observed (lottery losers) and those whose P_N is a counterfactual (lottery winners), the predicted $\hat{P}_N$'s will be used in the final models for all students.

The main treatment indicators d_i are dummy variables for enrollment in a magnet school. Students are regarded as enrolled in a magnet school if they finished the school year at the magnet. The relatively small number of students who started the year at a magnet before transferred out are treated as non-magnet students.

All models include a full set of grade by year interactions in order to control for differences in test forms across grades and year. In addition, a set of lottery participation indicators are also controlled in all models because there are separate lotteries for each magnet school, and randomization only happens to the participants in a given lottery.

II. Relax the assumptions

From now on, let's relax the provisional assumptions. First, there are some non-compliers among the lottery participants. The self selection of non-compliers means that the treatment variable d_i is no longer exogenous and neither is the peer term. Therefore, OLS methods will not yield unbiased estimates of model (3.11). The solution for this problem is to implement an instrument variable model, in which the lottery outcomes are used to construct the instrument variables. The following is the first stage models to predict the endogenous covariates (both treatment indicator and the peer variable) using the instrumental variables:

(a) Estimate magnet school treatment variable d_i from model

$$d_i = \pi r_i + \varphi X_i + \varepsilon_i \qquad (1^{st} \text{ stage IV model1})$$

where the lottery result indicator r_i serves as the external instrument variable (r_i =1 if student i won the lottery; 0 otherwise).

(b) Estimate the peer variable $P_M d_i + P_N(1-d_i)$ from model:

$$P_M d_i + P_N(1-d_i) = \pi[\overline{P}_M \hat{d}_{iM} + \hat{P}_N(1-\hat{d}_{iM})] + \varphi X_i + \varepsilon_i \qquad (1^{st} \text{ stage IV model2})$$

Where the external IV for the endogenous peer variable is $[\overline{P}_M \hat{d}_{iM} + \hat{P}_N(1-\hat{d}_{iM})]$.

The instrument variable includes three parts: $\hat{d}_{iM}$ is an prediction of the probability that student i attend the magnet school, which is estimated based on students' lottery outcome indicators and demographic variables[13]; $\overline{P}_M$ is the mean value of school level peer variables in the magnet school; and $\hat{P}_N$ is the estimated school level peer variable in the neighborhood school.

If we replace the endogenous regressors with their IV estimates, the instrumented model (second stage model) can now be expressed as:

$$Y_{ij} = \mu(X_i) + \delta\hat{d}_i + \beta[P_M \hat{d}_i + \hat{P}_N(1-\hat{d}_i)] + \gamma\hat{P}_N + \eta_i \hat{d}_i + v_{iN} \quad (2^{nd} \text{ stage model, 3.12})$$

Given the use of lottery outcomes as instruments and the inclusion of $\hat{P}_N$ in the regression, we obtain unbiased estimates of peer effects free of self-selection bias even with the lottery non-compliance.

Let's now relax the second assumption. Instead of having one magnet program and a single lottery, the school district operates multiple magnet schools, including one academic magnet program and several non-academic magnet programs. Separate lotteries are conducted for each school. Although a student can only attend one magnet school, lots of students apply for multiple lotteries, which causes three problems: (1) Because of multiple lottery applications, the chances for a student failing to obtain a place in at least one of the non-academic magnet schools are small; therefore, the counterfactual sample

[13] Appendix B provides the prediction models for $\hat{d}_{iM}$, the detailed procedures of calculating $\hat{d}_{iM}$, and the descriptive statistics.

is small and thus the power of detecting the treatment effects and peer effects in each of the non-academic magnet school is limited. (2) When the groups are small, the individual characteristics among treatment and control groups are likely to be unbalanced even with random assignment. (3) Lottery outcomes are multivalued for students who apply for several lotteries, which challenges the validity of lottery outcomes as instruments for d_i. The first two problems do not bias the estimations on δ and β, but they increase the sampling error. Combining several non-academic magnet programs into one single treatment provides the solution[14]: it increases the sample sizes of both treatment and control groups. In addition, including individual variables (X_i) in the regression also corrects the imbalances on these characteristics between treatment and control groups.

The third issue is most problematic. For lottery outcomes to be a valid instrument for the treatment variable d_i, it is required that $E[\eta_{iM} - E(\eta_{iM} \mid d_{iM} = 1) \mid d_{iM} = 1, R_i] =$ $E[\eta_{iM} - E(\eta_{iM} \mid d_{iM} = 1) \mid d_{iM} = 1] = 0$. That is, the lottery outcome variable R_i should not carry any information about the heterogeneous response term $\eta_i d_i$. However, there are two treatment groups now: one is the academic magnet, and the other is a composite non-academic magnet. Suppose that a student applied for both magnet programs, then the lottery outcome indicator is an ordered pair $R_i = (r_{i1}, r_{i2})$, with r_{i1} indicating the lottery results for the first magnet school, and r_{i2} for the second magnet school. This pair of lottery outcomes now may no longer be valid instruments because the combination of

[14] The numbers of non academic magnet programs vary across years. For each lottery year, a student is defined as a composite lottery participant if he/she applied for at least one non-academic magnet program; a student is defined as a composite magnet enrollee if he/she is enrolled in one of the non-academic programs. The peer characteristics in the composite magnet program is a weighted mean of peer characteristics in the non-academic magnet schools, and the construction of the composite magnet peer variables is explained in second part of Appendix B.

two indicators may convey some information about the heterogeneous response term $\eta_i d_i$. For example, let η_{i1} denote response heterogeneity to magnet school 1. Compare now

$E[\eta_{i1} - E(\eta_{i1} \mid d_{i1} = 1) \mid d_{i1} = 1, R_i = (1,0)]$ with $E[\eta_{i1} - E(\eta_{i1} \mid d_{i1} = 1) \mid d_{i1} = 1, R_i = (1,1)]$.

When $R_i = (1,0)$ student i is not offered the opportunity to attend school 2, but when $R_i = (1,1)$ he is. If the choice of school is based on private information about η_{i1}, the conditional expectation will be greater when student i have two options (school1 and school 2) than when school 2 is not an option. Then

$E[\eta_{i1} - E(\eta_{i1} \mid d_{i1} = 1) \mid d_{i1} = 1, R_i = (1,0)] \neq E[\eta_{i1} - E(\eta_{i1} \mid d_{i1} = 1) \mid d_{i1} = 1, R_i = (1,1)]$ and neither equals zero; therefore R_i violates the requirements of a valid instrument.

There are two circumstances under which the third problem would not exist. First, parents and students do not have any private information about η_{i1} when they make the enrollment decisions. In fact, this situation is very plausible. For example, it could happen when every family shares the same perception of the differences in school quality among the choice schools. It also could happen when parental/student's preferences for one magnet school over another are based on factors unrelated to achievement (e.g., distance from home). In these scenarios, because η_{ij} does not impact the enrollment decisions, R_i is uninformative about η_{ij}. Therefore, the IV approach of using lottery outcomes as instrument works. Second, all lottery applicant families have same preferences over magnet schools: everyone prefers A to B, and no one with a choice of both selects the latter. Then

$E[\eta_{i1} - E(\eta_{i1} \mid d_{i1} = 1) \mid d_{i1} = 1, R_i = (1,0)] = E[\eta_{i1} - E(\eta_{i1} \mid d_{i1} = 1) \mid d_{i1} = 1, R_i = (1,1)]$ — school

2 as a second option is simply irrelevant and contains no information about response heterogeneity η_{i1}. Note that the two treatment groups in current data are defined as the academic magnet and a composite of the non-academic magnets. Very few students admitted to the former choose to attend the latter --- the required condition for the second solution is very nearly met[15].

The same argument on the validity of instrument arises when we relax the third assumption of one binary indicator for lottery outcomes. Instead of a binary indicator for lottery results, there are multiple possible indicators for the outcomes of a single lottery. Let $R_i = (R_{i1}, R_{i2})$ denote lottery outcomes for students i: $R_{i1} = 1$ if student i is an outright winner (offered a place in magnet school M on the lottery day); $R_{i2} = 1$ if student i is a delayed winner (district reaches student i's position on the wait list at some point during the school year). Many students on the wait list make other plans and do not take up the position in the magnet school when notice arrives late. Students who do accept the delayed offers may expect unusual benefits from attending. If we compare $E[\eta_{i1} - E(\eta_{i1} | d_{i1} = 1) | d_{i1} = 1, R_i = (1,0)]$ with $E[\eta_{i1} - E(\eta_{i1} | d_{i1} = 1) | d_{i1} = 1, R_i = (0,1)]$ now, it is arguable that the latter conditional expectation is larger than the former one, and neither equals zero. When the lottery outcome indicator is not binary, both indicators 'outright win' and 'delayed win' are included to construct the instruments --- the validity of the instrumental variables needs to be tested using over-identification test after each regression model.

[15] For all 220 students who won both the academic magnet and the non-academic composite lotteries, 71.4% (157) enrolled in the academic magnet in 5th grade, 7.7% (17) enrolled in the non-academic composite, 15% enrolled in regular public schools in the same district, and 5.9% (13) left the system after 4th grade.

III. Complications caused by the presence of a second selective magnet school

The school district operates another academic magnet school structured from grade 7 to grade 12, which attracts a lot of 5^{th} grade (middle school) lottery participants (especially lottery losers). The admission lottery for this new selective school is conducted in the spring of the 6th grade for the following academic year. In each cohort of middle school (5^{th}-8^{th} grade magnet schools) lottery participants, about 10% students applied for the second academic magnet schools when they were in 6^{th} grade; the number ranges from 12% to 19% across cohorts for the first academic magnet middle school applicants. Lottery losers for the first selective magnet middle school are more likely to apply to this second academic magnet high school: among all 5 cohorts of lottery participants in 6^{th} grade, almost a quarter of lottery losers (in total 232 lottery losers) in 6^{th} grade applied for the second selective school; when students reached 7^{th} grade, 180 lottery losers switched to the new magnet school, which counts for 20% of lottery losers in 7^{th} grade. In comparison, many fewer winners were interested in the second school: in 6^{th} grade, only 63 winners (6% of all winners in 6^{th} grade) applied for the second academic school lottery; and only 38 (4% of the winners in 7^{th} grade) switched to the new school in 7^{th} grade.

The presence of the second selective magnet school causes some methodological challenges. To simplify the discussion, let's make two assumptions: (1) there is only one magnet school at the middle school level (from grade 5 to grade 8)--- the academic magnet middle school (M_1); (2) all students comply with the 5^{th} grade lottery assignments. The problems therefore arise when students reach later grades and have another selective magnet school (M_2) as a second option.

Problem1: For the first academic magnet middle school (M_1), there are a large number of lottery losers who applied for and then enrolled in this second academic school (M_2) in later grades, which changes the composition of the control group in higher grades: 80% of lottery losers remained in the regular neighborhood schools while the other 20% enrolled in the new academic magnet school.

This change to the control group challenges the original analytical strategy which identifies peer effects relying on the randomization through 5th grade lotteries. Let's recall the model in equation (3.5)

$$Y_{ij} = \mu(X_i) + \delta d_i + \beta[P_M d_i + P_N(1-d_i)] + \gamma P_N + \eta_i d_i + v_{iN}$$

Following the original strategy, the treatment variable d_i equals 1 for all 5th grade lottery winners (treatment group) and equals 0 for all 5th grade lottery losers (control group) even if they moved to the new school in later grades. To simplify the discussion, let's drop the peer term $\beta[P_M d_i + P_N(1-d_i)]$ and only estimate the magnet school effect d_{i} for now. The model then changes to:

$$Y_{ij} = \mu(X_i) + \delta d_i + \eta_i d_i + u_{iN} \qquad (3.13)$$

where the coefficient δ measures the treatment effects, and $\eta_i d_i + u_{iN}$ represents the error term. For the 20% lottery losers who switched to the new school, the outcome variable Y_{iN} is expected to be better than the Y_{iN} if they had remained in neighborhood schools, which improves the average Y_{iN} and in the meanwhile reduces the difference between Y_{iM} and Y_{iN} at 7th and 8th grades. When we estimate model (3.8), the smaller gap between Y_{iM} and Y_{iN} then yields a smaller coefficient on d_i.

For the full model with peer variables controlled, the estimation of peer effects β could be biased toward either direction. Note that the causal effect of peer groups is estimated from the difference between P_M and P_N. With more lottery losers enrolled in the second academic magnet school, P_N is improved for the whole control group, which decreases the gap between P_M and P_N. If the change in peer gap is smaller than the change in outcome gap, the estimate of β is biased downward; otherwise, the estimate is biased upward.

Problem 2: The challenge to IV validity emerges if the second selective school M_2 is treated as an additional magnet option in 7^{th} and 8^{th} grades. Let's also use the simple model with no peer variables for discussion now. Instead of having a single treatment variable d_i, the model now includes two treatment variables: d_1=1 if a student enrolled in the first academic middle school M_1; d_2=1 if a student enrolled in the second academic middle school M_2.

$$Y_{ij} = \mu(X_i) + \delta_1 d_{i1} + \delta_2 d_{i2} + \eta_{i1} d_{i1} + \eta_{i2} d_{i2} + u_{iN} \qquad (3.14)$$

As noted in previous discussion, self-selectivity problem arises when there are multiple choices, which challenges the validity of using lottery outcomes as instruments for the treatment variables. However, because there are few lottery winners applied for the second school, the group of students with multiple choices (students with $r_{i1} = 1$ and $r_{i2} = 1$) is very small --- the multiple-choice challenge does not pose a severe problem at this point.

However, the fact that the two options do not happen simultaneously causes more complications. The choice to participate in the second lottery (and therefore, the lottery outcomes of the second lottery) is conditional on the outcomes of the first lottery, which results in 6 different combinations of lottery outcomes in later grades:

Table 3.4 Lottery outcome combinations with the second academic school as an option

Combinations	**Descriptions**
(r_1=0, (r_2=0|p_2=0))	1st school lottery losers who did not apply for the second school
(r_1=0, (r_2=0|p_2=1))	1st school lottery losers who also lost the 2nd school lottery
(r_1=0, (r_2=1|p_2=1))	1st school lottery losers who won the 2nd school lottery
(r_1=1, (r_2=0|p_2=0))	1st school lottery winners who did not apply for the second school
(r_1=1, (r_2=0|p_2=1))	1st school lottery winners who lost the 2nd school lottery
(r_1=1, (r_2=1|p_2=1))	1st school lottery winners who won the 2nd school lottery

When the participation variable p_2 is omitted from the regression model (3.14), the estimation of δ_2 will be biased upward if p_2 carries some unobservable individual information that is positively related to the outcome variables[16]. For example, if all 5th grade lottery losers (the first three groups in the table) who then applied for the second school (the 2nd and 3rd groups) have more motivated parents, dropping p_2 from the model will compare students in the third group to both groups in the first two rows and thus enlarge the estimation of δ_2. However, with p_2 controlled in the model, the make-up of the control group for lottery M_1 is changed: instead of having two randomly-assigned groups with $r_1 = 0$ or $r_1 = 1$, there are four groups of $(r_1 = 0, p_2 = 0)$, $(r_1 = 0, p_2 = 1)$, $(r_1 = 1, p_2 = 0)$, and $(r_1 = 1, p_2 = 1)$. The two pairs $(r_1 = 0, p_2 = 0)$ and $(r_1 = 1, p_2 = 0)$ are not randomly determined; neither are the groups of $(r_1 = 0, p_2 = 1)$ and $(r_1 = 1, p_2 = 1)$.

16 As mentioned before, although not expressed in model (3.11), all regression models include lottery participation indicators and year/grade indicators.

The strategy to solve the problems caused by the second selective school is to pre-control the treatment effects from the second selective school --- that is, to make the coefficient δ_2 in equation (3.14) equal to 0. If there are no special impacts (including peer effects s and impacts from other school factors) from the second academic magnet school, it can be treated as a regular neighborhood school which students would have attended if they lose the middle school lottery; therefore, the original models can yield unbiased treatment effects (equation (3.13)) and peer effects (equation (3.11)) for the magnet middle school. Specifically, there are three steps included in this strategy:

First, estimate the treatment effect of the second academic school (δ_2) from the following model:

$$Y_{ij} = \mu(X_i) + \delta_1 d_{i1} + \delta_2 d_{i2} + \eta_{i1} d_{i1} + p_{i2} + \eta_{i2} d_{i2} + u_{iN} \quad (3.15)$$

where the peer variables are excluded in the regression. Therefore, coefficient δ_2 measures the total treatment effects from this new selective school, including both peer effects and effects from other school factors. With p_{i2} controlled in the model and lottery outcomes serving as instruments for the treatment variables, model (3.15) estimates the treatment effect δ_2 free of bias.

Second, subtract the coefficient δ_2 from Y_{ij} for each student who is enrolled in school M_2. By subtracting δ_2, students in the second selective school are assigned new values (lower values when δ_2 is positive) for the outcome variables, which represent the outcomes they would have had if they had not been in M_2 but instead attended their neighborhood schools. Therefore, these students can now be treated no differently from the situation in which they remained in their 6th grade schools --- the schools that are assigned by the middle school (5th grade) lotteries. If the peer effect is not the regressor of interest, the treatment effect for the first selective school δ_1 can then be estimated from the model (3.13):

$$Y'_{ij} = \mu(X_i) + \delta d_i + \eta_i d_i + u_{iN}$$

where Y'_{ij} is the new outcome variable (Y'_{ij} equals to the original Y_{ij} for students in schools other than M_2). The treatment indicator d_i is determined by the 5th grade lottery: $d_i = 1$ for students who won the lottery and enrolled in the first academic school M_1; $d_i = 0$ for students who lost the middle school lottery and enrolled in other schools (including the second selective magnet school which now is treated as a regular school).

Finally, add peer terms back into the model (3.13). Since the students in school M_2 are now assigned new outcome values that are comparable to the values they would have in regular neighborhood schools, the peer values for this school should be changed in the same way too. Students in M_2 are assigned new peer variables that are predicted from a model based on their 6th grade peers, with all demographic characteristics, and 6th grade academic/behavioral variables included as covariates. After all three steps, the peer effects can be estimated from the original model (3.11):

$$Y_{ij} = \mu(X_i) + \delta d_i + \beta[P_M d_i + P_N(1-d_i)] + \gamma P_N + \eta_i d_i + v_{iN}$$

where the variables d_i, P_M, and P_N are determined by the first school (M_1) lottery.

In addition, I'll also implement another strategy to address the problems caused by the new academic magnet school. The second strategy avoids the existence of the new school by limiting the peer effect investigation only to the lottery participants in the first two grades (Grades 5 and 6), for whom the 5th grade lottery ensures the exogeneity in both treatment variable d_i and peer variable $[P_M d_i + P_N(1-d_i)]$.

IV. Other Complications

There is another potential source of bias in estimating peer effects. Note that in equation (3.11), the model identifies peer effect through the interaction of $(P_M - \hat{P}_N)$ and d_i, which means that it might be confounded with unobserved response heterogeneity (η_i) which is also interacted with d_i. For example, suppose that magnet schools have higher academic expectations and more rigorous curricula. It is possible that students from a neighborhood with high proportion of low SES families (indicated by $\hat{P}_N$) are less ready to take advantage of the magnet program (they have a lower value of η_i) than those from more affluent neighborhoods. If so, $(P_M - \hat{P}_N)d_i$ then serves as a proxy for $\eta_i d_i$, which biases the estimate of β. In order to test this possibility, I will include more controls for observable heterogeneity in treatment responses in the basic model (3.11), such as the interactions between observable individual characteristics (X_i) and the treatment variable d_i, to see if this reduces the correlation between $(P_M - \hat{P}_N)d_i$ and the unobservable heterogeneity response $\eta_i d_i$.

Second, as discussed before, peer effects may also proxy for the omitted school factors that influence student achievement, such as the quality of teachers. For example, if the district tends to assign less qualified teachers to schools with a high proportion of poor students, peer effects then may signal teacher quality if it is omitted from the model. Since many studies on teacher quality finds that the conventional measures on teacher quality (such as experience and certification) explain very little about student achievement, teacher fixed effect will be added to the basic model (3.11) to check for the omitted variable bias problem.

Finally, studies exploiting social experiments are facing a common problem --- attrition. In current data, it is very plausible that lottery losers are more likely to seek other out-of-district options. The model removes the source of greatest potential bias by including student covariates of prior achievement and behavioral measures in the regression. Whether the estimation of peer group effects is impacted by attrition will be examined in later analyses.

2. Classroom Level Analysis

In addition to measure the peer effects at school level, this study is also going to estimate the classroom level peer effects. Estimating peer effects at classroom level poses more econometric challenges. First, students in the same classroom are subject to some common shocks, such as the quality of the instructor, or additional resources allocated to a particular classroom. Omitting or mismeasuring these factors in the model may bias the estimation of peer effects if they are correlated with peer characteristics. Second, across classroom placement is often not random. A large amount of education literature has shown that on the whole U.S. schools, students are assigned to classrooms at least partially based on their previous achievement (Oaks, 1990; Argys, Rees & Brewer, 1996; Betts & Shkolnik, 2000). Some parents may even exert influence over the placement of their child to a particular classroom if they believe certain teacher is highly qualified. Principals may choose class composition based on various considerations, such as racial diversity, gender balance, or maximizing the school wide accountability measures of student achievement etc.

In this study, the identification of classroom level peer effects follows the same strategy used in estimating school level peer effects --- exploiting randomization through admission lotteries. Note the facts that randomization only happens at school level --- lotteries randomly assign students into a magnet school and a regular school. The classroom placement within each school, however, it is not random as we found in the data. Then, how does the identification strategy overcome the endogenous sorting problem at the classroom level?

Here is the basic model estimating classroom level peer variables:

$$Y_{cij} = \mu(X_i) + \beta P_{cij} + u_{cij} \qquad (3.16)$$

where the subscript c means classroom c in school j. P_{cij} is calculated from the math or reading classes that student i attends; if a student has multiple classes in the same subject, the peer variable is a weighted average of the class peer attributes (class size used as weight). A student's own value is excluded in the calculation of classroom peer variables.

Same as the strategies for school level analyses, the identification of classroom peer effects also relies on the sample of magnet school lottery participants; therefore, the model needs to control for both the treatment indicator d_i and the neighborhood school peer variables P_N:

$$Y_{cij} = \mu(X_i) + \delta d_i + \beta P_{cij} + \gamma P_N + \eta_i d_i + v_{cij} \qquad (3.17)$$

where the coefficient β measures the causal effect from classroom peers.

Model (3.17) includes two endogenous regressors --- the treatment variable d_i and the peer variable P_{cij}. The endogenous treatment indicator d_i is instrumented by lottery outcomes following the IV models used for school level analyses. The biggest challenge then is to find exogenous sources of variation in classroom peer characteristics as the external IV for the endogenous class room peer variable P_{cij}. The following is the first stage IV model for the classroom peer variable P_{cij}:

$$P_{cij} = \pi[\hat{P}_{cM}\hat{d}_{iM} + \hat{P}_{cN}(1-\hat{d}_{iM})] + \varphi X_i + \varepsilon_i \qquad \text{(1}^{\text{st}}\text{ stage IV model, 3.18)}$$

where the external instrument variable $\hat{P}_{cM}\hat{d}_{iM} + \hat{P}_{cN}(1-\hat{d}_{iM})$ includes three components: $\hat{d}_{iM}$ is an prediction of the probability that student i attend the magnet school, which is estimated based on students' lottery outcome indicators and demographic variables; $\hat{P}_{cM}$ is the predicted classroom peer variable in magnet schools, which serves as the counterfactual class peers that lottery losers would encounter in classes if they had won the magnet school lottery; $\hat{P}_{cN}$ is the predicted classroom peer variable in neighborhood schools, which serves as the counterfactual class peers that lottery winners would encounter in classes if they had lost the magnet school lottery and enrolled in a neighborhood school. Every lottery applicant is assigned both values of $\hat{P}_{cM}$ and $\hat{P}_{cN}$, as well as the predicted enrollment probability --- all three predicted variables are used to construct the external instrumental variable $\hat{P}_{cM}\hat{d}_{iM} + \hat{P}_{cN}(1-\hat{d}_{iM})$. As the literature suggests, within each school, students are likely to be grouped based on their academic abilities as well as behavior problems. Therefore, the predictions of counterfactual class peers (both $\hat{P}_{cM}$ and $\hat{P}_{cN}$) utilize all individual information of the counterpart students, including prior test scores, prior behavioral outcome measures, and demographic characteristics. Also, the prediction of class peers in non-magnet schools ($\hat{P}_{cN}$) is conducted within each elementary school, so that there are more heterogeneous class assignments for students from the same elementary school. The prediction of $\hat{P}_{cM}$ is not conducted within each elementary school; instead, the elementary school indicator is included as an independent variable as other individual covariates in order to capture some unobservable factors that may influence student outcomes as well as their class placement[17].

There are two crucial questions querying the credibility of using the same school-level analysis strategy to estimate classroom peer effects. The first one asks whether the instrument for the classroom peer variables is valid. As introduced in previous part, for the instrumental variable $\hat{P}_{cM}\hat{d}_{iM} + \hat{P}_{cN}(1-\hat{d}_{iM})$ to be valid, it has to meet two requirements: it must not be correlated with the error term and it should be correlated with the endogenous peer variable. First, because the prediction of the three components ($\hat{P}_{cM}$, $\hat{P}_{cN}$, and $\hat{d}_{iM}$) in the instrumental variables is based on the lottery outcomes and controls for all observable individual covariates (including prior test scores and behavioral outcomes), the instruments are exogenous. Second, because the instrumental variable is constructed as an interaction between the predicted class peer value in either treatment or control group with the enrollment probabilities, it is highly correlated with the endogenous variable P_{cij} .

Table 3.5 provides some descriptive results of the classroom peer characteristics by different categories for the three enrollment groups: the academic magnet middle school, the composite non-academic middle schools, and non-magnet middle schools.

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[17] The procedures of constructing $\hat{P}_{cM}$, $\hat{P}_{cN}$, and classroom level instruments are included in Appendix C.

Table 3.5 Classroom Peer Characteristics (Math Class)

	Black		Low SES		G4 Math		G4 Read		G4 Suspension		G4 Absence	
	G5	G8	G5	G8	G5	G8	G5	G8	G5	G8	G5	G8
Math Class Peers for Academic Magnet Enrollees												
	.204	.194	.119	.104	1.16 1	1.150	1.235	1.257	.026	.011	.028	0.028
Actual	(.063)	(.116)	(.062)	(.074)	(0.147)	(0.294)	(0.153)	(0.211)	(.036)	(.022)	(.006)	(.007)
	.205	.172	.111	.093	1.160	1.152	1.234	1.257	.026	.011	.028	.028
Academic	(.065)	(.116)	(.056)	(.097)	(0.114)	(0.220)	(0.112)	(0.156)	(.025)	(.015)	(.004)	(.007)
	.610	.512	.392	.342	0.500	0.619	0.534	0.619	.041	.083	.030	.029
Non-academic	(.107)	(.177)	(.094)	(.111)	(0.360)	(0.497)	(0.301)	(0.482)	(.103)	(.103)	(.006)	(.006)
	.387	.305	.381	.305	0.321	0.674	0.335	0.641	.094	.060	.035	.034
Counterfactual	(.193)	(.254)	(.195)	(.213)	(0.359)	(0.414)	(0.372)	(0.442)	(.142)	(.027)	(.007)	(.008)
	.244	.214	.167	.153	0.995	1.01	1.060	1.086	.040	.027	.029	.029
Instrument	(.067)	(.123)	(.072)	(.110)	(0.146)	(0.228)	(0.179)	(0.220)	(.036)	(.049)	(.004)	(.004)
Math Class Peers for Non- Academic Magnet Enrollees												
	.621	.578	.378	.336	0.389	0.528	0.423	0.563	.090	.079	.029	.027
Actual	(.159)	(.272)	(.168)	(.205)	(0.426)	(0.546)	(0.398)	(0.536)	(.115)	(.096)	(.009)	(.008)
	.212	.198	.115	.117	1.150	1.150	1.227	1.278	.032	.009	.028	.028
Academic	(.073)	(.175)	(.073)	(.123)	(0.130)	(0.230)	(0.122)	(0.171)	(.032)	(.021)	(.005)	(.005)
	.610	.613	.394	.397	0.387	0.376	0.438	0.404	.069	.095	.030	.027
Non-academic	(.100)	(.170)	(.115)	(.124)	(0.302)	(0.422)	(0.260)	(0.427)	(.087)	(.089)	(.006)	(.006)
	.519	.507	.500	.488	0.103	0.291	0.102	0.243	.139	.128	.036	.035
Counterfactual	(.214)	(.278)	(.215)	(.233)	(0.387)	(0.458)	(0.425)	(0.449)	(.166)	(.180)	(.008)	(.008)
	.564	.549	.431	.436	0.285	0.348	0.314	0.338	.096	.109	.032	.031
Instrument	(.119)	(.195)	(.122)	(.160)	(0.290)	(0.404)	(0.281)	(0.401)	(.094)	(.118)	(.005)	(.006)
Math Class Peers for Non Magnet Enrollees												
	.433	.431	.435	.451	0.193	0.421	0.209	0.384	.126	.112	.036	.035
Actual	(.225)	(.274)	(.228)	(.245)	(0.457)	(0.565)	(0.462)	(0.550)	(.191)	(.225)	(.010)	(.011)
	.215	.228	.121	.125	1.154	1.127	1.233	1.267	.028	.011	.028	.028
Academic	(.071)	(.186)	(.079)	(.172)	(0.133)	(0.246)	(0.127)	(0.207)	(.028)	(.017)	(.005)	(.007)
	.607	.626	.406	.422	0.385	0.334	0.428	0.358	.063	.099	.030	.027
Non-academic	(.098)	(.172)	(.116)	(.134)	(0.401)	(0.455)	(0.313)	(0.464)	(.096)	(.092)	(.007)	(.013)
	.430	.438	.433	.471	0.207	0.377	0.209	0.350	.119	.121	.035	.035
Non-Magnet	(.214)	(.278)	(.218)	(.245)	(0.401)	(0.528)	(0.441)	(0.523)	(.155)	(.194)	(.014)	(.014)
	.450	.464	.395	.416	0.346	0.459	0.362	0.461	.094	.103	.034	.032
Instrument	(.186)	(.240)	(.179)	(.203)	(0.390)	(0.488)	(0.402)	(0.492)	(.114)	(.137)	(.007)	(.009)

As we see from this table, for all three groups, the instrumental variable values are very close to the values of the actual peer variables, which suggests that the instrumental variables meet the validity requirement of being correlated with the endogenous regressors. Table 3.5 also reveals another significant patterns if we compare the standard deviations between 5th grade class peer variables with 8th grade class peer variables: the between-class variance in peer composition is bigger in later grades, which provides some evidences that there is more tracking based on ability when students reach higher grades.

The second question asks whether there is enough within-school variance in classroom peer variables. If students in the same school have very similar class peers, there is no need to examine the peer group effects at classroom level, as they will be virtually the same as the school level analysis. In order to answer this question, I examine the variance of 11classroom level peer variables. As Table 3.6 suggests, there is significant within-school variance in all variables. For some variables like peer prior math scores or reading scores, there is greater within-school variance than between-school variance.

Table 3.6 Variance in Class Peer Variables

	Black	Low SES	Female	G4 Math	G4 Read	G4 Suspension	G4 Absence	G4 Math (St.d)	G4 Read (St.d)	G4 Suspension (St.d)	G4 Absence (St.d)
Math Class											
Between-school	0.05	0.05	0.01	0.09	0.12	0.03	0.00	0.01	0.01	0.10	0.00
Within-School	0.02	0.02	0.01	0.15	0.13	0.02	0.00	0.03	0.03	0.09	0.00
Fraction due to Within-school	0.27	0.26	0.50	0.61	0.53	0.34	0.50	0.70	0.83	0.47	0.77
Reading Class											
Between-school	0.05	0.05	0.01	0.09	0.12	0.03	0.00	0.01	0.01	0.10	0.00
Within-School	0.02	0.02	0.01	0.10	0.11	0.01	0.00	0.03	0.03	0.08	0.00
Fraction due to Within-school	0.24	0.23	0.47	0.52	0.49	0.30	0.43	0.66	0.78	0.44	0.74

After predicting the endogenous treatment variable and peer variable from the first stage instrumental models, the next step is to estimate the causal effects of classroom peers form the instrumented model (2^{nd} stage model):

$$Y_{cij} = \mu(X_i) + \delta\hat{d}_i + \beta\hat{P}_{cij} + \gamma\hat{P}_N + \eta_i\hat{d}_i + v_{cij} \qquad (3.18)$$

where $\hat{d}_i$ and $\hat{P}_{cij}$ are instrumented treatment indicator and peer variables, and $\hat{P}_N$ is an predicted neighborhood school peer variable that is included to remove residential location selectivity. Model (3.18) eliminates the endogeneity in classroom peer composition by exploiting randomly determined lottery outcomes to construct instruments.

The estimation of classroom level peer effects deals with all other complications following the steps in the school level investigation. One thing should be emphasized is that classroom level peer effect estimation faces more severe omitted variable biases. For example, if the majority of students in a classroom are above the average academic level (measured by prior achievement) and all high quality students are assigned to the best teachers, then failure to account for teacher characteristics will falsely attribute the teacher's contribution to peer influences, which will bias the peer effect estimation upwardly. Fortunately, I can use the course data to match each student to the class instructor for both math and reading courses, and therefore to control for teacher fixed effect T_c in the model:

$$Y_{cij} = \mu(X_i) + \delta d_i + \beta P_{cij} + \gamma P_N + \eta_i d_i + \lambda T_c + v_{cij} \qquad (3.18)$$

With T_c included in the model, teacher's influence on student achievement no longer enters through the error term, which reduces the omitted variable bias.

3. Peer Effects on Non-Academic Outcomes

In this study, the district administrative data sets provide student attendance records and discipline records, which enables me to derive two non-academic outcome variables:

the first one is attendance, measured by student absence rate (absent days/ total enrollment days) in each school year; the second one is disciplinary infraction, presented by the year-total times of suspension that a student was given as consequences to his/her misconduct at school.

The two behavioral outcome variables, attendance and disciplinary infractions, are important indicators for student participation and engagement in schools (Rowley, 2005). They are also significant factors associated with student educational attainment. Research has found that students with higher attendance rate are more likely to have better academic achievement (Anderson, Christenson, Sinclair, and Lehr, 2004; and Lamdin, 1995) and less likely to drop out from school (Alexander, Entwisle, & Horsey, 1997; Rumberger, 1995). Similarly, students with frequent misconduct have been found significantly associated with low academic achievement (Finn & Rock, 1997; Wentzel, Weinberger, Ford, & Feldman, 1990; Wentzel, 1993) and high drop-out rate (Rumberger, 1995).

The estimation of peer effects on behavioral outcomes at both school and classroom levels will follow the same strategies described in above sections; but the dependent variable in the two models (3.13) and (3.17) are now replaced by the attendance or delinquency outcomes for student i.

4. The Impact of Peer Composition Heterogeneity

In addition to examining the influences from average peer group qualities on student outcomes, this study also intends to investigate the impacts from the dispersion of peer characteristics. For example, do students benefit more from an ability-tracking class or from a homogeneous class?

Previous studies have mixed conclusions on the influences of heterogeneity in peer composition. In the Texas study, Hanushek and colleagues (2003) find no impacts from the dispersion in school level peer achievement. Fertig (2003), however, provides some evidences that heterogeneous peer academic ability composition at school level detriment individual student reading achievement. Kang's study (2007) on Korean schools reveals that weak students are negatively impacted by the dispersion of peer qualities within classroom, while strong students are positively impacted by the dispersion.

This project will examine the relationship between the dispersion of peer composition and student outcomes at both school and classroom level. The dispersion of peer composition is measured by the standard deviation in four major peer variables: prior math scores, prior reading scores, prior absence rate, and prior disciplinary infractions. The analytical strategies are very similar to those used for estimating average peer effects except that the regression model includes both the mean peer variable and the standard deviation of the peer variable**:**

$$Y_{ij} = \mu(X_i) + \delta d_i + \beta[P_M - P_N]d_i + \tau[st.d(P_M)d_i + st.d(P_N)(1-d_i)] + (\gamma+\beta)P_N + \eta_i d_i + v_{iN} \quad (3.19)$$

$$Y_{cij} = \mu(X_i) + \delta d_i + \beta P_{cij} + \tau st.d(P_{cij}) + \gamma P_N + \eta_i d_i + v_{iN} \quad (3.20)$$

5. Heterogeneous Peer Effects

Although the existence and magnitude of peer effects would be significant findings, another important objective of this project is to examine whether peer qualities impact a particular student group more significantly than other groups depending on student's own characteristics such as gender, race, or initial position in achievement distribution. That is to answer the second research question *"to whom do peer effects matter the most?"*.

Previous studies have revealed some evidences of nonlinearity in the correlations between peer group compositions and student achievement. For example, Hoxby (2000) and Hanushek et al (2009) find stronger intra racial group effects for certain minority groups; Zimmerman (2000) suggests that students at top academic ability level are less likely to be impacted by peer abilities; and Kang (2007) shows that while weak students are negatively influenced by the dispersion of classroom peer qualities, strong students benefit from the heterogeneity.

As these studies suggest, students are not always impacted by the peer qualities uniformly: the magnitude of peer effects are associated with some of student own characteristics. To examine this possibility, I'll implement new models by interacting peer variables with individual student characteristics for both school and classroom level peer variables:

$$Y_{ij} = \mu(X_i) + \delta d_i + \beta[P_M - P_N]d_i + \tau[P_M d_i + P_N(1-d_i)] * X_i + (\gamma + \beta)P_N + \eta_i d_i + v_{iN} \quad (3.21)$$

$$Y_{cij} = \mu(X_i) + \delta d_i + \beta P_{cij} + \tau P_{cij} * X_i + \gamma P_N + \eta_i d_i + v_{iN} \quad (3.22)$$

Given the example of equation (3.21) with the dependent variables Y_{ij} representing a student's mathematic achievement, a positive coefficient on the interaction of average peer academic achievement with low SES (X_i) indicates that being placed in a school with better performing peers, students from low social economic families gain more in math test scores than other students. Or if Y_{ij} indicates a student's behavioral problem, a positive interaction of percent Black peers with Black (X_i) suggests that black students are more likely to have disciplinary problems if they go to a school with a high proportion of black students.

CHAPTER IV

PEER EFFECTS ON ACADEMIC ACHIEVEMENT ---RESULTS FROM SCHOOL LEVEL ANALYSIS

This study is part of a big project investigating magnet school impacts and peer effects on student achievement. While the first part study on magnet school effect aims to answer the question whether students in magnet schools perform better than their counterparties in neighborhood schools, this dissertation intends to find out if peer effects play a significant role influencing student outcomes.

This chapter reports the findings of school level peer group impacts on student academic achievement. The first section presents descriptive statistics, including lottery participant characteristics, school level peer characteristics, and student achievement in math and reading. The second section reports the regression results on magnet school treatment effects, wherein the regressor of interest is only the treatment indicator d_i and no peer variables are included. The purpose of presenting the findings from the treatment effect models is to provide a baseline for further peer effect investigation, which examines to what extent the magnet effects are attributable to its peer composition. The subsequent sections report and discuss the findings as they are related to the following questions:

- How do average school level peer characteristics impact student academic achievement in math and reading?
- How does the variance of school peer characteristics influence student achievement?

- Which group of students is more significantly impacted by school peer composition?

Descriptive Results

Table 4.1 presents descriptive statistics of the demographic characteristics and prior outcomes of lottery participants in 5th grade. There are ten variables reported in this table, all of which are used to construct peer variables at both school and classroom levels and are also included in final regression models to control for individual background. As shown in this table, compared to the academic magnet, non-academic magnet schools drew more applicants who are blacks and from low income families.[18] There are very few special education or ELL students applying for the magnet programs. Moreover, students who applied for the non-academic magnet schools also have lower test scores and higher disciplinary incidences in 4th grade. There is no significant difference on prior absence rate between the two participant groups.

Enrollees in the academic magnet school are similar to the applicant population in most background measures. However, all non-academic magnet schools have a higher percentage of black students among enrollees than among participants, which may signal the perceptions of the quality of neighborhood schools, leading fewer blacks to turn down places in a magnet school. Among the non-academic magnet applicants, black and low-income students are more likely to lose all lotteries they entered. Given that the possibility of losing all lotteries are strongly related to the number of lotteries entered, the

[18] A student is identified as low income or low social economic status (SES) based on the eligibility to Federal Free and Reduced Lunch program.

higher percentage of black/low-income students losing all lotteries reflects more about the application behavior than the fairness of the lotteries.[19]

Table 4.2 presents school peer characteristics for lottery participants in 5th grade. School/grade level peer variables are calculated by averaging over all students in each grade in the schools attended. For students who switched schools during the school year, the peer variables are weighted averages reflecting the proportion of school days spent in each school. A student is defined as a magnet school enrollee if his/her end-of-year (EOY) school is recorded as a magnet school.[20]

For all lottery participants, the peer characteristics for the enrollees are of differences from the general applicants. Students who attend the academic magnet school have more favorable peers than the applicants over almost all dimensions. However, Compared to the general participants in non-academic magnets, enrollees in these schools are more likely to have black and low SES peers and peers with lower prior test scores. All magnet school enrollees have lower percentage of peers identified as special education or ELL students. Because the enrollees for each magnet program attend the same school, the standard deviations of all peer variables are much lower. Since the sample included in Table 4.2 is pooled across all five cohorts of 5th graders, the non-zero standard deviation reflects the variation from one cohort to the next. The big difference between peer characteristics for enrollees and lottery losers suggests that the qualities of one student's peers are very much affected by the lottery outcomes.

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[19] Among all the entrants to the non-academic magnet lotteries, 59% Blacks applied for at least 2 schools, while 75% non-black students applied for 2 schools or more. Similarly, 59% low income students and 71 non low income students applied for multiple lotteries.

[20] A student may attend a non magnet school at the beginning of the school year, but receive the notice during the school year and switched to the magnet school then.

Table 4.1 Lottery Participant Characteristics

	Black	Low Income	Special Education	ELL	Female	Hispanic	G4 Math		G4 Reading		G4 Suspension		G4 Absence	
	Pct	Pct	Pct	Pct	Pct	Pct	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Academic														
Academic														
Participants[1]	20.6	14.8	1.1	0.9	53.1	1.1	1.13	0.68	1.22	0.68	0.03	0.20	0.03	0.03
Losing Participants[2]	22.1	15.5	1.0	1.0	49.9	1.1	1.11	0.68	1.20	0.68	0.02	0.19	0.03	0.03
Losing Participants[3]	17.7	13.6	1.2	1	49.9	1.2	1.13	0.68	1.20	0.70	0.02	0.19	0.03	0.03
Enrolling Participants[4]	20.6	12.0	0.1	0.1	53.8	1.3	1.15	0.67	1.23	0.66	0.03	0.22	0.03	0.03
Non-Academic														
North														
Participants[1]	51.3	34	4.3	0.4	60.2	1.5	0.43	0.83	0.53	0.85	0.08	0.39	0.03	0.03
Losing Participants[2]	54.5	35.2	1.4	0	63.8	1.4	0.53	0.83	0.59	0.81	0.08	0.37	0.03	0.03
Losing Participants[3]	72.2	53.7	3.7	0	62.9	0	0.09	0.90	0.21	0.72	0.19	0.62	0.04	0.04
Enrolling Participants[4]	65.2	36.1	4.3	0.3	62.4	1.8	0.23	0.76	0.37	0.80	0.09	0.47	0.03	0.03
South														
Participants[1]	42.6	24.9	2.5	1.7	46.4	1	0.75	0.83	0.76	0.82	0.07	0.38	0.03	0.03
Losing Participants[2]	42.1	26.3	3.1	1.4	47.7	1.2	0.77	0.84	0.78	0.80	0.06	0.33	0.03	0.03
Losing Participants[3]	47.7	27	4.5	2.7	40.5	1.8	0.73	0.79	0.76	0.71	0.07	0.37	0.03	0.03
Enrolling Participants[4]	49.2	21.4	1.6	3.1	44.8	1	0.69	0.79	0.66	0.75	0.05	0.35	0.02	0.03
East														
Participants[1]	43.6	30.7	2.6	1	54.7	1.1	0.63	0.83	0.74	0.85	0.05	0.32	0.03	0.03
Losing Participants[2]	43.5	27.2	3.3	1.2	53.4	1.4	0.65	0.85	0.79	0.81	0.06	0.34	0.03	0.03
Losing Participants[3]	52.1	32.9	1.4	1.4	52.1	1.4	0.54	0.95	0.77	0.83	0.04	0.26	0.02	0.02
Enrolling Participants[4]	50.6	32.1	2.1	0.8	53.5	0.4	0.62	0.73	0.72	0.78	0.02	0.13	0.03	0.03
West														
Participants[1]	63	36	5.9	1	51.9	1.4	0.36	0.80	0.49	0.86	0.07	0.33	0.03	0.03
Losing Participants[2]	59.6	36.3	5.2	1.6	48.2	1.5	0.44	0.80	0.54	0.91	0.10	0.39	0.03	0.03
Losing Participants[3]	77.8	61.1	0	0	44.4	0	0.35	0.55	0.39	0.63	0.22	0.55	0.03	0.03
Enrolling Participants[4]	93.3	40	6.6	0	60	0	-0.08	0.69	0.05	0.64	0.00	0.00	0.03	0.02

Table 4.1 Continued

Composite Non-Academic														
Participants[1]	45.4	29.4	3.2	1.2	52.5	1.2	0.60	0.84	0.67	0.84	0.06	0.37	0.03	0.03
Losing Participants[2]	40.8	27.2	2.5	2	47.6	0.5	0.79	0.89	0.88	0.78	0.07	0.34	0.03	0.03
Losing Participants[3]	56.4	36.4	2.7	1.8	45.8	0.9	0.48	0.87	0.62	0.77	0.10	0.43	0.03	0.03
Enrolling Participants[4]	56.8	29.9	2.9	1.4	53.8	1.1	0.48	0.79	0.55	0.79	0.06	0.36	0.03	0.03

1.Counted as 4th graders who participated the magnet school lottery and had non-missing test scores in 4th grade.

2. Students who lost this lottery (neither outright winner nor delayed winner by the start of 5th grade).

3. Students who lost all lotteries they entered (neither an outright winner nor a delayed winner by the start of 5th grade).

4.Counted only 5th graders who enrolled in the magnet school and have non-missing test scores in 4th and 5th grades.

Table 4.2 School Peer Characteristics in Grade 5 [1]

	Black		Low Income		Special Education		ELL		Female		Hispanic		G4 Math		G4 Reading		G4Suspension		G4 Absence	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Academic	pct		pct		pct		pct		pct		pct									
Academic																				
Participants	35.8	18.5	30.1	19.3	6.7	6.8	2.1	3.3	51.3	4.7	2.7	3.9	0.56	0.54	0.61	0.56	0.09	0.10	0.03	0.01
Enrollees	20.5	3.7	12.0	2.9	0.5	0.6	0.1	0.2	54.2	2.0	1.3	0.7	1.16	0.08	1.23	0.06	0.03	0.01	0.03	0.00
Losers[2]	41.5	16.2	43.1	17.8	13.1	4.8	4.2	4.2	48.4	4.5	4.7	5.3	0.08	0.26	0.11	0.31	0.14	0.13	0.04	0.01
Non-Academic																				
North																				
Participants	56.3	21	44.3	17.4	8.7	5.9	1.9	3	52.8	5.5	2.5	3.7	0.20	0.42	0.45	0.43	0.14	0.10	0.03	0.01
Enrollees	73.1	5.4	51.2	7.4	8.9	4.5	0.3	0.2	57.7	4	1.3	1	-0.02	0.11	0.08	0.11	0.18	0.04	0.04	0.00
Losers[2]	59.8	20.9	50.3	19.1	10.7	4.6	3.7	4.9	49.8	4	3.5	4.8	0.05	0.36	0.08	0.40	0.15	0.10	0.04	0.01
South																				
Participants	49.9	20.6	39.1	17.3	7.2	5.9	2.3	2.9	50.6	4.5	2.3	2.9	0.37	0.45	0.40	0.47	0.12	0.10	0.03	0.01
Enrollees	61.4	1.6	44	7.3	6.2	4.1	2.8	1.5	48.6	2.3	1.2	0.4	0.32	0.16	0.29	0.18	0.13	0.08	0.03	0.00
Losers[2]	51.4	21.9	43.3	17.9	11.7	5.5	3.7	3.7	48.8	3.5	3.4	4.6	0.10	0.32	0.12	0.36	0.15	0.09	0.04	0.01
East																				
Participants	50.4	20.2	40.1	18.8	7.2	6.5	2	2.9	51.6	4.8	2.3	3.5	0.36	0.46	0.40	0.48	0.11	0.11	0.03	0.01
Enrollees	52.9	6.4	33.3	5	1.7	1.6	0.9	1	53.2	3.6	0.7	0.6	0.57	0.10	0.65	0.09	0.04	0.01	0.03	0.01
Losers[2]	59.2	21.5	53.2	20.2	12.9	5.9	2.4	2.7	50.2	4.3	2.8	2.8	-0.00	0.31	0.01	0.33	0.18	0.14	0.04	0.01
West																				
Participants	59.4	22.5	45.6	16.6	7.8	5.5	1.8	3	51.1	5.1	2.7	3.9	0.27	0.41	0.30	0.45	0.10	0.08	0.03	0.01
Enrollees	95.2	0	54.2	0	9.5	0	0	0	55	0	1.7	0	-0.05	0.00	-0.05	0.00	0.11	0.00	0.03	0.00
Losers[2]	61.3	20.7	61.3	18.6	12.3	4.6	3.4	4	50.4	3.7	3.4	4.7	-0.03	0.41	-0.02	0.44	0.19	0.12	0.04	0.00
Composite Non-Academic																				
Participants	52.2	21.5	41.2	18.2	7.8	6.2	2	2.9	51.6	5	2.4	3.5	0.30	0.45	0.34	0.47	0.12	0.10	0.03	0.01
Enrollees	64.7	10.5	44.4	9.7	6.3	4.7	1.4	1.5	53.2	5.1	1.13	0.8	0.24	0.27	0.29	0.26	0.13	0.08	0.03	0.01
Losers[2]	55.7	21.5	49.2	19.7	12.4	5.4	3.3	3.8	49.4	3.9	3.4	4.3	0.03	0.31	0.05	0.34	0.17	0.11	0.04	0.01

1.Counted as 5th graders with non-missing test scores in both grades 4 and 5.

2. Students who lost all lotteries (neither as outright winner nor delayed winner by the start of 5th grade).

Sample limited to 4th graders with non-missing test scores.

There are some discrepancies between Table 4.1 and 4.2, notably for the non-academic magnet schools. Overall, the percentage of black peers or low SES peers is higher than the percentage of black or low SES lottery participants enrolling in the non-academic magnet schools. As noted before, many non-academic magnet schools have places filled by students from other channels such as geographic priority zones, sibling preference, or a feeder school, which causes school peer characteristics to differ from the characteristics of lottery participants.

Table 4.3 presents the descriptive statistics on the two dependent variables --- student test scores in math and reading. As I mentioned in Chapter III, I standardized student test scores by grade and year: for every grade (from grade 4 to grade 8) in each school year from 1999 to 2007, the district mean score is 0 with a standard deviation of 1. As shown in Table 4.3, overall, lottery participants score higher in both math and reading than other middle school students in the district. Academic magnet school enrollees appear to perform better in both subjects than the lottery losers who enrolled in other schools. Also, the standardized scores for the academic magnet enrollees are less dispersed than the scores for lottery losers enrolled in neighborhood schools. The non-academic composite enrollees, however, only significantly outperform their counterpart students in neighborhood schools in 6^{th} grade; the achievement differences between the enrollees and lottery losers are not notable in other grades.

Table 4.3 Descriptive Statistics on Academic Achievement

	Grade 5		Grade 6		Grade 7		Grade 8	
Math Scores	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Lottery Participants								
All	0.82	0.86	0.86	0.85	0.86	0.84	0.86	0.85
Academic								
Participants	1.22	0.70	1.21	0.75	1.23	0.72	1.22	0.71
Enrollees	1.33	0.65	1.28	0.73	1.32	0.69	1.36	0.65
Enrollees in Other Magnet Schools	1.09	0.70	1.23	0.74	1.19	0.70	1.19	0.76
Enrollees in Non-Magnet Schools	1.17	0.73	1.15	0.75	1.16	0.75	1.12	0.73
Composite Non-Academic								
Participants	0.62	0.85	0.69	0.85	0.69	0.84	0.70	0.87
Enrollees	0.49	0.81	0.66	0.82	0.60	0.81	0.63	0.86
Enrollees in Other Magnet Schools	1.33	0.71	1.23	0.74	1.33	0.69	1.37	0.69
Enrollees in Non-Magnet Schools	0.50	0.81	0.50	0.83	0.57	0.83	0.55	0.83
Reading Scores	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Lottery Participants								
All	0.89	0.81	0.89	0.84	0.90	0.84	0.87	0.82
Academic								
Participants	1.26	0.67	1.26	0.72	1.28	0.71	1.22	0.69
Enrollees	1.35	0.63	1.33	0.69	1.45	0.67	1.38	0.63
Enrollees in Other Magnet Schools	1.15	0.68	1.22	0.74	1.13	0.68	1.07	0.67
Enrollees in Non-Magnet Schools	1.21	0.70	1.21	0.73	1.18	0.71	1.14	0.72
Composite Non-Academic								
Participants	0.71	0.81	0.73	0.84	0.73	0.86	0.70	0.81
Enrollees	0.60	0.76	0.65	0.78	0.59	0.74	0.58	0.72
Enrollees in Other Magnet Schools	1.35	0.74	1.33	0.68	1.52	0.74	1.37	0.62
Enrollees in Non-Magnet Schools	0.58	0.80	0.57	0.86	0.60	0.85	0.58	0.82

*Count only students who have non-missing 4th grade test scores.

Magnet School Treatment Effects

The estimation of magnet school treatment effects follows the same strategy as the peer effect estimation except that the peer term is dropped from the model. Because there are two treatment groups (the academic magnet, and the composite non-academic magnet) in the data, the regressors of interest include two treatment indicators: d_{i1} equals 1 if student i enrolled in the academic magnet; d_{i2} equals 1 if student i enrolled in the non-academic composite. The regression model estimating magnet school treatment effects is expressed as

$$Y_{ij} = \mu(X_i) + \delta_1 d_{i1} + \delta_2 d_{i2} + \eta_{i1} d_{i1} + \eta_{i2} d_{i2} + u_{iN} \qquad (4.1)$$

In order to eliminate the possible selection bias arising from non-compliances, lottery outcomes are exploited to form the instruments for both treatment indicators d_{i1} and d_{i2}.

Table 4.4 reports the estimates of magnet treatment effect models on student achievement in math and reading. For both subjects, I ran the regression models on two different samples separately[21]: students in the first two grades (Grades 5 and 6), and students in all four grades (Grades 5 to 8). The point estimates of treatment indicators suggest that the academic magnet school improves student standard score by almost 0.1 point of standardized scores in both math and reading. This implies that students enrolling in the academic magnet school achieve 0.1 point higher in standardized score than the test scores they would achieve if they had lost the academic magnet lottery and enrolled in the neighborhood schools. The estimates of treatment effects for the non-academic magnet composite are not statistically significant in either subject.

[21] As mentioned in Chapter III, the purpose of limiting the investigation to 5th and 6th graders is to avoid the complications arising from the presence of the second academic magnet school in later grades. I also ran a set of grade-specific models, wherein the treatment indicator is interacted with grade indicators (for grades 6 to 8) to examine whether the treatment effect varies across grades. Since the treatment effects in grades 6 to 8 are not found different from 5th grade, I'm not going to report the results from the grade specific models in the text.

Table 4.4 Magnet School Treatment Effects on Academic Achievement

	Math		Reading	
Independent Variables	First 2 grades	All 4 grades	First 2 grades	All 4 grades
Academic Magnet Treatment				
Academic	0.09*	0.08*	0.07*	0.09**
	(0.04)	(0.03)	(0.03)	(0.03)
Non-Academic Magnet Treatment				
Composite	0.11	0.07	0.02	-0.01
	(0.08)	(0.08)	(0.08)	(0.07)
Student Characteristics				
Black	-0.19***	-0.20***	-0.17***	-0.17***
	(0.02)	(0.02)	(0.02)	(0.01)
Hispanic	-0.24***	-0.12*	0.01	0.02
	(0.07)	(0.05)	(0.06)	(0.05)
Special Education	-0.20***	-0.17***	-0.12**	-0.11***
	(0.05)	(0.03)	(0.04)	(0.03)
Low SES	-0.09***	-0.12***	-0.12***	-0.14***
	(0.02)	(0.01)	(0.02)	(0.01)
ELL	0.17	0.16*	-0.10	-0.11
	(0.09)	(0.07)	(0.08)	(0.07)
Female	-0.03	-0.03**	0.01	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
Grade 4 Reading	0.18***	0.18***	0.53***	0.50***
	(0.01)	(0.01)	(0.01)	(0.01)
Grade 4 Math	0.44***	0.43***	0.11***	0.12***
	(0.01)	(0.01)	(0.01)	(0.01)
Grade 4 Absence	-0.86**	-1.19***	-0.31	-0.44*
	(0.29)	(0.21)	(0.26)	(0.20)
Grade 4 Suspension	0.00	0.00	0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)
Over-Identification Test	0.61	0.57	0.75	0.82
N	6267	11869	6270	11885

* p<0.05, ** p<0.01, *** p<0.001

The other coefficients reported in Table 4.4 pertain to student characteristics. Black students and students from low SES families score significantly below other lottery participants in both mathematics and reading. This is also true of special education students in both subjects. Although Hispanic students score significantly lower in math, their reading achievement does not differ from other students. ELL students in our sample outperform other students by 0.16 standard scores in mathematics[22], but not in reading. As one would expect, prior achievement is significantly associated with later performance, with a more substantial intra-subject correlation. For example, a 0.5 point increase in 4th grade standardized math score is related to 0.2 point of standardized score increase in middle school math test. Moreover, students with a high absence rate in 4th grade have lower achievement in middle school grades, while prior disciplinary infractions are not found significantly related to middle school performance.

The P-values of the omnibus over-identification tests are reported in the second panel of the table.[23] The results indicate that the instruments used in all four models are exogenous.

Impacts of Average School Peer Characteristics

This section investigates the average school level peer effects on middle student achievement in both math and reading. All ten specifications of peer characteristics (including 6 predetermined peer characteristics, and 4 lagged values of peer outcome measures) are examined. First, I run a set of single variable models with only one peer variable included in the regression; I then include all specifications of peer characteristics in one regression. Coefficients from all 11 models are reported in this part. Similar to the treatment effect investigation, all models are run twice using both the small sample comprising grades 5 and 6 and the big sample of all four grades. Overall, the findings from both samples are close, but the estimates from the models using the big sample have

[22] In the regression sample, 164 (1.25%) observations are Hispanic and 79 (0.6%) observations are ELL students, but only 9 Hispanic observations are identified as ELL students.

[23] Recall that the lottery outcomes are not binary, and there are two excluded instruments (outright-win and delayed-win) for each treatment variable. Therefore, the validity of the external instruments is tested for all regression models using the over-identification tests.

smaller standard errors and higher P-values from the omnibus over-identification tests. Therefore, I'm only going to report and discuss the results from the full sample analysis.

Math Achievement

Estimates of average school level peer effects on math achievement are presented in Table 4.5. As introduced in Chapter III, there are two types of peer variables included in the model: the exogenous, lottery-based variation in peer characteristics $[P_M d_i + P_N(1-d_i)]$; and the endogenous, residence-based variation in peer characteristics P_N. The causal relationship between peer group composition and student outcomes are obtained from the estimates on the first exogenous peer term; and the second peer term is included to control for residence choice (or neighborhood school choice). The estimated effects of both peer terms are reported in Table 4.5 along with the magnet school treatment effects. Individual characteristics are also controlled in the regression models, but are not reported in the table since the estimates are quite similar to those in previous treatment effect models.

Several estimates from Table 4.5 are noteworthy. The first result to draw is from model 1, which suggests that students tend to perform worse in mathematics if they are in a school with a high proportion of black students. The estimate of this racial composition effect is quite large. For instance, increasing the share of black schoolmates from 25% to 75% would reduce student math scores by 0.25 points of standardized score. Although the estimate of percent low SES student effect is not statistically significant (Model 2), the coefficient on this variable is negative. Also note that in model 1 and model 2, including the peer variables --- percent Black and percent low income students ---

completely overturns the positive treatment effect from the academic magnet school reported in Table 4.4. This implies that if the academic magnet school lottery losers attend a neighborhood school where peer racial or Social Economic Status composition is very similar to the academic magnet school, they would do as well in mathematics as those students enrolled in the academic magnet.

The second significant estimate is on school gender composition effect (Model 3), which shows that having more girls in a school decreases average math scores for the lottery participants. While some previous studies find female peers help to improve student achievement (e.g., Hoxby, 2000; Lavy & Scholosser, 2007), results in Table 4.5 do not support that conclusion. The coefficient is strikingly large --- for 50 percent point increase of female students in the school, the average math score falls by almost 0.8 standard deviations. However, a 50% change in percent school female population far exceeds anything observed in our data.

Model 5 and Model 6 find significant and positive coefficients on school percent special education students and school percent ELL students. For example, the estimate from Model 5 suggests that double the special education population in the academic magnet school (from 0.5% to 1%) would increase the average math scores by 0.02 points of standardized scores. However, although the school level estimates suggests positive peer effect from percent special education or ELL students, the impact of having special education peers or ELL peers is more likely to work through the classroom. For instance, a teacher may spend disproportional time on a leaning disabled student, which would have a great impact on the children in the same classroom but not on other children in the school. Therefore, it would be more meaningful to draw the conclusion on the effect of these two peer variables after comparing the results at both school and classroom levels, which will be done in next chapter.

Table 4.5: Average School Peer Effects on Math Achievement

	Math Scores										
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
Magnet School Effect											
Academic	-0.05	-0.03	0.19**	0.12**	0.46***	0.18**	0.08	0.00	0.09	0.11	-0.38
	(0.04)	(0.08)	(0.06)	(0.04)	(0.13)	(0.06)	(0.19)	(0.19)	(0.04)	(0.07)	(0.30)
Non-Academic Composite	0.05	0.04	0.14	0.10	0.25*	0.11	0.08	0.06	0.09	0.09	0.12
	(0.08)	(0.09)	(0.10)	(0.09)	(0.12)	(0.09)	(0.11)	(0.11)	(0.08)	(0.10)	(0.16)
Peer Effect (Lottery Based)											
Black (proportion)	-0.49**										0.75
	(0.15)										(0.40)
Low SES (proportion)		-0.33									0.10
		(0.18)									(0.43)
Female (proportion)			-1.52**								-0.83
			(0.57)								(0.65)
Hispanic (proportion)				0.83							3.14
				(0.63)							(2.54)
Special ED (proportion)					3.45**						8.33***
					(1.08)						(2.33)
ELL (proportion)						2.43*					2.69
						(1.18)					(3.83)
Grade 4 Math (Mean)							0.04				0.80*
							(0.17)				(0.33)
Grade 4 Reading (Mean)								0.10			0.80*
								(0.15)			(0.39)
Grade 4 Suspension (Mean)									-0.08		-0.17
									(0.20)		(0.32)
Grade 4 Absence (Mean)										2.10	-1.96
										(4.68)	(7.76)
Peer Effects (Residence Based)											
Black (proportion)	-0.02										0.01
	(0.04)										(0.06)
Low SES (proportion)		-0.15***									-0.20*
		(0.04)									(0.10)
Female (proportion)			0.18								0.09
			(0.13)								(0.16)
Hispanic (proportion)				-0.35***							-0.20
				(0.07)							(0.11)
Special ED (proportion)					-0.23**						-0.21*
					(0.07)						(0.10)

Table 4.5 (Continued)

ELL (proportion)						-0.24***					-0.04
						(0.06)					(0.08)
Grade 4 Math (Mean)							0.04*				-0.26*
							(0.02)				(0.10)
Grade 4 Reading (Mean)								0.05*			0.18
								(0.02)			(0.10)
Grade 4 Suspension (Mean)									-0.08		0.11
									(0.06)		(0.08)
Grade 4 Absence (Mean)										-2.53*	0.94
										(1.00)	(1.52)
Student Characteristics											
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Over-identification Test	0.6	0.53	0.71	0.8	0.45	0.34	0.58	0.58	0.81	0.57	0.96
Sample Size[24]	11780	11780	11780	11780	11780	11780	11778	11778	11778	11778	11778

* p<0.05, ** p<0.01, *** p<0.001

School level models do not find significant estimates of average peer achievement variables (Models 7 and 8), average peer disciplinary infractions (Model 9)[25], or average peer absence rate (Model10). Because middle school students (especially students in later grades) are often grouped for instruction based on their academic abilities[26] --- students with lower prior achievement (or behavioral problems) are often placed in a class with other disadvantaged students, it is possible that average school level peer academic ability or behavioral measures may only have a moderate or no impact on these variables.

[24] The regression sample is limited to lottery participants to all magnet middle schools. The sample size in the peer effect models is 11780, which is smaller than the sample size in the treatment effect models (11885) in Table 4.4. The difference is caused by the creation of peer variables --- there are about 100 observations whose End-of-Year school can not be identified.

[25] In order to circumvent the reflection problems, I use student 4th grade test scores and behavioral records to construct these peer variables. Given that students in elementary schools are less likely get suspension, I also constructed another peer disciplinary variable representing average 4th grade punishment numbers including severe punishments such as suspension and other less severe punishments. However, the conclusion from the average punishment peer variable is very similar to that from the suspension variable except that the coefficient is smaller. Since suspension is more often used in other studies measuring student disciplinary infractions (e.g., Figlio, 2005; Rowley, 2005), I follow the literature and keep only the suspension variable in the result table.

[26] It is also possible that some schools keep students with behavioral problems in one class just because the teacher is more experienced with disruptive students.

Note that although there is no significant peer effect from these four peer variables, the positive treatment effect of the academic magnet program vanishes in all four models while the peer variables are controlled. Because of the tracking police in middle school, the influence of peer achievement or misconduct is more likely to work through a classroom rather than through the school/grade level; therefore, I expect significant estimates of peer ability effects at the classroom level. This of course will be examined in next chapter.

Model 11 incorporates all peer variables in one regression. Among the four peer variables that are found significantly influencing student math achievement in the single variable models, only the estimate of percent special education peers remains significant and positive. With all specifications of peer characteristics controlled in one model, the estimates of prior peer math and reading scores become weakly significant ($P<0.05$) and show positive impact on student math achievement. Due to the correlations among the peer variables, the estimates of most peer characteristics are insignificant in the full model. Moreover, with all peer variables controlled in the same equation, the coefficient on the academic magnet school treatment effect turns to -0.38. This result suggests that if school peer compositions are held constant, lottery winners enrolling in the magnet schools do not perform better than their counterpart students enrolling in neighborhood schools, at least not in math test.

The lower part of Table 4.6 presents the estimates on the residence based peer variables. Four residence-based pre-determined (contextual) peer characteristics, percent low SES students, percent Hispanic students, percent special education students, and percent ELL students, have shown negative impacts on student math achievement in middle school; the estimates on the two residence-based peer ability variables (Model 7 and Model 8) suggest that students tend to score slightly higher in math if their neighborhood schools have more high performing students. The residence-based peer absence rate is also found reducing student achievement. However, compared to the estimated effects from the exogenous, lottery-based peer variables, the estimates of residence based peer characteristic effects are smaller in magnitude.

The findings from the residence-based peer variables suggest that although the admission lotteries randomly assign students between a magnet school and a neighborhood school, family residential choice (indicated by neighborhood school characteristics) is still correlated with some factors influencing student achievement --- although the correlation is smaller than the peer effects identified from the exogenous peer characteristics.

Reading Achievement

Estimates of peer effect models on reading achievement are presented in Table 4.6. Compared to the estimates from the math achievement models, there are fewer significant peer variable coefficients on reading achievement. The first significant estimate is percent ELL students, which shows positive impact on student reading achievement in middle school. Surprisingly, Model 6 suggests that school level average peer reading achievement reducing student reading scores in middle school: if average school peer 4^{th} grade reading scores increase by 1 point of standardized scores, student reading achievement in middle school falls by almost 0.3 points of standardized scores. The last significant coefficient is from model 10, which finds a positive association between individual reading achievement and prior peer absence rate.

Table 4.6: Average School Peer Effects on Reading Achievement

Independent Variables	Reading Scores										
	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
Magnet School Effect											
Academic	0.04	0.12	0.15**	0.13***	0.28*	0.21***	0.41*	0.44*	0.06	0.21***	0.53
	(0.04)	(0.07)	(0.06)	(0.04)	(0.12)	(0.05)	(0.18)	(0.18)	(0.04)	(0.06)	(0.27)
Non-Academic Composite	-0.02	0.01	0.03	0.03	0.09	0.05	0.07	0.09	-0.03	0.10	0.25
	(0.07)	(0.08)	(0.09)	(0.08)	(0.11)	(0.08)	(0.10)	(0.11)	(0.08)	(0.10)	(0.14)
Peer Effect (Lottery Based)											
Black (proportion)	-0.20										0.21
	(0.14)										(0.37)
Low SES (proportion)		0.05									-0.54
		(0.17)									(0.38)
Female (proportion)			-0.88								-1.23*
			(0.54)								(0.57)
Hispanic (proportion)				0.90							-1.70
				(0.59)							(2.25)
Special ED (proportion)					1.72						1.69
					(1.01)						(2.14)
ELL (proportion)						3.18**					5.70
						(1.13)					(3.45)
Grade 4 Math (Mean)							-0.29				-0.04
							(0.16)				(0.30)
Grade 4 Reading (Mean)								-0.29*			-0.02
								(0.14)			(0.36)
Grade 4 Suspension (Mean)									-0.27		-0.64*
									(0.18)		(0.29)
Grade 4 Absence (Mean)										12.20**	14.38*
										(4.45)	(7.03)
Peer Effects (Residence Based)											
Black (proportion)	-0.06										-0.04
	(0.04)										(0.06)
Low SES (proportion)		-0.18***									-0.08
		(0.04)									(0.09)
Female (proportion)			-0.03								-0.09
			(0.12)								(0.14)
Hispanic (proportion)				-0.32***							-0.25**
				(0.07)							(0.10)
Special ED (proportion)					-0.16*						-0.03
					(0.07)						(0.09)

Table 4.6 (Continued)

ELL (proportion)						-0.21*** (0.05)					-0.06 (0.07)
Grade 4 Math (Mean)							0.06** (0.02)				-0.19* (0.09)
Grade 4 Reading (Mean)								0.07*** (0.02)			0.18* (0.09)
Grade 4 Suspension (Mean)									-0.06 (0.06)		0.11 (0.07)
Grade 4 Absence (Mean)										-3.35*** (0.95)	-0.82 (1.35)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Over-identification Test	0.72	0.84	0.73	0.79	0.86	0.91	0.84	0.98	0.89	0.84	0.85
Sample Size[27]	11796	11796	11796	11796	11796	11796	11794	11794	11794	11794	11794

* p<0.05, ** p<0.01, *** p<0.001

In the full variable model (column 11), the estimated effect of average peer absence rate remains big and significant; and the estimate of average peer suspension records becomes negative and weakly significant at 5% statistical level. The variable of percent ELL students is no longer significant; but having more female peers seems to reduce individual reading scores in middle school while all other peer variables are controlled.

Since most models do not find advantaged peers improving student reading scores in middle school, the positive academic magnet school treatment effect becomes larger in some models while peer characteristics are controlled. Two exceptions are model 1 and model 9: the former one suggests that lottery losers in neighborhood schools would do just as well in reading as the lottery winners in the magnet schools if the school racial composition (percent black students) is similar; the later one suggests no evidence of magnet school effects on reading achievement if peer behavioral records in the magnet school and neighborhood schools are alike. Although the estimate of academic magnet

[27] The sample size for reading models is slightly larger than that in the math models because fewer students have missing reading scores.

school treatment effect is particularly big in model 11 where all peer variables are held constant, it is not statistically significant.

Seven out of ten estimates of residence based peer effects are significant. Although the size of the coefficients on the residence based peer variables are smaller, the coefficient signs are as expected. This again suggests that residential choice is correlated with other factors influencing student achievement, although the impact is in a small magnitude.

All 11 models in both Table 4.6 and Table 4.7 have high P-values from the omnibus over-identification tests, which support the exogeneity of the instruments used in the peer effect models on both math and reading achievement.

In conclusion, the estimates of average school peer effects on math and reading achievement are not quite similar. Overall, a student's math score is more likely to increase if the individual is in a school with more advantaged peers; this, however, is not true on student reading achievement in our sample. Several estimates of peer effects on student academic outcomes are noteworthy. First, although percent black students is significant only in the math equation, controlling for this school racial composition variable totally overturns the positive magnet school treatment effect in both math and reading. Second, school SES composition does not show significant impact on student in either subject. Third, there is no evidence that average school peer academic abilities or average school peer disciplinary records are associated with student math achievement in middle school, while average school peer reading achievement is even negatively correlated with student reading scores in middle school. However, as I mentioned before, the peer group impacts on student academic achievement may mostly come from classroom where the direct peer interaction on learning actually takes place. Therefore, it would be more interesting to see how different the estimates of peer effects at the classroom level are from the estimates at the school level.

Impacts from Dispersion of Peer Characteristics

The last section finds some important peer group effects on student academic outcomes from average school peer characteristics. In this part, I am going to explore whether student academic achievement is influenced by the variance of peer characteristics. The heterogeneity of peer composition is measured by the standard deviations in four lagged peer outcome variables: peer math achievement, peer reading achievement, peer disciplinary infractions, and peer absence Rate. One thing needs to be kept in mind is that this chapter focuses on peer effects at the school level. The variance specifications are aggregated grade level measures of variance, not variance within classes.

Table 4.7 presents some descriptive statistics on the four school level heterogeneity variables in 5^{th} grade. The mean values reported in the table are across-cohort means of standard deviations in all four peer outcome variables. As shown in the table, the average standard deviation of peer math achievement is about 0.82 for the applicants of the academic magnet program, and 0.86 for lottery participants of the non-academic magnet composite. There is less variance in peer outcomes (all 4 specifications) for academic magnet school enrollees than for lottery losers attending neighborhood schools. However, the differences in the dispersion measures are not very significant between the non-academic magnet enrollees and losers.

Table 4.7 Heterogeneity of School Peer Characteristics in Grade 5[1]

	Standard Deviation in G4 Math		Standard Deviation in G4Readng		Standard Deviation in G4 Suspension		Standard Deviation in G4 Absence	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Academic								
Academic								
Participants	0.82	0.18	0.82	0.17	0.38	0.27	0.03	0.01
Enrollees	0.66	0.09	0.66	0.04	0.21	0.06	0.03	0.00
Losers[2]	0.97	0.12	0.96	0.12	0.53	0.31	0.04	0.01
Non-Academic								
North								
Participants	0.88	0.13	0.89	0.13	0.52	0.26	0.03	0.01
Enrollees	0.90	0.06	0.93	0.07	0.63	0.12	0.04	0.02
Losers[2]	0.89	0.14	0.92	0.12	0.53	0.28	0.04	0.01
South								
Participants	0.88	0.15	0.88	0.14	0.46	0.26	0.03	0.01
Enrollees	0.95	0.09	0.93	0.09	0.48	0.21	0.03	0.00
Losers[2]	0.92	0.12	0.92	0.07	0.56	0.22	0.04	0.01
East								
Participants	0.83	0.15	0.85	0.14	0.42	0.27	0.03	0.01
Enrollees	0.71	0.10	0.78	0.06	0.20	0.04	0.03	0.01
Losers[2]	0.91	0.11	0.92	0.14	0.59	0.34	0.04	0.01
West								
Participants	0.81	0.14	0.85	0.12	0.39	0.22	0.03	0.01
Enrollees	0.75	0.00	0.83	0.00	0.37	0.00	0.03	0.00
Losers[2]	0.93	0.10	0.92	0.10	0.66	0.29	0.03	0.00
Composite Non-Academic								
Participants	0.86	0.15	0.87	0.14	0.46	0.26	0.03	0.01
Enrollees	0.87	0.12	0.89	0.1	0.47	0.22	0.03	0.01
Losers[2]	0.91	0.17	0.92	0.11	0.58	0.28	0.04	0.01

1. Counted as 5th graders with non-missing test scores in both grades 4 and 5.
2. Students who lost all lotteries (neither as outright winner nor delayed winner by the start of 5th grade).
3. Sample limited to 4th graders with non-missing test scores.

<u>*Math Achievement*</u>

The models estimating the dispersion of peer composition effects are reported in Table 4.8. The regression model is equation 3.19 in Chapter III, wherein both the average peer characteristics and the heterogeneity of peer composition are included in the same equation. Since the conclusions on the estimates of the magnet school treatment effects and the residence based peer effects are quite similar to those in average peer effect models, Table 4.8 only reports the estimates of the two regressors of interest: the mean peer outcome variables, and the variance of peer outcome variables (from the exogenous, lottery based peer characteristics).

Table 4.8 Impacts from Dispersion of Peer Characteristics on Math Achievement

	Math Scores			
Independent Variables	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	Yes	Yes	Yes
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	0.05			
	(0.16)			
Grade 4 Reading (Mean)		0.16		
		(0.14)		
Grade 4 Suspension (Mean)			-0.47	
			(0.49)	
Grade 4 Absence (Mean)				0.30
				(5.71)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	0.06			
	(0.14)			
Grade 4 Reading (Standard Deviation)		0.60***		
		(0.13)		
Grade 4 Suspension (Standard Deviation)			0.16	
			(0.18)	
Grade 4 Absence (Standard Deviation)				1.76
				(2.31)
Peer Effects (Residence Based)				
	Yes	Yes	Yes	Yes
Student Characteristics				
	Yes	Yes	Yes	Yes
P-value for Over-identification Tests	0.58	0.5	0.52	0.59
Sample Size	11778	11778	11778	11778

* p<0.05, ** p<0.01, *** p<0.001

Only one dispersion variable is found significantly influencing student math achievement. Model 2 suggests that the heterogeneity of school peer reading ability levels is positively associated with student math scores in middle school. For instance, a 0.5 point increase in the standard deviation of school peer reading achievement (this is about 3 standard deviation of this dispersion variable) will improve student math achievement by 0.3 point in standardized scores. There is little evidence that changes in the heterogeneity of peer math ability or student behaviors (discipline or attendance) affect individual math performance in middle school.

The estimates of average peer math and reading achievement remain almost unchanged after controlling for the variance of both variables. However, due to the high collinearity between the average term and the dispersion term[28], the coefficient on average peer suspension times increases substantially, but it is still insignificant. Similarly due to the collinearity, the estimated effect of average peer absence rate drops while controlling for the variance variable and also remains insignificant.

Reading Achievement

Table 4.9 reports the results from the reading achievement models. Column 1 and 2 suggest that the heterogeneity of peer academic achievement in both subjects tends to increase student reading performance in middle school. Although the coefficient on the standard deviation of peer disciplinary infractions is negative, the estimate is not statistically significant. The estimated effects from most specifications of average peer outcomes do not change much with the variance terms included in the regression, except that the coefficient on average peer absence rate is no longer significant now in model 4.

[28] The average peer suspension times is highly correlated with the standard deviation of peer suspension times (0.56 in our sample). Given the fact that 90% students never received a suspension in 4th grade, a school with a high value of average peer suspension times is most likely to have a high value in the dispersion term.

Over-identification tests indicate that the instruments (for both the average peer terms and the variance terms) are exogenous in all 4 models on both subjects.

Table 4.9 Impacts from Dispersion of Peer Characteristics on Reading Achievement

	Reading Scores			
Independent Variables	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	Yes	Yes	Yes
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	-0.27			
	(0.15)			
Grade 4 Reading (Mean)		-0.24		
		(0.14)		
Grade 4 Suspension (Mean)			-0.20	
			(0.46)	
Grade 4 Absence (Mean)				10.33
				(5.39)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	0.26*			
	(0.13)			
Grade 4 Reading (Standard Deviation)		0.55***		
		(0.13)		
Grade 4 Suspension (Standard Deviation)			-0.03	
			(0.17)	
Grade 4 Absence (Standard Deviation)				1.92
				(2.19)
Peer Effects (Residence Based)				
	Yes	Yes	Yes	Yes
Student Characteristics				
	Yes	Yes	Yes	Yes
P-value for Over-identification Tests	0.96	1.00	0.90	0.83
Sample Size	11794	11794	11794	11794

* p<0.05, ** p<0.01, *** p<0.001

Heterogeneous Peer Effects

The results in previous part reveal significant effects from some average peer characteristics and variation in peer variables. However, peer groups may impact some students more than others depending on a student's own background, such as race, gender, or academic ability. This section answers the third research question “to whom do peer effects matter more”. To examine this possibility, I interact all peer group variables with individual characteristics, including black, Low SES, female, and indicators of student academic ability.[29] The student academic ability indicators are measured by student initial position in each cohort lottery participants’ achievement distribution in 4th grade: if a student’s prior math score is in the bottom quartile (1st) of math score distribution among lottery participants, this student is coded as a low math achiever; if a student’s prior math score is in the top quartile (4th) of the distribution, he/she is coded as a high math achiever. The same procedure is also applied to reading achievement. Therefore, there are four academic ability indicators for one student: high math achiever, low math achiever, high reading achiever, and low reading achiever. The four indicators along with three demographic variables are interacted with all 10 peer variables to investigate if there are heterogeneous peer effects.

As shown in previous part, not every peer characteristic has shown a significant impact on student outcomes. Therefore, although all 10 specifications of peer characteristics are examined in the investigation of heterogeneous peer effects, I’m only going to report the results from two widely expressed contextual peer characteristics (percent black students and percent low SES students[30]) and from the other four peer

[29] Because there are very few Hispanic, Ell, and Special Education students among the lottery participants (especially for the academic magnet program), I only choose three student demographic variables (black, female, and SES) to interact with the peer characteristics. Also, due to the fact that most lottery participants never received suspension and have very low absence rate, more than 80% students will receive the value of 1 as low disciplinary incidences based on the quartiles, and very few will be coded as high misbehaving group. Therefore, I only create the academic ability indicators, which are also used in Haneshek et al (2002) and Kang (2007) in examining heterogeneous peer effects.

[30] Another widely expressed peer variable is peer gender composition (percent female students), I did not include in the tables here because most models (at both levels) find no significant effect from this variable.

outcome variables (peer math and reading achievement, peer disciplinary incidence, and peer absence rate).

Math Achievement

The regression model examining the heterogeneous peer effect is expressed in equation (3.17) of Chapter III, in which the interaction of the peer variables with individual characteristics is added into the model. The two regressors of interest then include the average peer characteristics and the interaction term. As usual, since the estimates of magnet school treatment effect, residential choice effect, and individual characteristics do not change much from previous average peer effect models, Table 4.10 omits the coefficients on these variables.

Each column in Table 4.10 represents an individual background variable; each horizontal panel represents a peer variable. The coefficients on the average peer term are reported in each row of the mean effect; the coefficients on the interaction terms are reported in each row of the heterogeneous effect. However, note that the coefficients on the average peer term in Table 4.10 can not be interpreted as the overall estimates of average peer effects on all students; instead, they should be explained as the average peer effects on students of the group not indicated at the top of each column. The coefficients on the heterogeneous terms stand for the peer effect differences between the two groups. The estimated peer effects on the specific student group in each column are obtained as the linear combination of the coefficient on the average term and the coefficient on the interaction term[31].

[31] The combination test results will be reported in Appendix E.

Table 4.10 Heterogeneous School Level Peer Effects on Math Achievement

	Math Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion of Black Students							
Mean Effect	-0.43**	-0.48**	-0.66***	-0.47**	-0.46**	-0.39**	-0.45**
	(0.18)	(0.16)	(0.16)	(0.15)	(0.15)	(0.15)	(0.15)
Heterogeneous Effect	-0.11	-0.04	0.32***	-0.09**	-0.16**	-0.22***	-0.18***
	(0.09)	(0.10)	(0.07)	(0.03)	(0.05)	(0.03)	(0.05)
Proportion of Low SES Students							
Mean Effect	-0.33	-0.36*	-0.48**	-0.30	-0.30	-0.22	-0.28
	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)
Heterogeneous Effect	-0.00	0.14	0.22**	-0.11**	-0.18**	-0.27***	-0.20***
	(0.10)	(0.13)	(0.08)	(0.04)	(0.06)	(0.04)	(0.05)
Peer Prior Math Achievement							
Mean Effect	0.04	0.04	0.14	0.05	0.06	0.07	0.03
	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)
Heterogeneous Effect	0.06	-0.01	-0.08*	-0.09*	-0.09**	-0.05	-0.13***
	(0.04)	(0.05)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)
Peer Prior Reading Achievement							
Mean Effect	0.10	0.10	0.18	0.12	0.12	0.13	0.10
	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)
Heterogeneous Effect	0.04	-0.03	-0.07*	-0.08*	-0.08**	-0.06	-0.12***
	(0.04)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Peer Prior Disciplinary Records							
Mean Effect	-0.17	-0.14	-0.23	-0.03	-0.04	0.13	0.09
	(0.23)	(0.21)	(0.23)	(0.20)	(0.20)	(0.20)	(0.21)
Heterogeneous Effect	0.18	0.17	0.25	-0.20	-0.28	-0.58***	-0.71***
	(0.20)	(0.23)	(0.19)	(0.12)	(0.18)	(0.12)	(0.16)
Peer Prior Attendance Records							
Mean Effect	0.97	0.89	0.31	2.48	2.08	3.01	2.35
	(4.86)	(4.69)	(5.08)	(4.69)	(4.69)	(4.68)	(4.68)
Heterogeneous Effect	4.86	5.48	2.60	-1.83***	-2.50***	-3.45***	-3.51***
	(4.05)	(4.55)	(3.44)	(0.51)	(0.63)	(0.50)	(0.61)

* p<0.05, ** p<0.01, *** p<0.001

The first peer variable reported in the table is percent Black students. The results from model 1 and model 2 suggest that although individual math score appears to be lower in a school with high percentage of black students, there is little evidence showing that Blacks or low income students are more severely impacted by school racial composition. This is different from previous findings of Hanueshek et al (2002) and Hoxby (2000), who both find stronger intra-racial group peer effects for Black students. However, although the heterogeneous effect is not statistically significant for black students, the linear combination test in Model 1 shows a coefficient of 0.55 (P<0.01) on black students, which is still bigger than the coefficient on non-black students (0.43). Female students, however, are much less strongly impacted than males by school percent black students. For instance, switching from a school with 25% black students to another school with 75% black students is likely to lower the math scores for boys by 0.33 points of standardized scores; but for girls, the achievement reduction is only 0.17, about half the size of the impact on boys. Interestingly, models 4 to 7 suggest that the estimated effect of school percent black students appears to be stronger on both low achievers and high achievers. Recall that the estimate of percent Black student effect on all lottery participants is 0.49 in the average peer effect model (model 1 in Table 4.5). The heterogeneous peer effect models find the percent black student coefficients (linear combination coefficients) are higher on students at both the bottom and the top quartiles of achievement distribution: 0.56 for low reading achievers, 0.61 for high reading achiever, 0.61 for low math achievers, and 0.63 for high math achievers. All 4 coefficients are statistically significant at 1% level.

The second peer variable examined here is percent low SES students. Although there is no significant effect of percent low SES students on the whole lottery participant sample (as shown in Model 2 of Table 4.5), the heterogeneous models find that school percent low income students does have significant impact on some student groups. First, the estimate of percent low SES student effect is significant ($P<0.05$) on students from more affluent families (students who are not eligible for free and reduced lunch programs). Second, boys are much more likely to score lower if they are in a school with high percentage of low SES peers. The coefficient is quite large (-0.48), which suggests that a 50 percentage point change in school low SES population will affect the average math performance by 0.24 points of standardized score for boys; in contrast, the estimated effect is much less sizeable and not significant for girls. Third, students in both the bottom and top quartiles of achievement distribution seem to be more impacted by the changes in school low SES population. For instance, the linear combination coefficients for both low math achievers and high math achievers are -0.49 and are statistically significant at 1 percent level.

The next two peer variables are the lagged measures of peer math and reading achievement. The results from all models on these two variables are very similar. First, school average peer achievement (in both math and reading) does not have significant influence on individual math scores; and there is no heterogeneous effect on black students or low SES students. Second, although girls and students in both the bottom and top achievement quartiles appear to be more negatively impacted by average peer achievement, the linear combination tests do no reveal significant effects on these groups of lottery participants.

The fifth peer variable is peer average disciplinary infraction, which has shown no significant impact on student math scores from the average peer effect model (Model 9 in Table 4.5). Heterogeneous effect of this peer discipline variable is only found in two models: both high math achievers and low math achievers are much more strongly influenced by peer disciplinary behaviors than other students. The linear combination coefficient for students in the top quartile of math achievement distribution is -0.62 ($P<0.01$), which suggests that if half of the school peers received 1 more suspension in 4th grade, high math performing lottery participants are likely to score lower by 0.3 points of standardized score in math.

The last peer variable is average school peer absence rate, which represents peer attendance behavior. There is no significant influence from the average attendance behavior of peers on student math achievement in middle school. No heterogeneous effects are found from the interactions of this peer variable with the three demographic characteristics. Although the estimates of peer attendance behavior appear to be different on students in both the bottom and top quartile of achievement distribution, the linear combination tests do not find the coefficients significant.

Reading Achievement

The estimates of heterogeneous peer effect models are presented in Table 4.11. The first peer variable is percent Black students. Recall that the average peer effect model (model 1 in Table 4.7) does not reveal significant peer effect from this variable on student reading achievement. Table 4.11, however, finds some significant heterogeneous effects. First, boys tend to have worse math performance if they attend a school with high proportion of black peers, while the negative impact is much smaller and insignificant (the linear combination coefficient) on girls. Second, the estimates of heterogeneous peer racial composition effects are significant on both low and high reading achievers. Especially for the students in the top quartile of reading score distribution, the linear combination coefficient is -0.54 ($P<0.001$), which is more than 5 times of the coefficient for other students whose prior reading scores are lower. Low math performing students also appear to be more negatively impacted by average school percent black students than other students, but the linear combination coefficient is not significant.

The second peer variable is percent low SES students. Significant heterogeneous effects are found for female students, students in both the bottom and top quartiles of reading achievement distribution, and low math achievers. However, the estimated effect of average school percent low SES students is only significant on the reading achievement of high reading achievers --- the linear combination coefficient is -0.34 ($P<0.05$), suggesting that increasing the school low SES population from 25% to 75% tends to reduce the average reading score by 0.17 points of standardized scores for students with high reading skills.

The third peer variable is peer math achievement. Both high and low reading achievers are more negatively impacted by average peer math achievement than other students. The linear combination coefficients on both groups are -0.38 and significant at 5% statistical level, suggesting that lottery participants at both the top and bottom math achievement distribution are likely to perform worse in reading if their school peers have better prior math achievement. There are no significant findings for other groups of students.

Table 4.11 Heterogeneous School Level Peer Effects on Reading Achievement

	Reading Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion of Black Students							
Mean Effect	-0.15	-0.21	-0.30*	-0.16	-0.10	-0.17	-0.19
	(0.16)	(0.15)	(0.15)	(0.14)	(0.14)	(0.14)	(0.14)
Heterogeneous Effect	-0.09	0.06	0.19**	-0.18***	-0.43***	-0.07*	-0.05
	(0.09)	(0.09)	(0.06)	(0.03)	(0.04)	(0.03)	(0.04)
Proportion of Low SES Students							
Mean Effect	0.06	0.05	-0.05	0.11	0.14	0.09	0.06
	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)
Heterogeneous Effect	-0.04	0.01	0.15*	-0.23***	-0.48***	-0.10**	-0.03
	(0.09)	(0.12)	(0.08)	(0.04)	(0.05)	(0.04)	(0.05)
Peer Prior Math Achievement							
Mean Effect	-0.30	-0.30	-0.28	-0.27	-0.27	-0.31	-0.30
	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)	(0.16)
Heterogeneous Effect	0.06	-0.01	-0.01	-0.15***	-0.13***	0.03	-0.05
	(0.04)	(0.05)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
Peer Prior Reading Achievement							
Mean Effect	-0.30*	-0.30*	-0.29*	-0.26	-0.27	-0.31*	-0.29*
	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
Heterogeneous Effect	0.05	-0.00	-0.00	-0.14***	-0.11***	0.03	-0.04
	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Peer Prior Disciplinary Records							
Mean Effect	-0.25	-0.36	-0.11	-0.09	-0.13	-0.24	-0.18
	(0.21)	(0.20)	(0.22)	(0.19)	(0.19)	(0.19)	(0.19)
Heterogeneous Effect	-0.04	0.24	-0.26	-0.56***	-0.82***	-0.09	-0.37*
	(0.19)	(0.21)	(0.18)	(0.11)	(0.16)	(0.11)	(0.15)
Peer Prior Attendance Records							
Mean Effect	12.64**	13.47**	14.02**	13.10**	12.28**	12.51**	12.28**
	(4.62)	(4.47)	(4.83)	(4.46)	(4.45)	(4.47)	(4.47)
Heterogeneous Effect	-1.96	-5.74	-2.66	-3.65***	-4.93***	-0.97*	-0.69
	(3.83)	(4.30)	(3.26)	(0.48)	(0.59)	(0.47)	(0.58)

* p<0.05, ** p<0.01, *** p<0.001

Average peer prior reading achievement has been found negatively associated with student reading scores in middle school. The estimates of heterogeneous models suggest that both high and low reading achievers are 50% more negatively impacted by average school peer prior reading achievement --- the linear combination coefficients are about 0.40 and significant at 1% level.

Average school peer disciplinary infractions (the fifth peer variables), significantly reduces the reading test scores of both high and low reading achievers. The heterogeneous effect is particularly large for students who are in the top quartile of reading achievement distribution. Linear combination test of Model 5 finds that when average school peer disciplinary infractions increase by 0.5, reading scores of high reading performing students falls by almost 0.47 in standardized scores ($P<0.001$) --- the estimate if very substantial.

The last peer variable is peer attendance behavior, measured by average peer absence rate. Overall, there is a positive correlation between average peer absence rate and student reading achievement. However, three subgroups, the high and low reading achievers, and low math achievers, appear to be less strongly impacted by peer attendance behavior than other students. Moreover, the linear combination tests suggest no significant effects of average peer absence rate on the reading scores of low income students or students with high reading abilities.

The heterogeneous peer effect models on both math and reading achievements reveal that peer influences do affect students differently depending on their backgrounds. Overall, the academic outcomes (in both math and reading) of female students are less impacted by school peer compositions. Both strong and weak ability students (measured by both math and reading skills) seem to be more strongly impacted by school racial and SES composition, as well as peer disciplinary infractions; and interestingly, both groups of students seem to be more negatively impacted by average school peer academic achievement.

Recall that the regression sample in this study is limited to lottery participants and the identification of peer effects relies on the differences between lottery losers in neighborhood schools and lottery winners in magnet programs. Overall, lottery participants attending neighborhood schools are superior (in terms of social economic status, prior achievement, and prior disciplinary records etc.) than other students in the same school, and are more likely to be grouped with other advantaged peers for classes. Especially for high performing lottery participants, because they are really advanced compared to other students in their neighborhood school, they may be placed with the best teacher, receive special instructions and more attention. This may less likely happen to high performing lottery participants in magnet schools because the overall peer quality is high. Similarly, even a student is at the bottom quartile of the prior achievement distribution among the lottery participants; he/she may still be a high performer in his neighborhood school and be assigned to classes with high quality peers and good teachers; while this again may not be true for the low performing lottery participants attending magnet programs. These are the possibilities that cause the large negative effect of peer achievement on students at both the top and bottom quartiles of prior achievement distribution. Given that students are often tracked within schools, high ability students are likely to be grouped with more advantaged students (e.g., fewer black students or low

SES students, and fewer students with behavioral problems), the conclusions from the classroom level may be different, which will be examined in next chapter.

Robustness Checks

Previous sections have shown that some of the estimated peer effects are quite large, which raises the question whether the estimates capture something other than the effect of peers. In this section, I am going to check the robustness of the peer effect estimates. In specific, there are three major problems as discussed in Chapter III. First, whether the estimated peer effects are confounded with some unobserved heterogeneity in treatment responses? Second, whether the estimates of peer effects capture some other unobserved school factors, especially teacher qualities? Third, whether the estimated peer effects are biased by sample attrition?

This section will address the above three problems in order. Although I did the robustness checks for all models related to the three research questions, in order to save some space, the following discussion will only focus on the average peer effect models on math achievement[32].

Heterogeneity in Treatment Response

Recall that in the average peer effect model (equation 3.11) the causal effect of peer group is identified from the interaction between the lottery based peer term $(P_M - \hat{P}_N)$ and the treatment indicator d_i. Therefore, the estimate of peer group effect could be confounded with unobserved heterogeneity in the treatment response ($\eta_i \, d_i$). It is hard to

[32] The conclusions from other models are similar to the average peer effect model on math achievement; and the results are available upon request.

directly test this hypothesis. However, controlling for the observable heterogeneity in treatment response can help to indirectly check whether the estimated peer effects are proxies for the treatment response heterogeneity. In specific, we are going to control for the interactions of the treatment indicators[33] (d_i) with all individual characteristics in the model, including prior achievement and behavioral outcomes. If controlling for all these observable treatment response heterogeneity in the regression leads to a diminished estimate of peer effect, then it indirectly proves that the estimated peer effect is in fact confounded with the way different individual respond to treatment.

Table 4.12 presents the estimates of average school peer effect models on math achievement, with the observed heterogeneity in treatment response controlled in the regression. As shown in the table, including the interaction of treatment indicator with individual variables in the model does not produce the expected changes in most of the estimates of peer effects, especially not for the widely expressed peer characteristics, such as peer race and SES composition, and peer academic achievement.

Some of the point estimates of peer effects become even larger than before (results in Table 4.5), e.g., percent black students and percent low SES students. Especially, the estimated effect of percent low SES students turns bigger and significant while controlling for the heterogeneous treatment response term. The estimates of peer academic ability measures (models 7and 8), and peer attendance behavior are also larger, but they are still insignificant.

The point estimates of variables like percent female students, percent special education students, and prior student discipline infractions, are a little smaller, but they

[33] There are two treatment indicators, one for the academic magnet, and the other for the non-academic composite.

still remain significant. Including treatment response heterogeneity only overturns the significant impact from one variable --- percent ELL students. The point estimate of percent ELL students is about 40% smaller than the estimate from the original model (model 6 in Table 4.5), and is not statistically significant.

Table 4.12: Average School Peer Effects on Math Achievement (Treatment Response Heterogeneity)

	Math Scores									
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10
Magnet School Effect										
Academic			Treatment indicators are interacted with individual characteristics							
Non-Academic Composite			Treatment indicators are interacted with individual characteristics							
Peer Effect (Lottery Based)										
Black (proportion)	-0.59***									
	(0.18)									
Low SES (proportion)		-0.41*								
		(0.18)								
Female (proportion)			-1.15*							
			(0.54)							
Hispanic (proportion)				-0.11						
				(0.63)						
Special ED (proportion)					2.33*					
					(0.98)					
ELL (proportion)						1.40				
						(1.22)				
Grade 4 Math (Mean)							0.22			
							(0.15)			
Grade 4 Reading (Mean)								0.20		
								(0.14)		
Grade 4 Suspension (Mean)									-0.04	
									(0.20)	
Grade 4 Absence (Mean)										3.85
										(4.58)
Peer Effects (Residence Based)										
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics										
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	11780	11780	11780	11780	11780	11780	11778	11778	11778	11778

* p<0.05, ** p<0.01, *** p<0.001

Overall, the results from Table 4.13 do not support that the estimates of average school peer effects are confounded with unobservable heterogeneity in treatment response.

Unobserved School Factors

The second problem is whether the estimates of peer effects pick up some other unobserved school characteristics, especially teacher qualities. A lot of research (e.g, Ingersoll 1999, 2004; Peske & Haycock, 2006) has shown that low quality teachers are more likely to be assigned to schools with high percent poor and minority students. In our sample, if the neighborhood schools that lottery losers must attend (those schools usually have higher percentage of black and low income students) tend to have less effective teachers, then the estimates of peer effects may just be proxies for the school teacher qualities.

To examine this possibility, I re-estimate the average peer effect models with teacher fixed effects included in the regression. Introducing teacher fixed effect into the model greatly reduces the variation in the data to estimate peer effects because the between-teacher differences in peer characteristics are absorbed by the estimates of teacher effect. Fortunately, we have 5 cohorts of students in the sample, which ensures a great within-teacher variation in peer characteristics. Also, the district was undergoing a school reassignment during the investigation years[34], which also contributes to a great variation in peer characteristics over the course of a teacher's career.

[34] The district moved from a court-ordered desegregation plan to a neighborhood-based school system during these years.

Table 4.13 presents the results from the teacher fixed effect models. Overall, the estimates of peer effects have larger standard errors, which are expected given that estimating teacher fixed effects requires a large amount of data. However, the new models do not find any evidence that the estimated peer effects in the original models are picking up the effects from school teachers. Instead, while teacher fixed effect is controlled in the regression, some estimates of peer effects are even larger than earlier, such as percent black students, percent special education peers and percent ELL students.

Table 4.13: Average School Peer Effects on Math Achievement (Teacher Fixed Effect)

	Math Scores									
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10
Magnet School Effect										
Academic			Treatment indicators are interacted with individual characteristics							
Non-Academic Composite			Treatment indicators are interacted with individual characteristics							
Peer Effect (Lottery Based)										
Black (proportion)	-1.47*									
	(0.63)									
Low SES (proportion)		-0.16								
		(0.69)								
Female (proportion)			-1.44							
			(1.11)							
Hispanic (proportion)				4.38*						
				(2.13)						
Special ED (proportion)					5.49**					
					(2.17)					
ELL (proportion)						7.22*				
						(3.52)				
Grade 4 Math (Mean)							-0.34			
							(0.74)			
Grade 4 Reading (Mean)								-0.34		
								(0.74)		
Grade 4 Suspension (Mean)									0.03	
									(0.51)	
Grade 4 Absence (Mean)										19.13
										(11.74)
Peer Effects (Residence Based)										
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics										
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	11764	11764	11764	11764	11764	11764	11762	11762	11762	11762

* p<0.05, ** p<0.01, *** p<0.001

In addition, I also implemented a set of school fixed effect models to check if the estimated peer effects are confounded with some unobserved time-invariant school factors, such as school policy and principal leadership. The school fixed effect model results are very similar to the results from the teacher fixed effect models, and do not support that the large estimates of peer effects are proxies for unobserved school factors.

Attrition Problems

All studies exploiting randomized experiment face a common problem --- attrition. In our data, there are two types of attrition: (1) students do not continue to enroll in the district schools in a year subsequent to the lottery --- they may switch to a private school, or move to another district; (2) students do not have spring test scores. Not all of the students in the second group are attritors for that they may still stay in the system --- there are other reasons that no test scores may be reported. However, since a student without test scores could not contribute to the estimation of peer effects, they pose the same problem as the attritors.

If attrition from the lottery winners is the same as from the lottery losers, it will not impact the difference in outcomes between these two groups and will not bias the estimates of treatment effect and peer effect. Unfortunately, as shown in table 4.14, the attrition rate of lottery winners is quite different from the rate of the lottery losers. The discrepancy is particularly pronounced for lottery participants in the academic magnet program --- losers are 50 percent more likely to leave the district than winners between 4^{th} and 5^{th} grade. The attrition rate of lottery losers continues to be larger than lottery winners in later grades. For non-academic composite lottery participants, although the attrition gap is smaller, lottery losers still have an attrition rate 40 percent higher than lottery winners in the year subsequent to the lottery.

Table 4.14 Attrition Rates, by Lottery Outcomes[1,2]

	Lotteries			
	Academic		Non-academic Composite	
	Winners	Losers	Winners	Losers
Leave the system after grade				
4	11.50%	17.45%	7.45%	10.58%
5	6.05%	12.01%	9.85%	6.57%
6	7.79%	9.94%	9.18%	6.23%
7	4.61%	8.20%	7.97%	6.02%
No spring Math scores in grade				
5	4.00%	6.16%	6.60%	3.14%
6	1.73%	2.46%	3.46%	2.37%
7	1.61%	2.50%	2.24%	1.51%
8	1.08%	1.79%	2.13%	2.49%

1.Counts only students who were in the system as 4th grade students when the lotteries were conducted and do not miss 4th grade math scores.
2. A student is defined as a lottery winner if he/she won the lottery outright on the lottery day or received the delayed offer in 5th grade.

The lower part of Table 4.14 shows the percentage of students missing math scores in the spring[35]. There is less significant difference between winners and losers who do not have spring test scores. In 5th grade, academic magnet lottery losers are more likely to miss math scores than lottery winners, and the pattern is opposite for the non academic composite lottery participants. The between group gap is even smaller in later grades.

[35] Students who miss spring test scores are different from the first type of attritor (those who left the district before a new school year started as indicated in the upper part of the table). They have records in the district attendance file and student file, but do not have the record of test scores in the state standardized tests. Although there are many reasons that test scores are not reported, many students may have left the system during the school year. For example, there are 337 academic magnet lottery participants who never enrolled in the system as 5th graders (they are considered as the first type of attritors); there are 100 participants who enrolled in district schools but were not present for testing (considered as students with missing scores). Among these 100 students, the majority of them had probably left the system prior to the test date, as 65% of them were never enrolled in 6th grade.

Therefore, I'll only focus on the first type of attritors (those who do not return to the system in the fall) in the following analysis.

Apparently, attrition is not random among lottery winners and lottery losers. Lottery outcomes do affect the decision whether to stay or leave the system. The problem is whether the correlation between attrition and student outcomes is systematically different between lottery winners and lottery losers. For example, if the lottery losers who left the system have families that are more concerned than the average about the quality of their children's schools (especially, more concerned about the peers in the school), attrition may introduce systematic differences in outcomes between treatment group and control group, and then bias the estimates of treatment effect and peer effect.

In this study, all regression models control for a large set of observed individual characteristics, including prior achievement and behavioral records, which greatly reduces attrition-induced bias[36] . However, if attrition is correlated with some unobservable variables, the differences between winners and losers may not be a simple linear function of the observed student characteristics, then even these controls are not enough to offset the bias caused by attrition. For example, if high achieving lottery losers are more likely to leave the system when they have to return to a neighborhood school that has a high percentage of disadvantaged peers, then attrition difference may bias the estimate of peer effect upwards by removing high achievers from schools that have a large population of minority and poor students.

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[36] To the extent that attrition-related outcome differences are a linear function of student demographic characteristics and 4th grade test scores and behavioral records, including these individual variables restore the balance between treatment and control groups.

To examine the attrition problem further, I estimate a model of attrition between fourth and fifth grades among the academic magnet participants.[37] The dependent variable is a dichotomous variable indicating attrition: 1 if the student left the system in 5th grade; 0 otherwise. The independent variables are mostly limited to the regressors already included in the achievement models, including lottery outcome indicator[38] and individual characteristics. If the results do not find a significant correlation between the attrition and the observable characteristics that predict achievement, it is less plausible that other unobservables with comparable predictive power could drive the decision of attrition. In addition, four variables as functions of peer characteristics are also included in the model to explore the possibility whether there is selective attrition among high achieving students when they have had to attend a school with disproportional minority and low income students. The four variables include: the school peer characteristics (percent black and percent low income students) a student will face if he/she loses the lottery (P_N in the above notation); the interaction of lottery outcome indicator with P_N; the interaction of individual prior math achievement with P_N; and a three way interaction of individual prior math scores, lottery outcome indicator, and P_N. Since the neighborhood school peer characteristics should not affect a student's attrition decision if he won the lottery, one would expect the estimate on the second term (the interaction of winning with P_N) to offset the effect of the stand-alone P_N. Moreover, if the previously described scenario is true, the coefficient on the interaction of prior math achievement with P_N should be positive, while the coefficient on the three way interaction are

[37] The discrepancy in attrition rate is more pronounced in the year subsequent to the lottery between lottery winners and lottery losers in the academic magnet lottery.
[38] Given that students have to make the private school enrollment decision in the spring, while the delayed win notice usually comes at the beginning of the fall semester, I only use the outright win indicator.

expected to be negative. Only two models are estimated--- one with the peer variable of percent black and the other with percent low SES students.[39]

Table 4.15 presents the results from the two attrition models. Given the numerous interactions included in the model, all coefficients have large standard errors and are very imprecise. While the point estimate of percent school black students is positive as expected, the estimate of percent low SES students is negative; the coefficient on winning the lottery is negative which offsets the impact from the stand-alone peer characteristics. However, the coefficients on the interaction of peer characteristics with prior math achievement are negative, which works opposite to the hypothesis that high achieving students are more likely to leave the system if they had to return to a neighborhood school with more disadvantaged students; the coefficients on the three way interaction are also unexpectedly positive.

[39] The reasons to choose these two peer variables include: (1) these are two school characteristics that are usually released to the public, such as posted on the district website. It is easier for parents to check school racial composition and SES composition than to check other school peer characteristics, such as average prior achievement, disciplinary records, or special education students. Therefore, they are likely to be the major factors influencing parents' decision on school enrollment. (2) these two peer variables are found significantly influencing student math achievement as shown in Table 4.5. Although the overall coefficient on average percent low SES peers is not significant in Table 4.5, the heterogeneous effect table (Table 4.10) shows that many groups of students are negatively impact by school low income population. (3) these are commonly expressed peer characteristics examined in the literature.

Table 4.15 Effects of Lottery Outcomes and Peer Characteristics on Attrition

	Attrition	
	Model 1	Model 2
Independent Variables		
Outright Winner	-0.43	-0.3
	(0.48)	(0.47)
Percent Black	0.32	
	(0.48)	
Percent Black * Winner	-0.38	
	(0.91)	
Percent Black * 4th grade math	-0.22	
	(0.35)	
Percent Black * 4th grade math*winner	0.02	
	(0.62)	
Percent low SES		-0.47
		(0.51)
Percent low SES * Winner		-0.74
		(0.96)
Percent LOW SES * 4th grade math		-0.16
		(0.37)
Percent LOW SES * 4th grade math*winner		0.49
		(0.66)
4th grade math scores	0.06	0.02
	(0.17)	(0.17)
4th grade math scores *winner	0.12	-0.07
	(0.29)	(0.29)
4th grade reading scores	0.05	0.04
	(0.06)	(0.07)
4th grade reading scores *winner	0.14	0.15
	(0.11)	(0.11)
Black	-0.49**	-0.44**
	(0.13)	(0.13)
Black*winner	0.13	0.08
	(0.24)	(0.23)
Low SES	-0.09	-0.02
	(0.13)	(0.13)
Low SES*winner	0.01	0.03
	(0.26)	(0.26)
Special Ed	-0.57	-0.62
	(0.53)	(0.54)
Special Ed*winner	0.52	0.58
	(0.79)	(0.79)
ESL	-0.33	-0.31
	(0.51)	(0.51)
ESL*winner	0.63	0.60
	(0.78)	(0.78)
Female	0.03	0.02
	(0.08)	(0.08)
Female*winner	0.10	0.07
	(0.15)	(0.15)
No. of Observation	2275	2275

* p<0.05, ** p<0.01, *** p<0.001

To make the interpretation of the regression results easier, I calculated the attrition probability for high performing students and low performing students[40] under different situations: lottery losers whose neighborhood schools have high proportion of unfavorable peers[41]; lottery losers with more favorable neighborhood school peers; lottery winners with more unfavorable peers; lottery winners with more favorable neighborhood peers.

Table 4.16 Attrition Probabilities as Function of Prior Achievement, Peer Characteristics and Lottery Outcomes (Outright Win, Academic Magnet)

	Percent Black			**Percent Low SES**		
Prior Achievement	High	Low	Difference	High	Low	Difference
Lottery Losers						
High	12.1%	13.0%	-0.9%	8.2%	16.0%	-7.8%
Low	13.7%	11.3%	2.4%	9.9%	14.8%	-4.9%
Difference	-1.6%	1.7%	-3.3%	-1.7%	1.2%	-2.9%
Lottery Winners						
High	9.1%	9.9%	-0.8%	5.5%	11.6%	-6.1%
Low	10.4%	8.5%	1.9%	6.8%	10.6%	-3.8%
Difference	-0.7%	1.4%	-2.7%	-1.3%	1.0%	-2.3%
Difference, Losers - Winners						
High	3.0%	3.1%		2.7%	4.4%	
Low	3.3%	2.6%		3.1%	4.2%	

The attrition probability results are reported in Table 4.16. Overall, high achievers are less likely to leave the system when their neighborhood school peers are unfavorable

[40] High performing students are those who scored one standard deviation above the sample mean in mathematics; low performing students score one standard deviation below the sample mean in math.
[41] A neighborhood school that has 75 percent black student or low SES students is defined as a school with unfavorable peers; a neighborhood school with 25 percent black or low SES students is coded as a school with favorable peers.

(column 1 and column 4); and low achievers are less likely to exit when the situation is reversed --- with more favorable neighborhood school peers (column 2 and column 5). This pattern holds for both lottery winners and lottery losers in both peer characteristics; and it works against finding large peer effects.

Under any scenario, lottery losers are more likely to leave the system. However, a more important problem is whether the attrition rate gap between high performing lottery losers and lottery winners is different from the gap between lottery losers and lottery winners who are low performers. Overall, the loser-winner difference is modest as reported in the bottom panel. When the neighborhood school peers are unfavorable, the attrition gap is larger for low achievers than for high achievers; the reverse is true when peers are favorable --- the loser-winner difference is bigger for high achievers than for low achievers. These findings again, do not support the hypothesis that differential attrition biases the estimates of peer effects upward by removing high performing lottery losers from schools serving high proportion minority and poor students.[42]

The final step to assess the attrition impact is to re-estimate the achievement model (with only two peer characteristics examined, percent Black and percent low SES students) using a weight option. Each observation is weighted by the inverse of the predicted probability that the student remains in the school system; therefore, the weighted sample resembles what the sample would be if no student had left the system. The stay-in-system probability (the inverse of the weight) equals 1 minus the attrition

[42] However, one thing should be mentioned that the linear combinations of the coefficients reported in Table 4.17 have large standard errors and overlapping confidence intervals, so we can not reject the hypothesis that high performing students do not differ from low performing students in attrition rate, regardless of the peers and lottery outcomes.

probability, which is calculated from a more general attrition model than the one shown in Table 4.15.[43]

The estimates of peer effects are quite similar to those in Model 1 and Model 2 of Table 4.6. The coefficient on percent Black from the weighted model is -0.53 (P<0.001), virtually identical to the coefficient (-0.49, P<0.001) in Table 4.6. The coefficient on percent low SES students from the weighted model is -0.43 (P<0.05), which is even larger than the one in original model (-0.33) and is weakly significant (at 5% level) in statistical sense.

To conclude, although there is significant attrition from our data, with more lottery losers leaving the system than lottery winners, there are no systematic differences between the remaining (or attrited) lottery losers and lottery winners in the correlation between all observed characteristics and achievement. Therefore, there is no evidence that attrition has biased the estimates of magnet school treatment effects and the peer group effects.

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[43] The attrition model is very similar to the one shown in Table 4.15, but includes lottery participants in both the academic magnet and the non-academic composite, and has separate equations for attrition after each grade. For instance, the attrition equation after grade 5 includes 5th grade test scores as additional controls; the one after grade 6 includes 6th grade achievement; the one after grade 7 includes 7th grade test scores.

CHAPTER V

PEER EFFECTS ON ACADEMIC ACHIEVEMENT ---RESULTS FROM CLASSROOM LEVEL ANALYSIS

This chapter explores how the classroom peer composition impacts student academic achievement in math and reading. Last chapter finds significant peer effects from some school peer characteristics, such as percent Black students, but no evidences that average school peer academic abilities or peer disciplinary records influence student achievement in middle school. Intuitively, it is expected that peer effects at the classroom level should be stronger than the school level peer effects since classroom is the place where instruction happens and peer interactions in academics mostly take place. In this chapter, I am going to examine how classroom peers influence student achievement and whether the magnitude of classroom peer effects is larger than that of school peer effects.

As discussed in Chapter III, identifying classroom peer effects poses more econometrical challenges, including selection bias arising from the non-random classroom placements and omitted variable bias which is likely to be stronger at the classroom level. This section overcomes these methodological challenges by exploiting the admission lotteries that randomly assign students to a magnet school or a neighborhood school. In particular, each student in both the treatment group and the control group is assigned a predicted value of counterfactual classroom peers, which is close to the class peers that a student would encounter if he had been in the opposite situation. The prediction of the counterfactual peer characteristics utilizes a rich set of information including lottery outcomes, individual characteristics, prior outcomes, and 4^{th}

grade schools. The exogenous estimates of the classroom peer effects on student achievement can then be derived from the differences between the actual classroom peer characteristics and the counterfactual classroom peer characteristics.

The descriptive statistics of classroom peer characteristics has already been reported in Chapter III; therefore this chapter will skip the section of descriptive results. The first three sections in this chapter are organized to answer the three research questions on: (1) average classroom peer effects on student achievement; (2) the impact from dispersion of classroom peer characteristics; (3) the heterogeneous classroom peer effects. The fourth section checks the robustness of the estimates with a focus on whether the estimated classroom peer effects are proxies for classroom instructors. The final section discusses the differences between school level peer effects and classroom level peer effects.

Impacts from Average Classroom Peer Characteristics

The model used to estimate average classroom peer effect is equation 3.17. The major regressor of interest is P_{cij}, the actual classroom peer characteristics. To deal with he endogeneity of the classroom peer variable, I constructed an instrumental variable consisting of the enrollment probability and counterfactual class peer characteristics. Other important variables included in the equation are the magnet school treatment indicators, residence based school peer characteristics[44], and individual characteristics (including prior outcomes).

The majority of middle school students usually rotate through classrooms for different subjects, so the achievement models in this chapter estimate subject-specific

[44] The residence based school peer characteristics are included in the model in order to control for the correlation between family residence choice and achievement.

peer effects on student academic achievement. The peer variables used in the math achievement model are the average peer characteristics of one's math classes; the variables used in the reading achievement model are the average peer values of the reading/language classes.[45,46]

Math Achievement

Table 5.1 presents the estimates of classroom peer effects on math achievement. Only coefficients on the lottery based classroom peer variables and the magnet school treatment indicators are reported in the table.[47] Model 1 and model 2 suggest that student math achievement is negatively impacted by classroom percent Black students and percent low income students. The estimates of peer effects from these two classroom peer variables are quite large: the average math score decreases by 0.33 points in standardized score if the classroom black students increase from 25% to 75%; and a 50 percent point change in classroom low income students is related to 0.4 point change in standardized math scores. Moreover, the treatment effect of the academic magnet school turns to negative (significant at 5% level) when classroom percent low income student is controlled. This implies that a student would do even better in his neighborhood school if he attends a class in the neighborhood school that has the same percent of low income peers as his class in the academic magnet school.

[45] If a student took multiple classes in one subject, the peer variable is an average value across all courses in this subject.
[46] Schools have different titles for reading and language classes, for example, the same class may be titled Reading in some school, but as Language Arts (or English) in other schools. Therefore, we coded the class as reading class if the course id starts with ENG (the district course id for reading/language arts classes), but excludes some selective classes such as Creative Writing or Theater.
[47] The coefficients on student characteristics are not included in the table because they are quite similar to previous results. The coefficients on majority of the residence based school peer characteristics are quite small and insignificant; and the conclusions on these estimates are similar to those from the school level models.

Table 5.1 Average Classroom Peer Effects on Math Achievement (Math Class)

	Math Scores										
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
Magnet School Effect											
Academic	0.00	-0.09*	0.12**	0.09*	0.09*	0.21***	-0.09	-0.19**	0.08*	0.07	-0.08
	(0.03)	(0.05)	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)	(0.05)	(0.04)	(0.05)	(0.07)
Non-Academic Composite	0.11	0.02	0.13	0.11	0.09	0.18	0.06	0.00	0.11	0.06	0.02
	(0.08)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.09)	(0.10)	(0.11)
Peer Effect (Lottery Based)											
Black (proportion)	-0.66***										-0.08
	(0.09)										(0.43)
Low SES (proportion)		-0.81***									-0.49
		(0.10)									(0.33)
Female (proportion)			-0.17								-0.00
			(0.25)								(0.26)
Hispanic (proportion)				-0.88							-2.28*
				(0.70)							(1.07)
Special ED (proportion)					-0.87*						-0.43
					(0.34)						(0.52)
ELL (proportion)						2.82*					4.47
						(1.29)					(2.45)
Grade 4 Math (Mean)							0.32***				0.03
							(0.05)				(0.22)
Grade 4 Reading (Mean)								0.40***			0.15
								(0.05)			(0.14)
Grade 4 Suspension (Mean)									-0.65***		0.24
									(0.20)		(0.30)
Grade 4 Absence (Mean)										-6.73	1.99
										(4.05)	(6.85)
Peer Effects (Residence Based)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Over-identification Test	0.77	0.75	0.68	0.69	0.73	0.62	0.8	0.85	0.54	0.69	0.36
Sample Size[48]	10224	10224	10224	10224	10224	10224	10224	10224	10224	10224	10224

* p<0.05, ** p<0.01, *** p<0.001

[48] The sample size for classroom level analysis is smaller than that for school level analysis is due to two reasons: (1) Some lottery participants are not included in the course file so we are not able to construct classroom peer variables for them. (2) The academic magnet school did not report course information during school years 1999-2003. We are able to use homeroom number to calculate class peer variables for students in this school. However, due to the tracking policy in later grades, homeroom peer characteristics are quite different from the true class peer characteristics in grades 7 and 8. After consulting with the school former principle, we decided to include only 5^{th} and 6^{th} graders in school year 2000 to 2003 in the regression sample. Therefore, we exclude lottery participant observations from the following cohort: lottery year 1999 participants (7^{th} graders in 2002 and 8^{th} graders in 2003) and lottery year 2000 participants (8^{th} graders in 2003).

The third significant finding is on classroom percent special education students (Model 5), which is found significantly decreasing student math scores in middle school. For instance, a 10 point percentage change in classroom percent special education students is associated with a 0.09 point standardized score change in math achievement. The estimated effect is quite large. Note that students eligible for special education programs are often placed in the same class to meet their needs: in our data, the average class percent special education peers is 0.33 for lottery participants who are also identified as special education; and it is only 0.03 for non special education lottery participants. The problem then is whether the negative impact of classroom percent special education peers mainly works on those who are identified as special education students. This will be addressed in the third section of heterogeneous peer effects. Also note that the estimate of percent special education peers at the classroom level is opposite to the estimate at school level which shows positive impact on student achievement. The difference between the classroom estimate and school level estimate will also be discussed later.

Model 6 in Table 5.1 finds that student math achievement tends to increase if there are more ELL students in their class. The estimated effect is substantial: if the classroom percent ELL students increases by 5% (one standard deviation of this peer variable in our sample), the average math score improves by 0.14 standardized score. Interestingly, the estimate of percent ELL students at the classroom level is very close to the estimate at the school level, both indicating a positive correlation between student math scores and percent ELL peers, at least in our sample.

The two peer academic ability variables, measured by average 4th grade math and reading scores, have shown significant and positive impact on individual math achievement in middle school. The sizes of the estimated effects from both peer academic ability indicators are substantial. For example, a student's math score in middle school improves by 0.32 points in standardized scores if the average class prior math achievement increases by 1 point (also in standardized scores); the estimated effect of average class reading achievement is even larger. Also note that the treatment effect of the academic magnet school is negative when the academic quality of class peers is controlled. Model 8 finds a coefficient of -0.19 ($P<0.01$) on the academic magnet treatment indicator, which suggests that lottery losers of the academic magnet school would perform 0.19 standardized score higher in mathematics than the winners enrolling in the academic program if the average class peer prior reading achievement in the neighborhood school is the same as the academic magnet school. This implies that once the classroom peer academic ability is controlled, students enrolled in the academic magnet program are actually worse off in academics than their counterpart students in neighborhood schools.

The result from Model 9 shows that average classroom peer disciplinary infractions greatly decrease student math achievement in middle school. The coefficient on this variable is -0.65 ($P<0.001$), implying that if half of the peers in the same class received one more suspension in 4th grade, the average math score will decrease by 0.33 standardized score --- the effect is very large.

The classroom level analyses do not find significant impacts from percent Female students, percent Hispanic students, and peer attendance behaviors. In column 11, all specifications of peer characteristics are incorporated in one model. Due to the collinearity of all these variables[49], the estimated effects of most peer variables are no longer significant. Only the variable percent Hispanic student shows weakly significant and negative effect in the full variable model. Moreover, with all peer characteristics included in one equation, neither of the two magnet programs is found influencing student achievement --- the coefficient on the treatment indicator is negative for the academic magnet. Again, the findings suggest that once we control for the class peer composition, the magnet program is no more successful than the regular public schools in this district, at least not in improving student math achievement.

Reading Achievement

Table 5.2 reports the regression results of classroom peer effects on student reading achievement. Most estimates of peer effects on reading achievement are similar to the estimates from the math achievement models. First, classroom race composition and SES composition have strong and negative impact on student reading scores as shown in model 1 and model 2. Second, peer academic abilities (model 7 and model 8), measured by average peer math and reading scores in 4^{th} grade, significantly improve individual reading achievement in middle school. Third, average class peer disciplinary behavior has shown strong and negative influence on student reading score. Interestingly, the point estimate of the peer behavior effect on reading achievement (0.95) is almost 50% larger than the estimated effect on math achievement (0.65): if every peer in the class received 1

[49] Given the ability grouping policies in many schools, it is likely that the classroom peer characteristics are highly correlated.

more suspension in 4th grade, the average class reading score decreases almost 1 point in standardized score. [50]

Table 5.2 Average Classroom Peer Effects on Reading Achievement (Reading Class)

	Reading Scores										
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
Magnet School Effect											
Academic	0.02	-0.07	0.13***	0.11**	0.11**	0.10	-0.04	-0.11*	0.06	0.12*	-0.06
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.05)	(0.03)	(0.06)	(0.09)
Non-Academic Composite	0.05	-0.02	0.07	0.06	0.06	0.06	0.01	-0.02	0.04	0.06	0.01
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)	(0.08)	(0.08)	(0.08)	(0.11)	(0.12)
Peer Effect (Lottery Based)											
Black (proportion)	-0.49***										-0.18
	(0.10)										(0.28)
Low SES (proportion)		-0.71***									-0.32
		(0.10)									(0.27)
Female (proportion)			-0.32								-0.34
			(0.25)								(0.26)
Hispanic (proportion)				-0.67							0.08
				(0.85)							(1.48)
Special ED (proportion)					-0.16						-0.11
					(0.48)						(0.67)
ELL (proportion)						-1.13					-1.85
						(1.90)					(3.10)
Grade 4 Math (Mean)							0.23**				-0.24
							(0.07)				-0.21
Grade 4 Reading (Mean)								0.30***			0.22
								(0.05)			(0.20)
Grade 4 Suspension (Mean)									-0.93***		-0.55*
									(0.18)		(0.25)
Grade 4 Absence (Mean)										-0.54	0.90
										(6.01)	(8.77)
Peer Effects (Residence Based)											
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics											
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Over-identification Test	0.7	0.57	0.81	0.79	0.83	0.81	0.72	0.6	0.74	0.81	0.62
Sample Size	10246	10246	10246	10246	10246	10246	10246	10246	10246	10246	10246

* p<0.05, ** p<0.01, *** p<0.001

[50] The vast majority students in our sample took multiple reading/language classes, but only one (15% of the students took two) math class. The reading class peer characteristics assigned to one student is an averaged value across all the reading/language courses, so one point change in the average reading class peer disciplinary infraction usually means that the student has encountered more misbehaving peers in classes than one point change of average disciplinary infraction in math class. Therefore, it is not surprising the estimate of reading class peer behavior problem is larger.

Other peer variables, including percent special education students and percent ELL classmates (they have shown significant impact on math achievement), are not found significantly associated with student reading achievement. When all peer characteristics are included in one model (Column 11), only the estimate of average peer disciplinary infraction still remains strong and negative. Again, when the classroom peer composition is controlled, neither of the magnet programs has shown any positive impact on student reading achievement.

All models from both Table 5.1 and Table 5.2 are over-identified. The omnibus over-identification tests suggest that the instruments (for both the treatment indicators and the peer variables) in both math and reading achievement models are exogenous.

Impacts from Dispersion of Peer Characteristics

Ability tracking in schools has long been a controversial topic. Supporters of within school tracking argue that putting similar students in classes can improve instructional efficiency, while opponents argue that ability grouping would essentially harm low-ability students by isolating them from peers with high ability and high motivations. In this section, I am going to use the classroom data to examine how the heterogeneity of classroom peer composition influences student academic achievement --- whether students benefit from a more homogeneous class setting or they perform better in a class with students at various levels in academic qualities.

Table 5.3 presents some descriptive statistics on the peer composition heterogeneity variables, measured by the standard deviations in peer math and reading achievements, peer disciplinary infractions, and peer absence rates. Overall, the classroom peer academic qualities (measured by prior math and reading achievement) are more dispersed in 5th grade than in 8th grade, which reflects that students in higher grades are more likely to be tracked based on their ability. Enrollees in the academic magnet have more homogeneous classes than the lottery losers enrolled in neighborhood schools; however, the heterogeneity gap is very small in 8th grade. The same pattern is also applied to the lottery participants of the non-academic composite, but with a smaller enrollee-loser gap.

There is a large gap between enrollees and losers in the heterogeneity of peer disciplinary infractions in both 5th and 8th grades. However, note that the average value of class peer disciplinary records is highly correlated with the dispersion value in our data.[51] Therefore, although many schools may also group students for instruction based on their behavioral problems (note that the mean variance is smaller in 8th grade), the heterogeneity of peer disciplinary behavior is still much larger in neighborhood school classes because the average value is bigger.

Finally, there is no significant difference in peer attendance behavior (measured by prior absence rate) between the enrollees and losers, or across grades.

[51] The correlation between the mean value and the standard deviation in peer disciplinary infractions is 0.90 for both math and reading classes in our sample.

Table 5.3 Heterogeneity of Classroom Peer Characteristics[1]

	Standard Deviation in G4 Math				Standard Deviation in G4 Reading				Standard Deviation in G4 Suspension				Standard Deviation in G4 Absence			
	G5		G8		G5		G8		G5		G8		G5		G8	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Math Class																
Academic																
Participants	0.72	0.18	0.60	0.14	0.74	0.18	0.65	0.16	0.23	0.32	0.14	0.25	0.03	0.01	0.03	0.01
Enrollees	0.66	0.12	0.59	0.12	0.65	0.10	0.64	0.12	0.13	0.17	0.06	0.11	0.03	0.01	0.03	0.01
Losers[2]	0.79	0.20	0.61	0.18	0.82	0.20	0.65	0.15	0.32	0.41	0.18	0.32	0.03	0.01	0.03	0.01
Non-Academic Composite																
Participants	0.73	0.19	0.60	0.16	0.75	0.20	0.65	0.17	0.30	0.36	0.27	0.36	0.03	0.01	0.03	0.01
Enrollees	0.71	0.18	0.59	0.15	0.75	0.20	0.64	0.15	0.29	0.31	0.27	0.31	0.03	0.01	0.03	0.01
Losers[2]	0.78	0.18	0.59	0.17	0.82	0.17	0.64	0.17	0.45	0.44	0.29	0.42	0.03	0.01	0.03	0.01
Reading Class																
Academic																
Participants	0.72	0.17	0.65	0.14	0.72	0.18	0.65	0.14	0.22	0.31	0.16	0.27	0.03	0.01	0.03	0.01
Enrollees	0.66	0.12	0.64	0.14	0.65	0.10	0.66	0.13	0.13	0.17	0.06	0.11	0.03	0.01	0.03	0.01
Losers[2]	0.78	0.18	0.67	0.18	0.78	0.21	0.66	0.17	0.30	0.40	0.23	0.35	0.03	0.01	0.03	0.01
Non-Academic Composite																
Participants	0.73	0.19	0.65	0.14	0.74	0.19	0.64	0.15	0.29	0.34	0.27	0.33	0.03	0.01	0.03	0.01
Enrollees	0.72	0.17	0.63	0.12	0.74	0.19	0.63	0.13	0.28	0.29	0.26	0.28	0.03	0.01	0.03	0.01
Losers[2]	0.78	0.18	0.65	0.15	0.19	0.18	0.65	0.14	0.40	0.40	0.33	0.38	0.03	0.01	0.03	0.01

1.Counted only lottery participants with non-missing test scores in 4th grade.

2. Students who lost all lotteries (neither as outright winner nor delayed winner by the start of 5th grade).

Math Achievement

The model estimating the impacts of the dispersion in classroom peer composition is equation 3.16, wherein the regressors of interest include both the average term and the dispersion term of peer outcome variables. Regression results are reported in Table 5.4. Overall, there is no evidence that the heterogeneity of classroom peer characteristics influence student math achievement in middle school. First, results in model 1 and model 2 suggest that although students tend to have higher math scores when they are in classes with high performing peers, the variation in peer academic qualities does not show significant impacts on student math achievement. Second, controlling for the heterogeneity of peer disciplinary infractions overturns the strong impact from the average discipline measures, even though the coefficient on the dispersion term is not significant. This is due to the high collinearity between the average term and the heterogeneity term, which causes big standard errors. However, the linear combination test on both the average term and the standard deviation term still finds a significantly negative coefficient, which indicates that students do perform worse in a class with more disruptive peers. Third, no evidence shows that either the mean or the variation of class peer absence rate has significant impact on student math achievement in middle school.

Table 5.4 Impacts from Dispersion of Peer Characteristics on Math Achievement (Math Class)

	Math Scores			
	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	Yes	Yes	Yes
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	0.31***			
	(0.05)			
Grade 4 Reading (Mean)		0.41***		
		(0.05)		
Grade 4 Suspension (Mean)			-0.61	
			(0.69)	
Grade 4 Absence (Mean)				-11.79
				(9.33)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	-0.22			
	(0.21)			
Grade 4 Reading (Standard Deviation)		0.22		
		(0.37)		
Grade 4 Suspension (Standard Deviation)			-0.01	
			(0.31)	
Grade 4 Absence (Standard Deviation)				5.17
				(6.52)
Peer Effects (Residence Based)				
	Yes	Yes	Yes	Yes
Student Characteristics				
	Yes	Yes	Yes	Yes
P-value for Over-identification Test	0.78	0.88	0.52	71
Sample Size	10215	10215	10215	10215

* p<0.05, ** p<0.01, *** p<0.001

Reading Achievement

Table 5.5 presents the estimated effects of the heterogeneity of classroom peer characteristics on reading achievement. The regression results are very similar to those from the math achievement models. One exception is from Model 3, wherein the estimate of average peer disciplinary infractions increases while controlling for dispersion. This is also caused by the high collinearity between the average term and the dispersion term in peer disciplinary records --- when the positive effect of the dispersion value (although it is not significant) is controlled, the negative impact of the average term rises.

Table 5.5 Impacts from Dispersion of Peer Characteristics on Reading Achievement (Reading Class)

	reading Scores			
	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	Yes	Yes	Yes
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	0.21***			
	(0.06)			
Grade 4 Reading (Mean)		0.30***		
		(0.05)		
Grade 4 Suspension (Mean)			-1.57**	
			(0.58)	
Grade 4 Absence (Mean)				1.08
				(13.77)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	-0.27			
	(0.20)			
Grade 4 Reading (Standard Deviation)		-0.01		
		(0.25)		
Grade 4 Suspension (Standard Deviation)			0.31	
			(0.23)	
Grade 4 Absence (Standard Deviation)				-2.03
				(8.62)
Peer Effects (Residence Based)				
	Yes	Yes	Yes	Yes
Student Characteristics				
	Yes	Yes	Yes	Yes
P-value for Over-identification Test	0.73	0.58	0.66	0.79
Sample Size	10243	10243	10243	10243

* p<0.05, ** p<0.01, *** p<0.001

Overall, the results from both the math and reading achievement models reveal little evidence that the heterogeneity of class peers (measured by the standard deviation in peer achievement levels and behavioral records) affect individual academic outcomes. While this finding implies that ability tracking per se has little effect on average achievement in our sample, another question emerges: how are students at different academic levels impacted by class grouping? For example, if high ability students benefit but low ability students suffer from being in an ability grouping setting, it is likely to get a zero net effect. Therefore, it is important to explore the differential effects of classroom peer heterogeneity.

Heterogeneous Peer Effects

This section examines whether students from different background (race or SES groups) or at different achievement levels are influenced by classroom peer composition uniformly. The model estimating the heterogeneous class peer effects is equation 3.18, which includes two sets of regressors of interest --- the average peer characteristics and the interaction of peer characteristics with individual variables.

Math Achievement

Table 5.6 reports the estimates of heterogeneous classroom peer effect models on math achievement. Same as Table 4.12 in Chapter IV, Table 5.5 only presents 6 peer variables interacted with 7 individual characteristics.

The first peer variable reported in the table is average percent black students. While model 1 and model 2 do not find that Black students or low income students are differently impacted by percent black classmates, model 3 suggests that the negative impact of having more black peers is stronger on boys: the coefficient is -0.75 ($P<0.001$) for boys and 0.58 for girls (linear combination coefficient, $P<0.001$). Model 4 to Model 7 suggest that both high and low performing students (depending on their initial position in achievement distribution among lottery participants) appear to be more negatively impacted by class Black population; and the negative impact is even stronger on students with high prior achievement. For example, model 7 finds that the estimated effect of percent black classmates is 30% stronger on students who are in the top quartile of math achievement distribution.

Table 5.6 Heterogeneous Class Level Peer Effects on Math Achievement (Math Class)

	Math Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Percent Black Students							
Mean Effect	-0.69***	-0.68***	-0.75***	-0.64***	-0.63***	-0.61***	-0.62***
	(0.10)	(0.09)	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)
Heterogeneous Effect	0.06	0.10	0.17**	-0.08*	-0.14*	-0.17***	-0.22***
	(0.08)	(0.08)	(0.06)	(0.03)	(0.06)	(0.03)	(0.06)
Percent Low SES Students							
Mean Effect	-0.81***	-0.84***	-0.93***	-0.78***	-0.78***	-0.73***	-0.78***
	(0.11)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Heterogeneous Effect	-0.01	0.11	0.23**	-0.09*	-0.17*	-0.20***	-0.15*
	(0.09)	(0.11)	(0.07)	(0.04)	(0.07)	(0.04)	(0.07)
Peer Prior Math Achievement							
Mean Effect	0.31***	0.32***	0.35***	0.33***	0.34***	0.33***	0.35***
	(0.06)	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)	(0.05)
Heterogeneous Effect	0.06	0.04	-0.05	-0.03	-0.08**	-0.04	-0.10***
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Peer Prior Reading Achievement							
Mean Effect	0.39***	0.40***	0.43***	0.41***	0.41***	0.41***	0.42***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Heterogeneous Effect	0.02	0.00	-0.05	-0.05*	-0.05*	-0.05*	-0.08***
	(0.03)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
Peer Prior Disciplinary Records							
Mean Effect	-0.86***	-0.72**	-0.70**	-0.58**	-0.65**	-0.47*	-0.62**
	(0.24)	(0.27)	(0.23)	(0.22)	(0.20)	(0.23)	(0.21)
Heterogeneous Effect	0.42	0.13	0.10	-0.19	-0.06	-0.46**	-0.20
	(0.25)	(0.27)	(0.23)	(0.17)	(0.37)	(0.17)	(0.31)
Peer Prior Attendance Records							
Mean Effect	-7.25*	-8.97*	-7.78*	-6.34	-6.25	-5.62	-5.48
	(3.57)	(4.08)	(3.53)	(4.03)	(4.08)	(4.02)	(4.10)
Heterogeneous Effect	2.06	8.28	2.13	-2.32***	-2.69***	-3.44***	-3.21***
	(6.13)	(5.78)	(4.68)	(0.61)	(0.76)	(0.59)	(0.73)

* p<0.05, ** p<0.01, *** p<0.001

The second peer variable is percent low income students. Similarly, the first two models do not find significant differential effect of this variable on Blacks or low income students; but model 3 suggests that female students are less impacted by class low income peers than male students. Heterogeneous effect of percent class low income students is also found in models 4 to 7 --- both high and low performing students are more negatively impact by percent low income classmates. The linear combination of the coefficients on the mean term and the interaction term is 0.93 for students with high prior math achievement (model 7), which is about 20% larger than the coefficient on other students. The estimated effect of percent low income class peers is very substantial for boys and high performing students: increasing the class low income peers from 25% to 75% is associated with a 0.47 point (standardized scores) fall in math achievement.

The next two peer variables are average class peer math and reading test scores in 4th grade. The coefficients on the main effect terms of two peer variables are positive and significant for all 7 models. The first two models reveal little evidence of heterogeneous effects on black or low income students. Although high performing students (in two subjects, as shown in column 5 and column 7) appear to be less strongly influenced by average class peer achievement, the linear combinations of both coefficients are still positive and statistically significant.

The fifth class peer variable is peer disciplinary infractions (average class peer suspensions in 4th grade). Overall, average class peer disciplinary infractions exert very strong and negative impact on student math scores. The differential effect is only significant from model 6 --- low math achievers are much more strongly influenced by peer disciplinary problems. The linear combination test finds a coefficient of 0.92

(P<0.001) on students at the bottom quartile of math achievement distribution, doubling the estimate of average peer disciplinary behavior effect on other students (0.47, P<0.05).

The last peer variable is average class peer absence rate. The estimate of class peer attendance behavior is not found significant from the average model (Model 10 in Table 5.1); however, Table 5.5 suggests that some groups of students tend to score lower if the class peers have high absence rate in 4th grade. For example, the negative effects of average peer absence rate are significant (P<0.05) on non-black students, non-low-income students, and male students, with coefficients slightly higher than the overall estimate (-6.60, model 10 in Table 5.1). Moreover, both the low achievers and higher achievers (in both subjects) appear to be more negatively impacted by peer absence rate than other students. The estimated effect of class peer absence rate is about -8.5 (P<0.05) for students at the two ends of achievement distributions. This implies that if the class peers were absent from schools for 2 more days (with an absence rate of 1.14% in a school year of 175 days), the average math achievement of high or low performing students falls by 0.1 point of standardized scores.

Although it is not reported in the table, the heterogeneous effects of two other peer variables are worth mention. The first one is percent special education students, which significantly decreases average student math scores (-0.88, P<0.05; model 5 in Table 5.1). The heterogeneous effect model finds that the coefficient on the mean effect term is -0.46 (for non-special education students), and the coefficient on the interaction term (percent special education * special education indicator) is -0.57. Although neither of the coefficients is statistically significant, the linear combination coefficient is -1.03 (P<0.001), which suggests that the negative effect of percent special education peers mainly works on special education students themselves. The other peer variable is percent class ELL students, which has shown a positive impact on student math scores (2.83, $P<0.05$; model 6 in Table 5.1). Interestingly, the positive influence of having more ELL classmates, however, mainly woks on non-ELL students: the coefficient on the average term is 3.16 ($P<0.05$); and the coefficient on the interaction of the peer variable with ELL indicator is -12.96, but is not statistically significant at 5% level.

Moreover, I also ran a set of models to check if there are differential effects from the heterogeneity of classroom peer characteristics on students from various backgrounds. In order to examine this possibility, I add the interaction of the standard deviation of peer achievement (in both math and reading) with individual characteristics to equation 3.16. Therefore, there are three regressors of interest in the model: average class peer achievement, standard deviation of class peer achievement, and the interaction of the standard deviation with individual characteristics.

Table 5.7 presents the differential effects of classroom peer achievement heterogeneity on student math achievement. The first peer variable is the variation of class peer math achievement. The results suggest that some students are impacted differently by the heterogeneity of classroom peer math achievement. First, Black students and low income students appear to be more negatively impacted by the heterogeneity of class peer achievement. The linear combination of the coefficients on the standard deviation term and the interaction term is -0.75 ($P<0.05$) for low income students, which implies that low income students would have better math performance in a more homogeneous class. Second, the negative impacts from the variation in class peer achievement seem to be stronger on both high and low performing students than on other students in the middle of the achievement distribution. However, the linear combination tests from model 4 to model 7 find little evidence that the heterogeneity of peer achievement influence the math achievement of students from either the bottom or the top achievement quartiles. Finally, the conclusions from the models of classroom peer reading achievement are very similar to those from the math achievement models, suggesting that the heterogeneity of peer reading achievement has a stronger negative impact on black and low income students.

Table 5.7 Differential Effect from Dispersion of Class Peer Achievement on Math Achievement (Math Class)

	Math Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
Peer Effects (Lottery Based)	**Black**	**LSES**	**Female**	**Low Reading achiever**	**High Reading Achiever**	**Low Math achiever**	**High Math Achiever**
Peer Prior Math Achievement							
Mean	0.32***	0.32***	0.31***	0.32***	0.31***	0.32***	0.31***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Standard deviation	-0.02	-0.10	-0.14	-0.20	-0.20	-0.12	-0.18
	(0.20)	(0.21)	(0.27)	(0.21)	(0.21)	(0.21)	(0.21)
Standard deviation * individual variables	-0.54*	-0.65*	-0.13	-0.11***	-0.09*	-0.19***	-0.10*
	(0.24)	(0.27)	(0.19)	(0.03)	(0.03)	(0.03)	(0.04)
Peer Prior Reading Achievement							
Mean	0.44***	0.41***	0.40***	0.41***	0.40***	0.41***	0.40***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Standard deviation	0.61	0.52	0.22	0.25	0.24	0.28	0.22
	(0.31)	(0.37)	(0.59)	(0.38)	(0.38)	(0.37)	(0.37)
Standard deviation * individual variables	-1.18	-1.46*	-0.00	-0.12***	-0.06	-0.18***	-0.10**
	(0.63)	(0.60)	(0.44)	(0.03)	(0.04)	(0.03)	(0.03)

* p<0.05, ** p<0.01, *** p<0.001

Reading Achievement

Estimates of heterogeneous effect of average classroom peer characteristics on student reading achievement are reported in Table 5.8. Many of the estimates are quite similar to those on math achievement. First, there is no significant differential effect of class racial or SES composition on the reading achievement of Blacks or low income students (model 1 and model 2); and the negative impacts from both peer variables are only 70% as great on female students as compared to males. Second, heterogeneous effects of percent black or low SES classmates are significant on both low and high reading achievers. Especially for students with high prior reading scores (model 5), the magnitude of the estimated effect of percent black peers is double the effect on other students.

Heterogeneous effects of average peer academic qualities (measured by peer achievement in math and reading) are only found significant from model 4 and model 5. The results suggest that students at both the bottom and the top quartiles of reading achievement distribution are less likely to be impacted by average classmate academic abilities, although the linear combination coefficients still suggest positive influence from average class peer achievement on both groups.

The fifth peer variable is average peer disciplinary records. Model 1 shows that the average reading scores for black students tend to decrease more with the same change in peer prior suspension records. Moreover, the negative impacts are much stronger on both high and low reading achievers. Especially for students at the top reading achievement quartile, the linear combination test suggests that the average reading score for these students would drop by 1.6 point of standardized scores if every student in the class received 1 more suspension in 4th grade, which almost doubles the estimated effect on other students below the 75th percentile of prior reading achievement.

Table 5.8 Heterogeneous Class Peer Effects on Reading Achievement (Reading Class)

	Reading Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading achiever**	**High Reading Achiever**	**Low Math achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Percent Black Students							
Mean Effect	-0.48***	-0.49***	-0.59***	-0.43***	-0.39***	-0.47***	-0.49***
	(0.11)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Heterogeneous Effect	-0.02	0.01	0.19**	-0.19***	-0.44***	-0.07*	-0.03
	(0.08)	(0.08)	(0.06)	(0.03)	(0.05)	(0.03)	(0.05)
Percent Low SES Students							
Mean Effect	-0.68***	-0.70***	-0.88***	-0.62***	-0.60***	-0.67***	-0.72***
	(0.11)	(0.11)	(0.11)	(0.10)	(0.10)	(0.10)	(0.10)
Heterogeneous Effect	-0.06	-0.01	0.33***	-0.23***	-0.48***	-0.09*	0.09
	(0.08)	(0.10)	(0.07)	(0.04)	(0.07)	(0.04)	(0.06)
Peer Prior Math Achievement							
Mean Effect	0.21**	0.23**	0.25***	0.25**	0.24**	0.22**	0.24**
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Heterogeneous Effect	0.08	0.02	-0.05	-0.08*	-0.14***	0.02	-0.04
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Peer Prior Reading Achievement							
Mean Effect	0.28***	0.30***	0.32***	0.33**	0.31***	0.30***	0.31***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Heterogeneous Effect	0.07*	0.01	-0.03	-0.09***	-0.11***	0.01	-0.02
	(0.03)	(0.40)	(0.03)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Disciplinary Records							
Mean Effect	-0.64**	-0.91***	-1.04***	-0.57**	-0.82***	-0.83***	-0.95***
	(0.22)	(0.23)	(0.20)	(0.21)	(0.18)	(0.22)	(0.18)
Heterogeneous Effect	-0.55*	-0.02	0.25	-0.76***	-0.76*	-0.20	0.11
	(0.22)	(0.23)	(0.20)	(0.15)	(0.30)	(0.16)	(0.26)
Peer Prior Attendance Records							
Mean Effect	0.69	1.59	-4.54	0.75	0.42	0.05	-0.41
	(4.90)	(6.03)	(4.56)	(5.99)	(6.05)	(6.05)	(6.09)
Heterogeneous Effect	-2.37	-7.14	8.03	-4.02***	-5.17***	-1.29*	-0.43
	(7.67)	(8.02)	(6.26)	(0.58)	(0.75)	(0.58)	(0.70)

* p<0.05, ** p<0.01, *** p<0.001

Finally, heterogeneous effects of average peer absence rate are only found significant on students at the top and bottom quartiles of reading achievement distribution. However, the linear combination tests do not suggest significant effect of peer absence rate on any student group specified in the table.

Unlike the models on math achievement, there is no evidence that special education students themselves are more strongly influenced by class percent special education students in reading achievement. Similarly, there is no heterogeneous effect of percent class ELL students.

Table 5.9 reports the estimates of heterogeneous effect from the dispersion of class peer achievement on student reading scores. First, differential effects of peer math achievement variation are found for female students and students at the two end quartiles of reading achievement distribution. The linear combination tests find that if the standard deviation of peer math achievement increases by 0.5, the average reading scores for girls, or students in either the bottom or top quartile of reading achievement will fall by 0.2 point of standardized scores ($P<0.05$). This implies that female students, both high and low reading performers, will achieve higher reading scores if the peers in their reading classes are more alike in math abilities. Second, the dispersion of peer reading achievement shows more negative impact on both high and low reading achievers, but the coefficients from the linear combination tests are not significant.

Table 5.9 Differential Effect from Dispersion of Class Peer Achievement on Reading Achievement (Reading Class)

	Reading Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
Peer Effects (Lottery Based)	**Black**	**LSES**	**Female**	**Low Reading achiever**	**High Reading Achiever**	**Low Math achiever**	**High Math Achiever**
Peer Prior Math Achievement							
Mean	0.22***	0.21***	0.20***	0.21***	0.20***	0.21***	0.21***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Standard deviation	-0.11	-0.22	0.03	-0.23	-0.21	-0.25	-0.28
	(0.19)	(0.20)	(0.26)	(0.20)	(0.20)	(0.20)	(0.20)
Standard deviation * individual Variable	-0.41	-0.2	-0.48*	-0.19***	-0.19***	-0.03	0.02
	(0.25)	(0.27)	(0.20)	(0.03)	(0.03)	(0.03)	(0.04)
Peer Prior Reading Achievement							
Mean	0.31***	0.30***	0.30***	0.31***	0.28***	0.30***	0.30***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Standard deviation	0.12	-0.04	0.21	0.07	0.09	0.02	0.01
	(0.21)	(0.24)	(0.36)	(0.25)	(0.25)	(0.25)	(0.25)
Standard deviation * individual Variable	-0.27	0.29	-0.30	-0.19***	-0.19***	-0.03	0.01
	(0.42)	(0.41)	(0.29)	(0.03)	(0.04)	(0.03)	(0.03)

* p<0.05, ** p<0.01, *** p<0.001

Overall, the heterogeneous peer effect models find some evidences that the academic outcomes of students from various backgrounds are differently impacted by class peer composition. In general, female students are less likely to be impacted by average class peer characteristics. Both high achievers and low achievers appear to be more negatively influenced by disadvantaged class peers. The heterogeneous effect is particularly significant on the reading achievement of students with high prior reading scores. Finally, some groups of students tend to be more negatively impacted by the heterogeneity of classroom peer achievement. For example, black and low income students will do better in mathematics if the peer math achievement is more homogeneous; females and both high and low reading achievers will score higher in reading if their reading class peers are less dispersed in prior math achievement.

Robustness Checks

Following the procedures in last chapter, I also ran a set of models to check if the strong estimates of class peer effects pick up the effects of some other factors than the classroom peer characteristics. First, I include the interaction of the treatment indicators with individual characteristics in the regression to test if peer effects are confounded with the heterogeneity in treatment responses. However, it turns out that the point estimates of classroom peer effects become even larger (and remain significant) when the heterogeneous interaction term is controlled. The results do not support that the estimated classroom peer effects are proxies for treatment response heterogeneity.

Second and more importantly, I implemented teacher fixed effect model to test if classroom peer effects are confounded with teacher impacts. One of the challenges faced by peer effect identification at the classroom level is the non-random matching between teachers and students within each school. For example, schools may place students with disciplinary problems with one teacher who is more experienced with misbehaving students; or schools may assign the best teachers to classes with high performing students. Therefore, the estimation of classroom peer effects is likely to pick up some effects of teacher characteristics.[52]

[52] For example, the Florida study by Burk and Sass (2004) finds that adding teacher fixed effect totally overturned the significant estimates of peer influence on student math achievement. They argue that the apparent peer impacts found in other studies may just reflect the endogenous matching between teachers and students within a school.

The estimates of classroom peer effect models (on math achievement) with teacher fixed effect are presented in Table 5.10[53]. Due to the large amount of data required by teacher fixed effect models, the standard errors of the peer effect estimates arise. However, the point estimates of some major classroom peer characteristics, including percent black students, percent low income students, average peer math achievement, average peer reading achievement, and average peer disciplinary records become even larger and still remain statistically significant. Therefore, there is little evidence that the large estimated effects of classroom peers are signals for teacher qualities or the endogenous matching between teachers and students.

Table5.10 Average Class Peer Effects on Math Achievement (Teacher Fixed Effect)

	Math Scores									
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10
Magnet School Effect										
Academic	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Non-Academic Composite	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Effect (Lottery Based)										
Black (proportion)	-0.96***									
	(0.24)									
Low SES (proportion)		-1.39***								
		(0.27)								
Female (proportion)			0.26							
			(0.36)							
Hispanic (proportion)				-3.59						
				(2.49)						
Special ED (proportion)					-1.18*					
					(0.54)					
ELL (proportion)						6.45				
						(3.57)				
Grade 4 Math (Mean)							0.42**			
							(0.10)			
Grade 4 Reading (Mean)								0.50***		
								(0.09)		

[53] The findings from the teacher fixed effect models on reading achievement are quite similar, except that the estimate of average peer disciplinary infraction effect is larger and significant.

Table 5.10 Continued

Grade 4 Suspension (Mean)									-1.23**	
									(0.47)	
Grade 4 Absence (Mean)										-8.93
										(6.18)
Peer Effects (Residence Based)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Size	10219	10219	10219	10219	10219	10219	10219	10219	10219	10219

* p<0.05, ** p<0.01, *** p<0.001

Differences between School Level Estimates and Classroom Level Estimates of

Although both school level analysis and classroom analysis find some important influences of peer groups, there are differences in the magnitude and nature of the estimated peer group effects between the two levels. In this section, I am going to compare the peer effect estimates at both levels and discuss the possibilities that cause the differences. The following discussions mainly focus on the estimated average peer characteristic effects on math achievement. The estimates can be referred to the models in Table 4.5 (school level analysis) and Table 5.1 (classroom analyses).

First, both the school level and classroom level analyses find that percent Black students (Model 1) are negatively associated with student math achievement. However, the magnitude of estimate of percent Black effect is about 30% stronger at the classroom level (0.66) than at the school level (0.49). The difference in the size of peer racial composition effect supports the hypothesis that peer group influence on student academic achievement mainly works through classrooms where the direct peer interactions in learning actually take place. Peer racial composition at school level, may impact student achievement mostly through the indirect channels, such as shaping the culture or environment of the schools (e.g., low expectations and fewer resources in a school with disproportional black students). However, peer racial composition at classroom level, in addition to the indirect influence such as teacher expectations, may also have direct impact on student achievement through the interactions between students. For example, black students are more likely to have disciplinary problems (as found in next chapter), which can directly impact student learning if the disruptive behavior happens in the classroom.

Second, while there is little evidence that the percentage of low income students (model 2) at school level impacts student math achievement, the estimated effect at the classroom level is large and significant. The same patterns are also found from model 7 and model 8 --- school level estimates of average peer academic abilities (in both subjects) do not show significant influence on student math achievement, but the classroom level estimates are positive and significant. One possibility is due to the tracking practice in middle schools. As suggested by many studies (e.g., Gamoran, 1986; Oaks, 1990; Argys, Rees & Brewer, 1996; Mickelson, 2001), students are often tracked in schools (especially in middle and high schools). Therefore, if disadvantaged students, such as low SES students or students with low academic abilities, are grouped with other disadvantaged students for classes no matter what school they attend (regardless of the school type or the average school peer characteristics), then the school level peer composition may only have a moderate or no influence on student outcomes. Moreover, the estimated differences between school level and classroom level further suggest that the causal relationship between peer groups and student achievement mostly comes from classrooms where students directly interact with each other on learning activities.

Third, although school level analysis finds that high percent female students decrease individual math scores, the significance vanishes at the classroom level (Model 3). Although it might be true that schools with more female students have relatively lower expectations on math achievement and put more emphasis on reading or language arts classes, I would not draw the conclusion from the school level estimate that there is a strong and negative causal relationship between percent female students and average student math achievement. As I mentioned in the previous chapter, because the analytical strategy in this paper identifies peer effect from the average difference in peer characteristics between the treatment group and the control group, the small gap in school gender composition between the two groups is likely to yield a large coefficient on the peer variable percent female students. However, although it may also be true that there are low expectations and motivations in a female dominate math class, the negative impact can be canceled out because there are fewer classroom disruptions.

Fourth, the school level models find significant and positive effects from percent school special education students and percent ELL students. In our data, many of the special education and ELL lottery participants chose to attend neighborhood schools even when they won the lottery of the academic magnet school, which may signal the perceptions of certain school qualities of some neighborhood schools, such as good special education or ELL programs, or teachers more experienced with special need or ELL students. However, I still would not conclude that the causal relationship between student achievement and school special education students or ELL students is strongly positive. In fact, the classroom estimate of percent special education students tells a totally different story: having more students with special needs in a class reduces average student math achievement in middle school. For example, when the instructor has to slow down her class pace or spend disproportional time teaching students with learning disabilities, it will likely have a negative impact on other students in the same classes regardless of their own academic abilities. Interestingly, the classroom heterogeneous peer effect model finds that in our sample, the negative influence of percent special education students mainly works on the lottery participants who are also special education students. This is not surprising because the special education lottery participants perform much better in both subjects than other special education students in the same district.[54] Therefore, if special education lottery participant are put in a special education class where most of other students are at extremely low academic levels[55], it is very likely that they are going to achieve lower than what they could do in a class with regular students. Moreover, although the classroom estimate of percent ELL student is also positive and very close to the school level estimate in magnitude, the impact of having more ELL peers in the same classroom might work differently from the impact of school level ELL peers. Note that in our sample, ELL lottery participants outperform other students in mathematics[56]. Due to the tracking policy in middle school, having more ELL classmates may also signal a high average class math achievement in the magnet schools where all ELL students are enrolled through admission lotteries.

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[54] The average prior scores for special education lottery participants are 620 (math) and 646 (reading); and the average prior scores for non-lottery participant special education students are 572 (math) and 585 (reading). The average scores for the special education lottery participants are even higher than the average scores for the non lottery participant regular students (623 in math, 635 in reading).

[55] This is most likely to happen in neighborhood schools where many special education lottery participants choose to attend and other special education enrollees (the majority) did not participated in the magnet school lotteries.

[56] Although the average prior math achievement for ELL lottery participants is slightly lower than other lottery participants, they achieve higher math scores in middle schools than other lottery participants in our sample.

Fifth, average peer disciplinary infractions only show significantly negative impact on student achievement at classroom level (model 9). One possibility that causes the insignificant school level estimate is also due to the school tracking policy. If students with behavioral problems are often placed in the same class (maybe because the teacher is experienced with disruptive students), it is likely to find little impact from the average school level disciplinary variable. The big difference in the estimates of average peer disciplinary records at both levels again suggests that peer group influence on student achievement mostly comes from classroom level instead of school level.

Finally, although the estimates of average peer absence behavior are not statistically significant at both school and classroom levels, the signs of the coefficients are opposite. Moreover, the heterogeneous peer effect models at the classroom level (Table 5.5) find that some groups of students, such as non-Blacks, high income students, boys, and students at the two ends of achievement distributions, are negatively impacted by average peer absence rate (significant at 5% level). For example, if the class teacher has to spend disproportional time helping a frequent absent student catch up with school work, it will definitely slow down the study of other students in the same class. However, this negative impact is unlikely to be spread to students in other classes of the same school.

Overall, the comparison of peer effect estimates between school level and classroom level suggests that stronger and direct influence of peers on student academic achievement comes from classrooms where the direct peer interaction in learning mostly takes place. The school level peer composition, however, has a relatively moderate impact on student academic outcomes through shaping school culture and atmosphere.

CHAPTER VI

PEER EFFECTS ON BEHAVIORAL OUTCOMES --- RESULTS FROM BOTH SCHOOL AND CLASSROOM LEVEL ANALYSES

Although a large body of research has extensively examined the presence of peer effects in education, most studies have focused on student academic outcomes. Due to data limitations, the question of whether peers influence student non-scholastic outcomes in school is much less studied in literature. In this project, I am able to derive two behavioral outcome variables from the district admission data. The two outcomes of interest include student discipline and attendance, which are considered as important indicators of student social engagement and participation in schools. (Rowley, 2005)

This chapter seeks to explore the impacts of peer groups on student disciplinary and attendance behaviors in school. The first part of this chapter presents some descriptive statistics of the two outcome variables. The second part reports the magnet school treatment effects, with no peer variables controlled. The following sections are organized to answer the three research questions of: (1) the influence on peer behaviors from average peer composition; (2) the impact from the dispersion of peer composition; and (3) the heterogeneous peer effects. Different from previous two chapters, the regression results from both school and classroom analyses will be reported and discussed together in this chapter.

Descriptive Statistics of the Outcomes Variables

Disciplinary Infraction

The district discipline data keep the records of the disciplinary actions given to a student as a consequence to his/her misconducts. The actions are taken based on the severity of student behavior, including less severe punishments, such as warning, call to parents, or corporal punishment, and severe punishments, like suspension (both in school and out of school) and expulsion.[57] While well behaved students do not have any record in the discipline data, many students experienced more than one disciplinary action in a given year. To measure student disciplinary infractions, I coded two variables from these records: annual number of times punished, which sums all disciplinary actions for every student in each year; and annual number of times suspended, counting only in-school and out-of-school suspensions. A zero value is given to the students who did not receive any disciplinary actions in the school year.

The current value of the discipline variable is used as dependent variable to measure student behavior in middle school. The 4th grade value of the discipline variable is included in the regression models as an independent variable, and is also aggregated at both school and classroom level to form the peer behavioral variable. I ran the regression analyses on both discipline variables (total punishment numbers and total suspension numbers) and obtained very similar conclusions. Following Figlio's 2005 study in which suspension is used as the outcome of interest, I report only the results for the suspension variable in this chapter.

[57] Since there are only 2 students (among 5 cohorts) expelled from middle schools in our data, I did not include the expulsion variable in discipline measures.

Table 6.1 Descriptive Statistics of the Dependent Variable: Number of Suspensions

	Suspension Numbers (per student per year)							
	Grade 5		Grade 6		Grade 7		Grade 8	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Lottery Participants								
All	0.25	0.94	0.35	1.21	0.61	1.81	0.72	1.87
Academic								
Participants	0.14	0.61	0.20	0.92	0.32	1.13	0.39	1.12
Enrollees	0.11	0.54	0.10	0.45	0.10	0.54	0.18	0.63
Enrollees in Other Magnet Schools	0.19	0.64	0.33	1.35	0.49	1.76	0.57	1.48
Enrollees in Non-Magnet Schools	0.14	0.66	0.25	1.03	0.44	1.20	0.51	1.27
Composite Non-Academic								
Participants	0.31	1.05	0.42	1.32	0.76	2.07	0.88	2.12
Enrollees	0.29	1.02	0.43	1.42	0.75	1.96	0.80	1.69
Enrollees in Other Magnet Schools	0.14	0.64	0.13	0.50	0.08	0.47	0.12	0.47
Enrollees in Non-Magnet Schools	0.39	1.20	0.52	1.40	0.99	2.40	1.19	2.63

*Counted only students who were in the system as 4th graders in the lottery year.

Table 6.1 presents the descriptive results of the suspension variable for lottery participants. Clearly, the disciplinary infractions increase as the students progress through grades. The biggest jump is between 6th grade and 7th grade --- the average number of suspensions for 7th graders is almost double the number for 6th graders. While this suggests that older students are more likely to have disciplinary problems in school, it may also reflect that schools become stricter with student behavioral problems in higher grades and issue more suspensions. Academic magnet school enrollees appear to receive fewer suspensions than their counterpart students enrolled in neighborhood schools; this pattern is also true of non-academic magnet lottery participants. The large standard deviation of this variable suggests that there is a great variation in behaviors among all lottery participants.

Absence Rate

The district keeps a daily base attendance record for each student. For each required school day, I code the absence variable as 1 if a student was absent (either unexcused or excused), 0 otherwise. I then sum the values of the absence variable across the days in the school year that the student was enrolled in the district. The total is then divided by the days enrolled for each student to calculate the annual absence rate.

Similar to other outcome variables, the current value of this variable enters the regression model as a dependent variable. The lagged measure (4^{th} grade) of this variable is employed to form the peer variable measuring peer attendance behavior, and is also included as an individual control in the regression.

Table 6.2 Descriptive Statistics of the Dependent Variable: Absence Rate

	Annual Absence Rate							
	Grade 5		Grade 6		Grade 7		Grade 8	
Absence Rate	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Lottery Participants								
All	0.03	0.05	0.03	0.04	0.04	0.05	0.04	0.05
Academic								
Participants	0.03	0.04	0.03	0.03	0.03	0.05	0.04	0.04
Enrollees	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Enrollees in Other Magnet Schools	0.03	0.04	0.03	0.03	0.03	0.05	0.03	0.03
Enrollees in Non-Magnet Schools	0.04	0.06	0.04	0.04	0.04	0.06	0.04	0.05
Composite Non-Academic								
Participants	0.03	0.04	0.03	0.05	0.04	0.05	0.04	0.05
Enrollees	0.03	0.03	0.03	0.03	0.03	0.04	0.03	0.03
Enrollees in Other Magnet Schools	0.03	0.03	0.03	0.03	0.03	0.03	0.03	0.03
Enrollees in Non-Magnet Schools	0.04	0.06	0.04	0.06	0.04	0.06	0.05	0.06

*Counted only students who were in the system as 4th graders in the lottery year.

As shown in Table 6.2, the average absence rate of the lottery participants is 0.035 across grades. The difference in average absence rate between magnet school enrollees and lottery participants enrolled in other schools is small --- the average gap is 0.01 for both academic and non-academic magnet schools. If all lottery participants were enrolled in the district for a full school year (175 school days)[58], a difference of 0.01 in absence rate would mean that lottery participants enrolled in neighborhood schools were absent 1.75 days more than the winners enrolled in the magnet programs. There is also more variation in attendance rate among the lottery losers in non-magnet schools than among the magnet school enrollees.

Magnet School Treatment Effects

Although academic outcome improvement is often used to evaluate the effectiveness of school choice programs, many studies also suggest that choice schools outperform their counterpart neighborhood schools in issues like student behavior and parental satisfaction (e.g., Cullen et al., 2003; Peterson, 1998; Smrekar and Goldring, 1999). This section examines whether the magnet schools in the district reduce student misconduct in middle schools and improve student attendance. The regression strategy is the same as the one used in the academic achievement models.

Disciplinary Infractions

Table 6.3 presents the estimated treatment effects on student disciplinary infractions from the models using both the small sample and the large sample. When I estimate the model using the sample of 5th and 6th graders, I do not find significant treatment effects

[58] The school year length varies across years, from 169 to 180 days.

for either the academic or non-academic magnet schools. However, results for the large sample indicate that enrolling in a magnet school substantially reduces the number of suspensions. The estimated treatment effect is larger for the non-academic magnet composite --- with all individual background variables held constant, students who enroll in the non-academic magnet composite schools are likely to receive 0.4 fewer suspensions than lottery losers who attend a neighborhood school.

The second panel of Table 6.3 shows the coefficients on individual characteristics. Compared to other students, black students, special education program students, and students from low SES families are more likely to receive disciplinary actions. Female students, and students with higher prior test scores, not surprisingly, appear to behave better. Also as it is expected, prior behavioral outcome is strongly correlated to student current behavior problems in middle school.

Table 6.3 Magnet School Treatment Effects on Disciplinary Infractions

	Suspension Numbers	
	First 2 Grades	All 4 Grades
Independent Variables		
Academic Magnet Treatment		
Academic	-0.043	-0.262***
	(0.06)	(0.07)
Non-Academic Magnet Treatment		
Composite	0.066	-0.401*
	(0.14)	(0.19)
Student Characteristics		
Black	0.229***	0.375***
	(0.03)	(0.04)
Hispanic	0.035	0.048
	(0.11)	(0.11)
Special Education	0.397***	0.561***
	(0.07)	(0.07)
Low SES	0.198***	0.348***
	(0.03)	(0.03)
ELL	-0.196	-0.13
	(0.14)	(0.17)
Female	-0.190***	-0.245***
	(0.03)	(0.03)

Table 6.3 Continued

Grade 4 Reading	-0.051*	-0.058**
	(0.02)	(0.02)
Grade 4 Math	-0.062**	-0.069***
	(0.02)	(0.02)
Grade 4 Absence	0.61	0.975
	(0.48)	(0.50)
Grade 4 Suspension	0.593***	0.669***
	(0.04)	(0.04)
Over-Identification Test	0.06	0.3
N	6558	12286

* p<0.05, ** p<0.01, *** p<0.001

<u>*Absence Rate*</u>

The estimated magnet school treatment effects on student attendance are reported in Table 6.4. In neither sample do I find significant magnet school effects on student absence rate. The coefficients on individual characteristics suggest that with all other factors held constant, blacks, girls, and ELL students are less likely to be absent from schools. Special education program students and students from low SES background, however, have higher absence rates than other students. As expected, students with higher prior math scores show better attendance behavior, and students who were more often absent in 4^{th} grade continue to have higher absence rates in middle school.

Table 6.4 Magnet School Treatment Effects on Attendance Behavior

	Absence Rate	
	First 2 Grades	**All 4 Grades**
Independent Variables	Model1	Model2
Academic Magnet Treatment		
Academic	-0.005	-0.004
	(0.00)	(0.00)
Non-Academic Magnet Treatment		
Composite	0.000	-0.003
	(0.01)	(0.01)
Student Characteristics		
Black	-0.008***	-0.008***
	(0.00)	(0.00)
Hispanic	0.007	0.002
	(0.00)	(0.00)
Special Education	0.011***	0.011***
	(0.00)	(0.00)
Low SES	0.005***	0.010***
	(0.00)	(0.00)
ELL	-0.015**	-0.013*
	(0.01)	(0.01)
Female	-0.002*	-0.002**
	(0.00)	(0.00)
Grade 4 Reading	-0.001	0.001
	(0.00)	(0.00)
Grade 4 Math	-0.002**	-0.003***
	(0.00)	(0.00)
Grade 4 Absence	0.603***	0.587***
	(0.02)	(0.01)
Grade 4 Suspension	-0.000	0.002
	(0.00)	(0.00)
Over-Identification Test	0.34	0.31
N	6558	12286

* p<0.05, ** p<0.01, *** p<0.001

Impacts from Average Peer Characteristics

This section presents the estimated effects of average peer characteristics on student discipline and attendance behaviors. As in the previous two chapters, I present the results from the large sample models only.

Disciplinary Infractions

1. School Level Analysis

The coefficients on school level peer characteristics are reported in Table 6.5. Each peer characteristic is examined separately in Model 1 to Model 10, while all peer variables are incorporated in Model 11.

The top 2 rows of Table 6.5 present the estimated magnet school treatment. These effects continue to be negative, which indicates that students in the magnet schools have fewer behavioral problems even with peer variables controlled. Indeed, the magnet school treatment effects are even stronger in many equations.

Many of the estimated effects have unexpected coefficients. First, Increases in the percent low income (column 2), percent Hispanic (column 4), and percent special education students (column 5), are associated with fewer incidences of measured misbehavior (measured by the numbers suspended). Second, average school peer academic achievement (measured by 4^{th} grade test scores in both math and reading) is positively correlated with measured misbehavior. A 1 point (in standardized scores) difference in average peer prior math achievement produces 1.5 suspensions. Third, students whose peers were more frequently absent appear to behave better in middle school. Finally, model 9 suggests that there is no significant correlation between school peer prior disciplinary records and individual behavioral outcomes in middle schools.

There are some possible reasons for the unexpected coefficients on the school level peer variables. First, although district has a uniform discipline code, it is the school that takes disciplinary actions based on student behavior. The way in which each school deals with misbehaving students can vary: the same kind of misconduct may receive different consequences in different schools. Second, it is possible that the student body composition influences a school's discipline decisions. For example, a neighborhood school with a higher proportion of low-income and low achieving students may be less willing to punish a lottery participant for his misbehavior just because this student performs and behaves better than other students in this school. However, if this student enrolled in a magnet school, it is less likely that he will get an exemption.

Although most peer characteristics have a significant influence on individual behaviors in the single variable models, this is true less often in column 11 where all peer characteristics are included. Due to the high collinearity among the peer variables, the estimated effects of most peer characteristics are insignificant. As shown in Column 11, there are three significant coefficients. The estimated effects of percent female students and average peer math achievement are positive and significant at 5% statistical level. Average peer prior disciplinary record shows a strong and positive impact on individual behaviors while all school peer characteristics are controlled.

Six residence based peer attributes show significant impact on student disciplinary infractions. Although the estimates of residence based peer effects are less sizeable than the estimates of lottery based peer effects, they all have expected coefficient signs. In none of the 11 models does the inclusion of school peer characteristics overturn the magnet school effects; rather, the treatment effects are even bigger in most models with school peer variables controlled.

Table 6.5 Average School Peer Effects on Disciplinary Infractions

Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
	Total Suspension Numbers										
Magnet School Effect											
Academic	-0.23*	-0.65***	-0.42**	-0.39***	-1.26***	-0.46***	-2.39***	-2.04***	-0.29**	-0.61***	-2.82***
	(0.11)	(0.19)	(0.14)	(0.10)	(0.33)	(0.14)	(0.48)	(0.46)	(0.10)	(0.16)	(0.70)
Non-Academic Composite	-0.40*	-0.58**	-0.52*	-0.53*	-0.92**	-0.51*	-0.99***	-0.97***	-0.42*	-0.71**	-1.40***
	(0.19)	(0.21)	(0.23)	(0.21)	(0.29)	(0.20)	(0.27)	(0.28)	(0.19)	(0.25)	(0.37)
Peer Effect (Lottery Based)											
Black (proportion)	0.13										0.45
	(0.35)										(0.94)
Low SES (proportion)		-1.02*									1.34
		(0.42)									(0.99)
Female (proportion)			2.28								2.94*
			(1.35)								(1.47)
Hispanic (proportion)				-3.24*							-3.06
				(1.55)							(5.83)
Special ED (proportion)					-8.96***						-6.12
					(2.64)						(5.38)
ELL (proportion)						-5.23					2.55
						(2.78)					(8.97)
Grade 4 Math (Mean)							1.97***				1.74*
							(0.41)				(0.76)
Grade 4 Reading (Mean)								1.50***			0.62
								(0.36)			(0.92)
Grade 4 Suspension (Mean)									-0.21		2.21**
									(0.46)		(0.73)
Grade 4 Absence (Mean)										-34.32**	7.95
										(11.08)	(18.20)

Table 6.5 (Continued)

Peer Effects (Residence Based)											
Black (proportion)	0.22*										0.10
	(0.10)										(0.14)
Low SES (proportion)		0.36***									0.06
		(0.10)									(0.23)
Female (proportion)			-0.14								-0.40
			(0.31)								(0.34)
Hispanic (proportion)				0.36*							0.44
				(0.17)							(0.23)
Special ED (proportion)					0.67***						0.20
					(0.18)						(0.25)
ELL (proportion)						0.05					-0.23
						(0.13)					(0.18)
Grade 4 Math (Mean)							-0.27***				-0.70**
							(0.05)				(0.23)
Grade 4 Reading (Mean)								-0.22***			0.45
								(0.05)			(0.23)
Grade 4 Suspension (Mean)									0.12		-0.55**
									(0.15)		(0.18)
Grade 4 Absence (Mean)										4.64	1.88
										(2.42)	(3.50
Student Characteristics											
	Yes	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	yes
Over-identification Test	0.3	0.51	0.25	0.26	0.4	0.41	0.85	0.78	0.35	0.33	0.54
Sample Size	12195	12195	12195	12195	12195	12195	12195	12195	12195	12195	12195

* p<0.05, ** p<0.01, *** p<0.001

2. Classroom Level Analysis

Although most middle school students in the district rotate through classes on different subjects, a student's classroom peers are relatively stable across subjects given that many schools practice tracking or ability grouping[59]. Therefore, the interactions between one student and his classmates are more frequent than the interactions with the average school peers; and the impacts from one's class peers are expected to be stronger and more meaningful even on discipline and attendance outcomes.

Because the classroom peer variables are subject based, there are two sets of peer variables from math classes and reading/language arts classes. I ran separate models using peers in both subjects and obtained very similar estimates of peer effects. Here I report the results for only math class peers. [60]

Table 6.6 presents estimates of models that include classroom peer variables. The estimated effects of peer characteristics at the classroom level are quite different from those at school level and all of the significant coefficients have the expected signs. A higher proportion of black and low SES classmates is associated with more disciplinary infractions: a student is likely to receive 1 more suspension if he moves from a class with no low income peers to a class with 80% low income students[61]. Second, peer academic ability (measured by prior reading achievement) appears to decrease individual misdemeanors. A 1 point increase in peer prior reading scores (standardized scores) yields a 0.4 decrease in student suspension times. Third, classmates' prior disciplinary records are strongly correlated with individual behavioral problems in middle school. The

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[59] In some schools, there is less ability-grouping in 5th and 6th grades, and students tend to stay with the same group of peers for all subjects just like elementary schools.
[60] The results from the reading class peer models are included in Appendix F.
[61] The estimated effect is quite large given that about 78% lottery participants never received any suspension during the years of investigation.

estimated effect of peer's prior behavioral problems is very substantial --- if every peer in the same class received 1 more suspension in 4^{th} grade, the average class number suspended in middle school increase by 1.8. Finally, being with classmates with past history of higher absence rates also increases individual misconduct as measured by suspension numbers. Overall, the single variable models find significant and expected peer effects on student disciplinary infractions from both peer racial/SES composition variables and peer prior behaviors.

Model 11 incorporates all classroom peer characteristics. Most peer variables are individually insignificant in the full model except peers' prior absence rate, which remains positive and weakly significant. Some of this may be due to the collinearity among the class peer variables, which increase the standard errors.

In an equation where a classroom peer characteristic has a significant influence on student behavior, magnet school treatment effects are insignificant. One exception is model 9, where the estimated treatment effect of the academic magnet program remains negative and significant, but less sizeable than the estimate in Table 6.3. When the model controls all peer characteristics, the point estimate of the non-academic magnet effect falls nearly to zero.

Table 6.6 Average Classroom Peer Effects on Disciplinary Infractions (Math Class)

	Total Suspension Numbers										
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
Magnet School Effect											
Academic	-0.10	0.05	-0.28**	-0.36***	-0.25**	-0.33*	-0.13	0.05	-0.18*	-0.08	0.13
	(0.08)	(0.11)	(0.09)	(0.10)	(0.09)	(0.13)	(0.12)	(0.13)	(0.08)	(0.11)	(0.17)
Non-Academic Composite	-0.33	-0.21	-0.36	-0.45*	-0.35	-0.39	-0.33	-0.24	-0.33	-0.09	0.02
	(0.20)	(0.20)	(0.20)	(0.21)	(0.21)	(0.21)	(0.20)	(0.21)	(0.20)	(0.24)	(0.26)
Peer Effect (Lottery Based)											
Black (proportion)	0.96***										0.85
	(0.21)										(1.01)
Low SES (proportion)		1.23***									0.76
		(0.24)									(0.79)
Female (proportion)			0.12								0.03
			(0.57)								(0.62)
Hispanic (proportion)				-3.05							-4.12
				(1.64)							(2.56)
Special ED (proportion)					1.30						0.83
					(0.82)						(1.24)
ELL (proportion)						-1.92					3.63
						(2.95)					(5.97)
Grade 4 Math (Mean)							-0.22				0.97
							(0.13)				(0.52)
Grade 4 Reading (Mean)								-0.42***			-0.51
								(0.11)			(0.33)
Grade 4 Suspension (Mean)									1.75***		0.60
									(0.46)		(0.69)
Grade 4 Absence (Mean)										28.37**	36.48*
										(9.58)	(16.68)
Peer Effects (Residence Based)											
	Yes	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Student Characteristics											
	Yes	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	yes
Over-identification Test	0.11	0.1	0.2	0.28	0.2	0.18	0.18	0.14	0.35	0.21	0.26
Sample Size	10369	10369	10369	10369	10369	10369	10368	10368	10368	10368	10368

* p<0.05, ** p<0.01, *** p<0.001

The results from both school and classroom level analyses on student discipline problems are very interesting. Although the school level results suggest many unfavorable peer attributes decreasing student misdemeanors, the classroom level results find the opposite. Moreover, classroom analyses find strong and expected effects from peer prior achievement and behaviors. Again, the results suggest that the stronger and direct impacts of peer group effects come from classroom level, even on individual disciplinary outcomes. The estimates of school level peer effects, in this case, reflects more about how school student composition impact one's chances of receiving disciplinary actions (suspensions, as measured in this section).

<u>Attendance Behavior</u>

1. School Level Analysis

Table 6.7 reports the effects of school level peers on student attendance behavior. Only one peer characteristic is found significantly correlated with student attendance behavior: peers with a history of behavioral problems tend to increase the individual absence rate in middle schools. Specifically, the coefficient suggests that a student will be absent for 2.5 more days from school if peer prior suspensions increase by 0.5 on average.

Several residence-based peer characteristics have shown significant associations with student absence rate. Students who are from a neighborhood with more low SES families, or more Hispanic and ELL peers, or more special education peers, are more likely to be absent from schools than other students. Residence-based peer math achievement is negatively correlated with student absence rate. But the magnitudes of the coefficients are quite small.

Finally, including peer characteristics does not change the estimated magnet school treatment effects, which remain insignificant in all models.

Table 6.7 Average School Peer Effects on Absence Rate

					Absence Rate						
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
Magnet School Effect											
Academic	-0.01	-0.00	-0.01	-0.00	-0.01	-0.01	0.01	0.01	-0.00	-0.00	0.01
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.02)
Non-Academic Composite	-0.00	-0.00	-0.01	-0.00	-0.01	-0.01	-0.00	0.00	0.00	-0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Effect (Lottery Based)											
Black (proportion)	-0.00										-0.03
	(0.01)										(0.03)
Low SES (proportion)		0.01									-0.03
		(0.01)									(0.03)
Female (proportion)			0.01								0.01
			(0.04)								(0.04)
Hispanic (proportion)				0.02							0.00
				(0.05)							(0.15)
Special ED (proportion)					-0.07						-0.25
					(0.08)						(0.14)
ELL (proportion)						-0.02					-0.14
						(0.08)					(0.22)
Grade 4 Math (Mean)							-0.00				-0.01
							(0.01)				(0.02)
Grade 4 Reading (Mean)								-0.01			-0.04
								(0.01)			(0.02)
Grade 4 Suspension (Mean)									0.03*		0.05**
									(0.01)		(0.02)
Grade 4 Absence (Mean)										0.32	0.15
										(0.33)	(0.50)
Peer Effects (Residence Based)											
Black (proportion)	0.00										0.00
	(0.00)										(0.00)
Low SES (proportion)		0.01**									0.02***
		(0.00)									(0.01)
Female (proportion)			-0.01								-0.01
			(0.01)								(0.01)

Table 6.7 Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Hispanic (proportion)				0.013*							(0.00)
				(0.01)							(0.01)
Special ED (proportion)					0.01*						0.01
					(0.01)						(0.01)
ELL (proportion)						0.01**					0.00
						(0.00)					(0.00)
Grade 4 Math (Mean)							-0.00*				-0.01*
							(0.00)				(0.01)
Grade 4 Reading (Mean)								0.00			0.02**
								(0.00)			(0.01)
Grade 4 Suspension (Mean)									0.01		0.00
									(0.00)		(0.01)
Grade 4 Absence (Mean)										0.10	-0.08
										(0.07)	(0.09)
Student Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Over-identification Test	0.26	0.21	0.27	0.3	0.25	0.2	0.24	0.23	0.22	0.23	0.29
Sample Size	12195	12195	12195	12195	12195	12195	12195	12195	12195	12195	12195

* p<0.05, ** p<0.01, *** p<0.001

2. Classroom Level Analysis

Classroom estimates of peer characteristic effects on student absence behavior are presented in Table 6.8. None of the ten single variable models has found significant coefficients on the lottery-based exogenous classroom peer variables. The full-variable model (model 11) at classroom level finds that a student has higher absence rate if his/her peers had higher rates of suspension in 4^{th} grade. Otherwise, student attendance behavior is quite impervious to peer influences.

Interestingly, although the classroom level models do not find significant peer effects on student participation behavior, controlling for classroom peer characteristics leads to significant magnet school effects in most models, especially for the academic magnet school. The implication is that a lottery loser enrolled in a neighborhood middle school tends to be absent from school for 2 more days even if he/she attends a class whose peers are identical to the class peers in the academic magnet.

Table 6.8 Average Classroom Peer Effects on Absence Rate (Math Class)

	Absence Rates										
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
Magnet School Effect											
Academic	-0.01**	-0.00	-0.01**	-0.00**	-0.01**	-0.00	-0.01*	-0.01	-0.00**	-0.01**	-0.01*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Non-Academic Composite	-0.01	-0.01	-0.01*	-0.01	-0.01*	-0.01	-0.01*	-0.01	-0.01	-0.01*	-0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Peer Effect (Lottery Based)											
Black (proportion)	0.00										0.03
	(0.01)										(0.03)
Low SES (proportion)		0.00									-0.01
		(0.01)									(0.02)
Female (proportion)			0.02								0.03
			(0.01)								(0.02)
Hispanic (proportion)				0.04							0.02
				(0.04)							(0.07)
Special ED (proportion)					-0.03						-0.00
					(0.02)						(0.03)
ELL (proportion)						0.08					0.20
						(0.07)					(0.15)
Grade 4 Math (Mean)							0.00				0.02
							(0.00)				(0.01)
Grade 4 Reading (Mean)								0.00			-0.00
								(0.00)			(0.01)
Grade 4 Suspension (Mean)									0.02		0.04*
									(0.01)		(0.02)
Grade 4 Absence (Mean)										-0.34	-0.05
										(0.24)	(0.43)
Peer Effects (Residence Based)											
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics											
	Yes	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Over-identification Test	0.12	0.11	0.11	0.11	0.14	0.15	0.15	0.14	0.15	0.1	0.19
Sample Size	10369	10369	10369	10369	10369	10369	10368	10368	10368	10368	10368

* p<0.05, ** p<0.01, *** p<0.001

Impacts from Dispersion of Peer Characteristics

In this section, I examine whether the variance of peer academic abilities and peer behavioral outcomes impacts student discipline and participation behaviors.

Disciplinary Infractions

1. School Level Analysis

In Table 6.9, peer heterogeneity is measured as the standard deviation of prior peer test scores (in math and reading), prior suspensions, and prior absence rate. Since the conclusions regarding magnet school effects and the residence-based peer variables are very similar to previous findings, Table 6.9 reports only the coefficients on the exogenous peer variables (means and standard deviations).

Columns 1-2 show the effect of peer achievement. The coefficient on the mean is positive, but the coefficient on the standard deviation is negative. The implication is that a lottery participant tends to have fewer measured misbehaviors (indicated by fewer suspensions received by the student) in a school where there is a greater variation in student academic ability. Recall my hypothesis that the misbehavior of a lottery participant enrolled in a neighborhood school may be overlooked because he performs better than his peers, while the participants enrolled in a magnet school are less lucky because students in the magnet school are more alike (e.g., all high performers). The negative coefficient on the standard deviation of peer achievement lends support to this explanation --- more dispersion in peer ability is associated with less individual disciplinary infractions as measured by number of suspensions.

Table 6.9 Impacts from Dispersion of Peer Characteristics on Disciplinary Infractions (School Level)

	Total Suspension Numbers			
Independent Variables	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	Yes	Yes	Yes
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	1.88***			
	(0.29)			
Grade 4 Reading (Mean)		1.38***		
		(0.35)		
Grade 4 Suspension (Mean)			0.33	
			(1.07)	
Grade 4 Absence (Mean)				-27.77*
				(13.53)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	-1.26***			
	(0.33)			
Grade 4 Reading (Standard Deviation)		-1.63***		
		(0.32)		
Grade 4 Suspension (Standard Deviation)			-0.22	
			(0.40)	
Grade 4 Absence (Standard Deviation)				-6.65
				(5.44)
Peer Effects (Residence Based)				
	Yes	Yes	Yes	Yes
Student Characteristics				
	Yes	Yes	Yes	Yes
P-value for Over-identification Tests	0.86	0.89	0.31	0.31
Sample Size	12192	12192	12192	12192

* p<0.05, ** p<0.01, *** p<0.001

Neither the mean nor the standard deviation of prior peer suspensions is significantly associated with student disciplinary infractions in middle school. Moreover, model 4 suggests no significant effect from the heterogeneity in peer attendance behavior on student disciplinary infractions, although the estimated effect from average peers attendance rate is still negative and significant.

2. Classroom Level Analysis

The results from the classroom level dispersion models are presented in Table 6.10. The heterogeneity of peer academic abilities (Model1 and model 2) does not exert significant impact on individual discipline behaviors, nor does its inclusion alter the coefficients on mean peer characteristics.

There is no evidence that the dispersion of peer disciplinary infractions at the classroom level has any significant impact on individual behaviors. However, controlling for the dispersion term overturned the effect of the average peer disciplinary record. This again is due to the high collinearity between the mean and the standard deviation, though now both terms have positive coefficient. When I test a linear combination of these two variables, I find a strong and positive correlation between the classmates' disciplinary records and individual behavior.

The standard deviation of peers' prior absentee has a significant negative effect on measured misconduct. However, notice that with the dispersion term in the model, the coefficient on the average peer attendance variable increases by almost 40 (28.3 in Table 6.6). Again, collinearity between the mean and the standard deviation is likely to cause this change.

The omnibus over-identification tests suggest that instruments used in all the models in both Table 6.9 and Table 6.10 are exogenous.

Table 6.10 Impacts from Dispersion of Peer Characteristics on Disciplinary Infractions (Math Class)

	Suspension Times			
	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	Yes	Yes	Yes
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	-0.21			
	(0.12)			
Grade 4 Reading (Mean)		-0.43***		
		(0.11)		
Grade 4 Suspension (Mean)			0.22	
			(1.51)	
Grade 4 Absence (Mean)				67.25**
				(22.62)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	0.53			
	(0.50)			
Grade 4 Reading (Standard Deviation)		-0.63		
		(0.89)		
Grade 4 Suspension (Standard Deviation)			0.70	
			(0.70)	
Grade 4 Absence (Standard Deviation)				-40.65*
				(15.86)
Peer Effects (Residence Based)				
	Yes	Yes	Yes	Yes
Student Characteristics				
	Yes	Yes	Yes	Yes
P-value for Over-identification Test	0.19	0.14	0.30	0.25
Sample Size	10359	10359	10359	10359

* p<0.05, ** p<0.01, *** p<0.001

<u>*Attendance Behavior*</u>

1. School Level Analysis

Table 6.11 shows the estimates of school level models on student attendance behavior. The firs 2 columns report the results from the peer achievement models. Neither the mean nor the standard deviation of peer academic abilities has significant influence on student absence rate in middle school. Similarly, there is no significant effect from either the mean or the standard deviation of peer prior disciplinary infractions (Model 3). In model 4, the average peer absence rate and its standard deviation work in opposite directions: while the average peer absence rate tends to raise the possibility of absence, its standard deviation dispersion has a negative effect. Again, the much larger and significant coefficient on the average peer absence rate is likely to be due to the high collinearity between the average term and the variance term.

Table 6.11 Impacts from Variance of Peer Characteristics on Absence Rate (School Level)

	Absence Rate			
	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	*Yes*	*Yes*	*Yes*
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	-0.01			
	(0.01)			
Grade 4 Reading (Mean)		-0.01		
		(0.01)		
Grade 4 Suspension (Mean)			0.04	
			(0.03)	
Grade 4 Absence (Mean)				0.80*
				(0.40)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	-0.01			
	(0.01)			
Grade 4 Reading (Standard Deviation)		-0.02		
		(0.01)		
Grade 4 Suspension (Standard Deviation)			-0.01	
			(0.01)	
Grade 4 Absence (Standard Deviation)				-0.47**
				(0.16)
Peer Effects (Residence Based)				
	Yes	*Yes*	*Yes*	*Yes*
Student Characteristics				
	Yes	*Yes*	*Yes*	*Yes*
P-value for Over-identification Tests	0.23	0.21	0.19	0.27
Sample Size	12192	12192	12192	12192

* p<0.05, ** p<0.01, *** p<0.001

2. Classroom Level Analysis

There is little evidence that changes in the heterogeneity of peer achievement or past behavior at the classroom level affects student attendance behavior. The coefficients on the average peer outcome variables remain insignificant, too. Although not shown in the table, controlling for peer heterogeneity does not change the coefficients on other variables: particularly, attending the academic magnet school appears to decrease student absenteeism.

Table 6.12 Impacts from Variance of Peer Characteristics on Absence Rate (Math Class)

	Absence Rate			
Independent Variables	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	*Yes*	*Yes*	*Yes*
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	0.00			
	(0.00)			
Grade 4 Reading (Mean)		0.00		
		(0.00)		
Grade 4 Suspension (Mean)			0.07	
			(0.04)	
Grade 4 Absence (Mean)				-0.07
				(0.55)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	-0.00			
	(0.01)			
Grade 4 Reading (Standard Deviation)		-0.00		
		(0.02)		
Grade 4 Suspension (Standard Deviation)			-0.02	
			(0.02)	
Grade 4 Absence (Standard Deviation)				-0.23
				(0.39)
Peer Effects (Residence Based)				
	Yes	*Yes*	*Yes*	*Yes*
Student Characteristics				
	Yes	*Yes*	*Yes*	*Yes*
P-value for Over-identification Test	0.14	0.14	0.16	0.11
Sample Size	10359	10359	10359	10359

* p<0.05, ** p<0.01, *** p<0.001

Heterogeneous Peer Effects

This section examines whether peer group characteristics effects on student discipline and attendance behaviors differ depending on individual background. The results in this part answer the third research question --- to whom do peer effects matter more.

<u>*Disciplinary Infractions*</u>

1. School Level Analysis

Many estimates of average school level peer effects are having unexpected coefficient signs in previous Table 6.5. This part will find out if different subgroups of students are impacted homogeneously by school level peer compositions. Regression results are reported in Table 6.13, which includes coefficients on both the mean peer terms and the interaction terms.

The first peer variable examined in this table is percent black students. Model 1 suggests that although other students are not significantly influenced by school black population (even with a negative coefficient), black students tend to have more suspensions while being in a school with disproportionately more Blacks. A similar conclusion can be drawn from model 2, wherein the peer variable of percent black students is found having a stronger and positive impact on students from low income families. The linear combination of the coefficients on main effect and the interaction with low SES variable is 0.9 ($p<0.05$), suggesting that low income students are likely to receive 1 more suspension if they attend a school where the population is 100% Black rather than 0% Black. Compared to boys, female students' behaviors are less influenced by black peers; and the influence from percent black students are stronger on low achieving students than on high achieving students. However, the linear combinations of the coefficients on the main effect and the interaction are not statistically significant.

Table 6.13 Heterogeneous Peer Effects on Disciplinary Infractions (School Level)

	Suspension Times						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading achiever**	**High Reading Achiever**	**Low Math achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Percent Black Students							
Mean Effect	-0.50	-0.12	0.48	0.09	0.13	0.03	0.08
	(0.41)	(0.37)	(0.37)	(0.36)	(0.35)	(0.36)	(0.35)
Heterogeneous Effect	1.06***	1.02***	-0.65***	0.16*	-0.01	0.20**	0.17
	(0.22)	(0.23)	(0.16)	(0.07)	(0.11)	(0.07)	(0.11)
Percent Low SES Students							
Mean Effect	-1.35**	-1.31**	-0.56	-1.09*	-1.03*	-1.16**	-1.09*
	(0.44)	(0.43)	(0.44)	(0.43)	(0.43)	(0.43)	(0.43)
Heterogeneous Effect	1.02***	1.12***	-0.67***	0.19*	-0.02	0.29**	0.19
	(0.23)	(0.31)	(0.19)	(0.09)	(0.13)	(0.09)	(0.13)
Peer Prior Math Achievement							
Mean Effect	1.99***	1.91***	1.73***	1.97***	1.94***	2.13***	1.99***
	(0.41)	(0.41)	(0.42)	(0.41)	(0.41)	(0.42)	(0.41)
Heterogeneous Effect	-0.37***	-0.32***	0.19*	0.02	0.20**	-0.29**	0.21*
	(0.10)	(0.13)	(0.08)	(0.09)	(0.07)	(0.09)	(0.07)
Peer Prior Reading Achievement							
Mean Effect	1.42***	1.39***	1.19***	1.41***	1.38***	1.51***	1.41***
	(0.36)	(0.36)	(0.36)	(0.36)	(0.36)	(0.36)	(0.36)
Heterogeneous Effect	-0.43***	-0.41***	0.20**	0.02	0.21**	-0.32**	0.21**
	(0.09)	(0.11)	(0.07)	(0.08)	(0.06)	(0.08)	(0.06)
Peer Prior Disciplinary Records							
Mean Effect	-0.77	-0.49	-0.14	-0.36	-0.22	-0.71	-0.35
	(0.53)	(0.49)	(20.55)	(0.47)	0.47	(0.47)	(0.48)
Heterogeneous Effect	1.19**	0.73	-0.11	0.31	-0.19	1.17***	0.36
	(0.47)	(0.52)	(0.44)	(0.28)	(0.40)	(0.28)	(0.39)
Peer Prior Absence Rate							
Mean Effect	-34.81**	-32.61**	-32.57**	-35.18**	-34.78**	-35.20**	-35.04**
	(11.46)	(11.17)	(12.00)	(11.12)	(11.10)	(11.11)	(11.10)
Heterogeneous Effect	2.37	-7.79	-2.59	1.85	1.95	1.76	4.18**
	(9.53)	(10.46)	(8.04)	(1.19)	(1.47)	(1.18)	(1.44)

* p<0.05, ** p<0.01, *** p<0.001

The second peer variable shown in this table is percent Low SES students. Recall that in Table 6.5, school percent Low SES population reduces suspensions. The models in Table 6.13 show that the negative association between percent low SES student variable and student disciplinary infractions does not hold for every group of students. First, black and low SES students appear to be not impacted by school low SES population: the positive coefficients on the interaction term almost cancel out the negative coefficients on the main effects in model 1 and model 2. Second, the negative impact of school percent low SES students is not significant on male students. Third, although both high and low achieving lottery participants seem to have fewer disciplinary infractions when there is a high proportion of low SES students in the school, the negative impact is stronger on students with higher prior test scores.

Both school level peer academic ability variables were found to increase student disciplinary infractions in our sample. There are some significant heterogeneous effects from both peer achievement variables. For example, the positive impact of peer achievement is weaker on Blacks, low income students (model 1 and model 2), and low performing students (model 6), but it is stronger on students with high prior achievement (model 5 and model 7).

Average school peer prior disciplinary records do not have a significant impact on student behaviors (as shown in Column9, Table 6.5). Although model 1 and model 6 show significant heterogeneous effect on Blacks and low math achievers, the linear combination of the coefficients on the main effect and the interaction finds no evidence that either of these groups is significantly influenced by peer prior disciplinary infractions.

The last peer variable reported in Table 6.13 is peer prior attendance record, which is negatively associated with individual disciplinary infractions for all groups of students. The effect is slightly weaker on students with higher math scores.

Again, the estimates from the heterogeneous models on school level peer characteristics support my previous hypothesis that advantaged students (e.g., students from non-low-income families or high performing students) are less likely to receive disciplinary actions if he is in a school with disproportional disadvantaged peers.

2. Classroom Level Analysis

Table 6.14 presents the estimates of classroom level heterogeneous peer effect models. As before, only results from math class peer variables are reported given that the conclusions from peer variables in reading classes are very similar.

First, although high percentage of Black or low SES class peers tends to increase individual misconduct for all students, the size of the impact is much stronger on Blacks, students from low income families, and low achieving students. For example, combined main effect and interaction for percent black students is 1.88 for low income students, almost 3 times of the estimated effect (0.66) on students from affluent families. Girls are less impacted by black or low SES peers than boys, although combined main effect and interaction is still positive and significant for girls for both variables.

Second, average peer math achievement significantly reduces the number of suspensions only for Blacks, boys, and low income students. However, the main effect of peer reading achievement is significant and negative in all models. There is also significant heterogeneity in this effect: Blacks and low income students appear to be more negatively impacted by class average peer reading achievement; the negative impact, however, is weaker on girls and students with high math scores.

Table 6.14 Heterogeneous Peer Effects on Disciplinary Infractions (Math Class)

	Suspension Times						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading achiever**	**High Reading Achiever**	**Low Math achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Percent Black Students							
Mean Effect	0.62**	0.66**	1.20***	0.88***	0.96***	0.87***	0.92***
	(0.24)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)
Heterogeneous Effect	0.81***	1.22***	-0.49***	0.23**	-0.01	0.27***	0.19
	(0.19)	(0.20)	(0.14)	(0.08)	(0.14)	(0.08)	(0.13)
Percent Low SES Students							
Mean Effect	0.79**	0.83***	1.60***	1.11***	1.25***	1.08***	1.24***
	(0.25)	(0.25)	(0.25)	(0.24)	(0.24)	(0.24)	(0.24)
Heterogeneous Effect	1.29***	1.43***	-0.71***	0.34***	-0.10	0.36***	-0.03
	(0.21)	(0.25)	(0.17)	(0.10)	(0.17)	(0.10)	(0.16)
Peer Prior Math Achievement							
Mean Effect	-0.14	-0.13	-0.33*	-0.18	-0.25	-0.19	-0.26
	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)	(0.13)
Heterogeneous Effect	-0.36***	-0.56***	0.22**	-0.17*	0.14*	-0.13	0.11*
	(0.13)	(0.10)	(0.07)	(0.07)	(0.06)	(0.07)	(0.06)
Peer Prior Reading Achievement							
Mean Effect	-0.30**	-0.34**	-0.52***	-0.38**	-0.44***	-0.38**	-0.45***
	(0.12)	(0.12)	(0.12)	(0.12)	(0.11)	(0.12)	(0.11)
Heterogeneous Effect	-0.41***	-0.50***	0.18**	-0.13	0.11*	-0.13*	0.11*
	(0.08)	(0.09)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)
Peer Prior Disciplinary Records							
Mean Effect	0.55	1.42*	2.10***	1.4**	1.76***	1.34*	1.68***
	(0.56)	(0.62)	(0.52)	(0.52)	(0.45)	(0.53)	(0.48)
Heterogeneous Effect	2.35***	0.66	-0.81	0.65	-0.11	0.89*	0.35
	(0.58)	(0.62)	(0.52)	(0.39)	(0.87)	(0.40)	(0.73)
Peer Prior Absence Rate							
Mean Effect	6.77	29.31**	20.24*	27.75**	28.08**	27.64**	27.47**
	(8.83)	(9.69)	(8.35)	(9.54)	(9.66)	(9.54)	(9.73)
Heterogeneous Effect	76.70***	-4.40	16.61	3.22*	1.23	2.02	2.11
	(14.81)	(13.35)	(11.06)	(1.41)	(1.80)	(1.38)	(1.73)

* p<0.05, ** p<0.01, *** p<0.001

Third, although the overall lottery participants seem to have more disciplinary incidents if they are placed in a class with more misbehaving peers, two subgroups of students seem to be impacted by peer disciplinary behaviors differently from other students. A black student tends to receive 2.4 more suspensions if the average prior peer suspensions increase by 1, while the coefficient on this variable is 0.55 and insignificant for non-black students. Also, the effect is larger on students with low math achievement: compared to other students, the students at the bottom quarter receive 1 more suspension if the average prior peer suspensions increase by 1.

Finally, conclusions from the models of peer prior absence rates are very similar to those from peer suspensions. The positive association between peer absence rate and individual disciplinary infraction works mainly on black students, and the positive impact is slightly larger on students with low reading achievement. Other models find no significant heterogeneity in the effect of prior peer absences.

Attendance Behavior

1. School Level Analysis

Results for student attendance are presented in Table 6.15. Most peer characteristics examined, including percent black students, percent low income students, average peer math scores, average reading scores, and average prior absence rate, do not show a significant influence on student attendance.

Interestingly, although Table 6.7 (column 9) suggests that average peer prior disciplinary infractions increase student absence rate, the heterogeneous models find that the estimated peer disciplinary record effect is not significant on every group of students.

For example, the attendances of black students, boys, and students in the middle quartiles of achievement distributions are not significantly influenced by peer behavioral records, but the linear combination test (see Appendix E) suggests that low income students, girls, and low achievers are likely to be more absent if the school peers received more suspensions in 4th grade.

Table 6.15 Heterogeneous Peer Effects on Absence Rate (School Level)

	Absence Rate						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading achiever**	**High Reading Achiever**	**Low Math achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Percent Black Students							
Mean Effect	0.00	0.00	0.00	-0.00	-0.00	-0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	-0.01	-0.01	-0.01	0.00	0.01*	-0.00	-0.00
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Percent Low SES Students							
Mean Effect	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	-0.02*	-0.01	0.01	0.01*	0.01*	0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Math Achievement							
Mean Effect	-0.00	-0.00	-0.00	-0.00	-0.01	-0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	0.00	-0.00	-0.00	-0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Reading Achievement							
Mean Effect	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	0.00	-0.00	-0.00	-0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Disciplinary Records							
Mean Effect	0.04**	0.02	0.02	0.02	0.02	0.02	0.04**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	-0.02	0.03	0.01	0.01	0.02	0.01	-0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Prior Absence Rate							
Mean Effect	0.42	0.34	-0.07	0.30	0.31	0.32	0.32
	(0.34)	(0.33)	(0.35)	(0.33)	(0.33)	(0.33)	(0.33)
Heterogeneous Effect	-0.44	-0.10	0.58*	0.05	0.10*	-0.01	-0.01
	(0.28)	(0.31)	(0.24)	(0.04)	(0.04)	(0.03)	(0.04)

* p<0.05, ** p<0.01, *** p<0.001

2. Classroom Level Analysis

As shown in Table 6.16, the estimates of heterogeneous peer effect models on student attendance behavior are quite simple: overall, most peer variables do not show significant impacts on individual attendance; there are no significant heterogeneous effects in most models either. Two exceptions are from average class prior absence rate. Non black students and male students seem to be less absent when class absence rate is higher.

Again, neither the heterogeneous peer effect models nor the average peer effect models (at both school and classroom levels) suggest that individual attendance behavior change significantly in response to the changes in school or class peer composition.

Table 6.16 Heterogeneous Peer Effects on Absence Rate (Math Class)

	Absence Rate						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Percent Black Students							
Mean Effect	0.00	0.00	0.01	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	-0.00	-0.00	-0.01**	0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Percent Low SES Students							
Mean Effect	0.01	0.00	0.01	0.00	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	-0.00	0.00	-0.01	0.00	0.00	-0.01	0.00
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Math Achievement							
Mean Effect	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Heterogeneous Effect	0.00	0.00	0.00	-0.00	0.004*	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Reading Achievement							
Mean Effect	0.00	0.00	0.00	0.00	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Heterogeneous Effect	0.00	-0.00	0.00	0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Disciplinary Records							
Mean Effect	0.01	0.01	0.02	0.01	0.02	0.02	0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	0.01	0.02	-0.00	0.02	0.01	0.00	-0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)

Table 6.16 Continued

Peer Prior Absence Rate							
Mean Effect	-0.49*	-0.24	-0.43*	-0.34	-0.35	-0.32	-0.36
	(0.21)	(0.24)	(0.21)	(0.24)	(0.24)	(0.24)	(0.24)
Heterogeneous Effect	0.49	-0.35	0.20	0.06	0.06	-0.03	0.06
	(0.36)	(0.33)	(0.28)	(0.04)	(0.04)	(0.03)	(0.04)

* p<0.05, ** p<0.01, *** p<0.001

Robustness Checks

Similar to the work on student academic achievement, I checked whether the estimates of peer group effects on student behavioral outcomes are confounded with heterogeneous treatment effects and teacher effects. There is no significant evidence that the estimated peer effects on student discipline or attendance behaviors are confounded heterogeneous treatment effects or teacher effects. In particular, with teacher fixed effects in the model, the coefficients on class peer characteristics in the discipline outcome models are even larger. Although including teacher fixed effect does not change the estimated peer effects on student absence behavior, the academic magnet school treatment effect became insignificant.

In addition, I also ran a set of school fixed effect models to see if the unexpected coefficients on school level peer variables are impacted by some time-invariant school factors (such as discipline policies). However, the conclusions regarding school level peer characteristics remained unchanged.

CHAPTER VII

CONCLUSIONS

This dissertation investigates the peer group effects on student outcomes (both academic and behavioral outcomes). Relying on the admission lotteries that randomly assign students to a magnet school or a neighborhood school, this study implements credible methods to identify peer effects free from selection bias and omitted variable bias; it also circumvents the simultaneity bias by using lagged values of student achievement and behavior records to form peer variables. The investigation of peer effects answers three research questions:

- What is the impact from average peer characteristics on individual student outcomes at both school and classroom levels?
- What is the impact of peer heterogeneity on student outcomes at both levels?
- To whom do peer effects matter the most? --- That is, which subgroups of students are more impacted by peer characteristics?

This chapter will summarize the key findings, implications, and limitations of this study.

Review of Findings

Average Peer Effects

This study examines peer group effects from a large set of specifications of peer characteristics at both school and classroom levels. Although both level analyses find some large estimates of peer effects, there are many differences between the school level estimates and the classroom estimates. The comparison of estimated peer effects at two levels shows that the stronger peer influences come from classroom.

First, student achievement tends to be lower if they are surrounded by more black peers at both school and classroom levels. The negative effect of percent black students agrees with the findings from some recent literature, such as the Texas studies by Hoxby (2004) and Hanushek, Kain, and Rivkin (2009). However, the two Texas studies also find stronger intra-racial effects of average school percent black students. In this study, the heterogeneous effect model does not suggest that black students are more negatively impacted by school or classroom black population. One possibility is that the sample in my study is limited to magnet school lottery participants. The black students in our sample may be different from other black students in motivations and parental support, and less susceptible to the influence of black peers.

Although school percent black students does not show significant impact on student disciplinary behavior, classroom level analysis finds that having more black students in a class increases disciplinary incidents. There is no evidence that school or classroom black population influences individual attendance behavior in middle school.

Second, the effect of low income peers is not significant at the school level. The school level finding agrees with previous studies by Hanushek et al (2003) and McEwan (2004) which did not find significant impacts of percent low income students. However, classroom level analysis found that percent low income student is associated with lower academic achievement. The estimated effect at the classroom level is substantial: a 50%

change in classroom low income students leads to a 0.4 point decline in standardized score changes in student math scores, and a 0.35 point decline in reading scores.

Third, although school level estimates of average peer achievement are not significant, the average class peer achievement has a positive impact on student test scores in both subjects. Because this study examines peer effects on middle school students who are often grouped for classes based on academic abilities, it is likely that average school level peer achievement has minimum impact on student outcomes.[62] The positive estimate of classroom peer achievement effect echoes the findings in some literature, such as Betts and Zau (2004), and Lefgren (2007).

Interestingly, the school level analyses find that percent low SES students decreases disciplinary problems in middle school and average peer achievement increases individual disciplinary infractions. However, the estimated effects of the same peer variables at the classroom level yield totally opposite conclusions: middle school students are more likely to misbehave (measured by number of suspensions) if there are more peers from low income families; and students tend to behave better if the classmates have higher prior test scores (significant only for peer reading achievement). These results suggest that the effects of peers are stronger and more direct at the classroom level. The estimates of school level peer effects may well reflect how school student composition impacts disciplinary actions (suspensions, as measured in this section).

Fourth, while classroom level prior peer disciplinary infractions significantly decrease individual academic achievement and increase individual misconducts in middle school,

[62] Both Hoxby (2001) and Hanushek et al (2000) found that school level average peer achievement improves student test scores. However, their studies focus on elementary school students. Given that there is often general teaching and few ability tracking, the mix of students in any class may just mirror the school as a whole in elementary schools.

the effect of peer disciplinary records at school level is not significant at all. This again suggests that a stronger and more direct peer effect happens at the classroom level.

Finally, there is no evidence in our data that percent female students improves student academic achievement or decreases disciplinary incidences; and average peer attendance behavior does not show any significant impact on individual achievement or behavior at either the school or classroom level.

Another important finding from the peer effect models is that adding peer characteristics into the model totally overturn the significant treatment effect of the academic magnet school in many models. In particular, when the average classroom peer reading achievement is controlled, the estimate of academic magnet treatment effect on student math achievement becomes significantly negative. This implies that lottery losers who attend the neighborhood schools can do even better in academics than the academic magnet enrollees if the classroom peers are the same. It also indicates that superior peer composition makes a large contribution to the success of magnet programs.

Effects of peer heterogeneity

School level analyses find some significant effects of peer heterogeneity. The average student test scores in both subjects tend to increase if the variation in prior peer reading achievement is larger; and student disciplinary incidents decrease when the variation in peer academic qualities is larger.

However, there is little evidence that changes in the heterogeneity of classroom peer characteristics (prior peer achievement, disciplinary records, or attendance records) impact student academic achievement or behavioral outcomes. The findings at the classroom level do not suggest that within school ability grouping has a significant influence on student outcomes.

Heterogeneous peer effects

The estimates of peer group effects are different on some students depending on their demographic characteristics and academic abilities. First, although no evidence shows that academic achievement of black or low income students decline with a high proportion of peers from the same group, there is a strong intra-group impact on individual disciplinary outcomes for Blacks and low income students at both the school and classroom levels.

Second, the estimates of peer group effects are less strong on female students than on male students. This is true of both academic achievement and disciplinary outcomes at both the school and classroom levels.

Third, academically, both high and low performing students appear to be more negatively impacted by percent black or low income students, but less positively impacted by average peer academic achievement. However, a significant heterogeneous effect on disciplinary outcomes is only found for low performing students, who are more likely to receive suspensions if there is a high proportion of disadvantaged peers.

Finally, there is no significant heterogeneous peer effect on student attendance outcomes, either with respect to mean peer characteristics or their variance within the classroom.

Implications and Limitations

Research Implications

This dissertation yields its research implications on two fronts. First, this study adds an empirical piece to the growing literature that implements credible strategies to identify peer group effects on student outcomes. Second, the various methods used in this study to deal with complications in randomized data have important implications for research with experimental design.

Empirical studies on peer effects are plagued by the critical issues of selection bias and simultaneity bias. This dissertation improves on previous attempts to eliminate self selectivity by employing both randomization and instrument variable methods. In specific, the identification strategies used in this study ensures the estimation of peer effects free of selectivity by exploiting randomly determined lottery outcomes to construct exogenous sources of variation in peer characteristics.

In recent years, two pieces of federal legislation the *No Child Left Behind Act of 2001* (NCLB) and the *Education Sciences Reform Act of 2002* (ESRA) have led to a rising demand of experimental research using random assignment to produce more rigorous evidences in evaluating education intervention programs. While randomization through experiments is considered as the most promising method to obtain unbiased estimates of causal effects, there are many complications in social experiments that may threat the validity of the randomness, such as non-compliance, selective attrition, and heterogeneous responses to treatment effects. This study presents detailed discussions of the complications that arise in the magnet program admission lotteries. The solutions to these complications adopted here contribute to the education research by offering workable approaches to using these sorts of data to address policy questions.

Policy Implications

From the perspective of practice, this study also has important policy implications. Although attending the academic magnet program in the district studied improves student academic achievement in both math and reading, this effect can be entirely accounted for by the peer characteristics. While this finding suggests that peer group characteristics make a large contribution to the magnet school success, it also implies that district administrators or policy makers need alternative assessment models while evaluating schools and teachers based on student test scores. Student characteristics or peer compositions should be incorporated in the models assessing teacher (school) effectiveness, or schools and teachers will be held accountable for factors that are beyond their control.

Second, scholars have argued that socioeconomic isolation of poor, minority students in urban school systems is a major cause of the continuing achievement gap (see Kahlenberg (2001) for a summary). Magnet program has been considered as an important mechanism to reform urban districts through decreasing racial or social-economical segregation as well as improving student achievement. However, this study finds that students from magnet schools are mostly benefiting from superior peer groups. This finding provides some evidences to the policy makers that magnet schools have not actually reduced the socioeconomic isolation but may have exacerbated the within-district inequality by creaming off good students from conventional schools.

Third, this study finds large and negative impact of average peer disciplinary infractions on student outcomes (both academic and behavior), which indicates that middle school students are very sensitive to the misconducts of their peers. Therefore, in addition to the standard test scores of students, reduction of student misbehavior and improvement of safe school environment should also be an important element in assessing school effectiveness.

Finally, there is no evidence from either the school level or classroom level analyses that change in the heterogeneity of peer composition influences student outcomes. Moreover, the heterogeneous effect models do not find that any specific student group is significantly impacted by the heterogeneity of peer characteristics. These findings suggest that ability tracking per se has no impact on student achievement, at least not on the lottery participants in the district studied.

Limitations

This study is limited in several respects. First, the investigation of peer effects in this study relies on data from only one district. Compared to other studies using state-wide data (such as the Texas studies by Hanushek et al. and Hoxby, and the North Carolina study by Vigdor and Nechby etc), this study contains far less observations. Although the district examined in this study is similar to other urban school systems in many ways, how well these findings generalized is unknown.

Second, because the research design relies on lottery randomization to estimate peer effects, the inferences are limited to a subset of students who participate in the lotteries. This strategy excludes the large number of students who do not express an interest in attending magnet schools, and the smaller number of students who are admitted in other ways (e.g., through sibling preferences or neighborhood zone preferences). Similarly, the conclusions about peer effects from this study can not be fully extended to students unlike those participating in the lotteries.

Third, in order to circumvent the simultaneity bias, this project does not estimate the endogenous peer effects --- contemporaneous peer achievement or behavioral measures. Instead, the investigation of peer effects focuses on contextual effects, represented by peer academic ability (measured by lagged academic outcomes) and other predetermined peer characteristics. While endogenous peer effects imply potentially large social multiplier effects and efficiency gains through the feedback in the behavior of individuals within an existing social network (e.g., Hoxby argues that positive student behavior leads to more positive behavior in the network), contextual peer effects do not have these dynamic implications.

Finally, using lagged values of peer scores and behavioral records may not completely remove the reflection problems due to the serial correlations between the measurement errors in the lagged outcomes and those in current outcomes. However, controlling for student prior outcomes in the data greatly reduces the correlation between current outcomes and the measurement errors in lagged peer outcomes. Moreover, given the facts that we use students' 4th grade scores to construct peer academic ability measures and most students in the district go to a new middle school after 4th grade, the serial correlation are less likely to cause significant simultaneity bias in the estimation of the effects from current peers.

Appendix A

Prediction of Peer Characteristics in Neighborhood Schools (P_N)

As introduced in Chapter III, although the admission lottery randomly assigns lottery participants to a treatment group (a magnet school) or a control group (a neighborhood school), neighborhood schools still vary across lottery participants. The neighborhood school where a lottery participant is expected to attend if not admitted by a magnet school reflects family residence choices, and is very likely to be related to unobserved factors that also influence student outcomes. In order to eliminate the selection bias arising from neighborhood choices, we include the variable P_N as an independent variable in the regression.

However, P_N is not observable for students who are enrolled in the magnet schools. Therefore, we need to predict the value of P_N. The prediction of P_N is based on the sample of students who lost all middle school lotteries and attended regular public schools in the district. The prediction model can be specified as:

$$P_{N,igl} = f(X_{igl}, S_{igl}) \qquad \text{(A.1)}$$

where $P_{N,igl}$ is a set of neighborhood school peer variables for lottery loser *i* in grade *g* and lottery year *l* (cohort indicator), X_{igl} is a vector of lottery losers' demographic characteristics and prior achievements, S_{igl} indicates the elementary school where lottery loser *i* attended in 4th grade. As indicated by the model, the prediction models are run for every grade and every lottery cohort separately. Although the regression is only limited to the sample of lottery losers, the predicted value of $\hat{P}_N$ is assigned to all lottery participants in the same grade and same cohort[63]. For example, if student A and student B are from the same elementary school and share very similar demographic attributes (race, gender, SES, special ed, and ELL) as well as 4th grade test scores, each of them is assigned a predicted $\hat{P}_N$ with close values regardless of their lottery outcomes.

The following table provides some descriptive statistics of predicted counterfactual neighborhood school peer variables ($\hat{P}_N$) for 5th graders in two cohorts: lottery year 2000 and lottery year 2001. The second column ("Group") indicates that students are from the same 4th grade school, same ethnicity group and same social economic status group. As we can see from the table, regardless of their lottery outcomes, the predicted value of neighborhood school peer characteristics are very similar for students sharing same attributes in race, SES and elementary schools.

Table A.1 Counterfactual School Peer Variables for 5th graders

Lottery Year	**Group**	**Lottery outcomes**	**Obs**	**Predicted Neighborhood school Peer Variables**							
				Black		Low SES		Grade 4 Math		Grade 4 Reading	
				Pct	SD	Pct	SD	Mean	SD	Mean	SD
2000	1	Win	11	33.1	2.7	11.4	1.9	0.67	0.06	0.82	0.07
		Lose	13	32.9	2.4	11.1	1.7	0.67	0.07	0.84	0.08
2000	2	Win	18	28.5	1.5	18.1	2.5	0.44	0.04	0.61	0.05
		Lose	7	28.9	1.4	19.1	2.2	0.42	0.04	0.58	0.05
2001	3	Win	20	31.6	1.9	30.0	2.3	0.42	0.02	0.42	0.02
		Lose	13	32.7	2.3	31.7	3.2	0.43	0.03	0.44	0.03
2003	4	Win	25	31.3	7.6	31.3	5.5	0.16	0.03	0.30	0.03
		Lose	8	30.6	4.4	31.0	4.4	0.15	0.01	0.29	0.02

Note: Lottery outcome 'WIN' means the student won at least one lottery;
'LOSE' means the student lost all lotteries.

[63] In order to avoid introducing a difference between students whose P_N is observed (lottery losers) and students whose P_N is a counterfactual prediction, the predicted $\hat{P}_N$ are used in all final models for all students.

Appendix B

Prediction of Enrollment Probability ($\hat{d}_{iM}$)
and Construction of School Level Peer Variable Instruments

I. Prediction of Enrollment Probability

In order to eliminate the endogeneity of the peer term $P_M d_i + P_N(1-d_i)$,[64] we instrumented the peer variable with an exogenous instrumental variable $[\overline{P}_M \hat{d}_{iM} + \hat{P}_N(1-\hat{d}_{iM})]$. The external peer term $[\overline{P}_M \hat{d}_{iM} + \hat{P}_N(1-\hat{d}_{iM})]$ contains three parts: the mean value of magnet school peer characteristics $\overline{P}_M$; the predicted counterfactual neighborhood school peer characteristics $\hat{P}_N$, and the magnet school enrollment probability $\hat{d}_{iM}$. Since there are two magnet programs in this study --- the academic magnet and the non-academic composite, the instrument for peer variables is actually constructed as $\sum P_{Mj}\hat{d}_{Mj} + \hat{P}_N(1-\sum \hat{d}_{Mj})$, where $\hat{d}_{Mj}$ is an estimate of the probability that a student attends magnet school j (j=A for the academic magnet, j=NA for the non-academic magnet as defined in this part).

Because winning the lottery is the only way through which a lottery participant got admitted by a magnet school, the prediction of magnet school enrollment probability is mainly based on lottery outcomes. First, we separate all lottery participants into 4 groups based on lottery outcomes[65]:

Group1: win_academic=1; win_nonacademic=0

[64] As introduced in chapter III, the endogeneity arises from the non-compliances among the lottery participants.
[65] The win variable is defined as 1 if either outright win or delayed win variable is 1.

Group2: win_academic=0, win_nonacademic=1

Group3: win_academic=1; win_nonacademic=1

Group4: win_academic=0; win_nonacademic=0

Accordingly, each student received 4 group indicators: group1=1 for students in the 1st group, 0 otherwise, and till group4.

Second, we define 4 magnet school enrollment probability variables for every lottery participants in the sample: d_{AM1} and d_{AM2} as the enrollment probabilities in the academic magnet school; and d_{NAM1} and d_{NAM2} as the non-academic magnet composite enrollment probabilities. The initial values of all four probability variables are set to be zero.

Third, we run the regression models to predict magnet school enrollment probabilities for students in each group separately. In specific, the equations are:

For Group1 students:

$$AM1_i = f(X_i, L_i, LO_i, G_i) \tag{B.1}$$

For Group3 students:

$$AM2_i = f(X_i, L_i, LO_i, G_i) \tag{B.2}$$

For Group2 students:

$$NAM1_i = f(X_i, L_i, LO_i, G_i) \tag{B.3}$$

For Group3 students:

$$NAM2_i = f(X_i, L_i, LO_i, G_i) \tag{B.4}$$

where $AM1_i$ is a dichotomous variable indicating whether student *i* from group 1 is enrolled in the academic magnet school, $AM2_i$ for student *i* from group 3; $NAM1_i$ indicates the non-academic magnet enrollment from students from group 2,

$NAM2_i$ for students from group 3. Other variables included in the models are: X_i is a vector of individual characteristics; L_i is a lottery indicator which combines the information of lottery year and lottery application; LO_i is the lottery outcome indicator including outright win and delayed win for both magnet programs; and G_i is a grade indicator. The predicted value of the dependent variable in each model is then assigned to every student to replace the initial zero value of the enrollment probability: $\hat{d}_{AM1} = A\hat{M}1_i$, $\hat{d}_{AM2} = A\hat{M}2_i$; $\hat{d}_{NAM1} = N\hat{A}M1_i$, $\hat{d}_{NAM2} = N\hat{A}M2_i$.

Based on the predicted magnet school enrollment probabilities, we also defined 4 variables representing the enrollment probabilities in non-magnet schools: $\hat{d}_{NM1} = 1 - \hat{d}_{AM1}$; $\hat{d}_{NM2} = 1 - \hat{d}_{NAM1}$; $\hat{d}_{NM3} = 1 - \hat{d}_{AM2} - \hat{d}_{NAM2}$; $\hat{d}_{NM4} = 1$. Therefore, each student in our sample has 4 group indicator variables, 4 magnet school enrollment probability variables, and 4 non-magnet school enrollment probability variables. Finally, the enrollment probabilities for the academic magnet school, the non-academic magnet composite and non magnet schools are:

Academic Magnet School: $\hat{d}_{AM} = \hat{d}_{AM1} * group1 + \hat{d}_{AM2} * group3$ (B.5)

Non-academic Magnet School: $\hat{d}_{NAM} = \hat{d}_{NAM1} * group2 + \hat{d}_{NAM2} * group3$ (B.6)

Non Magnet School:

$$\hat{d}_{NM} = \hat{d}_{NM1} * group1 + \hat{d}_{NM2} * group2 + \hat{d}_{NM3} * group3 + \hat{d}_{NM4} * group4 \quad \text{(B.7)}$$

The following Table B.2 presents the predicted enrollment probabilities for 5th grade students in different groups across lottery years. The predicted values of enrollment probabilities reveal similar messages as shown in Table 3.3 (magnet school lotteries and 5th grade enrollment) in Chapter III: there is a high non-compliance rate among lottery participants of the non-academic magnet composite; most students who won both lotteries chose to attend the academic magnet.

Table B.2 Enrollment Probability by lottery outcome Groups (5th Grade)

		Enrollment Probability					
Lottery Year	Group	Academic		Non-Academic		Non Magnet	
		Mean	SD	Mean	SD	Mean	SD
1999	1	0.77	0.09	0.00	0.00	0.23	0.09
	2	0.00	0.00	0.54	0.08	0.46	0.08
	3	0.74	0.20	0.04	0.12	0.22	0.13
	4	0.00	0.00	0.00	0.00	1.00	0.00
2000	1	0.76	0.12	0.00	0.00	0.24	0.12
	2	0.00	0.00	0.59	0.08	0.41	0.08
	3	0.68	0.13	0.07	0.05	0.26	0.10
	4	0.00	0.00	0.00	0.00	1.00	0.00
2001	1	0.82	0.09	0.00	0.00	0.17	0.09
	2	0.00	0.00	0.57	0.07	0.43	0.07
	3	0.71	0.16	0.16	0.12	0.13	0.09
	4	0.00	0.00	0.00	0.00	1.00	0.00
2002	1	0.87	0.07	0.00	0.00	0.13	0.07
	2	0.00	0.00	0.64	0.08	0.36	0.08
	3	0.84	0.12	0.06	0.06	0.11	0.08
	4	0.00	0.00	0.00	0.00	1.00	0.00
2003	1	0.86	0.07	0.00	0.00	0.14	0.07
	2	0.00	0.00	0.66	0.08	0.34	0.08
	3	0.70	0.16	0.10	0.13	0.20	0.09
	4	0.00	0.00	0.00	0.00	1.00	0.00

Note: Group1 students only won the academic magnet lottery;
Group2 students only won the non-academic lottery;
Group3 won both lotteries; Group4 students lost both lotteries

II. Construction of School Level Peer Variable Instrument

After predicting the enrollment probability, the next step is to construct the instrumental variable $\sum P_{Mj}\hat{d}_{Mj} + \hat{P}_N(1-\sum \hat{d}_{Mj})$, or $\sum P_{Mj}\hat{d}_{Mj} + \hat{P}_N \hat{d}_{NM}$. In specific, the instrumental variable for peer characteristics is constructed as:

$$\sum P_{Mj}\hat{d}_{Mj} + \hat{P}_N(1-\sum \hat{d}_{Mj}) =$$

$$\begin{aligned}&\overline{P}_{AM}*(\hat{d}_{iAM1}*group1+\hat{d}_{iAM2}*group3)+\overline{P}_{NAM}*(\hat{d}_{iNAM1}*group2+\hat{d}_{iNAM2}*group3)\\&+\hat{P}_N*(\hat{d}_{iNM1}*group1+\hat{d}_{iNM2}*group2+\hat{d}_{iNM3}*group3+\hat{d}_{iNM4}*group4)\end{aligned} \qquad \text{(B.8)}$$

where $\overline{P}_{AM}$ is the mean value of school peer variable in the academic magnet school, $\overline{P}_{NAM}$ represents mean school peer variable in the non-academic composite, and $\hat{P}_N$ is the predicted neighborhood school peer characteristics.

Note that there are 4 schools in the non-academic magnet composite. Therefore, the values of $\overline{P}_{NAM}$ is a weighted mean of the average peer characteristics in each non-academic magnet school, wherein the weight is the fraction of numbers of outright winner in each school over the total numbers of outright winners in the composite:

$$\overline{P}_{NAM} = \sum_{j=1}^{4} \overline{P}_{jNAM} *(No.outright\_winners_{jNAM} \Big/ \sum_{j=1}^{4} No.outright\_winners_{jNAM}) \qquad \text{(B.9)}$$

Appendix C

Construction of Instruments for Classroom Level Peer Variables

In order to eliminate the endogeneity of the classroom peer variable P_{cij}, we instrumented this variable using the model

$$P_{cij} = \pi[\hat{P}_{cM}\hat{d}_{iM} + \hat{P}_{cN}(1-\hat{d}_{iM})] + \varphi X_i + \varepsilon_i \qquad \text{(1}^{\text{st}}\text{ stage IV model)}$$

where $\hat{P}_{cM}\hat{d}_{iM} + \hat{P}_{cN}(1-\hat{d}_{iM})$ serves as the external instrumental variable. There are three components in this instrumental variable: $\hat{d}_{iM}$ is an prediction of the probability that student i attend the magnet school; $\hat{P}_{cM}$ is the predicted classroom peer variable in magnet schools, which serves as the counterfactual class peers that lottery losers would encounter in classes if they had won the magnet school lottery; $\hat{P}_{cN}$ is the predicted classroom peer variable in neighborhood schools, which serves as the counterfactual class peers that lottery winners would encounter in classes if they had lost the magnet school lottery and enrolled in a neighborhood school.

Since the prediction of the first component $\hat{d}_{iM}$ has been introduced in Appendix B, this part will focus on explaining the procedures predicting $\hat{P}_{cM}$ and $\hat{P}_{cN}$. The prediction of $\hat{P}_{cM}$ is done separately for each magnet school (the academic magnet and the four non-academic magnet schools) by cohort and by grade --- for example, the models that predict class peer characteristics in the academic magnet school $\hat{P}_{cAM}$ are only limited to a sample of students enrolling in this academic magnet school. Moreover, because students in middle schools are often grouped for instructions based on their academic ability (or possibly based on behavioral problem), the prediction models include all individual demographic variables as well as prior outcomes (academic and behavior outcomes); the prediction also controls for a set of student 4th grade school indictors given that student elementary school may carry some unobserved information influencing both outcomes and class assignments in middle school. The prediction model for P_{cM} can be expressed as the following:

$$P_{cjM,igl} = f(X_{igl}, S_{igl}) \qquad \text{(C.1)}$$

where $P_{cjM,igl}$ indicates the classroom peer characteristics in magnet school *j* for student *i* in grade *g* and lottery year (cohort) *l*, X_{igl} is a vector of individual demographic variables and prior outcomes, and S_{igl} represents 4th grade school indicators. Although the prediction of P_{cjM} is only limited to students enrolled in magnet school *j*, the predicted value is assigned to every student in the same grade and cohort regardless of the lottery status and enrollment status. Similarly, the predicted classroom peer characteristics in the non-academic magnet composite is a weighted mean of the class peer variables in all non-academic magnet schools

$$\overline{\hat{P}}_{cNAM} = \sum_{j=1}^{4} \hat{P}_{cjNAM} * (No.outright\_winners_{jNAM} \Big/ \sum_{j=1}^{4} No.outright\_winners_{jNAM}) \text{(C.2)+}$$

The prediction of classroom peer characteristics in the neighborhood schools $\hat{P}_{cN}$ focuses on the sample of lottery losers. Different from models predicting P_{cjM}, the prediction of P_{cN} is conducted within each elementary school, so that there are more heterogeneous class assignments for students from the same elementary school. In specific, the prediction model is:

$$P_{cN,igsl} = f(X_{igsl}) \qquad \text{(C.3)}$$

where $P_{cN,igsl}$ is the classroom peer characteristics for lottery loser *i* (who is from elementary school *s* in 4th grade) in grade g and lottery year l, and X_{igsl} is a set of individual factors. After each prediction, the predicted value of $\hat{P}_{cN}$ is assigned to both lottery winners and lottery losers in the same grade and cohort.

Each student in our sample is assigned both predicted values of $\hat{P}_{cM}$ and $\hat{P}_{cN}$, which are incorporated with the enrollment probabilities to construct instrumental variables for the true classroom peer characteristics:

$$\hat{P}_{cM}\hat{d}_{iM} + \hat{P}_{cN}(1-\hat{d}_{iM}) =$$

$$\begin{aligned}&\hat{P}_{ic\,AM} * (\hat{d}_{iAM1} * group1 + \hat{d}_{iAM2} * group3) + \overline{\hat{P}}_{ic\,NAM} * (\hat{d}_{iNAM1} * group2 + \hat{d}_{iNAM2} * group3) \\ &+ \hat{P}_{icN} * (\hat{d}_{iNM1} * group1 + \hat{d}_{iNM2} * group2 + \hat{d}_{iNM3} * group3 + \hat{d}_{iNM4} * group4)\end{aligned} \qquad \text{(C.4)}$$

As Table 3.4 in chapter III already shows, the instrumental variables for classroom peer characteristics are very close to the true values of classroom peer variables.

Appendix D

Validity of the Instrumental Variables

A valid instrumental variable has to meet two requirements: first, the IV must be orthogonal to the error term; second, the IV must be correlated to the endogenous regressor of interest. All peer effect models in this study include two sets of external instruments: lottery outcomes (r_{1i}, r_{2i}) as instruments for the magnet school treatment effect d_i; $\overline{P}_M \hat{d}_{iM} + \hat{P}_N (1 - \hat{d}_{iM})$ as the instruments for school level peer characteristics, or $\hat{P}_{cM} \hat{d}_{iM} + \hat{P}_{cN} (1 - \hat{d}_{iM})$ as the instruments for classroom level peer characteristics.

The omnibus over-identification tests of all regression models in Chapter IV to Chapter VI (As shown at the bottom of the result tables) have provided strong evidence that both sets of external instruments are orthogonal to the error terms and meet the first validity requirement of exogeneity. Therefore, in this part, I will examine whether the instruments meet the second validity requirement: correlated with the regressors of interest.

As introduced in Chapter II, the estimates of peer effect are yielded from a 2 Stage Lease Square (2SLS), wherein the first level model predicts the endogenous peer variables from the instrumental variables. Therefore, the coefficients on the instrumental variables from the first stage regression represent the correlation between the instruments and the regressors of interest.

Table D.1 presents the estimates of first stage models of school level peer effects on math achievement (the single variable models in Table 4.6 in Chapter IV). There are 10 regression models included in the table: one for each school level peer characteristics. Each regression model includes three endogenous regressors: the academic magnet school treatment indicator, the non-academic composite treatment indicator, and the peer variable. Accordingly, there are three separate first stage models for each regression. The first 1st stage model (dependent variable is d_{iAM}, the academic magnet treatment indicator) includes two external instruments: outright_win$_{AM}$, delayed_win$_{AM}$; the second 1st model (dependent variable is d_{iNAM}, the non academic composite treatment indicator) also includes two external instruments: outright_win$_{NAM}$, delayed_win$_{NAM}$; the third 1st stage model (dependent variable is each school level peer variable) has the instrument constructed as $\sum \overline{P}_{Mj}\hat{d}_{Mj} + \hat{P}_N(1-\sum \hat{d}_{Mj})$.

As we can see from Table D.1, the three sets of external instruments are significantly correlated with the regressors of interest in all 10 models at 0.1% statistical level, which implies that the instruments meet the second validity requirement. The magnitude of the coefficients on lottery outcome variables for the academic magnet school is much larger than for the non-academic composite. This is because of the high non-compliance rate among non-academic composite applicants and the multiple lotteries (students who won both the academic magnet and the non-academic composite rarely chose to attend the latter one). The coefficients on most peer variable IVs are larger than 0.75, suggesting a high correlation between the instruments and the endogenous peer variables. The coefficients on the peer variables Percent Special ED students, Percent Hispanics, and Percent ELL students are less sizeable, due to the high non-compliance rate among these students.

Table D.1 First Stage Regression (School Level): Correlations between Endogenous Regressors and the Instrumental Variables

	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10
Magnet School Treatment Indicators										
Academic Magnet										
Outright Win	0.67***	0.62***	0.73***	0.73***	0.76***	0.76***	0.53***	0.54***	0.74***	0.73***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Delayed Win	0.57***	0.53***	0.63***	0.62***	0.65***	0.65***	0.46***	0.46***	0.63***	0.62***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Non-Academic Composite										
Outright Win	0.27***	0.29***	0.25***	0.28***	0.29***	0.29***	0.29***	0.29***	0.29***	0.26***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Delayed Win	0.24***	0.26***	0.22***	0.25***	0.26***	0.27***	0.26***	0.26***	0.26***	0.23***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Variables										
Black (Proportion)										
Black IV	0.80***									
	(0.02)									
Low SES (Proportion)										
Low SES IV		0.81***								
		(0.02)								
Female (Proportion)										
Female IV			0.78***							
			(0.02)							
Special ED (Proportion)										
Special ED IV				0.29***						
				(0.02)						
Hispanic (Proportion)										
Hispanic IV					0.42***					
					(0.02)					
ELL (Proportion)										
ELL IV						0.19***				
						(0.01)				
Mean G4 Math										
Mean G4 Math IV							0.67***			
							(0.03)			
Mean G4 Reading										
Mean G4 Reading IV								0.69***		
								(0.01)		
Mean G4 Suspension										
Mean G4 Suspension IV									0.94***	
									(0.03)	
Mean G4 Absence										
Mean G4Absence IV										0.76***
										(0.03)

Table D.2 reports the estimates of first stage models of classroom level peer effects on math achievement (the single variable models in Table 5.1 in Chapter V). The coefficients on the lottery outcomes for both magnet programs are very similar to the estimates from school level models in last Table D.1, suggesting that the instruments and the endogenous magnet school treatment variables are highly correlated. Although the estimates of the classroom peer variable IVs are less sizeable than those from school level models, the coefficients from all 10 models are significant at 0.1% statistical level. The results again prove that the external instruments in our classroom level peer effect models meet the second validity requirement of high correlation with the regressors.

Table D.2 First Stage Regression (Classroom Level): Correlations between Endogenous Regressors and the Instrumental Variables

	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10
Magnet School Treatment Indicators										
Academic Magnet										
Outright Win	0.76***	0.75***	0.77***	0.77***	0.77***	0.77***	0.76***	0.74***	0.77***	0.77***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Delayed Win	0.68***	0.67***	0.69***	0.69***	0.69***	0.69***	0.67***	0.66***	0.69***	0.69***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Non-Academic Composite										
Outright Win	0.29***	0.29***	0.29***	0.30***	0.29***	0.30***	0.29***	0.29***	0.29***	0.29***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Delayed Win	0.27***	0.28***	0.28***	0.28***	0.28***	0.28***	0.28***	0.27***	0.28***	0.27***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Variables										
Black (Proportion)										
Black IV	0.56***									
	(0.01)									
Low SES (Proportion)										
Low SES IV		0.54***								
		(0.01)								
Female (Proportion)										
Female IV			0.34***							
			(0.01)							
Special ED (Proportion)										
Special ED IV				0.23***						
				(0.02)						

Table D.2 (Continued)

Hispanic (Proportion)						
Hispanic IV	0.15***					
	(0.02)					
ELL (Proportion)						
ELL IV		0.12***				
		(0.01)				
Mean G4 Math						
Mean G4 Math IV			0.42***			
			(0.01)			
Mean G4 Reading						
Mean G4 Reading IV				0.56***		
				(0.02)		
Mean G4 Suspension						
Mean G4 Suspension IV					0.28***	
					(0.01)	
Mean G4 Absence						
Mean G4Absence IV						0.24***
						(0.01)

In conclusion, the results from both the over-identification tests and the 1st stage models suggest that the three sets of external instrumental variables in our peer effect models are valid: they are exogenous to the error terms; and they are correlated with the endogenous regressors.

Appendix E

Linear Combination Results of Heterogeneous Peer Effects

Both school level and classroom level analyses find that some students are impacted by their peers more strongly than other students. The tables in Chapter IV (Table 4.10 and Table 4.11) and Chapter V (Table 5.5 and Table 5.6) present the estimates of heterogeneous effects for seven different student groups. This part will report the coefficients on peer variables from the linear combination tests, which shows the estimates of peer effects on different students depending on their own background.

Since I have discussed many of the linear combination test coefficients in Chapter IV to Chapter VI, this part will only reports the results.

I. Math Achievement

School Level

Table E.1 School Level Peer Effects on Math Achievement (Linear Combination Tests)

	Math Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Ahiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean + Interaction	-0.53***	-0.52***	-0.34*	-0.56***	-0.61***	-0.61***	-0.64***
	(0.14)	(0.15)	(0.15)	(0.15)	(0.16)	(0.15)	(0.15)
Proportion Low SES Students							
Mean + Interaction	-0.33	-0.22	-0.26	-0.42*	-0.48*	-0.49**	-0.49**
	(0.18)	(0.20)	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)
Peer Prior Math Achievement							
Mean + Interaction	0.09	0.03	0.06	-0.02	-0.04	0.01	-0.10
	(0.16)	(0.17)	(0.17)	(0.17)	(0.17)	(0.16)	(0.17)
Peer Prior Reading Achievement							
Mean + Interaction	0.14	0.07	0.10	0.04	0.04	0.07	-0.01
	(0.14)	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)
Peer Prior Disciplinary Records							
Mean + Interaction	0.01	0.02	0.02	-0.22	-0.32	-0.44*	-0.62**
	(0.21)	(0.25)	(0.20)	(0.21)	(0.24)	(0.21)	(0.22)
Peer Prior Attendance Records							
Mean + Interaction	5.83	6.36	2.91	0.65	-0.42	-0.44	-1.16
	(5.40)	(6.15)	(4.89)	(4.69)	(4.71)	(4.69)	(4.73)

* p<0.05, ** p<0.01, *** p<0.001

Classroom Level

Table E.2 Heterogeneous Class Level Peer Effects on Math Achievement (Linear Combination Tests) (Math Class)

	Math Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean + Interaction	-0.63***	-0.59***	-0.58***	-0.71***	-0.76***	-0.78***	-0.83***
	(0.10)	(0.11)	(0.10)	(0.09)	(0.10)	(0.09)	(0.10)
Proportion Low SES Students							
Mean + Interaction	-0.81***	-0.73***	-0.69***	-0.86***	-0.94***	-0.93***	-0.91***
	(0.11)	(0.13)	(0.11)	(0.10)	(0.11)	(0.10)	(0.11)
Peer Prior Math Achievement							
Mean + Interaction	0.37***	0.35***	0.29***	0.30***	0.25***	0.29***	0.25***
	(0.06)	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Peer Prior Reading Achievement							
Mean + Interaction	0.41***	0.40***	0.37***	0.36***	0.36***	0.36***	0.34***
	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Peer Prior Disciplinary Records							
Mean + Interaction	-0.44	-0.58**	-0.60**	-0.77***	-0.71	-0.92***	-0.77*
	(0.23)	(0.21)	(0.23)	(0.21)	(0.40)	(0.20)	(0.32)
Peer Prior Attendance Records							
Mean + Interaction	-5.08	-0.57	-5.49	-8.53*	-8.77*	-8.99*	-8.43*
	(7.28)	(6.32)	(5.65)	(4.18)	(3.94)	(4.10)	(3.99)

* p<0.05, ** p<0.01, *** p<0.001

II. Reading Achievement

School Level

Table E.3 Heterogeneous School Level Peer Effects on Reading Achievement (Linear Combination Tests)

	Reading Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean + Interaction	-0.23	-0.15	-0.11	-0.33**	-0.53***	-0.24	-0.24
	(0.13)	(0.15)	(0.14)	(0.14)	(0.15)	(0.14)	(0.14)
Proportion Low SES Students							
Mean + Interaction	0.02	0.06	0.09	-0.12	-0.34*	-0.01	0.02
	(0.17)	(0.19)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)
Peer Prior Math Achievement							
Mean + Interaction	-0.24	-0.30	-0.27	-0.39*	-0.38*	-0.26	-0.32*
	(0.15)	(0.17)	(0.16)	(0.17)	(0.16)	(0.16)	(0.16)
Peer Prior Reading Achievement							
Mean + Interaction	-0.24	-0.30*	-0.29*	-0.41**	-0.39**	-0.28	-0.33*
	(0.14)	(0.15)	(0.14)	(0.15)	(0.15)	(0.14)	(0.14)
Peer Prior Disciplinary Records							
Mean + Interaction	-0.29	-0.12	-0.37	-0.65***	-0.95***	-0.33	-0.55**
	(0.20)	(0.23)	(0.19)	(0.20)	(0.23)	(0.20)	(0.21)
Peer Prior Attendance Records							
Mean + Interaction	10.69*	7.72	11.36*	9.45*	7.36	11.54**	11.59**
	(5.12)	(5.84)	(4.65)	(4.45)	(4.46)	(4.47)	(4.50)

* p<0.05, ** p<0.01, *** p<0.001

Classroom Level

Table E.4 Heterogeneous Class Level Peer Effects on Reading Achievement (Linear Combination Tests) (Reading Class)

	Reading Scores						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean + Interaction	-0.50***	-0.49***	-0.39***	-0.62***	-0.83***	-0.54***	-0.51***
	(0.10)	(0.11)	(0.10)	(0.10)	(0.10)	(0.09)	(0.10)
Proportion Low SES Students							
Mean + Interaction	-0.74***	-0.71***	-0.54***	-0.85***	-1.07***	-0.76***	-0.63***
	(0.11)	(0.12)	(0.11)	(0.10)	(0.11)	(0.10)	(0.11)
Peer Prior Math Achievement							
Mean + Interaction	0.28***	0.25***	0.20**	0.17**	0.10	0.24**	0.20**
	(0.07)	(0.08)	(0.07)	(0.07)	(0.08)	(0.07)	(0.08)
Peer Prior Reading Achievement							
Mean + Interaction	0.35***	0.31***	0.29***	0.24***	0.20***	0.31***	0.29***
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.05)	(0.05)
Peer Prior Disciplinary Records							
Mean + Interaction	-1.22***	-0.96***	-0.81***	-1.36***	-1.61***	-1.05***	-0.85**
	(0.20)	(0.19)	(0.21)	(0.17)	(0.33)	(0.17)	(0.28)
Peer Prior Attendance Records							
Mean + Interaction	-1.50	-5.39	3.71	-3.21	-4.57	-1.16	-0.69
	(9.78)	(9.04)	(8.44)	(6.05)	(5.83)	(5.99)	(5.88)

* p<0.05, ** p<0.01, *** p<0.001

III. Disciplinary Infractions

School Level

Table E.5 Heterogeneous School Level Peer Effects on Disciplinary Infractions (Linear Combination Tests)

	Suspension Times						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean + Interaction	0.56	0.90*	-0.17	0.25	0.12	0.24	0.26
	(0.34)	(0.36)	(0.36)	(0.35)	(0.36)	(0.35)	(0.36)
Proportion Low SES Students							
Mean + Interaction	-0.33	-0.19	-1.23**	-0.91*	-1.05*	-0.87*	-0.90*
	(0.43)	(0.48)	(0.43)	(0.43)	(0.43)	(0.43)	(0.43)
Peer Prior Math Achievement							
Mean + Interaction	1.57***	1.50***	1.93***	1.99***	2.15***	1.82***	2.20***
	(0.40)	(0.43)	(0.41)	(0.43)	(0.43)	(0.41)	(0.42)
Peer Prior Reading Achievement							
Mean + Interaction	1.13***	1.09***	1.50***	1.50***	1.65***	1.36***	1.68***
	(0.35)	(0.37)	(0.36)	(0.38)	(0.38)	(0.36)	(0.36)
Peer Prior Disciplinary Records							
Mean + Interaction	0.42	0.24	-0.25	-0.05	-0.42	0.46	0.01
	(0.50)	(0.57)	(0.48)	(0.50)	(0.57)	(0.49)	(0.52)
Peer Prior Attendance Records							
Mean + Interaction	-32.44*	-40.50**	-35.17**	-33.33**	-32.83**	-33.45**	-30.85**
	(12.81)	(14.22)	(11.55)	(11.08)	(11.13)	(11.11)	(11.19)

* p<0.05, ** p<0.01, *** p<0.001

Classroom Level

Table E.6 Heterogeneous Class Level Peer Effects on Disciplinary Infractions (Linear Combination Tests) (Math Class)

	Suspension Times						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean + Interaction	1.43***	1.88***	0.71**	1.11***	0.95***	1.15***	1.11***
	(0.23)	(0.26)	(0.23)	(0.21)	(0.24)	(0.22)	(0.23)
Proportion Low SES Students							
Mean + Interaction	2.09***	2.26***	0.88***	1.45***	1.15***	1.44***	1.21***
	(0.27)	(0.29)	(0.26)	(0.24)	(0.27)	(0.24)	(0.26)
Peer Prior Math Achievement							
Mean + Interaction	-0.50**	-0.69***	-0.12	-0.35**	-0.11	-0.32*	-0.14
	(0.14)	(0.15)	(0.13)	(0.13)	(0.14)	(0.13)	(0.14)
Peer Prior Reading Achievement							
Mean + Interaction	-0.72***	-0.84***	-0.38**	-0.51***	-0.33*	-0.51***	-0.33**
	(0.12)	(0.13)	(0.12)	(0.12)	(0.13)	(0.12)	(0.13)
Peer Prior Disciplinary Records							
Mean + Interaction	2.90***	2.09***	1.28*	2.10***	1.65	2.23***	2.03**
	(0.53)	(0.48)	(0.53)	(0.46)	(0.95)	(0.45)	(0.75)
Peer Prior Attendance Records							
Mean + Interaction	83.47***	24.91	36.84**	30.97**	29.31**	29.66**	29.57**
	(17.71)	(14.69)	(13.43)	(9.86)	(9.31)	(9.71)	(9.43)

* p<0.05, ** p<0.01, *** p<0.001

IV. Attendance Behavior

School Level

Table E.7 Heterogeneous School Level Peer Effects on Attendance Behavior (Linear Combination Tests)

	Absence Rate						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean + Interaction	-0.00	-0.01	-0.00	0.00	0.00	-0.00	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Proportion Low SES Students							
Mean + Interaction	-0.00	0.00	0.01	0.01	0.02	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Prior Math Achievement							
Mean + Interaction	-0.00	-0.00	-0.00	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Prior Reading Achievement							
Mean + Interaction	-0.00	-0.01	-0.00	-0.01	-0.00	-0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Prior Disciplinary Records							
Mean + Interaction	0.02	0.04**	0.03*	0.04**	0.05**	0.03*	0.01
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Peer Prior Attendance Records							
Mean + Interaction	-0.02	0.24	0.50	0.35	0.41	0.31	0.30
	(0.37)	(0.42)	(0.34)	(0.33)	(0.33)	(0.33)	(0.33)

* p<0.05, ** p<0.01, *** p<0.001

<u>Classroom Level</u>

Table E.8 Heterogeneous Class Level Peer Effects on Attendance Behavior (Linear Combination Tests) (Math Class)

	Absence Rate						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean + Interaction	0.00	0.00	-0.00	0.00	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Proportion Low SES Students							
Mean + Interaction	0.00	0.01	0.00	0.01	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Peer Prior Math Achievement							
Mean + Interaction	0.00	0.00	0.00	0.00	0.01	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Reading Achievement							
Mean + Interaction	0.00	-0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Disciplinary Records							
Mean + Interaction	0.02	0.03*	0.02	0.03*	0.02	0.02	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
Peer Prior Attendance Records							
Mean + Interaction	-0.00	-0.60	-0.23	-0.29	-0.29	-0.36	-0.30
	(0.42)	(0.36)	(0.33)	(0.25)	(0.23)	(0.24)	(0.24)

* p<0.05, ** p<0.01, *** p<0.001

Appendix F

Reading Classroom Peer Effects on Behavioral Outcomes

Chapter VI reports and discusses the estimates of math class peer effects on student disciplinary infractions and attendance rates. This part presents the regression results of reading class peer effects on student behavioral outcomes. As usual, estimates of peer effects from all three parts (the average peer effects, the effects from dispersion of peer variables, and the heterogeneous peer effects) are reported in order. Although there are some differences in the magnitudes of the coefficients on some peer variables between math class estimates and reading class estimates, the overall conclusions are very similar. Therefore, I am not going to discuss the results again in this part since Chapter VI has provided detailed discussions.

I. Average Peer Effects

Disciplinary Infractions

Table F.1 Average Classroom Peer Effects on Disciplinary Infractions (Reading Class)

	Suspension Numbers										
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
Magnet School Effect											
Academic	-0.11	0.00	-0.24**	-0.32**	-0.21*	-0.24	-0.09	0.16	-0.20*	0.05	0.47*
	(0.08)	(0.11)	(0.09)	(0.10)	(0.10)	(0.15)	(0.16)	(0.13)	(0.08)	(0.15)	(0.23)
Non-Academic Composite	-0.34	-0.25	-0.33	-0.40	-0.33	-0.35	-0.31	-0.22	-0.33	0.06	0.27
	(0.20)	(0.20)	(0.20)	(0.21)	(0.21)	(0.22)	(0.21)	(0.21)	(0.20)	(0.27)	(0.31)
Peer Effect (Lottery Based)											
Black (proportion)	0.82***										0.84
	(0.24)										(0.76)
Low SES (proportion)		1.03***									-0.38
		(0.25)									(0.75)
Female (proportion)			-0.49								-0.52
			(0.63)								(0.69)
Hispanic (proportion)				-1.63							-4.94
				(2.15)							(3.91)
Special ED (proportion)					2.52*						1.24
					(1.26)						(1.88)
ELL (proportion)						1.21					8.43
						(4.82)					(8.38)
Grade 4 Math (Mean)							-0.25				1.01
							(0.18)				(0.56)
Grade 4 Reading (Mean)								-0.57***			-1.12*
								(0.12)			(0.52)
Grade 4 Suspension (Mean)									1.12*		-0.64
									(0.44)		(0.68)
Grade 4 Absence (Mean)										45.23**	66.04**
										(15.52)	(23.90)
Peer Effects (Residence Based)											
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics											
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Over-identification Test	0.14	0.1	0.21	0.22	0.14	0.19	0.16	0.1	0.29	0.36	0.38
Sample Size	10378	10378	10378	10378	10378	10378	10378	10378	10378	10378	10378

* p<0.05, ** p<0.01, *** p<0.001

Absence Rate

Table F.2 Average Classroom Peer Effects on Absence Rate (Reading Class)

	Absence Rates										
Independent Variables	Model1	Model2	Model3	Model4	Model5	Model6	Model7	Model8	Model9	Model10	Model11
Magnet School Effect											
Academic	-0.004*	0.00	-0.01*	0.00	-0.01**	0.00	-0.01	0.00	-0.01*	-0.01*	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Non-Academic Composite	-0.01	-0.01	-0.01	-0.01	-0.01*	-0.01	-0.01	-0.01	-0.01	-0.01*	-0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Peer Effect (Lottery Based)											
Black (proportion)	0.01										0.03
	(0.01)										(0.02)
Low SES (proportion)		0.01									-0.02
		(0.01)									(0.02)
Female (proportion)			-0.02								-0.02
			(0.02)								(0.02)
Hispanic (proportion)				0.09							0.00
				(0.05)							(0.10)
Special ED (proportion)					-0.04						-0.00
					(0.03)						(0.01)
ELL (proportion)						0.31*					0.51*
						(0.13)					(0.22)
Grade 4 Math (Mean)							-0.00				0.02
							(0.00)				(0.01)
Grade 4 Reading (Mean)								-0.00			-0.01
								(0.00)			(0.01)
Grade 4 Suspension (Mean)									0.02		0.03
									(0.01)		(0.02)
Grade 4 Absence (Mean)										-0.43	-0.05
										(0.38)	(0.62)
Peer Effects (Residence Based)											
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Student Characteristics											
	Yes	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Over-identification Test	0.11	0.1	0.14	0.12	0.16	0.2	0.14	0.12	0.15	0.09	0.33
Sample Size	10369	10369	10369	10369	10369	10369	10368	10368	10368	10368	10368

* p<0.05, ** p<0.01, *** p<0.001

II. Impacts from Dispersion of Peer Characteristics

Disciplinary Infractions

Table F.3 Impacts from Variance of Peer Outcomes on Disciplinary Infractions (Reading Class)

	Suspension Numbers			
	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	Yes	Yes	Yes
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	-0.27			
	(0.16)			
Grade 4 Reading (Mean)		-0.58***		
		(0.12)		
Grade 4 Suspension (Mean)			-0.09	
			(1.36)	
Grade 4 Absence (Mean)				76.18*
				(36.10)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	0.08			
	(0.49)			
Grade 4 Reading (Standard Deviation)		-0.64		
		(0.62)		
Grade 4 Suspension (Standard Deviation)			0.56	
			(0.54)	
Grade 4 Absence (Standard Deviation)				-33.36
				(22.62)
Peer Effects (Residence Based)				
	Yes	Yes	Yes	Yes
Student Characteristics				
	Yes	Yes	Yes	Yes
P-value for Over-identification Test	0.17	0.13	0.26	0.28
Sample Size	10374	10374	10374	10374

* p<0.05, ** p<0.01, *** p<0.001

Absence Rate

Table F.4 Impacts from Variance in Peer Outcomes on Absence Rate (Reading Class)

	Absence Rate			
Independent Variables	Model1	Model2	Model3	Model4
Magnet School Effect				
	Yes	*Yes*	*Yes*	*Yes*
Peer Effect (Mean, Lottery Based)				
Grade 4 Math (Mean)	-0.00			
	(0.00)			
Grade 4 Reading (Mean)		-0.00		
		(0.00)		
Grade 4 Suspension (Mean)			0.03	
			(0.03)	
Grade 4 Absence (Mean)				-0.00
				(0.87)
Peer Effect (Dispersion, Lottery Based)				
Grade 4 Math (Standard Deviation)	-0.00			
	(0.00)			
Grade 4 Reading (Standard Deviation)		0.00		
		(0.02)		
Grade 4 Suspension (Standard Deviation)			-0.01	
			(0.01)	
Grade 4 Absence (Standard Deviation)				-0.38
				(0.55)
Peer Effects (Residence Based)				
	Yes	*Yes*	*Yes*	*Yes*
Student Characteristics				
	Yes	*Yes*	*Yes*	*Yes*
P-value for Over-identification Test	0.13	0.11	0.15	0.10
Sample Size	10374	10374	10374	10374

* p<0.05, ** p<0.01, *** p<0.001

III. Heterogeneous Peer Effects

Disciplinary Infractions

Table F.5 Heterogeneous Class Level Peer Effects on Disciplinary Infractions (Reading Class)

	Suspension Numbers						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading achiever**	**High Reading Achiever**	**Low Math achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean Effect	0.54**	0.54**	1.11***	0.76**	0.81***	0.75**	0.79**
	(0.27)	(0.25)	(0.25)	(0.24)	(0.24)	(0.24)	(0.24)
Heterogeneous Effect	0.64**	1.21***	-0.59***	0.19*	0.05	0.21***	0.2
	(0.20)	(0.20)	(0.15)	(0.08)	(0.14)	(0.08)	(0.13)
Proportion Low SES Students							
Mean Effect	0.63*	0.65*	1.43***	0.93***	1.04***	0.90***	1.02***
	(0.27)	(0.27)	(0.27)	(0.26)	(0.26)	(0.26)	(0.26)
Heterogeneous Effect	1.11***	1.46***	-0.77***	0.27***	-0.05	0.32***	0.05
	(0.21)	(0.26)	(0.18)	(0.10)	(0.17)	(0.10)	(0.16)
Peer Prior Math Achievement							
Mean Effect	-0.12	-0.14	-0.37*	-0.20	-0.26	-0.22	-0.29
	(0.18)	(0.18)	(0.18)	(0.18)	(0.17)	(0.18)	(0.17)
Heterogeneous Effect	-0.47***	-0.77***	0.25**	-0.21**	0.14	-0.13	0.15*
	(0.09)	(0.11)	(0.08)	(0.08)	(0.07)	(0.08)	(0.06)
Peer Prior Reading Achievement							
Mean Effect	-0.42*	-0.45**	-0.66***	-0.53***	-0.58***	-0.53***	-0.59***
	(0.13)	(0.13)	(0.13)	(0.13)	(0.12)	(0.13)	(0.12)
Heterogeneous Effect	-0.44***	-0.61***	0.17**	-0.13	0.08	-0.11	0.12*
	(0.08)	(0.09)	(0.07)	(0.07)	(0.06)	(0.06)	(0.05)
Peer Prior Disciplinary Records							
Mean Effect	-0.20	0.5	1.55**	0.66	1.16**	0.52	1.07*
	(0.55)	(0.57)	(0.49)	(0.53)	(0.45)	(0.53)	(0.45)
Heterogeneous Effect	2.48***	1.24*	-0.98	0.91*	-0.36	1.17**	0.29
	(0.55)	(0.57)	(0.50)	(0.38)	(0.76)	(0.41)	(0.64)
Peer Prior Absence Rate							
Mean Effect	11.27	54.71***	26.61*	44.48**	45.30**	44.51**	44.98**
	(13.28)	(15.60)	(11.80)	(15.49)	(15.64)	(15.60)	(15.70)
Heterogeneous Effect	95.09***	-34.14	33.84*	2.39	-0.19	1.57	1.01
	(20.50)	(20.42)	(16.11)	(1.48)	(1.93)	(1.48)	(1.81)

* p<0.05, ** p<0.01, *** p<0.001

<u>Absence Rate</u>

Table F.6 Heterogeneous Class Level Peer Effects on Attendance Behavior (Reading Class)

	Absence Rate						
	Model1	Model2	Model3	Model4	Model5	Model6	Model7
	Black	**LSES**	**Female**	**Low Reading Achiever**	**High Reading Achiever**	**Low Math Achiever**	**High Math Achiever**
Peer Effects (Lottery Based)							
Proportion Black Students							
Mean Effect	0.01	0.01	0.01*	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	-0.01	-0.00	-0.01**	0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Proportion Low SES Students							
Mean Effect	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	-0.00	0.00	-0.01	0.00	0.00	-0.00	0.00
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Math Achievement							
Mean Effect	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Heterogeneous Effect	-0.00	-0.00	0.00	-0.00	0.00*	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Reading Achievement							
Mean Effect	-0.00	-0.00	-0.01	-0.00	-0.00	-0.00	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Heterogeneous Effect	0.00	-0.00	0.00	-0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Peer Prior Disciplinary Records							
Mean Effect	0.01	0.01	0.02	0.01	0.02	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Heterogeneous Effect	0.02	0.02	-0.01	0.02*	0.02	-0.00	-0.02
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
Peer Prior Absence Rate							
Mean Effect	-0.63*	-0.26	-0.56*	-0.45	-0.45	-0.42	-0.45
	(0.31)	(0.38)	(0.29)	(0.38)	(0.38)	(0.38)	(0.39)
Heterogeneous Effect	0.34	-0.64	0.19	0.06	0.07	-0.03	0.05
	(0.48)	(0.50)	(0.39)	(0.04)	(0.05)	(0.04)	(0.04)

* p<0.05, ** p<0.01, *** p<0.001

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