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Three Essays on the Economics of Education

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ABSTRACT

THREE ESSAYS ON THE ECONOMICS OF EDUCATION

BY

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This dissertation comprises essays that exploit geographic data in an effort to provide new causal evidence on three topics facing education policy makers.

Chapter 1 investigates the consequences of domestic violence exposure. I show that episodes of domestic violence cause a short-term increase in absences, but I do not find evidence that the events increase or decrease the number of disciplinary infractions, conditional on attending school. In addition, I measure spillovers to peers using plausibly exogenous daily and annual variation in peer group composition. In contrast to earlier research, my spillover results suggest that neither peers' behavior nor their test scores are impacted.

Chapter 2 analyzes the relationship between longer student commutes and outcomes including attendance and achievement. I find little evidence of a marginal effect when adding an additional mile to a students' commute on either academic achievement or attendance. In contrast to the null effects arising from a marginal increase in distance, I find robust evidence that being within walking distance to school affects attendance. Being able to walk to school increases attendance by 0.76 percentage points. It is not clear whether this increased attendance translates to higher achievement on annual exams. While point estimates are positive, the effects on achievement are not measured precisely enough to reject a null achievement effect.

Chapter 3 evaluates the effects of charter schools on New York City neighborhoods. Using unique New York City laws that impact geographical access to charter schools, I employ a new approach to identifying the causal effect of charter school entry on neighborhoods. I find that for every 10 percent increase in charter market share, neighborhood student achievement (i.e. students at both charter and traditional schools) increases 0.01 standard deviations in ELA and 0.04 standard deviations in Math. I find no evidence that charter schools causally reduce or improve achievement of students remaining in traditional public schools; however, charter schools do cause substantial sorting into the neighborhood's schools, greater concentration of students with disabilities in traditional public schools, and selection by black and Hispanic students into more segregated schools.

THREE ESSAYS ON THE ECONOMICS OF EDUCATION

BY

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A dissertation submitted in partial fulfillment for the degree of Doctor of Philosophy

in the

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Acceptance

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University

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Introduction

This dissertation comprises essays that exploit geographic data in an effort to provide new causal evidence on three topics facing education policy makers.

Chapter 1 investigates the consequences of domestic violence exposure. Despite the prevalence of domestic violence, little is known about how childhood exposure affects academic outcomes. Combining seven years of daily student observations with geocoded police records from a large urban school district, this study presents what I believe to be the first causal evidence on how domestic violence impacts a students' own academic outcomes. I show that episodes of domestic violence cause a short-term increase in absences, but I do not find evidence that the events increase or decrease the number of disciplinary infractions, conditional on attending school. In addition, I measure spillovers to peers using plausibly exogenous daily and annual variation in peer group composition. Peer effects stemming from domestic violence have been studied in one U.S. county. In contrast to earlier research, my spillover results suggest that neither peers' behavior nor their test scores are impacted. Estimates are precise enough to rule out peer spillovers of the magnitude found in prior work. I explore some possible explanations for the divergence from my findings and those found in the U.S. county previously studied.

Chapter 2 analyzes the relationship between longer student commutes and outcomes including attendance and achievement. The research design relies on exogenous variation in distance to school arising from 40 school closures, relocations, and consolidations occurring in a large urban school district during the years 2010 to 2017. Because these events could impact other education inputs (e.g. peer composition, facility quality, teaching staff), I focus on comparisons between students who attend the same school both before and after the event but experience different changes in their distance to school. I find little evidence of a marginal effect when adding an additional mile to a students' commute on either academic achievement or attendance. I am able to reject the null hypothesis that adding a mile to a students' commute reduces achievement by more than 0.009 standard deviations. For attendance, I am able to reject the null hypothesis that adding a mile to a students' commute reduces percent attendance by more than 0.04 percentage points. In contrast to the null effects arising from a marginal increase in distance, I find robust evidence that being within walking distance to school affects attendance. Being able to walk to school increases attendance by 0.76 percentage points. It is not clear whether this increased attendance translates to higher achievement on annual exams. While point estimates are positive, the effects on achievement are not measured precisely enough to reject a null achievement effect. I conclude that factors other than student commutes are likely more important for district leaders to consider when setting policies on school closure and school choice.

Chapter 3 evaluates the effects of charter schools on New York City neighborhoods. The most convincing charter school research focuses on how the schools affect students who attend them. Optimal policy should also weigh how charters impact nearby traditional public schools and how sorting on charter access impacts neighborhoods. Though studied by some, these questions have proven tougher to answer convincingly because of challenges dealing with endogenous charter school location. Using unique New York City laws that impact geographical access to charter schools, I employ a new approach to identifying the causal effect of charter school entry on neighborhoods. I find that for every 10 percent increase in charter market share, neighborhood student achievement (i.e. students at both charter and traditional schools) increases 0.01 standard deviations in ELA and 0.04 standard deviations in Math. I find no evidence that charter schools causally reduce or improve achievement of students remaining in traditional public schools; however, charter schools do cause substantial sorting into the neighborhood's schools, greater concentration of students with disabilities in traditional public schools, and selection by black and Hispanic students into more segregated schools. The findings are supported by a series of falsification tests.

The remainder of this dissertation is organized around the three chapters, setting out the background, existing literature, methods, available data, results, and conclusions for each.

1 The Effects of Domestic Violence on Children and Their Peers

1.1 Motivation and Existing Literature

It is estimated that one in six U.S. children witnesses domestic violence before reaching adulthood (Finkelhor et al., 2009). Medical research indicates that stressful situations trigger the release of cortisol, and repeated exposure may alter children's biological makeup in lasting ways. (Rogosch et al., 2011; Danese and McEwen, 2012; Hinnant et al., 2013). In academic settings, concerns about the consequences of domestic violence exposure are twofold. First, children who witness domestic violence may be directly harmed by the event, suffering worse academic outcomes as a result. Second, classmates and others the child comes in contact with may be indirectly harmed if exposure reduces children's ability to self-regulate, causing more behavior incidents and class disruptions. Thus, knowledge about how children's academic outcomes are affected by domestic violence exposure may motivate policymakers to direct additional resources to helping victims of domestic violence exit the abusive relationships quickly. Further, if student behavior is altered around the time an event is reported, school counselors may allocate their time more efficiently by using event data from other local agencies to target their efforts. In addition, knowledge about peer spillovers may inform school management decisions. If disruptive students cause significant harm to their peers, schools may want to develop policies that mitigate the negative externalities, either by reducing exposure to disruptive peers or adopting practices, such as the use of behavior aides, that attempt limit spillovers.

The purpose of this study is to measure the extent to which domestic violence causally affects students' own academic outcomes and/or the outcomes of the peers with whom they attend school. To explore these questions, I rely on daily administrative data that track all students in a large urban school district over a seven-year period. The data include residential histories which I match to geocoded crime incidence reports from the local police department to pinpoint children's exposure to incidents of domestic violence.

Little evidence exists on whether children's behavior or other school outcomes are affected by exposure to domestic violence. A sizable body of psychological research shows a negative association between domestic violence exposure and child development, but most of the work does not employ causal identification strategies.[1] Some causal evidence does exist on peer-spillovers. Using data from Alachua County, Florida Carrell and Hoekstra (2010) find that students exposed to domestic violence reduce their peers' contemporaneous test scores, on average, by 0.025 standard deviations and increase their disciplinary incidents by 17 percent. In later work, the authors find that the test-score effects persist as students age, and ultimately the negative labor-market externality caused by a year of exposure to each elementary-school peer who has or will experience domestic violence is an $80,000 discounted loss of future earnings for classmates (Carrell et al., 2016).

To measure how a child's own behavior and absences are affected by domestic violence exposure, I use a model with student-school-year fixed effects and evaluate whether attendance and behavior in the days following an incidence of domestic violence deviate from the student's typical pattern in that year. Observations from the 14 days preceding the incident serve as placebo tests for the potential that other events are happening in the child's life at the time.

To measure how peers' test scores and behavior are affected by classmates exposed to domestic violence, I employ two approaches. First, I use a triple-difference model with annual observations to identify effects from cohort-to-cohort variation in the share of schoolmates exposed to domestic violence. Second, I use a student-school-year fixed effects

[1] One exception is Koenen et al. (2003). The study relies on twins in an effort to control for genetic and environmental factors. The authors focus on IQ as an outcome. Their results suggest much of the negative association between IQ and domestic violence is related to genetic and environmental factors, though the authors suggest domestic violence may explain a small portion. See Wolfe et al. (2003) for a meta-analysis summarizing 41 studies from this literature.

model with daily behavior observations, measuring whether peers are more or less likely to have a behavior incident when the share of their peers exposed to domestic violence varies due to student absences and mobility during the school year.

The results I present contribute to the literature by offering what I believe to be the first causal evidence on how students' own outcomes are affected by domestic violence exposure. The peer effects results are the first evaluation of whether peers in a large urban school district are impacted by classmates exposed to domestic violence (extant research is limited to Alachua County). Additionally, to my knowledge, the methods relying on day-to-day variation in peer composition have not previously been used in the peer-effects literature.

For students' own outcomes, I find clear evidence that domestic violence causes students to miss school in the period immediately following the incident. On the day following a domestic violence episode, the rate of absence jumps four percentage points above the student's own typical level, a 90 percent increase. I find no evidence that students are more or less likely to get into disciplinary trouble conditional on attending.

For peer outcomes, the triple-difference analysis indicates that peer test scores are unaffected by the percentage of schoolmates exposed to domestic violence. Across six specifications, the point estimates range from effects of -0.009 to 0.004 standard deviations.[2] Five of the six estimates are measured precisely enough to reject the null hypothesis that exposure to students experiencing domestic violence reduces peer achievement by the -0.025 standard deviations measured in Alachua County. Similarly, my estimates range from disciplinary incidents falling seven percent to rising eight percent, all substantially lower than the 17 percent increase found in Alachua County (Carrell and Hoekstra, 2010). Four of the six specifications are measured precisely enough to statistically reject a rise of that magnitude. For the analysis using day-to-day variation in peer-group composition, I find no evidence that behavior of peers is impacted by the share of students exposed to domestic violence who are in attendance on a given day.

[2]For ease of comparison, I follow Carrell and Hoekstra (2010) and discuss effects of each additional peer exposed to violence by assuming a class size of 20, i.e. (-0.183 * 0.05)=-0.009.

The remainder of this chapter is structured as follows. Section 1.2 presents the prior literature, Section 1.3 describes the data used, Section 1.4 explains the identification strategies, Section 1.5 presents results, Section 1.6 introduces a series of robustness checks, Section 1.7 discusses findings in the context of prior work, and Section 1.8 concludes.

1.2 Prior Literature

Direct Effect on Student's Own Outcomes

While prior research has not evaluated how children's school outcomes are affected by episodes of domestic violence, a related line of inquiry has studied traumatic neighborhood events. The studies find that neighborhood crime impacts students' test scores, self-regulation, and attendance.

Sharkey (2010) exploits variation in the timing of at-home reading assessments in Chicago. The author finds that students who take the assessment in the week following a homicide on their block score lower than students in the same neighborhood who took the assessment at a different time. Sharkey et al. (2012) use the same dataset and identification strategy, but analyze measures of attention and impulse control, finding that both are reduced by recent local violence. Finally, Sharkey et al. (2014) look at performance on annual exams in New York City. The authors find that students who live on a blockface where a homicide occurred in the week before annual assessments score lower than students living on a blockface where a homicide occurred the week following annual assessments.

While related in its interest in how students respond to trauma, the present study is distinct from this research in its direct focus on the students who live in a home where violence occurs rather than a neighborhood where violence occurs. As a result, the students studied are more likely to have knowledge of the event and personally know those involved.

Indirect Effect on Peers' Outcomes

Extant research on elementary and secondary school peer effects in a variety of contexts has been mixed. Some studies find little or no effect (Angrist and Lang, 2004; Duflo et al., 2011; Abdulkadiroğlu et al., 2014; Dobbie and Fryer Jr, 2014) while others find meaningful impacts (Hoxby, 2000a; Figlio, 2007; Lavy and Schlosser, 2011; Imberman et al., 2012). One explanation for the mixed findings may be that some school districts are more or less adept at mitigating the effect of spillovers from disruptive or otherwise weaker peers. It is also possible that the conflicting findings result from varying approaches to identification and varying measures of peer-group quality.

Similar to the present study, three Alachua County studies (Carrell and Hoekstra, 2010, 2012; Carrell et al., 2016) analyze peer effects using students who have been or will be exposed to domestic violence as a source of exogenous variation in peer-group quality. Carrell and Hoekstra (2010) find that students exposed to domestic violence reduce their peers' contemporaneous test scores, on average, by 0.025 standard deviations and increase their disciplinary incidents by 17 percent. In later work, the authors find that the test-score effects persist as students age, and ultimately the negative labor-market externality caused by each elementary-school peer who has or will experience domestic violence is $80,000, measured as the classmates' discounted loss of future earnings (Carrell et al., 2016). Both the magnitude and the persistence of these spillovers are striking. The magnitude is striking because, if the evidence from Alachua County is generalizable, every year U.S. elementary students exposed to domestic violence cause their peers to lose at least $84 billion in discounted future earnings.[3] To contextualize that figure, public expenditures on elementary education total $280 billion annually.[4] The persistence is striking because the test score

[3]According to NCES estimates, there were 22,763,000 public school students in grades K-5 in 2016. Carrell and Hoekstra measure that 4.6% of students are exposed to domestic violence in Alachua County. The fact that some domestic violence goes unreported suggests this amount should be interpreted as a lower bound.

[4]According to NCES estimates, total education expenditures per pupil were $12,296 in the 2013 school year, the most recent year for which data was available. As noted above, there were 22,763,000 public school students in grades K-5 in 2016.

effects found by (Carrell et al., 2016) do not fade over time. This suggests that negative externalities associated with domestic violence exposure potentially stand in contrast to other important education inputs like preschool enrollment (Deming, 2009), class size (Krueger and Whitmore, 2001), and teacher quality (Jacob et al., 2010; Chetty et al., 2014), which are all characterized by fading impacts on test scores as years pass following exposure to the treatment.[5]

1.3 Data

My study is made possible by combining administrative datasets from two local government agencies. The first is a set of educational records maintained by a large urban school district covering the school years 2010 through 2016. It includes a history of student addresses, including start and end dates at each address, daily absences, and detailed records of each behavior infraction. In addition, it includes demographic data about each student (race, gender, subsidized lunch status, etc.) as well as sibling relationships, and annual performance on state standardized exams in English, Reading, Math, Science, and Social Studies.

My second data source comes from the local police department and includes records of all Part 1 offences occurring from January 1, 2009 through February 6, 2017.[6] In order to identify students exposed to violence in their homes, I compare the geocoded crime records to geocoded student residences on the date of each crime. For rapes and aggravated assaults occurring within 100 feet (30.48 meters) of a student's home, I then look for an exact street number match. For multi-family dwellings, I ensure that the apartment number in the police records matches the apartment number of the student. I omit any violence occurring in apartment common areas.

[5] Some of these studies find that despite the fact test score effects fade, labor market outcomes are impacted.

[6] Part 1 offences include homicide, rape, robbery, aggravated assault, burglary, larceny, auto theft, and arson. I limit my analysis to assaults and rapes occurring in the child's home.

The data yields a sample of 86,370 student-year observations.[7] Within the sample, approximately 6.0 percent of students are exposed to domestic violence at some point.[8]

It is important to consider the measure of domestic violence exposure and what that implies about the timing, extent of reporting, and potential circumstances surrounding each observed incident. A call to the police likely occurs in times of marked discord; however, exposure may be ongoing before or after the police are contacted. Another important factor is the relationship between the offender and the child. Domestic violence is violence occurring between members of the same family. Because I focus only on rapes and aggravated assaults occurring at home, the majority of offenders in my dataset should be family members, though not 100 percent.[9] Third, because police records specify the event's location, it is clear that the violence occurred at the home where the child lives. However, children may not be aware of the event in all cases.[10]

Students exposed to domestic violence, as measured by the police record data, have 118 percent more annual behavior incidents than their peers. Those exposed to domestic violence lag behind their peers' performance on achievement tests, scoring 0.56 standard deviations lower, on average.[11] Together these associations suggest that the method is identifying students who are substantially more disruptive and lag significantly academically.

[7] I collect data on all students in grades 3-8; however, I limit my sample to grades 3-5 which maintains consistency with the Alachua County work.

[8] This compares to 4.6 percent in the Alachua County studies.

[9] The police department I work with does not maintain records on the relationship between the offender and victim for aggravated assaults or rapes. The Bureau of Justice Statistics reports that 91.3 percent of violent crimes occurring at the victim's home are committed by individuals known to the victim, and other reports from that agency suggest most of the known offenders are family members.

[10] The Alachua County studies rely on a different measure: ex-post court filings (Temporary Protective Orders). These may be filed in the aftermath of a violent event or after a series of events lead the filer to seek relief. They also signal an end to the exposure and are limited to family members.

[11] The measure of domestic violence exposure used in Alachua County exhibited analogous associations with behavior and achievement. Exposed students had 97 percent more annual behavior incidents and score 0.49 standard deviations below their peers.

Approximately, 71 percent of students in the district I study qualify for free and reduced lunch while 73 percent of students are black.[12] A full set of descriptive statistics is presented in Table 1.1.

[12]The comparable percentages for the Alachua County studies are 40 and 38, respectively.

Table 1.1: Descriptive Statistics

Panel A: Student demographics		*Panel B: Academic outcomes by student type*		
Variable	Mean	Sample	Reading and math composite score	Number of disciplinary incidents
Black	0.73	All Students	-0.23	0.18
	(0.44)		(1.00)	(0.82)
Male	0.50	Subsidized lunch	-0.56	0.24
	(0.50)		(0.81)	(0.95)
Subsidized lunch	0.71	Unsubsidized lunch	0.56	0.03
	(0.45)		(0.97)	(0.22)
Exposed to domestic violence	0.06	All boys	-0.31	0.27
	(0.24)		(1.02)	(1.03)
Boys exposed to domestic violence	0.03	All girls	-0.14	0.08
	(0.17)		(0.97)	(0.50)
Girls exposed to domestic violence	0.03	Boys exposed to domestic violence	-0.91	0.54
	(0.17)		(0.75)	(1.50)
Peer domestic violence	0.06	Girls exposed to domestic violence	-0.67	0.20
	(0.06)		(0.76)	(0.87)
Cohort Size	86.61			
	(36.85)			

Notes: Each cell contains the mean with the standard deviation in parentheses. Demographic variables are based on 86,370 observations. There were 85,270 observations containing test scores. Discipline variables are based on 77,163 observations, and exclude charter schools which do not track discipline incidents in the same method as the traditional public schools. Cohort refers to a group of children in the same grade, in the same school, in the same year. Average cohort size was computed at the cohort level.

1.4 Identification Strategy and Methodology

Measuring the causal effect of domestic violence on a student's own behavior and absences presents some challenges. Primarily, the challenges arise because students exposed to domestic violence are likely to differ in a variety of ways from the students never exposed. Thus, the most convincing evidence will rely on strategies that compare the exposed students' behavior and absences to what would be expected from that same student had domestic violence not occurred.

Identifying the causal effect of a student on his or her peers is also challenging.[13] A first concern, known as the reflection problem, arises because a student's own actions reflect the impact of peers on them (Manski, 1993) as well as their impact on peers. A second challenge arises because peer formation is influenced by a variety of choices that can lead to selection bias. In the case of school peers, district leaders choose attendance boundaries, parents choose neighborhoods, administrators choose classroom assignment, and students choose friends from the peers they encounter. Because all of these choices can bias estimates of peer effects, the most convincing identification strategies rely on plausibly random variation in peer group formation.

In the sections below, I describe how my identification strategies attempt to address these challenges.

Direct Effect of Domestic Violence on Students

I attempt to disentangle domestic violence effects from other factors impacting student behavior and absences by analyzing daily deviations from a students' own typical behavior in the period immediately preceding or following an episode of domestic violence at their home. This identification strategy relies on the assumption that changes in student behavior or absences immediately following the domestic violence episode can be attributed to the

[13] See Hoxby (2000a) and Hoxby and Weingarth (2005) for a more robust discussion of these challenges and the different frameworks through which we may expect peers to impact each other.

event itself. As a placebo test for the possibility that other things are occurring in the student's life around the same time, I look at the days immediately before the event. Formally, I estimate the following equation using ordinary least squares:

$$y_{isgyd} = \alpha_0 + \alpha_{i,d-14}DV_{i,d-14} + ...\alpha_{i,d+14}DV_{i,d+14} + \lambda_{isy} + \sigma_d + \epsilon_{isgyd} \tag{1.1}$$

where y is the outcome variable for individual i, in school s, grade g, year y, and date d.

$\alpha_{i,d-14}DV_{i,d-14}...\alpha_{i,d+14}DV_{i,d+14}$ are a series of indicators for 14 days before and after domestic violence exposure. λ_{isy} and σ_d are student-school-year fixed effects and date fixed effects. ϵ_{isgytd} is the error term.

I argue that this approach to identification should give an unbiased causal estimate; however, its benefits come at the cost of only measuring acute responses to domestic violence around the time it is reported to the police. The specification cannot account for the fact that the student's experience of domestic violence may be ongoing before or after reporting. The existence of such circumstances would tend to attenuate my findings toward zero. It similarly does not measure more distant effects that would arise if there is a latent period between exposure and changes in behavior or absences.

Peer Effects Using Annual Variation

By relying on domestic violence exposure as a proxy for disruptive peers, my peer-effects identification strategy avoids the reflection problem. In order for the reflection problem to bias estimates relying on this strategy, it would have to be the case that a child's school peers cause adults to engage in more or less violent behavior in the home, which seems quite unlikely. However, endogenous peer formation is a more challenging problem to solve. I rely on variation in the composition of each cohort within a school across the period of my study.[14] The specification suggests that if one compares the outcomes of each cohort at a school to the average outcome from that grade/school combination over the

[14] The choice to rely in school-grade-year-level variation avoids bias potentially resulting from nonrandom assignment of students to classes.

seven years analyzed, any variation in outcomes correlated with years the grade/school had a greater share of students exposed to domestic violence, conditional on other controls, can be described as the causal effect of exposure to such peers. Formally, the I estimate the following equation using ordinary least squares:

$$y_{isgt} = \alpha_0 + \alpha_1 \frac{\sum_{k \neq i} DV_{ksgt}}{n_{sgt} - 1} + \alpha_2 \mathbf{X}_{isgt} + \lambda_{sg} + \sigma_{gt} + \alpha_{sg} t + \epsilon_{isgt}, \tag{1.2}$$

where y is the outcome variable for individual i, in school s, grade g, and year t. $\frac{\sum_{k \neq i} DV_{ksgt}}{n_{sgt}-1}$ is the proportion of peers in the school-grade cohort who were ever exposed to domestic violence, except individual i. $\mathbf{X}_{isgt}$ is a vector of individual i's specific characteristics, including own violence exposure, race, gender, and subsidized lunch. λ_{sg}, σ_{gt}, and $\alpha_{sg}t$ are school-grade fixed effects, grade-year fixed effects, and school-grade specific linear time trends. ϵ_{isgt} is the error term. Robust standard errors are clustered at the school-cohort level.

Expressing skepticism for several recent studies in the peer effects literature, Angrist (2014) suggests separating research subjects under study from the peers whose characteristics might influence them. Therefore, I exclude the students who were themselves exposed to domestic violence.

From this baseline, I introduce a number of alternative specifications. First, I replace the school-grade linear time trends with school-year fixed effects.[15] Next, I present specifications that use sibling fixed effects to control for unobserved family characteristics. This serves to shift the comparison from cohorts of students to siblings within the same family where one sibling was exposed to comparatively more students who experienced domestic violence. Finally, I present specifications using a student's own lagged scores and discipline as controls. This has the added benefit of better-defining the treatment period.

[15] Carrell and Hoekstra (2010) argue that such a specification may be biased toward zero if disruptive students impact kids enrolled in other grades. While I concede this possibility, I also acknowledge that linear time trends may fail to pick up unobserved changes in the student population that do not occuur linearly during the period of the study (for example, a new apartment, a charter school opening, or a school's attendance boundary being amended).

Without a lagged score, it is unclear how much exposure to the disruptive student has occurred.[16]

Peer Effects Analysis Using Daily Variation

The biggest threat to the identification strategy described above is the possibility that year-to-year variation in the share of students exposed to violence is nonrandom and instead reflects school composition changes. If the arrival of more students exposed to domestic violence coincided with a broader shift toward a school being comprised of relatively weaker students, the above identification strategy would fail to deliver unbiased results.

One way to address the potential for bias resulting from school composition changes is to rely on an alternative identification strategy that avoids variation arising over such long periods of time. Since I have access to daily student records, I exploit variation in school composition from one day to the next. Within a school year, on any given day, a student's peers vary depending on who is absent from school and in less-frequent cases which students have left or joined the school. Using this granular variation, I can avoid bias resulting from any broad changes in neighborhood or school composition over the seven years of my study. I estimate the following equation using ordinary least squares:

$$y_{isgyd} = \alpha_0 + \alpha_1 \frac{\sum_{k \neq i} DV_{ksgyd}}{n_{sgyd} - 1} + \alpha_2 \mathbf{X}_{sgyd} + \lambda_{isy} + \sigma_d + \epsilon_{isgyd}, \tag{1.3}$$

where y is the outcome variable for individual i, in school s, grade g, year y, and date d. $\frac{\sum_{k \neq i} DV_{ksgyd}}{n_{sgyd}-1}$ is the proportion of peers in the school-grade cohort each day who were ever exposed to domestic violence, except individual i. $\mathbf{X}_{sgyd}$ is a vector of cohort controls including percent of students present and cohort size. λ_{isy} and σ_d are student-school-year fixed effects and date fixed effects. ϵ_{isgyd} is the error term.

[16] Using a similar identification strategy without lagged outcomes as a control, Carrell et al. (2016) describe the effects measured as annual effects; however, if students are not highly mobile, it is likely that having an unusually high percentage of peers exposed to domestic violence in third grade, for example, would be correlated with having also had an unusually high percentage of such peers in second grade. Incorporating a lagged-score control changes the measure from the cumulative effect of an undefined treatment period to the marginal effect of a single year of exposure.

It is constructive to highlight what this method identifies relative to the annual approach. This strategy would pick up on any evidence that students exposed to domestic violence serve as instigators or ringleaders of disruption, and when present, cause peers to get into more trouble. It would not measure effects resulting from a scenario where cumulative exposure to a disruptive student alters a peer's behavior consistently through the year, whether the disruptive student is present or not.

1.5 Results

Direct Effect of Domestic Violence on Students

Because I analyze daily outcomes for 14 days before and after exposure, the large number of estimates and standard errors are more easily digested graphically. Figure 1.5 and 1.5 present the coefficients and confidence intervals for the full period for my analyses of absences and discipline, respectively.

In Table 1.2, I present the same evidence in a numeric format. The table shows how student behavior and absences are impacted in the days after exposure to domestic violence. I find a clear increase in absences on each of the four days following an episode of domestic violence that is reported to the police. On the first day following the event, students are absent 4.3 percentage points more than is typical.[17] The elevated absences persist for the following three days before returning to normal. The placebo tests covering the 14 days preceding the event (shown in Figure 1), support the idea that we can interpret this as the causal effect of domestic violence exposure, rather than simply a unique time in the life of the student. If this were an unusual time in the life of the student, I would expect to see absences in those days deviate from their typical level. Instead, I observe absences on those days as typical and only immediately after the domestic-violence episode do the absences rise.

[17]The students ever exposed to domestic violence are absent 4.7 percent of the school year, so a 4.3 percentage point rise in the rate of absences represents a 90 percent increase.

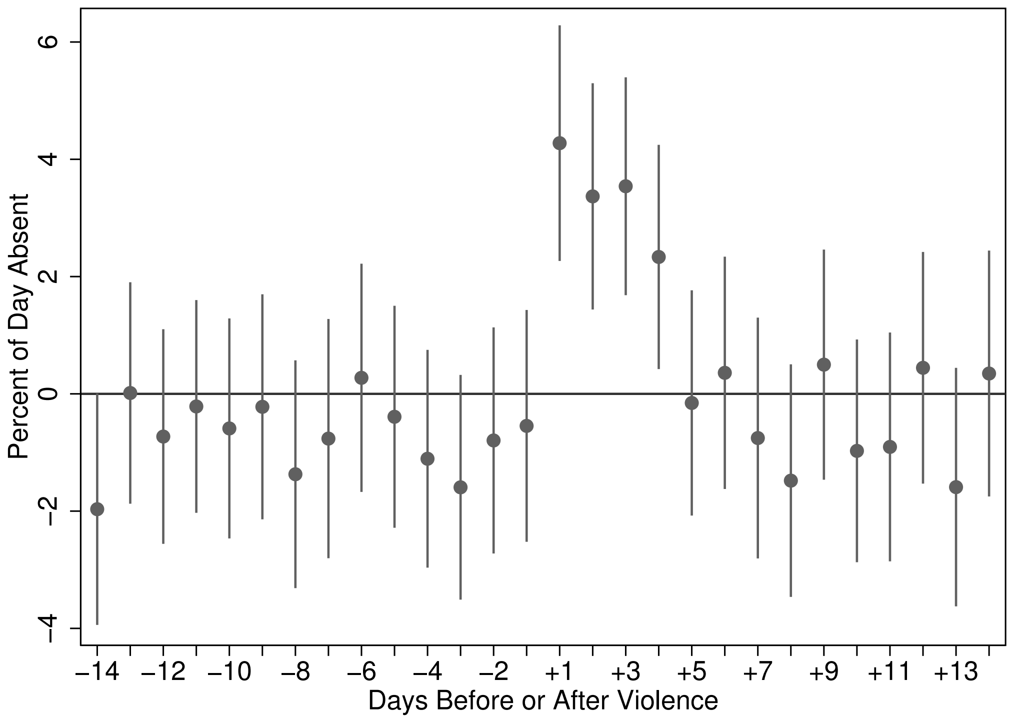

Figure 1.1: Student Absences Before and After Exposure to Violence

I find no evidence that student behavior, conditional on attendance, is affected in the days before and after domestic violence exposure. This suggests that either the episodes of domestic violence exposure I observe do not cause changes in a student's behavior or that such changes manifest themselves less acutely around the time domestic violence is reported.

Peer Effects Using Annual Variation

Results from the peer effects analysis using annual variation are presented in Table 1.3. Across all specifications, the findings suggest there is no evidence that peers exposed to domestic violence negatively impact students with whom they attend school in the district I analyze.

The point estimates for the impact of spillovers on peer test scores indicate that adding an additional peer exposed to domestic violence to a class of 20, affects peer achievement

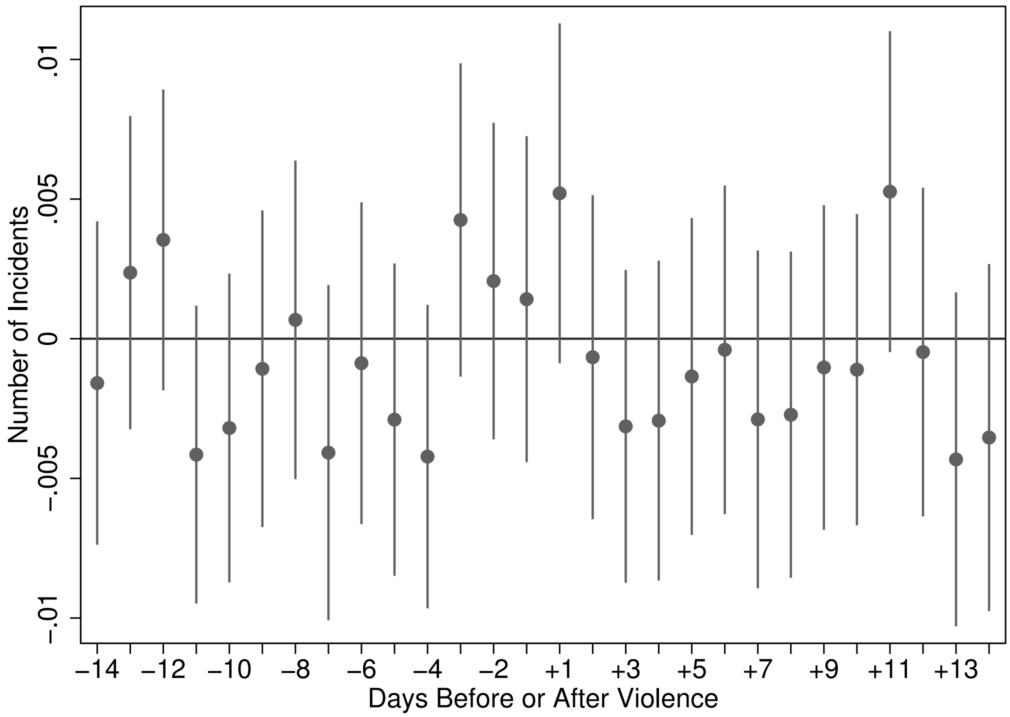

Figure 1.2: Student Disciplinary Behavior Before and After Exposure to Violence

by somewhere between -0.009 and 0.004 standard deviations, all statistically insignificant amounts.[18] In five of the six specifications, the precision of the estimates allows me to rule out the possibility that peer test scores are reduced by 0.025 standard deviations, the amount found in Alachua County.

The six behavior results presented in Table 1.4 range from a seven percent decrease in the number of disciplinary incidents per student to an increase of eight percent. In none of the specifications do I observe infractions rising by as much as 17 percent, the amount found in Alachua County. In four of the six specifications, I am able to reject a rise of that magnitude with 95% confidence.

Gender-specific estimates are noisier. Carrell and Hoekstra (2010) found that boys exposed to domestic violence caused particularly large spillovers. My findings are not

[18]Following Carrell and Hoekstra (2010), I discuss effects of each additional peer exposed to violence by assuming a class size of 20, i.e. (-0.183 * 0.05) = -0.009. I note that in their third paper using the Alachua County data, Carrell et al. (2016) discuss results based on a class size of 25.

Table 1.2: Domestic Violence and Students' Own Outcomes

	Number of disciplinary incidents	Absences
One Day After Domestic Violence Exposure	0.005* (0.003)	4.276*** (1.025)
Two Days After Domestic Violence Exposure	-0.001 (0.003)	3.368*** (0.984)
Three Days After Domestic Violence Exposure	-0.003 (0.003)	3.540*** (0.948)
Four Days After Domestic Violence Exposure	-0.003 (0.003)	2.334** (0.976)
Five Days After Domestic Violence Exposure	-0.001 (0.003)	-0.155 (0.980)
Observations	762,856	810,103
Student-school-year fixed effects	Yes	Yes
Date fixed effects	Yes	Yes

consistent with exposed boys causing peers to test substantially lower and get significantly more behavior infractions, though I cannot rule out the possibility of some small effects.

Peer Effects Analysis Using Daily Variation

The large sample of daily observations allows me to observe whether day-to-day changes in peer-group composition are associated with changes in a student's behavior. Results from this analysis are presented in Table 1.5.

The findings offer no evidence that behavior spillovers occur. In fact, the point estimates are negative and statistically insignificant in both specifications. In addition, I can reject the null hypothesis that each additional exposed student raises peer's contemporaneous behavior incidents by the magnitude measured in Alachua County.

Table 1.3: Effects of Peers Exposed to Domestic Violence on Test Scores (Annual Variation)

	Reading and math composite score					
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mean Peer Effects						
Proportion peers with family violence	-0.183	-0.072	0.038	0.082	-0.025	-0.022
	(0.168)	(0.164)	(0.166)	(0.169)	(0.140)	(0.132)
Observations	79,377	79,377	78,939	78,939	38,659	38,659
Panel B: Gender Differences						
Proportion boy peers with family violence	-0.351	0.0368	-0.214	-0.0705	-0.356*	-0.147
	(0.229)	(0.213)	(0.242)	(0.235)	(0.200)	(0.210)
Proportion girl peers with family violence	-0.009	-0.205	0.309	0.251	0.328	0.109
	(0.237)	(0.243)	(0.229)	(0.246)	(0.210)	(0.204)
Observations	79,370	79,370	78,932	78,932	38,657	38,657
School-grade fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Grade-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort controls	Yes	Yes	Yes	Yes	Yes	Yes
School-grade-specific linear time trends	Yes	No	Yes	No	Yes	No
School-year fixed effects	No	Yes	No	Yes	No	Yes
Sibling fixed effects	No	No	Yes	Yes	No	No
Lagged test score	No	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school-cohort level. Individual controls include race, gender, and subsidized lunch status. Cohort controls include race, subsidized lunch, gender, and size. All regressions are weighted by the inverse of the number of times a student is observed in the sample. We restrict the sample to individuals not exposed to domestic violence. * p<0.10, ** p<0.05, *** p<0.01.

1.6 Robustness Checks

The consistency of the estimates presented above provides strong evidence that my results are not driven by omitted variable bias. However, it remains possible that

Table 1.4: Effects of Peers Exposed to Domestic Violence on Discipline (Annual Variation)

	Number of Disciplinary Incidents					
Specification	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Mean Peer Effects						
Proportion peers with	0.243	0.302**	0.105	0.211	-0.256	0.060
family violence	(0.220)	(0.134)	(0.295)	(0.197)	(0.383)	(0.184)
Observations	72,210	72,210	71,793	71,793	35,092	35,092
Panel B: Gender Differences						
Proportion boy peers with	0.296	0.343*	0.306	0.274	0.230	0.186
family violence	(0.338)	(0.183)	(0.391)	(0.285)	(0.510)	(0.307)
Proportion girl peers with	0.191	0.257	-0.117	0.138	-0.785	-0.0736
family violence	(0.354)	(0.200)	(0.422)	(0.279)	(0.504)	(0.257)
Observations 72,203	72,203	71,786	71,786	35,090	35,090	
School-grade fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Grade-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
Cohort controls	Yes	Yes	Yes	Yes	Yes	Yes
School-grade-specific linear time trends	Yes	No	Yes	No	Yes	No
School-year fixed effects	No	Yes	No	Yes	No	Yes
Sibling fixed effects	No	No	Yes	Yes	No	No
Lagged test score	No	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school-cohort level. Individual controls include race, gender, and subsidized lunch status. Cohort controls include race, subsidized lunch, gender, and size. All regressions are weighted by the inverse of the number of times a student is observed in the sample. We restrict the sample to individuals not exposed to domestic violence. Charter schools are excluded because they do not record discipline consistent with traditional public schools.
* p<0.10, ** p<0.05, *** p<0.01.

unobserved shifts in the student population I analyze could be correlated with variation in exposure to domestic violence. In order for such bias to lead toward the null results I observe, it would have to be the case that when reported domestic violence rises, the student

Table 1.5: Effects of Peers Exposed to Domestic Violence on Discipline (Daily Variation)

	(1)	(2)
Proportion peers with family violence	-0.001	-0.001
	(0.002)	(0.002)
Observations	11,621,721	11,621,721
Student-school-year fixed effects	Yes	Yes
Date fixed effects	No	Yes

population becomes higher-achieving and better behaved as a result of changes in some unobserved characteristic. Because domestic violence is associated with lower scores and more disciplinary incidents, one's first instinct is probably for unobserved variable bias in the opposite direction, but there could be some cases where such events occur. To explore the possibility of bias arising from such events, I perform two robustness tests.

First, following Carrell and Hoekstra (2010), I analyze the "effect" of disruptive peers on exogenous student characteristics. Those results are presented in Table 1.6. None of the gender or free lunch results are statistically significant at the 95% level; however, three of the six race outcomes are statistically significant at the 95% level. This is primarily driven by precise estimation. The magnitude of even the significant effects is extremely small. For example, adding a peer exposed to domestic violence to a class of 20 is associated with a 0.225 percent reduction in a students' likelihood of being white. In the district I study, black students are 18 more times likely than white students to have been exposed to domestic violence. When this information is combined with residential segregation patterns in the district, it is not entirely surprising that some race-based association would remain even after applying the fixed-effect identification strategy. So, the evidence from the exogenous characteristics analysis suggests the strategy approaches quasi-randomization, but does not achieve a measure of treatment effects entirely consistent with random assignment. In addition to the small magnitude of the associations, it is important to note that the direction

Table 1.6: Effects of Peers Exposed to Domestic Violence on Exogenous Characteristics

	Male	Free Lunch	Black	White
Panel A: Peer Mean				
Proportion peers with	-0.009	-0.046	0.061*	-0.045**
family violence	(0.078)	(0.043)	(0.033)	(0.020)
Observations	80,380	80,380	80,380	80,380
Panel B: Gender Differences				
Proportion boy peers with	-0.120	-0.111*	-0.017	-0.062**
family violence	(0.108)	(0.061)	(0.048)	(0.024)
Proportion female peers with	0.113	0.027	0.149***	-0.025
family violence	(0.123)	(0.054)	(0.043)	(0.027)
Observations	80,373	80,373	80,373	80,373
School-grade fixed effects	Yes	Yes	Yes	Yes
Grade-year fixed effects	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Cohort controls	Yes	Yes	Yes	Yes
School-year fixed effects	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school-cohort level. Individual controls include race, gender, and subsidized lunch status. Cohort controls include race, subsidized lunch, gender, and size. All regressions are weighted by the inverse of the number of times a student is observed in the sample. We restrict the sample to individuals not exposed to domestic violence. * p<0.10, ** p<0.05, *** p<0.01.

of the results does not suggest upward bias in test scores or downward bias in disciplinary incidents.

Second, I implement a series of placebo tests similar to those used by Lavy and Schlosser (2011) in their study of peer effects using gender-composition variation. Specifically, I test effects from two years of data on the future and lagged share of students exposed to domestic violence within a school grade. An example will help illustrate the usefulness of this test. It should not be the case that a third grader attending school in 2012 would be impacted by the share of third graders exposed to domestic violence who will

Table 1.7: Placebo Tests Using Future and Lagged Cohorts

Specification	Lag2	Lag1	Future1	Future2
Panel A: Reading and Math Composite Score				
Proportion peers with family violence	-0.005	-0.191	0.359*	0.073
	(0.179)	(0.147)	(0.188)	(0.193)
Observations	30,406	37,609	30,688	23,724
Panel B: Number of Disciplinary Incidents				
Proportion peers with family violence	-0.375	0.267	0.073	-0.274
	(0.313)	(0.229)	(0.271)	(0.238)
Observations	27,854	34,399	28,364	21,891
School-grade fixed effects	Yes	Yes	Yes	Yes
Grade-year fixed effects	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes
Cohort controls	Yes	Yes	Yes	Yes
School-year fixed effects	Yes	Yes	Yes	Yes
Lagged test score	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school-cohort level. Individual controls include race, gender, and subsidized lunch status. Cohort controls include race, subsidized lunch, gender, and size. All regressions are weighted by the inverse of the number of times a student is observed in the sample. We restrict the sample to individuals not exposed to domestic violence. * p<0.10, ** p<0.05, *** p<0.01.

attend that school in 2014. However, if the identification strategy is picking up contemporaneous trends in the neighborhood, changes in school quality, or shifting school composition, I may observe some spurious effect. Table 1.7 presents the results of this analysis. I don't find evidence of bias in one direction or the other. Four of the coefficients are negative while four are positive. Out of the eight tests, none are statistically significant at the 95% level, though one is marginally significant. Together, the findings give additional support that the identification strategy is valid and unobserved factors are not driving my results.

1.7 Discussion

The fact that my findings differ from those of Carrell & Hoekstra motivates some discussion of how one might reconcile the divergent peer effects evidence.

Does The Measure of Domestic Violence Exposure Matter?

As explained in section 2, I rely on police records to identify students exposed to domestic violence while the Alachua County studies rely on court filings (Temporary Protective Orders). Because both methods identify peers who are similarly weak academically and similarly likely to get into trouble behaviorally, I argue that both measures succeed at identifying peers who may cause negative externalities using a source of variation exogenous to the school environment. However, it is possible that the peer effects measured in Alachua County are not caused by peers being more disruptive or weaker academically. If they are caused by unobserved differences in these peers uncorrelated with behavior problems and academic achievement, it is possible (though it seems unlikely) that the Temporary Protective Orders identify students uniquely harmful while my police records fail to identify such students.

Does Context Matter?

Context may play an important role in determining the extent to which students are impacted by a disruptive peer. Schools that rely more heavily on self-contained learning environments for disruptive peers would likely see fewer spillovers. The presence of paraprofessionals, special-education teachers, or other push-in resources to the class could also serve as mitigating forces. Districts that assign particularly disruptive students to alternative schools may limit peer interaction by isolating the most disruptive students from their peers.

Because research on peer effects themselves has been so inconclusive, even less is known about how policies schools set interact with peer effects. As a result, studies relying on data from different cities may reach divergent conclusions because each school district operates differently and very little is known about whether district policies and procedures impact the magnitude and existence of peer spillovers.

Do Student Characteristics Matter?

As mentioned in section 2, the students in Alachua County are less poor than the students in the district I analyze. 40 percent of students in the Alachua County study qualified for free and reduced lunch while 71 percent qualify in the district I study. Alachua County is also not a major metropolitan area. Its largest city is Gainsville, home to The University of Florida. I study a large urban district in a major metropolitan area.

It is possible that negative spillovers heterogeneously impact students at different income levels. One possibility to investigate this further might be to restrict the sample to a subset of schools that mirror Alachua County in observable ways. Unfortunately, the district that I analyze does not have many schools that mirror the Alachua County demographics. Instead, it is comprised of schools at both ends of the distribution (schools where virtually all the students are low income and schools where virtually no students are low income).

Further research in other contexts would shed more light on these possible explanations for the divergent results.

1.8 Conclusion

Domestic violence is a prevalent problem in the United States impacting a substantial share of students. From the medical literature and other evidence on student response to traumatic events, there is reason to think that student outcomes may be impacted by domestic violence exposure. However, the question has not been studied previously with causal identification strategies. Using seven years of daily student observations from a large urban school district, I have shown that domestic violence exposure causes a dramatic, short-term increase in a student's own absences spanning four days. This suggests that school districts may have an interest in working with police departments and other local agencies to quickly identify student victims and attempt to mitigate any negative consequences of the exposure to domestic violence. The finding may also motivate the allocation of additional resources to helping victims of domestic violence exit the abusive relationships quickly.

I have also presented robust evidence that peers in the district I study are not impacted by attending school with students exposed to domestic violence. This stands in contrast to the prior literature. Notably, the effects measured for both test scores and behavior are precise enough to rule out effects of a magnitude similar to what was measured in Alachua County. However, the reasons for the divergent findings remain unclear and warrant further research.

2 The Effect of Distance from School on Student Achievement and Attendance

2.1 Motivation and Existing Literature

Declining central-city student populations and the expansion of educational choice have both led to schools that serve students from wider geographic areas. In some cities, this development has been caused by school closure in the face of declining traditional public school enrollment (e.g. Atlanta, Chicago, District of Columbia, and Philadelphia). In other cities, is it a result of expanded choice. For example, New Orleans has moved from a system of traditional public schools with attendance zones to a city-wide, charter-only school choice plan. Meanwhile, New York City has implemented choice among traditional public schools in some of its Community School Districts as it attempts to reduce school segregation. In addition to these two phenomena, budgetary restraints have caused some school districts to reduce the number of busses employed. One unexplored consequence of these developments is longer student commutes.

There are at least four mechanisms through which longer commutes could impact student learning: reduced attendance, less time for home instruction, less physically active commutes, and sleep deprivation. The holistic consequences of longer student commutes have not been widely studied in the literature; however, a number of studies have evaluated these mechanisms individually, with some finding causal effects on student outcomes.

Because longer commutes necessitate earlier bus pick up times and longer drives to drop students off, the additional costs of commuting may result in lower rates of attendance or higher rates of tardiness. If students are present for less of the school year, they miss some direct instruction received by their peers. In one of the literature's only well-identified investigations of student absences, Goodman (2014) shows that when snow causes some students to miss school, achievement suffers. A related literature documents a relationship between instructional time and achievement by exploiting variation in weather-induced school closures and the timing of standardized assessments (Marcotte and Hemelt, 2008; Fitzpatrick et al., 2011; Hansen, 2011; Carlsson et al., 2015); however, this literature is distinct from Goodman's work which focuses specifically on students who miss school on a day when peers continue to learn. Such absences require teachers to coordinate make-up work and students may struggle to follow new material having missed an intermediate lesson. In contrast, school-wide closures reduce learning time, but keep all of the students on the same page. Absences resulting from different commutes are more similar to the absences arising from Goodman's incident because they occur when school remains in session.

A second result of longer commutes is that they reduce the time students have available for at-home learning. Economic theory suggests that home inputs are an important component of human capital development (Leibowitz, 1974), and more recent empirical work confirms an interplay between home and school inputs to education (Das et al., 2013). It is possible that students learn from peers during their commute; however, if time spent commuting is less productive than time spent at home, longer commutes may reduce student achievement.

Third, when students live further from school, fewer are able to use active means of commuting such as walking or riding a bike. The existing research linking student health to education outcomes is relatively small and tends to focus on variation in early-life health care access. Credibly designed empirical work in this area suggests that healthier students achieve at higher levels (Chay et al., 2009; Bharadwaj et al., 2013). Because longer commutes may result in some students switching from active commutes to bus ridership, they could result in declines in student health.

Finally, longer commutes likely mean that some students are picked up earlier in the morning. Two studies in the K-12 context find evidence that earlier start times impact student achievement. Cortes et al. (2012) uses variation in the order of classes to measure how student achievement varies through the day. The authors find that students who have math classes first, perform worse in math, and that weaker performance persists to the next year. Edwards (2012) exploits within-school variation in start times for Wake County, North Carolina and finds that a one-hour later start results in two percentile point gain in math achievement and similar effects for reading. A study in the college context finds start time is similarly important. Carrell et al. (2011) show that earlier start times substantially reduce student achievement by exploiting random classroom assignment and policy changes at the US Air Force Academy. If the effects found by these papers are in part a reflection of lack of sleep, one would expect that longer commutes may cause students to achieve at lower levels due to the earlier start time of their day.

Using data from a large urban school district, I evaluate how distance to school impacts attendance and student achievement on annual exams. To identify causal effects, I exploit exogenous changes to the distance between students' homes and their school. The prior research that shares the most with my empirical work is Gottfried (2010) who relied on distance from school as an instrument for attendance. My methods diverge from his in two important ways. First, Gottfried assumes that distance to school satisfies the exclusion restriction, affecting achievement only through attendance. The magnitude of the effect measured by his study (a 0.16 standard deviation math achievement reduction per missed day) is so large that the author's findings suggest missing two days of school is roughly equivalent to missing a whole year of education (Hill et al., 2008). This seems implausible. One explanation may be that distance does not satisfy the exclusion restriction based on its potential relationship with other educational inputs as described above. I avoid this problem by focusing on reduced form estimates rather than isolating a single mechanism and assuming that exclusion holds. A second difference in my analysis is that I do not assume distance to school follows a random allocation. Instead, I acknowledge that some parents may choose residential location based on proximity to school. By exploiting school renovations, closures, and consolidations, as a source of variation in the distance students must travel to school, I avoid bias resulting from residential location choices. I show the importance of using these exogenous shocks by illustrating that within school zones it is clear that students living closer to school achieve at higher levels and attend at higher levels. This fact pattern is consistent with the potential that residential location patterns reflect the importance families place on education.

Unlike residential location choices, the timing and choice of facility renovations/closures are made by district leaders, and cannot be easily anticipated by parents. As a result, variation in distance to school arising from these events is more plausibly exogenous than the distance chosen directly by families. In the district I study, a total of 40 temporary and permanent facility relocations occurred during the years 2010 through 2017. For some students the events reduced distance to school. For others, longer commutes resulted. The mean absolute value of the change in distance for those experiencing such an event is 4.0 miles.

I find little evidence of a marginal effect when adding an additional mile to a students' commute on either academic achievement or attendance. I am able to reject the null hypothesis that adding a mile to a students' commute reduces achievement by more than 0.009 standard deviations. For attendance, I am able to reject the null hypothesis that adding a mile to a students' commute reduces percent attendance by more than 0.04 percentage points.[1]

In contrast to the null effects arising from a marginal increase in distance across the full distrubution of distances, I find robust evidence that being within walking distance to school affects attendance.[2] Being able to walk to school increases attendance by 0.76 percentage points. It is not clear whether this increased attendance translates to higher

[1] For large changes in commuting distance, I am not able to rule out effects of non-trivial magnitude. For example, if a school closure, renovation, or consolidation results in a move of 4.0 miles, I would be able to reject an effect on achievement greater than 0.036.

[2] I define walking distance as residing within 0.5 miles from the school facility. In the technical appendix, I include results for 0.75 miles and 1 mile.

achievement on annual exams. While point estimates are positive, the effects on achievement are not measured precisely enough to reject a null effect.

The remainder of this chapter proceeds as follows. Section 2.2 describes the history of school renovation and closure in the large urban district where I propose to conduct this study. Section 2.3 describes the data I use in my analysis. Section 2.4 sets out my methodology and identification strategy. Section 2.5 presents results, and Section 2.6 concludes.

2.2 Renovations, Closures, and Consolidations in A Large Urban School District

As school districts age, grow, shrink, or expand, facility management choices change students' proximity to school. I study 40 events that impacted student commutes and arose from a large urban school district's choices about managing its facilities.

Of the events I study, the most common reason for a school facility move is a renovation. In the district I study, when schools are renovated, students are often temporarily relocated to another facility. This is particularly true for major renovations that can take a year or more to complete. Facility renovations in the district I work with are funded through an Education Special Local Optional Sales Tax (E-SPLOST) which was first authorized in 1997 and has remained in place for the past 21 years. Over that period, the tax has generated between \$90M and \$100M annually, or approximately \$1,900 per pupil. This has led to substantial investment in facilities. Using these funds, the district has newly constructed and/or renovated over 100 schools. One benefit of using renovations as a source of variation is that they result in temporary relocation with the students then returning to their original distance once the renovation is complete.[3] To illustrate the impact on students, consider the Jane Doe Elementary campus which was renovated during the

[3]Changing facilities may impact students in ways other than the commute. Therefore, my analysis will focus on variation in distance between students who attended the same school facility before and after the event. The proposed methodology is defined formally in section 2.4.

2013 school year. During the renovation, students were relocated to a facility 4.6 miles away from the Jane Doe campus; as a result, some students who were accustomed to walking no longer lived in walking distance and others experienced changes in the distance their parents needed to drive them and/or time they spent riding the bus. Once the renovation was complete, students returned to their typical commuting pattern. I observe the students at Jane Doe Elementary in the years before, during, and after the renovation.

In addition to renovations, students in the district I study have experienced significant changes in distance to school arising from school consolidation and closure. Beginning in the 1970s, the district experienced declining enrollment. Today enrollment is less than 50% of its peak, though some areas of the city have seen a recent reversal in these trends. As a result, the district embarked on a wide-ranging redistricting effort in 2010. The work began with a demographic study that ultimately led to the closure of seven schools. Additionally, schools were reorganized into clusters which impacted the feeder pattern for some elementary school zones. Not all of the schools recommended for closure by the superintendent were approved by the Board of Education in the original plan. As a result, schools continued to be closed through the year 2017.

Though school reorganization and renovation has been a feature in the district for much of its recent history, I focus my analysis on the period 2010 – 2017 because one of the outcomes I am most interested in exploring is achievement on standardized assessments, and reliable data from these assessments is available for this period.

The mean absolute value of the change in distance for those experiencing a school renovation, closure or consolidation is 4.0 miles in the district.

2.3 Data

For the 2010 through 2017 period I study, the district maintained an administrative data set for its students including their residential location, daily attendance, behavior, and performance on standardized assessments. In addition, the district has collected information on student race, ethnicity, free and reduced price lunch eligibility, and program participation (i.e. gifted, special education, etc.).

The district also maintains a record of schools and school addresses. In order to combine the school and student data sets, I converted street addresses to longitude and latitude locations, which allowed me to calculate the Euclidean distance between a school and households where students reside. Combining the district's data sets, I developed a panel of data for each student, year combination.[4]

Table 2.1: Descriptive Statistics

	Mean	Standard Deviation	N
Composite Normalized Score	-0.183	0.985	168,073
Percent Attendance	96.227	3.986	168,073
Distance to School	2.758	2.825	148,736
Black	0.742	0.437	168,062
Hispanic	0.070	0.255	168,062
White	0.158	0.365	168,062
Male	0.497	0.500	168,067
Free/Reduced Lunch	0.729	0.445	165,618
Students with Disabilities	0.139	0.346	168,073

Table 2.1 presents summary statistics for the district I study. A majority of students in the district are Black and quality for Free or Reduced Price lunch. The students' annual standardized assessments are normalized using the state mean and standard deviation. The mean of -0.183 shown in Table 2.1 suggests students in the district on average score approximately .2 standard deviations below the state average. I limit my analysis to students in grades 3-8 for whom testing data is available each year. The average distance to school is 2.76 miles.[5]

[4]I explored the possibility of using transit routes to improve distance measures. However, the large number of observations and available providers of such services made the approach cost prohibitive.

[5]A small number of students live unusually far from school. These cases tend to be unusual in nature. For example, a teacher whose child is allowed to attend the school even though they reside out of zone. In order to avoid the analysis being affected by these unusual cases, I calculate the mean and standard deviation of distance to school for each school and year. I then limit the analysis to students within 3 standard deviations from the mean.

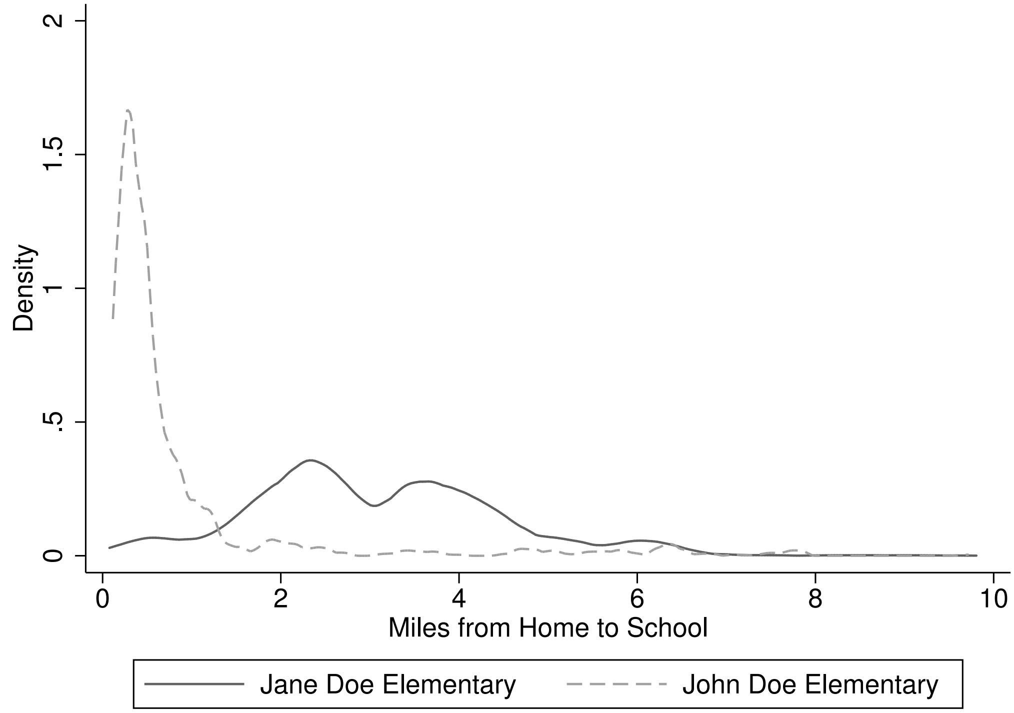

Figure 2.1: Distribution of Distance to School for Two Schools in Sample

Across the district, distance to school varies widely. Some school zones cover small, densely populated geographic areas. Others cover large geographic areas where single family homes on large pieces of land are the predominant residences. Figure 2.3 illustrates these differences from one school to another. Virtually all of the students attending John Doe Elementary live less than a mile from the school campus. Most of the students attending Jane Doe elementary live within 2 to 5 miles of the campus. Given this reality, it will be important to use an empirical estimation strategy that allows for disentangling school effects from the distance commuted by a school's students as the two are clearly correlated.

Another dimension across which distance varies is school type. As one would expect, students attending charter schools tend to live further from the campus than students attending a traditional public school. All of these schools accept applicants citywide; however, some rely on geographic boundaries when providing the first-available seats. Traditional public school students live on average 2.4 miles from campus while charter school students live 4.9 miles from campus. Figure 2.3 illustrates these patterns. Charter schools do not consistently provide transportation and tend to experience closure for reasons unrelated to facilities (e.g. poor academic achievement, low enrollment, or financial mismanagement), I limit my analysis to students in the traditional public schools.

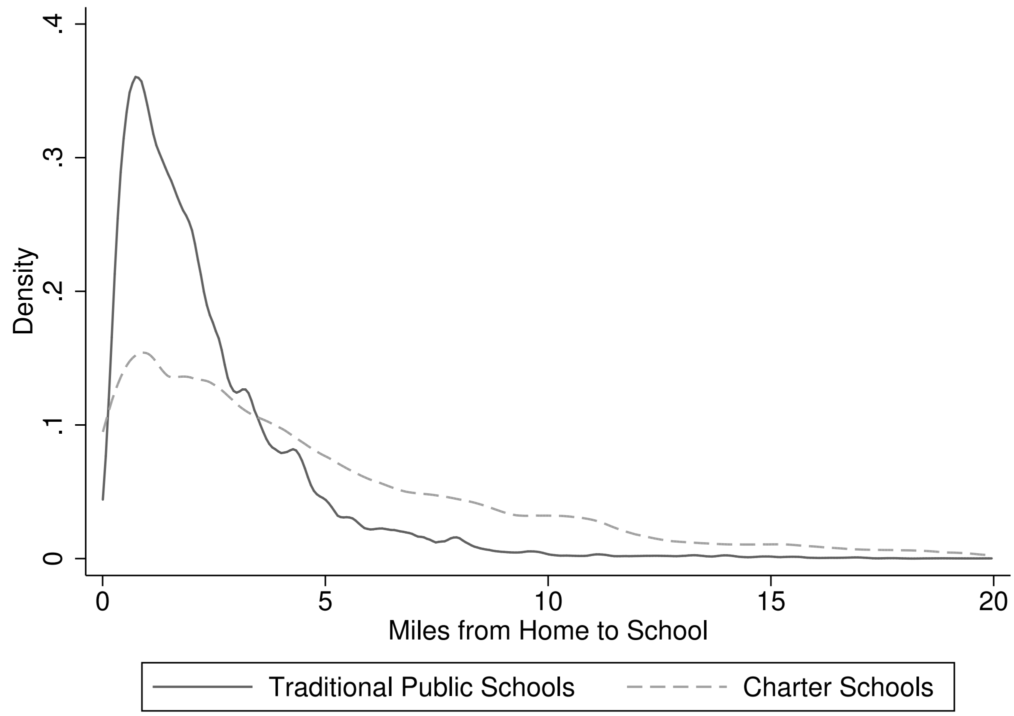

Figure 2.2: Distribution of Distance to School for Charters and Traditional Public Schools

A visual summary of the data reveals a clear relationship between distance to school and student achievement within each school community. The pattern presented in Figure 2.3 shows that being further from school is associated with lower attendance and lower achievement. This motivates further investigation into the relationship between distance to school and the outcomes of interest as it is not clear whether the relationship reflects a causal effect of distance or whether, consistent with the expectations set out above, parents who value education are choosing to locate near schools.[6] To the extent that the latter is true,

[6]In the appendix, Figure 3.6 presents similar information for walkers. Though the evidence is noisier, the figure shows a positive relationship between walking to school and both outcomes.

we would expect these students to score higher and attend more whether they lived near the school or not.

2.4 Methodology and Identification Strategy

The primary empirical challenge in identifying the effect of distance on student attendance and achievement arises because distance is a choice that students and/or their families make. Families who choose to rent or purchase houses further from school may have a lower preference for the acquisition of education capital, and therefore may contribute deferentially to their child's learning in a number of ways, including time and resources dedicated to home learning. By focusing on cohorts who experienced an exogenous change to their distance from school and controlling for lagged student data, I hope to properly identify the causal effect of distance on student attendance and achievement. I develop separate but similar models for analyzing distance and walking.

Distance Model

I begin my empirical approach to measuring the effect of commuting distance on student achievement and attendance by estimating the following model using ordinary least squares:

$$Y_{isgt} = \beta_1 + \beta_2 D_{isgt} + \beta_3 score_{isgt-1} + \beta_4 attend_{isgt-1} + \beta_5 X_{isgt} + \phi_s + \gamma_t + \eta_g + \epsilon_{isgt} \quad (2.1)$$

where Y_{isgt} is the outcome of interest (either achievement or attendance) for student i, in school s, in grade g, and year t. D_{isgt} is distance from the student's residence to the school facility. $score_{isgt-1}$ is the lagged standardized test score. $attend_{isgt-1}$ is the lagged attendance. X_{isgt} is a vector of student observables. ϕ_s, γ_t, and η_g are fixed effects for school, year, and grade, respectively.

This model is best thought of as a descriptive model and the resulting coefficient of interest, β_2, would pick up both any causal effect of distance as well as any associations

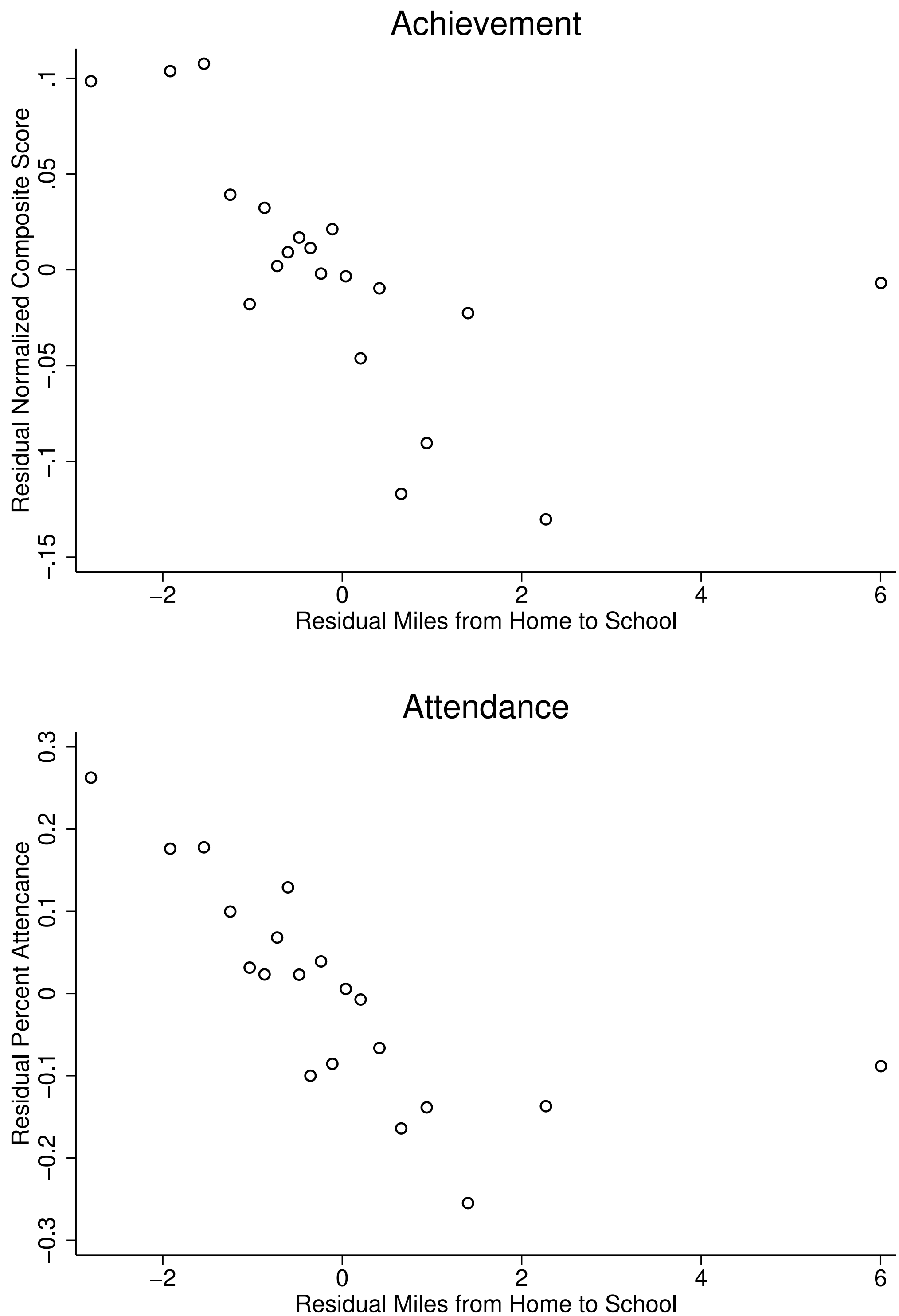

Figure 2.3: Binned Scatter Plots of Distance to School vs. Outcomes

with distance and the outcomes of interest that arise from family location choices. In order to more convincingly isolate causal effects coming from distance, I extend this model slightly and also estimate the following equation using ordinary least squares:

$$Y_{isgt} = \beta_1 + \beta_2 M_{isgt} \times D_{isgt} + \beta_3 score_{isgt-1} + \beta_4 attend_{isgt-1} + \beta_5 X_{isgt} + \phi_{sgt,sgt-1} + \eta_g + \epsilon_{isgt} \tag{2.2}$$

This model contains two important changes from the original specification. D_{isgt} is replaced with an interaction of distance to school with an indicator variable for whether or not a district facilities event occurred, $M_{isgt} \times D_{isgt}$. M_{isgt} is equal to zero when a facilities event did not occur and one when it did. This change serves to isolate effects arising from distance unrelated to parent choices. The second important change is that the school and year fixed effects, ϕ_s and γ_t, are replaced with an interaction of school, lagged school, and year, $\phi_{sgt,sgt-1}$ This change ensures that comparisons are using variation in distance for students who attended school at facility A in year t-1 and facility B in year t. Without this control, other factors such as changes in peer group composition or facility quality associated with the renovations, consolidations, and closures may bias my estimates.

Once these two changes are incorporated to the model, I argue that the specification should appropriately isolate causal effects of commuting distance on student achievement and attendance.[7]

Walking Model

To evaluate the causal effect of gaining or losing the ability to walk to school as a result of a school renovation, closure, or consolidation, I use a similar approach to the models above. I estimate the following equation using ordinary least squares.

[7] One may also think an intuitive approach is to use a first difference model of the form $\Delta Y_{isgt} = \beta_1 + \beta_2 \Delta \mathbb{1} D_{isgt} + \beta_3 X_{isgt} + \phi_{sgt,sgt-1} + \eta_g + \epsilon_{isgt}$ In addition to the results presented below, I set out results from this approach in the technical appendix. Qualitative conclusions are the same for either approach.

$$Y_{isgt} = \beta_1 + \beta_2 M_{isgt} \times W_{isgt} + \beta_3 score_{isgt-1} + \beta_4 attend_{isgt-1} + \beta_5 X_{isgt} + \phi_{sgt,sgt-1} + \eta_g + \epsilon_{isgt} \tag{2.3}$$

The only significant change to the empirical approach is that my measure of distance to school arising from a facilities event $M_{isgt} \times D_{isgt}$, is replaced with $M_{isgt} \times W_{isgt}$, where W_{isgt} is a dummy variable equal to 1 if a student lives within 0.5 miles of the school campus and 0 if a student lives further than 0.5 miles from the school campus.

2.5 Results

Distance Results

Table 2.2 presents the results from models evaluating the relationship between distance to school and performance on annual standardized exams. The results suggest there is little evidence of a marginal effect on achievement when adding an additional mile to a students' commute. The first three specifications present evidence on the relationship across the full sample, including distance chosen by families. The first column, shows a negative but statistically insignificant relationship. This attenuates as controls for other observable characteristics of the student are included.

Moving on to the evidence from facility decisions presented in columns 4 and 5, I find no evidence of a causal effect of distance on student achievement. The findings are measured precisely enough for me to reject the null hypothesis that adding a mile to a students' commute reduces achievement by more than 0.009 standard deviations. While I cannot rule out the possibility that living a mile further from school causes some small effect on student achievement, district policymakers should not anticipate that choices they make around school closure, consolidation, or choice are likely to result in major achievement impacts operating through the channel of commuting distance.

Table 2.2: Effects of Distance to School on Academic Achievement

	Composite Normalized Score				
Specification	(1)	(2)	(3)	(4)	(5)
Distance to School	-0.014	0.002	0.002		
	(0.012)	(0.004)	(0.001)		
Distance to School x School Relocation = 0				-0.014**	0.001
				(0.006)	(0.001)
Distance to School x School Relocation = 1				-0.027	-0.000
				(0.022)	(0.005)
Composite Normalized Score (Lagged)			0.806***		0.811***
			(0.004)		(0.004)
Attendance (Lagged)			0.005***		0.005***
			(0.001)		(0.001)
Observations	126,303	126,250	74,847	90,980	74,847
Grade fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
School fixed effects	Yes	Yes	Yes	No	No
Individual controls	No	Yes	Yes	No	Yes
School-lagged-school-year fixed effects	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-3 and the school-lagged-school-year level in specifications 4-5. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

For attendance, a similar picture emerges. Table 2.3 presents evidence on the relationship between distance to school and attendance. The first column shows that students who live a mile further from school generally attend school less frequently. They are 0.046 percentage points lower in their attendance record. Unlike the achievement data, individual controls do not move the coefficient much. Instead, a statistically significant relationship remains until lagged achievement and attendance are added to the model.

Table 2.3: Effects of Distance to School on Attendance

	Percent Attendance				
Specification	(1)	(2)	(3)	(4)	(5)
Distance to School	-0.046*	-0.041*	-0.024		
	(0.025)	(0.023)	(0.023)		
Distance to School x School Relocation = 0				-0.015	-0.010
				(0.011)	(0.008)
Distance to School x School Relocation = 1				0.026	0.011
				(0.055)	(0.026)
Composite Normalized Score (Lagged)			0.409***		0.379***
			(0.051)		(0.022)
Attendance (Lagged)			0.645***		0.676***
			(0.014)		(0.012)
Observations	126,303	126,250	74,847	90,980	74,847
Grade fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
School fixed effects	Yes	Yes	Yes	No	No
Individual controls	No	Yes	Yes	No	Yes
School-lagged-school-year fixed effects	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-3 and the school-lagged-school-year level in specifications 4-5. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

For the results using variation from facility moves (columns 4 and 5), I am able to reject the null hypothesis that adding a mile to a students' commute reduces percent attendance by more than 0.04 percentage points.[8]

[8]In the appendix, Tables A.1 and A.2 present evidence on achievement and attendance, respectively, using a first-difference approach. The results are qualitatively equivalent.

Walking Results

In contrast to the null effects arising from a marginal increase in distance across the full range of possible distances, I find robust evidence that being within walking distance to school does affect attendance.[9]

Table 2.4: Effects of Living Within Walking Distance on Attendance

	Percent Attendance				
Specification	(1)	(2)	(3)	(4)	(5)
Walking Distance	0.231***	0.218***	0.087*		
	(0.054)	(0.053)	(0.047)		
Walking Distance x School Relocation = 0				0.185***	0.046
				(0.046)	(0.045)
Walking Distance x School Relocation = 1				0.999**	0.756***
				(0.412)	(0.274)
Composite Normalized Score (Lagged)			0.409***		0.379***
			(0.050)		(0.022)
Attendance (Lagged)			0.645***		0.676***
			(0.014)		(0.012)
Observations	126,303	126,250	74,847	90,980	74,847
Grade fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
School fixed effects	Yes	Yes	Yes	No	No
Individual controls	No	Yes	Yes	No	Yes
School-lagged-school-year fixed effects	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-3 and the school-lagged-school-year level in specifications 4-5. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

[9] Walking distance is defined as residing within 0.5 miles from the school facility. The technical appendix includes Tables A.5 A.6 which present results for a distance of 0.75 miles and Tables A.7 A.8 which present results for a distance of 1.0 miles. The results remain statistically significant at 0.75 miles, but are not significant at a distance of 1.0 mile.

Columns 1 through 3 of Table 2.4 show that living within walking distance is associated with higher attendance across specifications. The evidence using variation from facility location choices shows that students who gained or lost the ability to walk to school as a result of a school renovation, closure, or consolidation saw their attendance change by 0.76 percentage points (increasing for those who gained the ability to walk, and decreasing for those who lost it). While an effect size of less than one percentage point sounds small, it is important to remember that the typical percent attendance is around 96 percent. Therefore, the change represents about 19 percent of missed days.

Evidence on how walking impacts test scores is less clear. The results presented in Table 2.5 show the relationship between walking to school and achievement on annual assessments. [10]

While point estimates are positive, the effects on achievement are not measured precisely enough to reject a null effect. These findings are interesting in light of the Goodman (2014) work that found missing a day of school led to a 0.05 standard deviation reduction in achievement.

The attendance findings above suggest that being within walking distance leads students to attend 1.37 (calculated as 180 * 0.75 percent) additional days. The point estimate then for the achievement effect relative to number of missed days is 0.020 (calculated as 0.027/1.37) or a little less than one half what Goodman found. Given the imprecision of my estimates, I cannot rule out the possibility that the achievement effect per day of missed school is comparable to that found by Goodman.

Nonetheless, if attendance does have an effect on achievement, it seems clear that there is little room for other mechanisms identified in the motivation for this paper (activity levels, sleep time, time at home) to have a very large effect.

[10] As with the distance results, the appendix includes Tables A.4 and A.3, which present the results of a first-difference analysis of attendance and achievement, respectively. Again, the results point to the same conclusions.

Table 2.5: Effects of Living Within Walking Distance on Academic Achievement

	Composite Normalized Score				
Specification	(1)	(2)	(3)	(4)	(5)
Walking Distance	0.040	0.020	0.004		
	(0.027)	(0.014)	(0.006)		
Walking Distance x School Relocation = 0				0.037**	0.006
				(0.015)	(0.006)
Walking Distance x School Relocation = 1				0.020	0.027
				(0.076)	(0.037)
[1em] Composite Normalized Score (Lagged)			0.806***		0.811***
			(0.004)		(0.004)
Attendance (Lagged)			0.005***		0.005***
			(0.001)		(0.001)
Observations	126,303	126,250	74,847	90,980	74,847
Grade fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
School fixed effects	Yes	Yes	Yes	No	No
Individual controls	No	Yes	Yes	No	Yes
School-lagged-school-year fixed effects	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-3 and the school-lagged-school-year level in specifications 4-5. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

2.6 Conclusion

Despite the fact that many school districts find themselves facing choices around whether to keep open or close school facilities in the face of charter school competition and declining central-city student populations, the holistic consequences of longer student commutes have not been widely studied in the literature.

Using student data and exogenous changes to school location from a large urban school district, I have evaluated how distance to school impacts attendance and student achievement on annual exams.

I found little evidence of a marginal effect when adding an additional mile to a students' commute on either academic achievement or attendance. I am able to reject the null hypothesis that adding a mile to a students' commute reduces achievement by more than 0.009 standard deviations. For attendance, I am able to reject the null hypothesis that adding a mile to a students' commute reduces percent attendance by more than 0.04 percentage points.

In contrast to the null effects arising from a marginal increase in distance, I find robust evidence that being within walking distance to school affects attendance.[11] Being able to walk to school increases attendance by 0.76 percentage points. It is not clear whether this increased attendance translates to higher achievement on annual exams. While point estimates are positive, the effects on achievement are not measured precisely enough to reject a null effect.

Ultimately, my findings suggest that choices about facilities remaining open or closing will best be made on the basis of factors other than the potential impact on student commutes. The availability of high-quality leaders, the costs of maintaining under-enrolled facilities, and the desire to provide access to school choice are likely more important considerations than how school closure or consolidation might impact students through the channel of commutes.

[11] Walking distance is defined as residing within 0.5 miles from the school facility.

3 The Effect of New York City Charter Schools: Evidence from Spatial Variation in Access

3.1 Motivation and Existing Literature

Rapid proliferation of charter schools has expanded school choice across the United States and resulted in greater variation in education delivery through increased autonomy. An emerging consensus suggests that "no excuses"[1] charter schools substantially raise achievement in some urban settings, but charters do not similarly raise achievement of students outside the urban core, where they are less likely to employ "no excuses" methods (Hoxby and Murarka, 2009; Clark et al., 2011; Angrist et al., 2013; Dobbie and Fryer, 2013).[2]

In addition to the impact that charter schools have on students who attend them, optimal policy should also consider any externalities affecting nearby traditional public schools and any sorting induced by the new school availability. Early educational choice theory suggested that policies increasing choice would result in positive externalities, with traditional public schools improving to remain competitive (Friedman, 1955; Hoxby, 2000b). Alternatively, if positive selection into the charter schools negatively affects the composition of the traditional public school population, those left behind in the traditional public school could be negatively affected by attending school with weaker peers.[3]

Several researchers have attempted to evaluate the competitive effect of charter schools on nearby traditional public schools using difference-in-difference approaches or school and

[1] Charter schools following a "no exucses" philosophy are more likely to emphasize behavior management, increased instructional time, and selective teacher hiring.

[2] In addition to these lottery studies, Abdulkadiroğlu et al. (2016) make a convincing case that charters benefit students who attend them relying on "grandfathering" as an instrument. Fryer (2014) shows that charter practices can benefit traditional public school students using a cluster-randomized trial.

[3] Research on peer effects in a variety of context has been inconclusive. Some studies find little or no effect (Angrist and Lang, 2004; Duflo et al., 2011; Abdulkadiroğlu et al., 2014; Dobbie and Fryer Jr, 2014) while others find meaningful impacts (Figlio, 2007; Carrell and Hoekstra, 2010; Lavy and Schlosser, 2011; Imberman et al., 2012).

student fixed effects. The findings vary from positive (Sass, 2006; Booker et al., 2008; Jinnai, 2014) to zero (Bifulco and Ladd, 2006) to negative (Ni, 2009; Imberman, 2011).[4] All of this research is exposed to the same threat to identification. While the approaches employed control for time-invariant characteristics of students and schools, they struggle to separate the causal effect of charter school entry from contemporaneous changes that may simultaneously impact traditional public school performance and charter availability. There are clear reasons to worry about this possibility. For example, the fact that a charter school gets authorized in one region but not another may signal that a district is adopting a wider-range of policies associated with education reform. Alternatively, districts may encourage charter schools to locate in neighborhoods more broadly targeted for improvement.

My research differs from the prior literature by using a triple-difference strategy to account for time-variant factors that may impact both charter school availability and nearby traditional public school achievement. Using data from New York City, I evaluate the impact of growing charter school market share on local communities as measured through overall student achievement and traditional public school achievement. This analysis is made possible because New York City is divided into 32 Community School Districts (CSD's) and over the past decade residents within these CSD's experienced remarkable spatial variation in access to new charter schools.[5] In Harlem, central Brooklyn, and the South Bronx, some grades saw charter market share rise from 0 percent to 50 percent from 2006 to 2015.[6] Meanwhile, adjacent districts and other grade levels within the same CSD experienced more modest changes in charter school access. Further, the timing of charter access for specific grades within a CSD varied as many charter schools roll out a grade each

[4]Additional papers in this literature find mixed results depending on specification (Holmes et al., 2003; Cremata and Raymond, 2014).

[5]This aspect of New York City's laws present an opportunity to conduct research similar to Abdulkadiroğlu et al. (2016) who exploit preferences given to students enrolled in New Orleans and Boston traditional public schools taken over by charters.

[6]Charter market share is defined as the number of charter school students enrolled in a CSD divided by the total number of students enrolled in the CSD, including charter and traditional public schools.

year after opening. Thus, this third dimension of variation (grades served) allows us to address endogenous charter school location in a new way: grades not served by the charter represent a control for events in the neighborhood not caused by the charter's entry.

My results suggest that for every 10 percent increase in charter market share, neighborhoods see a rise in overall student achievement of 0.01 standard deviations in ELA and 0.04 standard deviations in Math, equivalent to approximately one month of additional learning (Hill et al., 2008). I find no evidence that charter schools causally reduce or raise achievement of students remaining in traditional public schools; however, charter schools do cause substantial sorting into the neighborhood's schools, greater concentration of students with disabilities in traditional public schools, and selection by black and Hispanic students into more segregated schools. I implement a series of falsification tests that validate the robustness of my findings.

3.2 Context and Data

New York City's first charter school opened in the fall of 1999. Fifteen years later, the charter sector had grown to 197 schools serving 83,200 students. In addition to the year-to-year variation resulting from this rapid transition, there are two distinctive aspects of New York City's approach to education that facilitate identification of charter school effects. The first is the city's educational organization into CSD's. The second is the consistency of geographic preferences for charter school attendance.

In an attempt to allow for localized input on public school decisions, all of New York City's schools belong to one of 32 CSD's, and zoned school enrollment boundaries are also contained within that CSD. A council of eleven parents and community members represents each district. The average CSD covers approximately 9.5 square miles. The largest CSD by geography is CSD 31, which covers the entirety of Staten Island. The largest CSD by student enrollment is CSD 10, which covers the northwest corner of the Bronx.

It is a common practice nationally for charter schools to allow geographic enrollment preferences. These preferences provide students residing within an established geographic boundary an opportunity to attend the charter before students residing outside of the boundary are admitted. Most U.S. cities that permit geographic preference allow charter schools to choose how their geographic boundaries are developed, and many schools have no geographic preference at all. Exceptionally, New York City requires all charter schools to use a geographic preference consistent with the boundaries of the CSD in which the school is located. As a result, the impact of a charter school opening is most acutely felt in the immediate neighborhood where it locates.

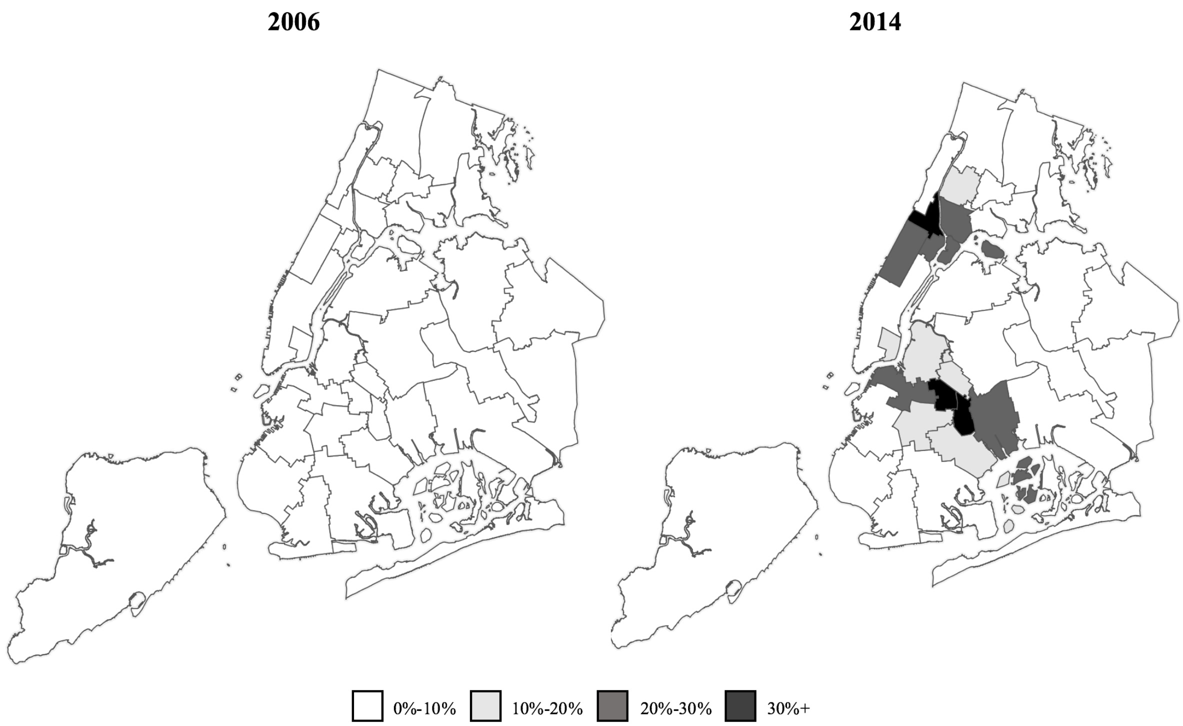

Figure 3.1: Charter School Market Share by CSD (Third Grade)

In Figure 3.2, I present the percentage of third graders within each CSD attending a charter school at two points: 2006 and 2014. This illustrates the magnitude of the transition experienced in the city over a short period as well the geographic variation in charter school access. Results for other grades are similar; importantly, grades within a CSD typically see growth in charter market share in different years. For example, a charter may open with 5th grade students and not serve an 8th grade cohort until its fourth year of operation.

My analysis relies on data from two sources. The first is a set of annual reports published by the New York State Education Department (NYSED) and includes performance and enrollment by grade for all charter and traditional public schools in the state for the period 2006 through 2015. For those traditional public schools located in New York City, the dataset identifies which CSD the school belongs to. Second, I use a charter school data set published by the New York City Charter Center that identifies the location of each charter school in the city as well as some basic information such as whether the school is associated with a for-profit or non-profit management company. Table 3.1 presents descriptive statistics from my dataset.

Table 3.1: Descriptive Statistics

Description	Mean	Standard Deviation
CSD Test Scores		
Normalized ELA Score	-0.178	(0.292)
Normalized Math Score	-0.119	(0.343)
Traditional Public School Test Scores		
Normalized ELA Score	-0.193	(0.304)
Normalized Math Score	-0.145	(0.362)
Demographics of Traditional Public School Students		
Fraction Students with Disabilities	0.190	(0.047)
Fraction Free Lunch	0.815	(0.141)
Fraction Black	0.341	(0.262)
Fraction Limited English Proficiency	0.122	(0.076)
Other Statistics		
Fraction Charter Students	0.055	(0.080)
Segregation Index	0.136	(0.084)

3.3 Identification Strategy and Methodology

The challenge of identifying the causal effect of charter schools on neighborhoods arises because of endogenous charter school location choices. There are a number of reasons one might expect charter schools to locate in atypical areas. For example, charter school leaders may anticipate having a greater impact on the academic outcomes of the students they serve by opening schools in the lowest-performing neighborhoods. Alternatively, charter schools may choose to locate in up-and-coming areas where they believe it will be easier to succeed. Therefore, a naïve strategy comparing average performance of districts with and without a charter presence would be unlikely to reflect the causal impact of charter schools on neighborhoods. Past research has dealt with this challenge by using school or student fixed effects. These approaches are successful at accounting for time-invariant unobservables; however, they do not effectively deal with changes over time. Fortunately, the New York City data contain sufficient information for us to improve upon past methods and more convincingly isolate the causal impact charter schools have on the neighborhoods where they locate. I use a triple-difference model that exploits variation in charter school access across three dimensions: time, geographic area, and grade.

Like the prior literature, the first two differences allow us to control for time-invariant characteristics that may be correlated with both charter location and the outcomes of interest. The third difference allows us to control for contemporaneous changes at nearby traditional public schools not caused by the charter school's entry. The intuition here is fairly simple. A charter school serving 3rd graders would be unlikely to have competitive or peer effects on a nearby middle school.[7] But both would be impacted by shifts in the neighborhood or other events that might impact traditional public school quality. When a charter school enters a CSD, it generally does not serve all grades. In some cases, the schools focus on a

[7] This identification strategy would not deliver unbiased estimates if competetive effects occured in grades not served by the charter. Jinnai (2014) finds no evidence of such effects.

subset of grades. Further, some charters use a roll-out strategy, beginning with a single grade and adding a grade each year. As a result, grades not experiencing changes in access to new charter seats during a given year serve as a control for concurrent events affecting the nearby traditional public schools and neighborhood, such as gentrification not induced by the charter or initiatives of the school district targeting a specific community. Finally, I use linear trends to account for any grade-specific, time-varying factors within the CSD that may bias my findings. Formally, I estimate the model below using ordinary least squares.

$$y_{cgt} = \beta_0 + \beta_1 \frac{\sum n_{scgt} * ch_{scgt}}{\sum n_{scgt}} + \beta_t \mathbf{X}_{cgt} + \lambda_{gt} + \sigma_{ct} + \phi_{cg} + \beta_{cg} T + \epsilon_{cgt} \tag{3.1}$$

where y is the outcome for each CSD c, for grade g, in year t. $\frac{\sum n_{scgt} * ch_{scgt}}{\sum n_{scgt}}$ is the percent of students within the CSD who are enrolled in a charter school. $\mathbf{X}_{cgt}$ is a vector of cohort controls including race, subsidized lunch status, and program participation.[8] λ_{gt}, σ_{ct}, and ϕ_{cg} are fixed effects for grade-year, CSD-year, and CSD-grade, respecitvely. $\beta_{cg} T$ is a CSD-grade specific linear time trend. ϵ_{cgt} is the error term. In specifications where I evaluate enrollment effects, the $\frac{\sum n_{scgt} * ch_{scgt}}{\sum n_{scgt}}$ term is replaced with $\sum n_{scgt} * ch_{scgt}$, the number of charter school seats.

The coefficient of interest is β_1 which reflects the amount the outcome being evaluated would be predicted to change should charter market share move from 0 percent to 100 percent. In reality, annual changes in charter market share are significantly smaller; therefore, I discuss results based on a 10 percent change in charter market share.

[8] In 2010, the federal government introduced the Community Eligibility Provision, which allowed schools with substantial numbers of students qualifying for free and reduced lunch to discontinue collection of eligibility data for each student and instead classify 100 percent of the students attending as eligible for free lunch. Because of this change, subsidized lunch data has become less reliable in recent years. I therefore allow the coeficients on my observable controls to shift over the period of the study.

3.4 Results

Neighborhood and Traditional Public School Achievement

Panel A of Table 3.2 presents results for the impact of an increase in charter school market share on overall neighborhood achievement in both ELA and Math. I present three different specifications: a typical difference-in-difference approach, my tripple-difference strategy without controls for observable characteristics (any sorting induced by the charter is fully reflected here), and my tripple-difference strategy with such controls (sorting on observables is removed; however, sorting on unobservables remains). The difference-in-difference approach is presented for information purposes, but I do not believe it results in causal evidence, and it is not robust to the falsification tests I introduce later.

The results of my preferred specification show that neighborhood achievement rises as charter school market share rises, with the effect greater in Math than ELA (a 10 percent increase is associated with achievement increases of 0.04 and 0.01, respectively). The positive coefficients at the neighborhood level suggest that to the extent that any negative impacts are realized in the traditional public schools, those are more than offset by achievement at the charters. Caution is warranted when trying to compare the results of my analysis to prior research on charter schools. While most studies evaluating charter schools attempt to measure school effectiveness, holding the population constant, I measure the causal effect of the schools entry to a neighborhood, including any sorting that entry may induce. Nonetheless, it is interesting to note that the magnitude of my findings is similar to what prior studies have found looking at the direct effect of charter schools on the students who attend them with lottery data (Dobbie and Fryer, 2011; Angrist et al., 2013).[9] In addition to consistency with these well-identified papers, the fact that my specification including observable controls (race, free lunch, program participation) results in measures

[9] Hoxby and Murarka (2009) measure smaller effects in their evaluation of several New York City charter schools. The difference between my findings and theirs may be attributed to the rapidly changing charter school market. The data used in my study comes from the decade following the Hoxby and Murarka work.

Table 3.2: Achievement Effects

Specification	English and Language Arts (1)	(2)	(3)	Math (1)	(2)	(3)
Panel A: Overall CSD Achievement						
Proportion of students attending a charter school	0.430***	0.169**	0.147**	0.537***	0.315***	0.367***
	(0.056)	(0.086)	(0.147)	(0.070)	(0.109)	(0.092)
Observations	1,920	1,920	1,920	1,920	1,920	1,920
Panel B: Traditional Public School Achievement						
Proportion of students attending a charter school	0.137**	-0.091	0.016	-0.030	-0.214**	-0.042
	(0.061)	(0.090)	(0.079)	(0.071)	(0.108)	(0.086)
Observations	1,920	1,920	1,920	1,920	1,920	1,920
CSD fixed effects	Yes	No	No	Yes	No	No
Year fixed effects	Yes	No	No	Yes	No	No
CSD-grade fixed effects	No	Yes	Yes	No	Yes	Yes
CSD-grade-specific linear time trends	No	Yes	Yes	No	Yes	Yes
Grade-year fixed effects	No	Yes	Yes	No	Yes	Yes
CSD-year fixed effects	No	Yes	Yes	No	Yes	Yes
Cohort controls	No	No	Yes	No	No	Yes

Notes: Robust standard errors in parentheses are clustered at the CSD-cohort level. Cohort controls include race, subsidized lunch, gender, and program participation.

almost identical to the specifications omitting observable controls suggests that most of what I measure is the result of school quality rather than positive sorting, though some positive sorting into the neighborhood on unobservable characteristics may be caused by the charter's entry. To the extend that charters do induce positive sorting, policymakers may be interested in their use as a component of neighborhood revitalization efforts.

Panel B of Table 3.2 presents results for the effect of an increase in charter school market share on student achievement for those remaining in the traditional public schools. The results provide evidence that those remaining in New York City's traditional public schools are unaffected when charter schools open in their neighborhood. The results from my preferred identification strategy are not statistically different from zero, with the point estimate in ELA slighlty positive and the point estimate in math slightly negative.

Enrollment, Sorting, and Composition

Table 3.3: Enrollment Effects

	(1)	(2)
Panel A: Overall Public School Enrollment		
One additional charter student enrollment	0.390***	0.576***
	(0.074)	(0.066)
Observations	1,920	1,920
Panel B: Private School Enrollment		
One additional charter student enrollment	-0.081***	-0.023
	(0.026)	(0.030)
Observations	1,536	1,536
Panel C: Contiguous CSD Enrollment		
One additional charter student enrollment	-0.026	-0.001
	(0.142)	(0.119)
Observations	1,920	1,920
Panel D: Segregation		
Proportion of students attending a charter school	0.092***	0.131***
	(0.027)	(0.040)
Observations	1,920	1,920
CSD-grade fixed effects	Yes	Yes
CSD-grade-specific linear time trends	No	Yes
Grade-year fixed effects	Yes	Yes
CSD-year fixed effects	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the CSD-cohort level.

In Table 3.3, I present results for a number of outcomes related to enrollment. The findings indicate that when a charter seat opens, overall enrollment in the CSD rises substantially. An additional charter seat, raises the total number of seats by 0.58. One might expect some of these students to come from nearby private schools or contiguous CSD's; however, panels B and C show little evidence that enrollment falls for private schools in the CSD or traditional public schools in contiguous CSD's. As a result, it appears that the sorting effects induced by charter entry involve residential location choices spanning a wider geography, perhaps families choosing to remain in urban areas rather than move to the suburbs. Panel D shows that charter school entry increases segregation within the CSD.[10] This is consistent with prior research in North Carolia that found charter schools increased racial isolation (Garcia, 2007; Bifulco and Ladd, 2007). It is worth pointing out that approximately 92 percent of charter school students in New York City are black or Hispanic. Thus, the segregation caused by charter schools in this context is largely driven by the choice of black and Hispanic students to attend the charter schools.

While I showed in Table 3.2 that traditional public school students are not harmed by charter school entry, their composition does change. Table 3.4 shows that a 10 percent increase in charter school market share results in a 0.58 percentage point increase in the share of students in the traditional public schools classified as students with disabilities, a 0.73 percentage point increase in the share of limited English proficiency students, and a 0.63 percentage point reduction in the share of black students (some charter schools specifically target recruitment of black and Hispanic students).

[10]To measure segregation at each grade-CSD-year, I first calculate diversity at both the school and the CSD level. Diversity is defined as the likelihood that two students selected at random will be of different ethnicity. I then measure segregation as one minus the ratio of the weighted average school-level diversity to the CSD's diversity. If the ethnic composition of the CSD were equally distributed across schools, the segregation indicator would be equal to zero. If each ethnicity attended their own school, it would be one.

Table 3.4: Effects on Student Observable Characteristics at Traditional Public Schools

	Students with Disabilities	Free Lunch	Black	Limited English Proficiency
Proportion of students attending a charter school	0.056***	-0.051	-0.063***	0.073***
	(0.022)	(0.036)	(0.022)	(0.026)
Observations	1,920	1,920	1,920	1,920
CSD-grade fixed effects	Yes	Yes	Yes	Yes
CSD-grade-specific linear time trends	Yes	Yes	Yes	Yes
Grade-year fixed effects	Yes	Yes	Yes	Yes
CSD-year fixed effects	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the CSD-cohort level.

3.5 Robustness Checks

As explained in Section 3.3, my empirical strategy is designed to deal with endogenous charter school location by using grade-CSD fixed effects to account for time invariant characteristics and using CSD-year fixed effects to account for unobservable variation in the neighborhood that may be associated with both charter school market share and my outcomes of interest.

One may worry that in addition to endogenous physical location, the charter's choice of which grades to serve is also strategically related to grade-specific events in the area. For example, it is possible that while choosing a location, a charter school decides to serve grade 5 because parents are dissatisfied with the traditional public school option for that grade.[11] In order to test the robustness of my empirical model to such possibilities, I test

[11] The fact that many charter management organizations use common practices around enrollment makes this less likely. For example, when KIPP schools began in New York City, they opened with 5th grade only and added grades up to grade 8. It was not left up to each location to choose which grades they wished to serve. Other Charter Management Organizations employ similarly uniform approaches to school roll-out and grades served. Success Academy begins its campuses with grades K-1 and adds a grade each year. Uncommon Schools initially began with middle schools, but has since added elementary schools.

Table 3.5: Placebo Test of Achievement Effects

Specification	English and Language Arts (1)	(2)	(3)	Math (1)	(2)	(3)
Panel A: Overall CSD Achievement						
Proportion of students attending a charter school	0.433***	-0.036	-0.021	0.459***	-0.052	-0.021
	(0.061)	(0.091)	(0.075)	(0.072)	(0.117)	(0.096)
Observations	1,728	1,728	1,728	1,728	1,728	1,728
Panel B: Traditional Public School Achievement						
Proportion of students attending a charter school	0.205***	-0.091	-0.068	0.038	-0.135	-0.100
	(0.065)	(0.085)	(0.071)	(0.075)	(0.112)	(0.093)
Observations	1,728	1,728	1,728	1,728	1,728	1,728
CSD fixed effects	Yes	No	No	Yes	No	No
Year fixed effects	Yes	No	No	Yes	No	No
CSD-grade fixed effects	No	Yes	Yes	No	Yes	Yes
CSD-grade-specific linear time trends	No	Yes	Yes	No	Yes	Yes
Grade-year fixed effects	No	Yes	Yes	No	Yes	Yes
CSD-year fixed effects	No	Yes	Yes	No	Yes	Yes
Cohort controls	No	No	Yes	No	No	Yes

Notes: Robust standard errors in parentheses are clustered at the CSD-cohort level. Cohort controls include race, subsidized lunch, gender, and program participation.

whether charter school market share has predictive power for something it could not have causally impacted: neighborhood student achievement in the year prior to the charter's entry.

Table 3.5 presents these placebo tests for achievement effects. Future charter school market share is not predictive of student achievement in either ELA or Math when my preferred specification is used. This lends support to a causal interpretation of the achievement results found in my main model. The results from the difference-in-difference approach suggest that charters do endogenously locate and that a triple-difference approach is needed to measure unbiased causal effects.

Table 3.6 presents placebo tests for enrollment and sorting outcomes. Again, the measured effects are null and indicate that my findings are unbiased by endogenous charter

Table 3.6: Placebo Test of Enrollment Effects

	(1)	(2)
Panel A: Overall Public School Enrollment		
One additional charter student enrollment	0.069	-0.017
	(0.078)	(0.067)
Observations	1,728	1,728
Panel B: Private School Enrollment		
One additional charter student enrollment	-0.068***	-0.028
	(0.025)	(0.028)
Observations	1,536	1,536
Panel C: Contiguous CSD Enrollment		
One additional charter student enrollment	0.069	0.101
	(0.136)	(0.109)
Observations	1,728	1,728
Panel D: Segregation		
Proportion of students attending a charter school	-0.051	-0.001
	(0.038)	(0.027)
Observations	1,728	1,728
CSD-grade fixed effects	Yes	Yes
CSD-grade-specific linear time trends	No	Yes
Grade-year fixed effects	Yes	Yes
CSD-year fixed effects	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the CSD-cohort level.

school location. The private school outcome demonstrates the importance of including school-by-grade linear time trends in the model for that specific outcome.[12]

[12]This may be the results of private schools choosing to operate in a model that maintains consistent size across grades even once demand falls in a given grade.

Table 3.7: Placebo Test of Observable Characteristics at Traditional Public Schools

	Students with Disabilities	Free Lunch	Black	Limited English Proficiency
Proportion of students attending a charter school	-0.002 (0.034)	-0.035 (0.024)	-0.029 (0.025)	-0.002 (0.024)
Observations	$1,728$	$1,728$	$1,728$	$1,728$
CSD-grade fixed effects	Yes	Yes	Yes	Yes
CSD-grade-specific linear time trends	Yes	Yes	Yes	Yes
Grade-year fixed effects	Yes	Yes	Yes	Yes
CSD-year fixed effects	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the CSD-cohort level.

Table 3.7 demonstrates that the effects I measure for how charter schools impact the composition of students left behind in the traditional public schools are also not biased by contemporaneous trends or spurious correlations.

3.6 Conclusion

Much of the most compelling research on charter school achievement has focused on the schools' impact on students who attend them. Broadly, the results using lottery data point to large positive average treatment effects on the treated in urban settings. Less convincing evidence has been presented on the holistic way charter schools impact neighborhoods, including those left behind in the traditional public schools and those who adjust residential choices based on access to charter schools. This is largely attributable to the difficulty researchers face in developing plausible identification strategies that deal with endogenous charter school location and contemporaneous events in the neighborhood where they choose to locate.

Using data from New York City and a new approach to identification, I have shown that charter schools raise the overall neighborhood achievement level and do not significantly impact achievement at the traditional public schools in the neighborhood. These findings suggest that charter schools are an effective means by which policymakers can raise overall student achievement and potentially cause revitalization of neighborhoods through positive sorting. The substantial gains other researchers (Dobbie and Fryer, 2011; Angrist et al., 2013; Dobbie and Fryer, 2013) have found for urban charter schools do not come at the expense of nearby traditional public schools. Instead, overall student achievement rises when a charter school enters a neighborhood, and some positive sorting into the neighborhood may occur as well.

While student achievement rises, other consequences of charter school entry may be of concern for policy makers. Particularly the fact that the schools cause significant increases in the concentration of students with disabilities and limited English proficiency in the traditional public schools may motivate policies which attempt to provide greater access for these students to charter school seats.

Appendix

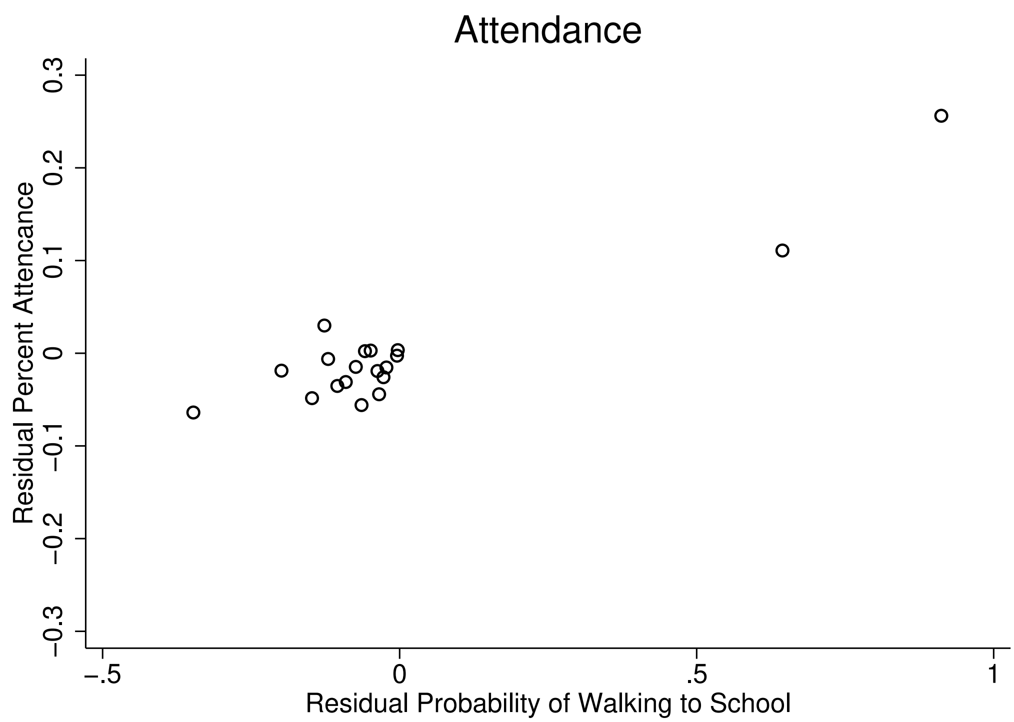

Figure A.1: Binned Scatter Plots of Walking to School vs. Outcomes

Table A.1: First Difference Effects of Distance to School on Academic Achievement

	Change in Composite Normalized Score		
Specification	(1)	(2)	(3)
Change in Distance	0.003	0.003	
	(0.002)	(0.002)	
Change in Distance x School Relocation = 0			-0.003*
			(0.002)
Change in Distance x School Relocation = 1			0.001
			(0.002)
Observations	71,171	71,151	71,151
Grade fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No
School fixed effects	Yes	Yes	No
Individual controls	No	Yes	Yes
School-lagged-school-year fixed effects	No	No	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-2 and the school-lagged-school-year level in specification 3. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

Table A.2: First Difference Effects of Distance to School on Attendance

	Change in Percent Attendance		
Specification	(1)	(2)	(3)
Change in Distance	-0.049**	-0.049**	
	(0.024)	(0.024)	
Change in Distance x School Relocation = 0			-0.027*
			(0.015)
Change in Distance x School Relocation = 1			0.028
			(0.023)
Observations	86,279	86,258	86,258
Grade fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No
School fixed effects	Yes	Yes	No
Individual controls	No	Yes	Yes
School-lagged-school-year fixed effects	No	No	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-2 and the school-lagged-school-year level in specification 3. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

Table A.3: First Difference Effects of Walking Distance on Achievement

	Change in Composite Normalized Score		
Specification	(1)	(2)	(3)
Change in Walking	0.002 (0.008)	0.002 (0.008)	
Change in Walking x School Relocation = 0			0.011 (0.009)
Change in Walking x School Relocation = 1			0.008 (0.018)
Observations	71,215	71,195	71,195
Grade fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No
School fixed effects	Yes	Yes	No
Individual controls	No	Yes	Yes
School-lagged-school- year fixed effects	No	No	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-2 and the school-lagged-school-year level in specification 3. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: First Difference Effects of Walking Distance on Attendance

	Change in Percent Attendance		
Specification	(1)	(2)	(3)
Change in Walking	0.328***	0.324***	
	(0.073)	(0.072)	
Change in Walking x School Relocation = 0			0.113
			(0.072)
Change in Walking x School Relocation = 1			0.358**
			(0.165)
Observations	86,332	86,311	86,311
Grade fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	No
School fixed effects	Yes	Yes	No
Individual controls	No	Yes	Yes
School-lagged-school-year fixed effects	No	No	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-2 and the school-lagged-school-year level in specification 3. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

Table A.5: Robustness of Walking Distance on Academic Achievement (0.75 Miles)

	Composite Normalized Score				
Specification	(1)	(2)	(3)	(4)	(5)
Walking Distance	0.061**	0.017	0.004		
	(0.026)	(0.012)	(0.004)		
Walking Distance x School Relocation = 0				0.058***	0.003
				(0.013)	(0.005)
Walking Distance x School Relocation = 1				0.135**	0.056**
				(0.066)	(0.026)
[1em] Composite Normalized Score (Lagged)			0.806***		0.811***
			(0.004)		(0.004)
Attendance (Lagged)			0.005***		0.005***
			(0.001)		(0.001)
Observations	126,303	126,250	74,847	90,980	74,847
Grade fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
School fixed effects	Yes	Yes	Yes	No	No
Individual controls	No	Yes	Yes	No	Yes
School-lagged-school-year fixed effects	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-3 and the school-lagged-school-year level in specifications 4-5. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

Table A.6: Robustness of Walking Distance on Attendance (0.75 Mile)

	Percent Attendance				
Specification	(1)	(2)	(3)	(4)	(5)
Walking Distance	0.213***	0.189***	0.076*		
	(0.059)	(0.055)	(0.040)		
Walking Distance x School Relocation = 0				0.151***	0.039
				(0.041)	(0.034)
Walking Distance x School Relocation = 1				0.775**	0.460**
				(0.281)	(0.189)
Composite Normalized Score (Lagged)			0.409***		0.379***
			(0.050)		(0.022)
Attendance (Lagged)			0.645***		0.676***
			(0.014)		(0.012)
Observations	126,303	126,250	74,847	90,980	74,847
Grade fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
School fixed effects	Yes	Yes	Yes	No	No
Individual controls	No	Yes	Yes	No	Yes
School-lagged-school-year fixed effects	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-3 and the school-lagged-school-year level in specifications 4-5. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

Table A.7: Robustness of Walking Distance on Academic Achievement (1 Mile)

	Composite Normalized Score				
Specification	(1)	(2)	(3)	(4)	(5)
Walking Distance	0.080***	0.026	0.004		
	(0.027)	(0.013)	(0.004)		
Walking Distance x School Relocation = 0				0.072**	0.003
				(0.012)	(0.005)
Walking Distance x School Relocation = 1				0.154*	0.035
				(0.079)	(0.022)
[1em] Composite Normalized Score (Lagged)			0.806***		0.811***
			(0.004)		(0.004)
Attendance (Lagged)			0.005***		0.005***
			(0.001)		(0.001)
Observations	126,303	126,250	74,847	90,980	74,847
Grade fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
School fixed effects	Yes	Yes	Yes	No	No
Individual controls	No	Yes	Yes	No	Yes
School-lagged-school-year fixed effects	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-3 and the school-lagged-school-year level in specifications 4-5. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

Table A.8: Robustness of Walking Distance on Attendance (1 Mile)

	Percent Attendance				
Specification	(1)	(2)	(3)	(4)	(5)
Walking Distance	0.229***	0.191***	0.078**		
	(0.056)	(0.052)	(0.038)		
Walking Distance x School Relocation = 0				0.200***	0.054*
				(0.037)	(0.030)
Walking Distance x School Relocation = 1				0.482**	0.050
				(0.190)	(0.197)
Composite Normalized Score (Lagged)			0.408***		0.379***
			(0.050)		(0.022)
Attendance (Lagged)			0.645***		0.676***
			(0.014)		(0.012)
Observations	126,303	126,250	74,847	90,980	74,847
Grade fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	No	No
School fixed effects	Yes	Yes	Yes	No	No
Individual controls	No	Yes	Yes	No	Yes
School-lagged-school-year fixed effects	No	No	No	Yes	Yes

Notes: Robust standard errors in parentheses are clustered at the school level in specifications 1-3 and the school-lagged-school-year level in specifications 4-5. Individual controls include race, gender, subsidized lunch status and disability indicators. * p<0.10, ** p<0.05, *** p<0.01.

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Vita

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