ESSAYS ON ECONOMICS OF EDUCATION

By

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ABSTRACT

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Chapter 1 analyzes how principals and teachers react when principals are given complete control over teacher retention decisions after a district-level restructuring policy has laid off all untenured teachers. I examine three questions related to increases in principal autonomy: Was there a change in the probability of an untenured teacher returning to the same school the next year? If so, were higher-value-added teachers retained at a higher rate? Lastly, what effect did the restructuring of staffs have on student performance? A conceptual framework based on Bayesian updating employer learning predicts that in the year of the layoffs fewer teachers would be retained in their position with the more-senior, untenured teachers being the least likely to be retained. Using a triple-difference identification strategy on data collected from the Rockford Public School District (RPS) and Illinois State Board of Education, I find that fewer untenured teachers were retained in the year of the restructuring with an inverse relationship between untenured teacher experience and retention rates. Additionally, there is suggestive evidence of an inverse relationship between value-added scores and retention rates. Finally, I find evidence that the policy had a negative impact on student test performance. A 10 percentage point increase in untenured teachers at a school in the year of the layoffs is associated with 0.02 - 0.04 standard deviations lower student test scores in math two years after the policy.

Chapter 2 examines whether layoff policies, like the one outlined in Chapter 1, affect teacher effort. I utilize a difference-in-difference identification strategy to estimate the effect of the RPS layoff announcement of this policy on untenured teacher effort. Both ordinary least squares and Poisson quasi-maximum likelihood estimation methods find that the there is little impact on the total teacher absences after the layoff announcement. However, when separated into weeks, I find that untenured teachers took, on average, 0.08 fewer absences in the week immediately following the layoff announcement. Additionally, looking at the daily-level, in the Friday after the layoff announcement teachers took, on average, 0.04 more absences. I hypothesize that this initial negative effect is due to lower teacher morale but the initial shock to morale dissipates and teachers change their behavior to apply more effort to attempt to win their jobs back. These results indicate that teachers are sensitive to policies regarding their job security, but their initial response to the policies fade quickly.

Chapter 3 discusses the impact of charter schools on property values. While prior research has found clear impacts of schools and school quality on property values, little is known about whether charter schools have similar effects. Using sale price data for residential properties in Los Angeles County from 2008 to 2011 we estimate the neighborhood level impact of charter schools on housing prices. Using an identification strategy that relies on census block fixed-effects and variation in charter penetration over time, we find little evidence that the availability of a charter school affects housing prices on average. However, we do find that when restricting to districts other than Los Angeles Unified and counting only charter schools located in the same school district as the household, housing prices fall in response to an increase in nearby charter penetration.

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Chapter 1

Restructured or Fractured: The Impact of a One-Time Increase in Principal Autonomy on Staffing Decisions and Student Outcomes.

1.1 Introduction

On March 23, 2010, the Rockford, Illinois, Public School District (RPS) announced that they would lay off every untenured teacher and give principals autonomy over rehire decisions in order to restructure the district. This policy is just one of the many examples of a nationwide trend to give principals more power over choosing their staff in order to improve student outcomes. Chicago in the mid-2000's made it considerably easier for principals to layoff untenured teachers. The Chicago policy made it such that at the end of the school year a principal would receive a list of all untenured teachers within the school and would check a box for the teachers to be laid off. Another policy being implemented in the U.S. that gives principals more autonomy is the removal of teacher tenure. Several states (such as California, Florida, Michigan, North Carolina, New Jersey, and New York) have considered removal of tenure, which would give principals more power to replace more-experienced teachers in their schools. In many cases, the proposed policies replace teacher tenure with a yearly evaluation system. Washington, DC has utilized this type of policy when it implemented IMPACT, which uses evaluations to determine whether a teacher is laid off, put on probation, or given a bonus. The current research increases our understanding of the impacts, on teacher retention and student test scores, and potential unintended consequences of implementing policies that give principals more autonomy by analyzing the impacts of the RPS policy.

The RPS restructuring in 2010 was part of their recently hired superintendent's plan to increase student test scores by 2015.¹ When the layoffs were announced, it was also announced that principals had complete autonomy over which teachers were retained.² From the teacher's

¹ The superintendent's tenure in the district was very short-lived. She was hired to a four-year contract over the summer of 2009, announced the layoffs in March of 2010, and resigned in April of 2011.

² Throughout this paper I refer to teachers being retained or rehired. This is a simplified notation for saying that the teacher returns to the same school the following year.

perspective: she would be told of the layoff on March 23, then at the end of the school year she would reapply to her old position through an online form and interview with her principal, and then the principal would decide whether to retain her or not. If the teacher did not get retained by her principal in the year of the restructuring, she could apply for a position at another school within the district or leave the district altogether.³ This policy provides a natural experiment to estimate the causal impacts of increases in principal autonomy on school staffing and student test scores. Note that there are two mechanisms through which the policy could have an effect on teacher retention: first, there is the direct effect of giving principals more autonomy that leads to changes in the principal decisions over staffing and, second, there is potential for an indirect behavioral response from teachers affected by the policy. In this study I ask three overarching questions pertaining to increases in principal autonomy: Did the sudden change in principal autonomy lead to a change in teacher retention rates? What were the characteristics of teachers who were more likely to return to their same school? And did the policy achieve the intended results of improving student test scores?

To answer these questions, I utilize a triple-difference identification strategy on data collected from the RPS and the Illinois State Board of Education. The triple-difference strategy compares the affected group, untenured teachers in RPS in 2010, to the comparison group, untenured and tenured teachers in other Illinois districts, tenured teacher in RPS, and untenured teachers in RPS in years leading up to the layoffs. In addition to the triple-difference identification, I use synthetic control and difference-in-difference methods as robustness checks and find reassuring consistency in the results. Furthermore, to identify the effect on student

³ Some may question how such a policy could get implemented. As discussed already, the policy stemmed from the superintendent. Additionally, it had the full backing of the school board. However, the policy was opposed by the teachers' union. Since the policy only targeted probationary (or untenured) teachers, it was within the power of the superintendent and school board.

performance, I utilize a difference-in-difference strategy that relies on variation in percent of untenured teachers within schools in RPS.

I find evidence that untenured teacher retention rates decline in the year of the layoffs in RPS. When principals are given autonomy over rehire decisions after all untenured teachers are laid off, untenured teacher retention declined 4 to 6 percentage points. Additionally, I find that the more experienced untenured teachers saw particularly lower retention rates. This implies that principals preferred to retain a first-year teacher over a fourth-year teacher, who would be up for tenure review the following year, with everything else equal.

While my identification strategy separates out the effects of those affected by the policy (untenured teachers in RPS in 2010) and compare them to others not affected, there are still limitations of the estimation. In particular, I am estimating a reduced form effect of the layoff policy. However, there may be confounding factors that are occurring in RPS simultaneously with the layoffs. As mentioned earlier, the layoffs were part of a recently hired superintendent's plan to improve student performance. So, any effects I find could be explained by the layoff policy implemented by the superintendent or the overall impact of a new superintendent. To help alleviate this concern, I include a specification that looks at how a new superintendent affects teacher retention rates in all of Illinois excluding RPS and find that, while retention rates decline, the decline in retention rates is considerably less than the main results. Additionally, the new RPS superintendent affected both untenured and tenured teachers but the layoffs only affect untenured teachers. So, the difference between untenured and tenured teachers in my estimation method removes the some of the effect of the new superintendent.

When I compare teacher retention to teachers' value-added scores, I find the highervalue-added teachers are retained at lower rates, but I fail to reject that the effect is different from

zero due to imprecision. The RPS did not give principals a criteria on which teachers should be retained and did not provide teacher value-added scores to aide in the principal's decision. This could partially explain why I find that if an untenured teacher had a one standard deviation higher math VAM score, then in the year of the layoffs, she would be 20 percentage points less likely to be retained.

Lastly, I find that student performance declined in the schools more affected by the layoffs, as measured by share of untenured teachers. For this estimate, I utilize variation in the percent of untenured teachers in the schools in a difference-in-difference strategy that uses only the RPS to identify the effect. I find that if a school has 10 percentage points more untenured teachers, their students' math test performance declines 0.02 to 0.04 standard deviations.

These results offer a counterpoint to the existing literature on the impact that laying off teachers has on student performance. Simulations have shown, under certain assumptions, that removing lower value-added teachers from school districts would lead to student test performance increases (Hanushek, 2009; Goldhaber, 2011). Additionally, studies on giving principals more autonomy to fire untenured teachers have suggested that such policies increase student test performance (Jacob, 2011). The results of the current study show the implementation of this type of policy can lead to lower retention of high-value-added teachers, which leads to the layoffs negatively impacting student performance. The main take away from this is that while there is potential for student improvement, my study shows that one real-life implementation of such a policy has led to negative effects.

1.2 Background Information

The Rockford Public School District (RPS) is a large school district in Northern Illinois. There were approximately 26,000 students enrolled in 28 elementary, 6 middle, 5 high, and 11 specialty schools in the district in fall 2011. In any given year, there are approximately 2,100 teachers in the district, of which about 550 are untenured. As seen in Figure 1.1, RPS students tend to underperform on standardized exams (shown on the graph as RPS being below adequate yearly progress - AYP - threshold). Additionally, Figure 1.1 shows that RPS is more racially diverse than the average Illinois school district. Lastly, Figure 1.1 shows that RPS is one of the largest districts in Illinois outside of Chicago. The size of the district is represented by the size of its circle in the figure.

A teacher's case for tenure is reviewed by the district after their fourth year of service in the RPS District – and effectively no one fails to receive tenure after four years of service (both before and after the policy), but this is in part due to a self-selection of teachers.⁴ According to Section D of the ISBE Non-Regulatory Guidance on The Performance Evaluation Reform Act (PERA) and Senate Bill 7 (SB 7), after four years of experience, untenured (probationary) teachers' contracts could be renewed or tenure granted at the discretion of the school district and the district was not required to take performance evaluations into account. After the restructuring, teachers retained in the same school or hired at a different school within the district would have their tenure clock continue uninterrupted. For example, if a teacher was in her fourth year in March 2010, she would have received a layoff notice. Then, if that teacher was not retained by her principal and subsequently hired at a new school within the district, the year following the layoffs (the 2010-2011 school year) she would be up for tenure review.

⁴ While I do not have specific data on tenure review successes, in conversations with RPS teachers it was made clear that teachers who stayed in the district for five years received tenure. In their minds, it was not a question of whether the tenure review was successful; it was only a matter of achieving enough experience.

The layoffs marked a change in principal autonomy and the way school staffing was determined. The hiring process in Rockford is a centralized process where a new teacher applies to the district human resources department. If hired, the central administration places the teacher into an open position in the district. Principals can affect whether that untenured teacher returns to the same position the following year by choosing to discharge them or not. However, there are substantial transaction costs associated with the involuntary removal of a teacher. Transaction costs may include formal evaluation, a written explanation sent by certified mail or delivered in person, complete review of staff member's files, and a hearing before the Rockford School Board (Rockford Collective Bargaining Agreement, 2011). However, teachers leaving voluntarily impose little to no costs on the administration and thus this occurs more frequently. Therefore, principals can use pressure (e.g., giving a teacher a harder group of students to teach) to coerce teachers to leave. With the district-wide layoffs, the principals no longer faced the large transaction costs for dismissal, which theoretically made it easier to remove marginal teachers and fill the position with a new teacher. So, all else equal, there should be more teacher turnover in the year of the restructuring. When a principal receives an application from an untenured teacher from another school in the year of the layoffs, the principal would not know whether the teacher was leaving voluntarily or was not rehired by their original principal. However, after receiving an application, in theory, the principal could contact the previous principal to decipher whether the teacher left voluntarily or not.

I am interested in whether there was a change in retention rates and whether higher teacher performance lead to higher retention rates. Teacher retention is a topic that is commonly studied in the literature (Guarino et al., 2006). Jacob (2011) looks at how principals select which teachers are laid off. The author finds that probationary teachers, level of absenteeism, and

value-added (i.e., performance) measures do play a role in the principal's layoff decision. Additionally, Grissom, Loeb, and Nakashima (2014) show that when principals are allowed to move teachers without discretion, the principals remove the lower-performing teachers and replace them with better teachers. However, Goldhaber and Theobold (2013) show that reduction-in-force notices (RIFs) in the state of Washington from 2008 to 2010 were given based on seniority more than value-added measures (VAM). Continuing in this vein of literature, I analyze whether a teacher's VAM aligns with the principal's retention decision in RPS. In the RPS, the district does not calculate out VAM, so the principals do not have access to the VAM scores, but the current economic literature supports the position that principals can correctly identify the highest VAM teachers in absence of explicit scores (Jacob and Lefgren, 2008).

The last area of analysis is whether the mass layoff achieved its main goal of improving student test scores. There are a few avenues through which the restructuring would affect student test scores. First, the increase in autonomy is a shift to less-centralized human resources. Decentralizing human resources has been shown to lead to student performance gains (Naper, 2010). Second, if principals choose to retain better teachers and replace below-average teachers – as measured by VAM – with random draws from the distribution of new teachers, there could be substantial gains to student performance. A review of the current economic literature on the efficacy of using VAM to measure teacher quality and how VAM is subsequently used in policy can be found in Jackson et al. (2014). Similarly, Staiger and Rockoff (2010) discuss the existing evidence of how policy can be used to affect teacher quality. Specifically, the authors suggest that instead of screening before hiring, it would be more efficient if principals were to evaluate teachers for their on-the-job performance and then make a retention decision. Hanushek (2009) and Goldhaber (2011) show evidence that if a district were to lay off the bottom value-added

teachers and replace them with average teachers, student test scores are predicted to increase in simulations. Other studies show that when principals use Bayesian updating of beliefs with new VAM information, student performances increase (Rockoff et al., 2012). Additionally, studies connect rewarding teachers for their VAM performance to student performance gains (Goldhaber and Hansen, 2010). However, some studies show that an increase in teacher turnover leads to lower student performance (Ronfeldt, Loeb, and Wyckoff, 2013). Rothstein (2015) shows, through simulations, that the efficacy of any policy used to improve teacher quality will depend on the interaction with the teacher labor markets. In particular, if teacher supply is not perfectly elastic, then performance-based policies will need a large compensation increase. If one looks at the broader economic literature, there is evidence that firms have lower productivity in the wake of mass layoffs (De Meuse et al., 1994). Applying this to an education production function would suggest that student test performance would decrease in the years following the layoffs, as the teachers within the schools are less productive.

1.3 Conceptual Framework

To model how principals act when given a sudden increase in autonomy over personnel decisions, I base my framework on the existing Bayesian employer learning literature. There is a long list of studies that look at employer's Bayesian updating and employee signaling (Spence 1973; Jovanovich, 1979; Harris and Holmstrom, 1982; Farber and Gibbons, 1996; and Gibbons et al., 2005). Even more recently, there have been some extensions of the model that look specifically at the principal-teacher relationship (Rockoff et al., 2012).

Assume that teachers are drawn from independent and identical distribution. In the first period, the principal hires a teacher with a random draw from the distribution. In the second

period, the principal does not observe the teacher's true quality but instead uses a Bayesian updating to form an expectation of the teacher's quality, based on the teacher's signals. For a principal to retain the teacher, the principal's expected quality of the teacher must be greater or equal to the outside option minus transaction costs. In the year of the layoffs, the transaction costs equal zero, which would make the principal's outside option more attractive. Without the transaction costs in the year of the layoffs, the level of teacher quality necessary for the principal to retain a teacher is higher.

In addition to the change in transaction costs, it is possible that the change in default option could lead to different outcomes in the year of the layoffs. In the organ donation literature, Abadie and Gay (2006) show that the difference between a default option of opt-out can lead to considerably different outcomes from an opt-in default option. Applying this to the RPS principals' decisions, in the years prior to the layoffs, the default option for untenured teachers is that they would be retained unless the principal actively decided to lay them off.⁵ However, in the year of the layoffs, the default option is that teachers would <u>not</u> be retained unless the principals decided to rehire them. Since the shift in default option moved to a position where action would need to be taken for a teacher to be retained in the same position, there is potential that fewer teachers would be retained as a result.

In addition to the change in principal decision in the year of the layoffs, there is potential for a behavioral response from teachers. While there was not a large change in economic incentives for the teachers in the year of the layoffs – only an online application was needed to reapply for the same position – there were larger behavioral disincentives. In particular, untenured teachers that were involved in the mass layoffs could be demoralized by the process. If

⁵ I am only considering the principal decision effect here, but in any given year, including the year of the layoffs, there would be some voluntary separations. I discuss this in further detail later in this section.

this is the case, then any sufficiently demoralized teacher would likely leave the school district voluntarily to move to another district or private school that does not use similar layoff strategies or leave the teaching profession altogether. Similar effects on teachers leaving the district are found by Adnot et al. (2016) when looking at the implementation of the D.C. IMPACT program.

All of these potential effects lead to the first empirically testable prediction: Prediction 1: *In the year of the restructuring, fewer teachers will be retained in their same position than in other periods.*

The next step in the conceptual framework is to extend the analysis to allow for experience heterogeneity. There are two effects to consider: 1) The effect of Bayesian learning on principals' decisions and 2) The effect of a change in present discounted value (PDV) of future transaction costs once a teacher gains tenure status. As each year of experience for a teacher goes by, the teacher's principal gains new information from the teacher's signals and the teacher gets a year closer to tenure. Rothstein (2015, p. 124) does an excellent job of explaining this tradeoff when he says,

"The option value of retaining an inexperienced teacher with low posterior mean but high variance is higher than for an experienced teacher with the same posterior mean (so better average performance to date) but low variance—the inexperienced teacher may turn out to be fine, and can always be fired next year if she doesn't."

The effects of Bayesian learning of principals lead to the principal's beliefs about the teacher's quality to start off with a wide range and slowly narrow towards the teacher's true quality. With this in mind, principals would prefer to retain a first-year teacher over a fourth-year teacher with the same signaled quality because the first-year teacher's signal is noisier. Thus it is more likely that the first-year teacher turns out to be "fine."

Tenure status dramatically increases the transaction costs associated with firing to the point where tenured teachers cannot be fired without a justified cause. Since a first-year teacher still has three more years before tenure and the transaction costs increase, the PDV of future transaction costs for the first-year teacher are lower than those of the fourth-year teacher. This gives the principal more incentive to retain a first-year teacher over a fourth-year teacher that signaled the same quality and all else is equal.

Empirical studies show that first-year teachers, particularly those with low-performing students, leave the profession voluntarily at a higher rate than more experienced teachers (Boyd et al., 2011). The probability of leaving the teaching profession declines as a teacher gains more experience. In any given year, there are more teachers that voluntarily leave than there are teachers that are fired. Thus, the standard pattern for teacher retention is one that is low for first-year teachers and increasing with experience. However, in the year of the layoffs, this dynamic shifts such that the principal's decision is more important than in previous years. Thus, in the year of the layoffs, we should see a reversal of the usual retention patterns.

All of the above effects lead to the second empirically testable prediction: Prediction 2: *In the year of the layoffs, there is an inverse relationship between an untenured teacher's experience and probability of returning to his or her same position.*

Principals use signals of teacher performance as measures of teacher quality. In particular, teachers that are more effective at increasing student test scores signal that they are teachers of higher quality. So, in the year of the layoffs, if a principal believes that teacher quality is signaled through students' test performance, there should be a positive correlation between probability the teacher is retained and the teacher's effect on student test performance. This leads to the third testable prediction:

Prediction 3: In the year of the layoffs, there is a positive relationship between probability of retention and teacher performance as measured by value-added score.

Several different effects of the policy on student performance could be supported. First, if principals retain teachers based on student test scores, then it is possible that student test scores will increase in the interim. Thus, the policy could achieve its goal, high-quality teachers are retained, and lower-quality teachers are not retained within the district and student test scores increase. On the other hand, the policy could be considered a large shock to the school staff and the resulting turmoil (in addition to an outflow of disgruntled good teachers) could lower student test scores. The large influx of teachers that are new to teaching – or teaching an unfamiliar subject – could have a short-run negative impact on students (Clotfelter et al., 2007). So, in the first year or two following the layoffs, there would be a decline in student test scores.⁶. Additionally, there could be student selection away from the public schools into private schools if the students and their parents do not approve of the way the restructuring was conducted. Furthermore, if principals do not sort teachers by their impact on student performance, there may not be any significant change in the student test scores. In this case the restructuring layoffs would just shuffle the teachers without making any improvements. If good teachers take the restructuring layoffs as a signal that the district does not value them appropriately, they could leave for other public districts (or private schools). Lastly, RPS may have a difficult time attracting good teachers, who may be wary of the potential of such restructuring being implemented again. This downward shift in the teacher supply for the RPS would lead to lower student test scores. Such a supply-side constraint is outlined in Rothstein (2015).

⁶ Appendix Table 1.A1 supports this hypothesis by showing a decline in teacher value-added scores in the years following the restructuring

1.4 Data

The data for this study come from two sources, the Illinois State Board of Education (ISBE) and the Rockford Public School District (RPS). Through the ISBE, I collected demographic data (including gender, race, salary, and subject taught) on all State of Illinois teachers and principals over six fiscal years, 2007 through 2012. Each fiscal year runs from July to June (e.g., the 2007 fiscal year runs from July 2006 through June 2007). So, the data range from July 2006 through June 2012.

The ISBE data identifies the years of experience for each teacher, which is split into experience in district, in state, and out of state, but does not identify teacher tenure status. To impute tenure status, I create a dummy variable for whether a teacher is untenured based on the in-district experience level.⁷ This variable generation is straightforward since, in RPS, teachers are only eligible for tenure after four years of experience. Any teacher with four years or fewer of experience is assigned a value of one in the new variable; everyone else is assigned a value of zero. This method does leave open the possibility of some teachers with fewer than four years of in-district experience to be misclassified as untenured when they are not. For example, a teacher with military experience before starting as a teacher at RPS may receive tenure after 2 years, but my dummy variable would mark them as untenured in years 3 and 4. However, the misclassification is likely a rare occurrence and any such cases would only lead to underreported effects because my comparison group, young tenured teachers, would be more similar to my treated group, untenured teachers.⁸

The data provided from RPS and ISBE do not include any variables that track whether a teacher is in the same school from one year to the next, which is my primary dependent variable.

⁷ As a robustness check, I also calculate the tenure dummy variable using in-state and total experience. The results are qualitatively the same.

⁸ I define young tenured teachers as teachers with five to ten years of in-district experience.

I generate an indicator variable that equals one if a teacher is observed in the same school from one year to the next.

In order to compare teacher retention rates to teacher performance, I use the teachers' value-added measurements (VAM). The RPS does not compute VAM for their teachers but instead rely on principal evaluations and in-class observation. However, from the RPS, I obtained student test score data linked to specific teachers, which I use to calculate the teachers' VAMs. In my calculation of VAM, I use the dynamic ordinary least squares (DOLS) estimator (DOLS is also sometimes referred to as OLS-lag) (Guarino et al., 2014). To calculate DOLS, the following equation is used: ⁹

(1.1)
$$A_{ig} = \tau_g + \lambda_1 A_{i,g-1} + \lambda_1 A_{i,g-2} + \boldsymbol{E}_{ig} \beta + \boldsymbol{X}_{ig} \gamma + \boldsymbol{e}_{ig}$$

In this equation, A_{ig} is student *i*'s test score in grade g, $A_{i,g-1}$ is student *i*'s test score in grade g - 1 (i.e., the one-year lag test score), $A_{i,g-2}$ is student *i*'s test score in grade g - 2 (i.e., the two-year lag test score), X_{ig} is a vector of student observable characteristics, c_i is student-level fixed effects, and e_{ig} is the idiosyncratic error term. The teacher VAM comes from the $E_{ig}\beta$ term. In particular, E_{ig} is a vector of teacher dummy variables that take on the value of one if student *i* had the teacher in grade *g*. When this equation is estimated using ordinary least squares (OLS), the resulting coefficient estimates, $\hat{\beta}$, are the VAM estimates. Thus, the VAM is a measure of the teacher's contribution to the change in a student's test performance after other factors have been taken into account.

One concern when using VAM is that the measurements may not accurately reflect the teacher's true quality (see Rothstein, 2009 and 2010). For example, teacher VAM may be biased by student selection (e.g., a teacher may have a higher VAM if they are given a group of students

⁹ An alternative version that drops the constant term was also used. The results were qualitatively and quantitatively similar and only the results with the constant included are reported.

that underperformed on their previous exam). However, Koedel and Betts (2011) show that with sophisticated models, bias from student selection becomes negligable. DOLS has the benefit that, regardless of student assignment mechanism used by the district, it has been shown to be more accurate than or as accurate as other VAM models in simulations (Guarino et al., 2014). Chetty et al. (2014) show evidence that DOLS estimators are unbiased. However, Rothstein (2014) refutes the evidence presented by Chetty et al. (2014). I proceed by using the calculated DOLS VAM scores, but caution the reader that the calculated VAM scores used in my analysis may be biased.

1.5 Empirical Strategy

The goal of this research is to analyze if the restructuring affected teacher retention rates, if teacher retention matched sorting on teacher performance, and how the increase in principal autonomy affected student test scores. To identify these effects, I utilize a difference-in-difference (triple-difference) identification strategy from the natural experiment of the district-level policy of restructuring. Since the restructuring only affected untenured teachers in the Rockford Public School District (RPS), they are my treatment group. Since tenured teachers were not laid off in the restructuring, they are the comparison group. I limit the comparison group to tenured teachers with 10 or fewer years of in-district experience because they are more similar to untenured teachers in movement patterns.¹⁰ I describe the group of tenured teachers with 10 or fewer years of experience as young tenured teachers going forward. The first difference is between the comparison and treatment groups. The second difference is the within-group change over time. The final difference compares RPS to all other districts with Illinois. The inclusion of this final difference controls for any state-wide

¹⁰ I also relax this restriction to include all tenured teachers as a robustness check.

contemporaneous effect on teacher retention rates such as an effect from the Great Recession and/or the American Recovery and Reinvestment Act of 2009. In my analyses that use student performance measures, my data limits the identification strategy to a difference-in-difference because only RPS student performance data was collected.

Figure 1.2 and Table 1.1 show the trends for young tenured and untenured teachers being in the same school from one year to the next for the RPS and all other Illinois school districts.¹¹ In the year of the restructuring, fiscal year 2010, there is a noticeable decline in the retention of untenured teachers in both the quantity and the percent terms in the RPS as measured by whether a teacher is observed in the same school in the following year. Note, though, that the data do not distinguish between teachers who leave voluntarily and those who were forced out. Since my main concern is on the reduced form effect the policy had on total retention, regardless of which mechanism is driving the impact, the analysis is still valid. Young tenured teachers in RPS also saw a single-year decline in the retention percentage in the year of the restructuring, but young tenured teacher retention percentage was not at its lowest in 2010 and the quantity of retained teachers was actually at its highest. In all of the other Illinois school districts, there was a similar, small decline in tenured teacher retention in the 2010 school year, but the decline in untenured retention rates is not as stark as the decline seen in the RPS. This evidence suggests that Prediction 1 from the conceptual framework section holds and principals retained fewer untenured teachers in the year of the layoffs than they otherwise would have.

I estimate the following equation to analyze the principal's decision of which teachers to rehire:

¹¹ In Figures 1.A1 and 1.A2 in the appendix, I show the trends in teachers leaving the district and teachers switching schools within the district.

(1.3)
$$Y_{it} = \beta_0 + \beta_1 Untenured_{it} + \beta_2 Untenured_{it} * 2010 + \beta_3 RPS_{it} + \beta_4 RPS_{it} * 2010 + \beta_5 RPS_{it} * Untenured_{it} + \beta_6 RPS_{it} * Untenured_{it} * 2010 + \delta_t + e_{it}$$

 Y_{it} is a dummy variable that equals one if teacher *i* was retained in year *t*. An additional specification replaces the retention variable with a dummy variable for whether teacher *i* leaves the district. *Untenured*_{it} is a dummy variable that takes on the value of one if teacher *i* was untenured in year *t*. 2010 is a dummy variable that takes the value of 1 in the year of the restructuring. *RPS*_{it} is a dummy variable that takes the value of one if teacher *i* was in the RPS in year *t*. δ_t is time fixed effects at the year level. I also utilize a model that includes district fixed effects to control for district characteristics that could affect teacher retention, such as average student quality. The coefficient of interest in this equation is β_6 , which will give an empirical test of Prediction 1. Furthermore, I estimate a version of this equation that breaks the *Untenured*_{it} dummy variable into separate dummies for each year of experience (i.e., a dummy for teachers in their first year with the district, and so on). Using the dummies for separate years of experience allows for an empirical test of Prediction 2.

My initial estimation is a linear probability model (LPM), which uses ordinary least squares (OLS) on the above equation. However, since my dependent variable is binary, there may be concern that the LPM estimates could yield fitted values that fall outside of the zero-one boundary. To account for this, I also use logit estimation. Additionally, I use a multinomial logit model to see the effect on the three possible teacher outcomes: retained in same school, switched schools within district, and left district.

As a robustness check to the main triple-difference identification strategy, I use a synthetic control matching method developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010). In this identification strategy, a synthetic RPS is generated to closely match the actual

RPS in the periods leading up to the layoff announcement. Then, in the year of the layoffs, comparisons can be made between the outcomes of the teachers in the actual RPS and the synthetic RPS, where the synthetic RPS is used as the counterfactual.

To calculate the synthetic control unit, I create a vector of weights for each district with 50 or more teachers in Illinois that minimizes the distance between the teacher observables for the synthetic control unit and those of the actual RPS. Put into mathematical terms:

(1.2)
$$\sum_{j=1}^{N_D} w_j^* \boldsymbol{X}_j = \boldsymbol{X}_i$$

where N_D is the total number of school districts with more than 50 teachers (excluding RPS), w_j^* are the optimal weights placed on the non-RPS districts, *i* represents RPS, and *X* are the district observables used in the matching process. The observables I use in the matching process include average teacher and student demographics for the district (e.g., log of salary, tenure status, experience, gender, race, socioeconomic status, and district enrollment) as well as lagged district-level retention variables. After calculating the correct w_j^* , I assign the w_j^* weights to the teachers' districts and then perform a weighted least squares estimation at the individual teacher level where the teachers are weighted by their district-level optimal weights. One major benefit of using this technique is that it eliminates any districts that are dissimilar to RPS. Thus, the comparison group is improved from the triple difference that includes all districts.

Since my data set has student test performance data on only RPS students, I cannot use a triple-difference identification strategy to analyze how the layoffs in RPS affected student test performance. Instead, the source of variation that I use to identify student performance is the percent of untenured teachers within a school. If a school had a larger percent of untenured teachers in the year of the layoffs, their teachers, and subsequently students, would be more affected by the policy.

To estimate the impacts of the district's policy on student test scores I use the following baseline equation:

$$(1.4) \qquad A_{ijst} = \beta_0 + \beta_1 \% Untenured_{s,2010} + \beta_2 \% Untenured_{s,2010} * 2010 + \beta_3 \% Untenured_{s,2010} * 2011 + \beta_4 \% Untenured_{s,2010} * 2012 + X_{jt}\gamma + A_{i,t-1} + \eta_s + \delta_t + e_{jt}$$

 A_{ijst} is student *i*'s standardized test performance in year *t*. The student test scale scores are standardized using the state average and variance for the student's grade. $\%Untenured_{s,2010}$ is the percentage of teachers in school *s* that were untenured in 2010. 2010 is a dummy variable that takes the value of 1 in the year 2010 (with similar variables for 2011 and 2012). The inclusion of multiple years allows me to track both the short- and long-run effects from the policy. X_{jt} is a vector of teacher characteristics. η_s and δ_t are school and year fixed effects, respectively. The coefficients of interest in this equation are the β_3 and β_4 terms. I additionally estimate a version of this equation that replaces the lagged student test performance, $A_{i,t-1}$, with student fixed effects, c_i .

There are a few reasons for choosing to analyze individual student performance by using variation in percent of untenured teachers within a school in the year of the layoffs. The obvious alternative regression would compare the performance of students of retained teachers to those of teachers that replaced non-retained teachers. However, this specification would be subject to endogeneity. Principals may remove teachers based on student performance. This would likely include teachers that have large numbers of harder-to-teach students. If the principals remove teachers in these more difficult classrooms, then the replacement teachers would be assigned to more-difficult-than-average classrooms. Thus, if we directly compare student performance of retained teachers to that of replacement teachers, the replacement teachers' performance may be lower simply due to being assigned harder-to-teach students. So, it would be unclear if the effect

is being driven by endogenous selection or by the true effect of replacing the teacher. A second reason for using the school-level analysis is the potential for spillover effects of the layoffs. In particular, if a tenured teacher works closely with untenured teachers, their dismissal could have an effect on the tenured teacher's performance. Lastly, since the policy gave the principals more control over their teaching staff, it is inherently a school-level policy. So, any analysis at the teacher level could miss the greater effect of the policy.

The empirical strategy in model (1.4) will only work if there is significant variation in the percent of untenured teachers across schools. In RPS in the year of the layoffs, 2010, an average of 27.06 percent of school staffs were untenured with a standard deviation of 21.02. Additionally, the minimum and maximum percent of untenured teachers in a school in RPS in 2010 were zero and 100, respectively. Thus, there is considerable variation in percent untenured in each school.

With model (1.4), one potential concern is that the percent untenured in 2010 only captures the intent to treat of the policy instead of the actual treatment intensity. The percent of untenured teachers in a school in 2010 tells how affected the school was by the layoffs, but does not say how much a school's principal chose to utilize the layoffs to restaff their school. For example, two schools could each have 50 percent of their teachers untenured, but one principal chose to retain all the untenured teachers while the other principal chose to retain none. However, simply using percent of untenured teachers leaving a school in 2010 as a regressor may be endogenous. Specifically, if a school has lower student test performance, the principal has more incentive to change the school's staff more drastically. Thus, any effect found from a regression that uses untenured teacher turnover as an independent variable could be due to lower performing schools more actively using the policy. In order to control for this endogeneity an

instrumental variable (IV) framework is used. In the proposed IV framework, the percent of untenured teachers in the year of the layoffs would be used as an instrument for the percent of teachers leaving a school in the layoffs. Since the untenured teachers were affected by the layoffs, the schools with higher percentage of untenured teachers had more opportunity to change their untenured teaching staff. Thus, there is a direct link between the percent untenured in a school and the percent of teachers leaving the school. Additionally, as already discussed, the percent untenured within a school in the year of the layoffs does not suffer from endogeneity of student test performance. Thus, the percent of untenured teachers in a school in the year of the layoffs fits the requirements for a valid instrument.

1.6 Results

1.6.1 Change in Retention Rates

To better understand the effects of the policy and the triple-difference identification strategy, Table 1.2 shows the mean retention rates for each group over time. Focusing on teachers within the Rockford Public School District (RPS), there is a large drop in retention rates for untenured teachers in the year of the layoffs. Additionally, there is a slight increase in retention rates for young tenured teachers in the year of the layoffs. This leads to a difference-indifference effect in RPS that suggests that untenured teachers were 10.28 percentage points less likely to be retained in the year of the layoffs. It is possible that a portion or all of this effect is due to a state-wide trend in teacher retention rates. With this in mind, I next show a differencein-difference effect for all non-RPS districts in Illinois. The results from this difference validate the concerns of a state-wide effect. In particular, there is a decline in retention rates of untenured teachers in all non-RPS districts in Illinois. Combining these results into a triple difference

shows a smaller decline of 5.9 percentage points in retention of untenured teachers in RPS. Even with this triple-difference result, there could be concerns that the results are being driven by the largest Illinois district, Chicago. The next difference shows that the results do not change when Chicago is removed from the sample. Lastly, one could be concerned that the triple difference with all the Illinois districts includes many districts that are not similar to RPS. Thus, there could be bias in the triple-difference results. To test this, I also include a synthetic control of RPS. The synthetic control uses an optimal weighting method to create a control district that best matches RPS on numerous dimensions. While the group differences of the synthetic control are vastly different from those that use all Illinois districts, the final triple difference coefficient is pretty similar to the other triple-difference results.¹² With this in mind, I proceed using the triple difference with all districts as my main results and use the other identification strategies as robustness checks.

Table 1.3 shows the results of the empirical tests of conceptual Predictions 1 and 2. In columns (1), (3), and (5) the coefficient on the interaction term between the untenured teacher dummy and the year of the restructuring is negative, but only statistically significant at the 10 percent level. This evidence upholds Prediction 1. The coefficients are quite similar across models and imply that untenured teachers in the Rockford Public School District (RPS) are between 4 and 6 percentage points less likely to return to their same school in the year of the layoffs, relative to the comparison groups.

Columns (2), (4), and (6) of Table 1.3 uphold Prediction 2. In the year of the restructuring, first-year teachers are not retained at a statistically differential rate than in other years. However, the more experienced untenured teachers are rehired at a lower rate in the year of the restructuring. In particular, third-year teachers in RPS were 16 to 19 percentage points less

¹² The composition of weights used in the synthetic control is shown in Appendix Table 1.A6.

likely to be retained in the year of the layoffs relative to the comparison groups. Fourth-year teachers were between 11 and 12 percentage points less likely to be retained in the year of the layoffs. Note that the inclusion of district fixed effects has almost no effect on the results when compared to the OLS without district fixed effects. As such, I proceed using only the OLS without district fixed effects and logit estimation techniques.¹³

Continuing the analysis of teacher outcomes, Table 1.4 shows the multinomial logit estimation of the effect of the layoffs on probability of an untenured teacher being retained at the same school, leaving the district, and switching schools. The results for teacher retention are similar to those found in Table 1.3 with a 4-percentage-point decline in probability of retention. One can interpret the multinomial logit results as the decline in retention due to the policy is split approximately 40 percent to teachers leaving the district and 60 percent to teachers switching schools within the district. This means the layoffs led to some reshuffling of teachers, but also a group of teachers leaving the district.¹⁴

1.6.2 Characteristics of Teachers Retained

In the conceptual framework, I developed a prediction (Prediction 3) that suggests that teacher retention rates would match with value-added measurements (VAM) sorting. Table 1.5 Panel A shows a negative effect of untenured teacher VAM in the year of the restructuring on the probability of an untenured teacher being retained, but only the single-lagged VAMs are

¹³ There may also be questions about whether certain teachers are more affect by the policy. In Table 1.A3, I find that teacher demographics such as gender, race, and salary do not have a statistically significant impact on teacher retention rates.

¹⁴ In Appendix Table 1.A4, I split the effect by years of experience. This table shows the effect is again split between teachers leaving the district and teachers switching schools. This is especially true for the third- and fourth-year teachers.

statistically different from zero.¹⁵ The interpretation of the single-lagged VAM OLS results suggest a one standard deviation increase in an untenured teacher's math VAM score is associated with a statistically significant 20 percentage point decline in probability of retention. However, the less biased (but also less precise) twice-lagged VAMs show smaller negative effects that are not statistically different from zero.¹⁶ Table 1.5 Panel B shows that teachers with higher VAM scores have a higher probability of leaving the district in the year of the layoffs.¹⁷

There are a couple of hypotheses over why I find that higher-value-added teachers are retained at a lower rate and leave the district at a higher rate. The first hypothesis is that principals are choosing teachers in a manner that opposes their VAM sorting. However, in the existing literature, Jacob and Lefgren (2008) have shown that principals can accurately identify the highest-value-added teachers. The second hypothesis for this negative relationship is that high-value-added teachers may self-select out of the district in response to the layoffs. Adnot et al. (2016) find evidence that when Washington, DC implemented a policy that removed teacher job security, many higher-value-added teachers voluntarily left the district. Additionally, there could be higher-value-added teachers voluntarily switching to more desirable schools which is supported by the research of Bates (2015). So, based on the existing literature, it seems more plausible that the higher-value-added teachers in RPS switched schools and left voluntarily in response to the policy, although I cannot confirm that this is necessarily the case.

¹⁵ These results may be subject to bias due to using only RPS in a difference-in-difference identification strategy. More discussion of the potential bias and its impact on these results are discussed in 1.6.5.

¹⁶ An additional analysis could look at how the VAM affects retention probability when untenured teachers are separated by years of experience, but this analysis is not feasible with my dataset due to small sample size and imprecision.

¹⁷ One concern about this VAM analysis is that the results could be driven by VAM scores calculated by teachers in non-tested subjects. To address this, I also performed a version of the analysis that limits the regressions to only reading and math teachers. The results suffer from small sample size, but generally support the main results.
1.6.3 Impact of Layoffs on Student Test Scores

The final main area of analysis is whether laying off all untenured teachers and allowing principals autonomy over rehiring decisions actually increased student test scores.¹⁸ However, there are two main sources of endogeneity to consider before looking at student test score results: first, principals may have selectively removed the lower-quality teachers – although this was not supported with the earlier empirical results – and second, student tracking. If principals actively select teachers based on quality, removing the lowest quality teachers, then any analysis of student performance that uses individual teachers' retention decisions will show higher student performance. Similarly, if students are put into different tracks based on their ability, teachers may leave if they teach a lower track of students (Feng, 2010). Thus, teacher-level analysis of student performance would estimate both the effect of the policy as well as the effect of the selection of teachers or student track. To control for this endogeneity, I focus on results that use school-level variation to identify individual student performance changes related to the policy. With this strategy, I can also account for the schools that used the restructuring most aggressively. Additionally, the student tracking endogeneity would be accounted for as long as the tracking remains consistent for each school over time. I first calculate the percent of teachers within a school that were untenured in the year of the layoffs. I then look at how percentage of teachers subject to the layoffs affects school test performance.

Table 1.6 shows evidence that for schools with more teachers affected by the restructuring, student math performance declined in the years following the restructuring.¹⁹ In

¹⁸ An additional question could be on how principals filled the open positions of the non-retained teachers. Appendix Table 1.A5 shows the summary statistics for the teachers that left the Rockford Public School District (RPS) in 2010 and compares it to all teachers in new schools in 2011 and all teachers new to RPS in 2011. Across most dimensions, there is no difference in the means. However, looking at the reading and math VAM averages, the new teachers tend to be lower value-added teachers than those that left RPS in 2010.

¹⁹ Table 1.12 offers a falsification test of these results by using 2008 as a placebo treatment year. These results are discussed in more complete detail later in this section.

column [3] of the table, the coefficient for two years after the layoffs can be interpreted as a 10 percentage point increase in untenured teachers in the year of the layoffs is expected to lower student performance in math at the school by 0.036 standard deviations two years after the layoffs. This suggests that the negative effect from destruction of human capital, the influx of new teachers, and/or teacher sorting that opposes VAM sorting drives the effect of the policy on student performance.²⁰

Another important aspect is how students of schools that used the restructuring more aggressively performed – or the effect on the treated. Note that there is endogeneity in this analysis. Specifically, lower-performing schools are more likely to use the policy to restaff their schools. While the results show how schools with higher turnover in the year of the layoffs performed in the following years, this could be simply due to selection of which schools chose to use the policy and not the effect of laying off a teacher. One method that can be used to control for this endogeneity is Two-Stage Least Squares (2SLS). In my 2SLS approach, I use the percent of untenured teachers interacted with year dummies within a school as an instrument for the percent of teachers leaving a school for each year. Table 1.7 shows that the 2SLS strategy estimates are similarly signed to the results in Table 1.6, but are larger in magnitude. In the 2SLS regression, a 10 percent increase in teachers leaving a school in the year of the layoffs is associated with a 0.0527 to 0.131 standard deviations decline on the standardized math exam two years after the policy. These larger effect sizes suggest that the schools that used the layoffs more aggressively performed worse in math test performance two years after the layoffs than the effects from all schools' untenured teacher population regardless of use of the policy. Additionally, there is now a negative impact to reading student test performance a year after the

²⁰ I also analyzed whether these effects are driven solely by percent of untenured teachers within tested grades (3-8) and found similarly signed coefficients, but they are less precisely estimated and not statistically different from zero.

layoffs in the model that uses a lagged student test score, but no effect is found when student fixed effects replace the lagged student test score. Also, there is now a positive and statistically significant effect to math test performance one year after the layoffs when lagged math test scores are included. This implies that the schools that used the layoff policy more aggressively may have had some short-run success at improving math test performance, but the effect is negated by declining student math performance two years after the policy.

The negative impact on student performance of the policy highlights the potential downfalls of RPS implementing the layoffs policy. Student math test performance in schools that were more affected by the policy declined in two years following the layoffs and there is some evidence of a short-run negative effect on reading test performance the year after the layoffs. However, there is an increase in math test performance the year immediately following the layoffs of schools that replaced more teachers. While simulations show that layoffs of teachers can improve student performance when lower value-added teachers are removed (Hanushek, 2009; Goldhaber, 2011) and some analysis of policies that give principals more autonomy have suggested positive effects on student performance (Jacob, 2011), these results highlight the potential danger of enacting a policy that gives principals autonomy without a criteria over which teachers should be retained or the potential for good teachers to leave voluntarily when such a policy is enacted.

1.6.4 Synthetic Control Model Robustness Check

One could argue that using all the districts in Illinois for my third difference would lead to inaccurate results because the comparison group imperfectly mirrors the Rockford Public School District (RPS) in the periods leading up to the layoffs. Figure 1.2 supports this argument

by showing that in the year before the layoffs, there was a larger increase in young-tenured retention rates in RPS than all other Illinois districts for the first time in my data set. Thus, the results could be biased due to an incorrect comparison group. One method to correct for this is to use a synthetic control district for the third difference. I use the synthetic control results as a robustness check for the main results.

Figure 1.3 shows the comparison between the actual RPS and the synthetic control RPS in respect to the number of untenured teachers retained in the same school each year. In the periods before the layoffs, 2007 through 2009, the actual and synthetic RPS are quite similar. However, there is a deviation in the year of the layoffs, 2010. In particular, while the synthetic RPS remains at the same retention level in 2010, the actual RPS's retention rate severely declined. This decline highlights the impact of the policy on untenured teacher retention rates. Appendix Table 1.A6 lists the composition of districts that make up the synthetic RPS.

Table 1.8 shows the comparison of results from using the triple difference with all districts (my main results) and the triple difference that uses weights based on the synthetic control approach. When the synthetic control approach is used, the effect size is in the same direction and qualitatively the same, going from a 5.75 percentage point decline in probability of retention to an 8.38 percentage point decline. This suggests that the main results may underestimate the impacts of the policy.

1.6.5 Additional Placebo and Robustness Tests

In order for a triple-difference identification strategy to be valid, there are a few assumptions that need to hold. First, there is an assumption that there are no parallel trends. Any parallel trend would cause the triple-difference estimator to be biased. For example, the city of

Rockford was affected by the Great Recession more than other Illinois cities (Lucci, 2014). In my analysis I offer a couple of checks for parallel trends. One of these methods is to look at a placebo treatment year. I use both 2009 and 2008 as placebo treatment years and find no statistically significant effect. Another approach is to use an event study approach. In this approach, the researcher tracks the effect of each period before, during and after the policy. I utilize this method as a robustness check and find no statistically significant effects in the years leading up to the policy, but in the year after the layoffs, there is a larger effect than in the year of the layoffs. This suggests that even after the policy was completed, there were still some lasting effects on the teacher population. This is discussed more in the results section.

A second assumption with the triple-difference strategy is that there are no contemporaneous changes that affected RPS untenured teachers in 2010. For example, RPS hired a new district superintendent for the 2010 school year. Thus, there is the possibility that any effects found are actually due to having a new superintendent and not due to the layoffs. To address this concern, I analyze the impact a first-year superintendent has on untenured teacher retention in other districts in order to eliminate that as the driver of the main results. Appendix Table 1.A2 shows that having a new superintendent is associated with a lower retention rates, but the effect sizes are about one-fifth to one-sixth the size of my main results. However, this does not rule out the possibility that a new low-quality superintendent could have negative impacts beyond the average new superintendent effect. So, I cannot rule out that the effects I find are solely from the layoff policy and not from a combined effect from the new superintendent as well as the superintendent's policies (including the layoffs).

Another contemporaneous shift that threatens estimation of the causal effect of principal autonomy is the effect of the Great Recession on the non-treated school districts. There is

evidence that in the years following the Great Recession, school district revenues and expenditures fell (Chakrabarti and Sutherland, 2012). This could lead to a decline in teacher retention in the comparison group in 2010, although Chakrabarti and Sutherland (2012) show evidence that the reduction in expenditures affected instruction expenses less than other expenses. However, this effect is not a concern because when I use a difference-in-difference strategy that uses only the RPS, the results are not different from my main result. This suggests that the Great Recession is not driving my main results through a differential change in retention rates across districts.

Another assumption needed for a valid triple-difference strategy is that there are no spillover effects to the comparison groups. In the RPS triple difference, there are two potential spillover effects. First, young tenured teachers may be close to some untenured teachers and would be distraught seeing their friends laid off. In this case, in the year of the restructuring, young tenured teachers may leave the district at a higher rate. A second, more likely, potential source for spillover would be opportunistic transfers. During the restructuring, there are more jobs open than normal. Additionally, tenure is given preference for filling job positions (Rockford Collective Bargaining Agreement, 2011). Thus, there may be more tenured teachers voluntarily transferring schools during the restructuring. If this effect occurs, it is likely that the young tenured teachers. To control for this possibility, I do a robustness check that drops all untenured teachers and compares young tenured teachers to older tenured teachers. This robustness check is to find whether these results are being driven by changes in retention rates of young tenured teachers (the comparison group) instead of the untenured (treated) group. This

robustness check leads to results similar to my main results. Thus, there is no evidence that my results are driven by spillovers.

The first robustness check analyzes whether the results change when the identification strategy changes. In particular, with the triple-difference strategy there may be two concerns with the comparison group. First, there is potential for the non-RPS districts in Illinois to have enacted somewhat similar policies. This would lead to underreported effects. A second concern is that the young tenured teachers' retention rate trends could also be affected in the 2010. To address the first concern, I redo the analysis by dropping all districts besides the RPS. Then to address the second concern, I bring back all the Illinois districts, but drop all tenured teachers from the analysis. Thus, I have two difference-in-difference (diff-in-diff) identification strategies to use as robustness checks. Table 1.9 shows that when the diff-in-diff identification strategies are implemented, the results are consistent. The coefficient for the RPS-only diff-in-diff is larger and now statistically significant at the 1 percent level. This larger coefficient suggests that when using the RPS-only diff-in-diff identification strategy, there may be a downward bias. This is important because the analyses that utilize teacher value-added measurements (VAM) and student test performance rely on the RPS-only diff-in-diff identification strategy. This means that those results may also suffer from a downward bias. In those results I found negative correlations between VAM and teacher retention as well as between percent untenured and student math test performance. So, these estimates could be thought of as a lower bound of the true effect. However, if we believe that the synthetic control matching results are a better measurement of the true parameter, the RPS-only diff-in-diff coefficient is very similar which suggests there would be no bias. The coefficient for the diff-in-diff that uses only untenured teachers is similar

to the triple-difference results in magnitude, but is a bit smaller than the synthetic control matching estimates.

An additional placebo test looks at the effect of the policy in the year before the policy took place. The traditional placebo test would drop the treatment year, 2010, and use the preceding year as the treatment period. However, in 2009, there was a statewide increase in teacher retention (see Figure 1.2). The increase in retention is more apparent in the young tenured teachers than the untenured teachers. One could speculate that the increase in retention in 2009 is because of the Great Recession. During the Great Recession, there were fewer job openings than in other years. Thus, teachers may have chosen to stay at their school because of a lack of outside option. So, in addition to using 2009 as a placebo, I also use the same approach with 2008 as the placebo. Table 1.10 shows the results of this placebo test. In this placebo test, all of the coefficients for untenured teachers in 2008 are not statistically different from zero. This suggests that my main results are not being driven by an existing pre-trend.

Another method to check the parallel-trend assumption is to use an event study. Table 1.11 shows the results from the event study. In particular, there are no statistically significant coefficients in the years prior to 2010. This suggests that there is no pre-trend effect. However, in the year following the layoffs, 2011, there is a large negative effect on probability of retention. This suggests that there is either an on-going effect from the layoffs or there is some other impact to teacher retention rates in 2011.

The next test analyzes the effect of a placebo layoff on student test scores. In a similar method to the previous placebo test, I drop all the years after and including the year of the layoffs. I then calculate the percent of untenured teachers and percent of untenured teachers that were not retained in 2008 instead of 2010. Table 1.12 shows how the percent of untenured

teachers in 2008 affects student performance in 2008 and 2009. The majority of coefficients are not statistically different from zero. However, there is a statistically significant negative impact to reading test scores the year following the layoffs. While this is a little disconcerting and could mean that there is an existing pre-trend for student reading performance, my main results show an impact only on math results.

The final test for validity looks at whether the policy impacted the characteristics of students at the schools. In particular, one should be concerned that after the policy was announced, there may be self selection of students leaving the public schools that were most affected by the policy. One example of this is if a parent is displeased with the layoffs, she may move her child to either a private school in or near the city of Rockford or to a neighboring public school district. The primary concern with this self-selection bias is if the highest-performing students self-select out of the schools most affected by the policy, then the main student performance results would be capturing the change in demographics instead of the effect of teacher turnover due to the layoffs. However, Table 1.13 shows that there is no statistically significant effect of percent of untenured teachers in the year of the layoffs on percent of students that are male, black, Hispanic, economically disadvantaged, or test performance in the years following the layoffs. This suggests that the main results are not being driven by self selection of students out of the schools most affected by the layoffs.

1.7 Conclusions

This research looks at a natural experiment caused by a unique local policy of restructuring a school district by laying off all untenured teachers. I address three questions important to the literature on education: When given more autonomy and reduced transaction

costs, is there a change in untenured teacher retention levels? Do teacher retention rates match with teacher performance measurements? And do student test scores increase due to the policy?

A conceptual framework based on Bayesian employer learning and teacher choice predicts that principals will retain fewer untenured teachers in the year of the restructuring. The conceptual framework is extended to show that the principals' retention rates will be inversely related to the untenured teacher's experience level. The empirical analysis supports both of these predictions. In regards to how principals decide which teachers to retain, I find some evidence that teacher retention is inversely related to their value-added score and teachers with higher VAM left the district at a higher rate in the year of the layoffs.

The final focus of this study analyzes the impact of the layoffs on student test scores. I find that students at the schools most affected by the policy tended to perform at lower levels in the years following the policy. A school with 10 percentage points fewer untenured teachers saw student test performance decline as much as 0.036 standard deviations in math. This lower performance holds even when I focus on schools that utilized the policy more (i.e., schools that retained fewer of their untenured teachers). When a two-stage least squares approach with student fixed effects is taken, I find that student math test performance two years after the layoffs declines as much as 0.131 standard deviations for a 10 percent increase in untenured teachers leaving the school in the year of the layoffs. So, while simulations have shown that teacher layoffs can improve student performance if the lowest-VAM teachers are removed (Hanushek, 2009, and Goldhaber, 2011) and other real-world studies have generally supported this (Jacob, 2011), these results suggest that student performance may actually be harmed by such policies if lower-VAM teachers are retained. The main point policy makers should take away from this is that gains in student performance from increases in principal autonomy through mass teacher

layoffs can depend on how the policy is implemented. My results show that a mass layoff of untenured teachers followed by giving principals autonomy over rehire decisions, without giving the principals a clear criteria for retaining teachers, leads to lower student performance. Chapter 2

Morale Hazard: The Impacts of a Layoff Announcement on Teacher Effort

2.1 Introduction

In recent years, there has been an increase in policies that negatively impact job security for teachers, such as removal of tenure, increasing the ease of firing a teacher, and reconstitution of school staffs, among other policies. While there is a debate over the efficacy of these policies, there has been limited research done on how teachers respond when their job security is threatened. Most of the research on this topic focuses on small changes in job security (Jacobs, 2013). This paper looks at how a publicly announced mass layoff of all untenured teachers affects untenured teacher effort levels. In March of 2010, the Rockford, Illinois, Public School District (RPS) announced a restructuring of the district.²¹ All untenured teachers within the district received layoff notice on March 23 – with principals given autonomy over whether a teacher would be retained in the teacher's pre-restructuring position. The principals' retention decisions occurred at the end of the school year. So, between March 23 and the end of the school year, the untenured teachers were working while uncertain about whether or not they would have a position going forward. The current research analyzes whether teachers adjusted their effort level during this period and which types of teachers changed their effort the most.

To answer these questions, I first develop a conceptual framework that leads to a couple of hypotheses. The first hypothesis is that teachers will increase effort to signal their quality in order to retain their current positions. An alternative hypothesis is that teachers reduce effort due to demoralization. In addition to demoralization, a reduction in teacher effort may come from principals tipping off teachers that they plan to retain which leads to lower effort because of an increase in job security over the normal level.

²¹ The local newspaper also reported that there had been rumors of potential layoffs in the report on the restructuring (Bayer, 2010). For this paper, I only focus on the effect in the time period after the actual layoff announcement. However, in Appendix Table 2.A1, I show that untenured teachers took fewer absences in 2010 school year before the layoff announcement occurred. This decrease in untenured teacher absences could be from untenured teachers increasing effort in response to the rumors, tenured teachers reducing effort in response to the rumors due to morale effects, or a spurious change in teacher efforts.

I base my analysis off of data collected from the RPS as well as the Illinois State Board of Education (ISBE). My data ranges from the 2006-2007 to 2009-2010 school year. The RPS provides teacher absences data, which is used as a proxy for teacher effort, while the ISBE provides information on demographic characteristics of teachers as well as experience. From the ISBE experience variable, I create a dummy variable for whether a teacher is untenured or not based on the teacher's years of experience within the district.

To test my hypotheses, I use a difference-in-difference identification strategy. In the first difference, I compare periods prior to the restructuring to the period between the layoff announcement and end of the school year. The second difference comes from comparing those directly affected by the policy, untenured teachers, to those that were not, tenured teachers with 10 years of experience or fewer.²² Additionally, as a robustness check, I adjust the comparison group to all tenured teachers and find similar results.

In my analysis, I find that the restructuring policy had no statistically significant effect on the average number of absences per untenured teacher in the period between the announced policy and the end of the school year. However, when I look at the results in smaller time spans, I find that there is a decline in untenured teacher absences in the periods immediately following the announcement. Specifically, I find that the absences in the month of April 2010 for untenured teachers decline and that this is driven by a decline in absences in the first week of April. In an attempt to determine if this result is driven by a specific group of untenured teachers, I test various sources of heterogeneity but only find marginal effects for science teachers. Additionally, I find opposing results that show lower value-added teachers take more absences.

²² My identification strategy assumes that tenured teachers are not affected by the policy at all. However, tenured teachers may be indirectly affected by the policy in that they were not in the group laid off, but they may be friends with those laid off or see the layoff as a bad working condition. This could lead to tenured teachers altering their effort due to indirect effects of the policy. However, if this is the case, it would lead to underestimating the effect of the policy on the untenured teachers.

In order to test the hypothesis that the decline in attendance is due to a principal tip-off, I look at whether a teacher's future retention had an effect on attendance or not. The test of the principal tip-off hypothesis has the expected sign, but is not statistically different from zero. Lastly, I perform several placebo and parallel trends tests and find that the results do not come from spurious differences in the groups.

2.2 Literature Review

The current research adds to the deep literature on how teachers respond to incentives. One relevant topic is incentive pay (which can be viewed as the carrot to the stick of teacher layoffs). For example, Imberman and Lovenheim (2015) find that when teachers are incentivized to improve exams, the incentivized exams show signs of improvement while non-incentivized exams do not. Additionally, Lavy (2009) finds evidence that when teachers in Israel are given financial incentives for student achievement, student test scores do increase. Similarly, Goodman and Turner (2010) find that when free-riding is controlled for, teachers respond to incentive pay programs by increasing effort (as measured by absenteeism).

My research focuses on the effects on teacher effort of a policy designed to increase teacher accountability through giving principals more control over personnel decisions. Looking at how accountability introduced in the No Child Left Behind Act (NCLB) affects effort, Finnigan and Gross (2007) show that when after NCLB went into effect, teachers applied more effort but also showed signs of demoralization. Similarly, Jacob (2005) shows that teachers respond strategically (e.g., teaching to the test) when faced with penalties for poor student performance. The Rockford Public School District (RPS) policy affects teachers with different experience levels differently. Specifically, the RPS layoffs target untenured teachers and do not affect the tenured teachers. The extant literature links teacher effort to teacher tenure within schools or districts. Hansen (2009) shows that teacher effort declines in tenure at a school as well as declines in total experience and that teacher effort levels respond significantly to incentives. Similar results are found when looking at teacher turnover via quits (Falch, 2011; Stinebrickner, 1998) and tenure decisions (Goldhaber and Hansen, 2010).

While the RPS policy is in a teacher labor market, the existing literature shows similar effects on effort have been found in other industries. Campbell and Kamlani (1997) surveyed 184 firms to determine the reason for wage rigidities. Among the strongest responses is how a decrease in wages would affect worker effort. In particular, the survey responses indicated that effort would decrease faster when wages declined than it would increase with an equivalent increase in wages. In the public school district, wages are set through the collective bargaining agreement with little room for change. However, one would expect a similar impact to worker effort when layoffs are announced.

One of the main reasons to study teacher effort is that it has been shown to impact student outcomes. Several studies show that when a teacher applies more effort on the job, student test score performance increases (Miller, et al., 2008a; Miller, et al., 2008b; Clotfelter, et al., 2009; Herrmann & Rockoff, 2012). In addition to the measurable student test performance gains, studies also show a link between teacher effort and student absences (Ehrenberg, et al., 1991).

An additional reason for studying teacher effort is that teachers can easily adjust effort in response to policies. For instance, Ballou and Podgursky (1995) show that the students assigned to a teacher affect the teacher's effort level. In particular, the authors show that when a teacher is

assigned a better group of students, the teacher responds by applying more effort. This implies that different student sorting policies can impact the effort levels of teachers. Other studies show that the school's general culture of teacher effort can influence individual teacher effort levels (Bradley, et al., 2007). This suggests that a school would be able to make gains in effort levels by changing the culture at the school.

In the following conceptual framework, I propose two hypotheses of how teachers will adjust their behavior given the layoff announcement. The first hypothesis says that teachers will use effort as a signal of quality to retain their jobs. Jacob (2013) finds evidence that supports this hypothesis in his study of how a change in employment protection policy – a collective bargaining agreement between the Chicago Public School district (CPS) and the Chicago Teachers Union that made it easier for principals to dismiss teachers – leads to a decrease in teacher absences (which are used as a proxy for effort). This suggests that the decrease in job security from the RPS restructuring would lead to fewer absences. One large difference between the current study and Jacob (2013) is the implementation of the policy. Jacob (2013) shows how giving principals control over untenured firings after the school year without a preceding layoff announcement affects teacher effort in subsequent years.

The second hypothesis says that teachers will reduce effort after the layoff announcement due to demoralization. This second area of analysis is based on the current literature that ties worker morale to productivity (see Frey, 1993, and Benabou & Tirole, 2003). In a more recent publication, Stowe (2009) develops a theoretical framework that supports a positive correlation between morale and effort. Applying worker morale theory to the restructuring policy suggests that when the teachers are given news of the restructuring and layoffs, their morale suffers. With a new lower morale, teachers will be less productive on the job. This lower productivity could

manifest as teachers applying less effort after the policy has been announced. Additionally, the decline in morale may disproportionately affect lower-quality teachers. Kennedy (1995) shows that when firms are affected by mass layoffs, lower-skilled workers' morale is more affected. Applying this to the RPS policy could suggest that teachers with lower value-added scores, which is a proxy for teacher quality, would have their morale more affected by the layoff announcements. To my knowledge, there is no existing literature that specifically studies teacher morale and effort, but there has been some work on the effects of morale-lowering policies on district's ability to attract new applicants (Delfgaauw & Dur, 2007).

2.3 Conceptual Framework

There are three hypothesized effects of the policy on teacher effort. First, in an attempt to improve their probability of being rehired, teachers may have increased their effort levels by taking fewer absences. The second potential effect is a decrease in teacher effort based on demoralization literature. Lastly, there is the potential for a principal tip-off effect where principals let teachers know before the end of the school year that they will be hired. This last effect would result in an increase in teacher effort right after the layoff announcement (similar to the signaling effect), but then later show a large decline in effort after the teacher is tipped off.

Applying the signaling models developed by Spence (1973), Greenwald (1986), and Gibbons and Katz (1991), among many others to the current situation shows why one could expect the increase in teacher effort. Consider a stylized model where there are two types of teachers, high- and low-quality, and effort is less costly for high-quality teachers. In this model, teachers would apply more effort until their marginal cost of another unit of effort equaled their expected probability of being rehired in the same school, conditional on their effort level,

multiplied by the utility they get from being retained. Thus, a separating equilibrium can be supported where the high-quality teachers apply more effort than the low-quality teachers. Based on the signal of teacher effort, principals can decide which teachers to retain based on their respective effort levels (i.e., principals prefer high-quality teachers and would prefer to retain teachers that exhibit high effort). However, the real situation is more complex in that there is a continuum of teacher quality and teacher effort is a noisy signal. This leads to a situation where principals make decisions using Bayesian updated beliefs of teacher quality based on the noisy signals.

In the period after the layoffs are announced, the probability of an untenured teacher being retained in the same school declines. With this change, teachers re-optimize until marginal cost equals expected marginal benefit as described in the previous paragraph. With increased effort, the teacher's probability of being retained increase. So, the teacher may attempt to counteract the drop in probability of retention from the policy by increasing effort levels. Thus, in the year of the layoffs, there would be an increase in effort. One concern is that if marginal cost is increasing in effort, then teachers would only apply more effort after the layoff period if the probability of retention increases at a faster rate than the marginal cost of effort.

The layoff announcement may have affected the cost of effort. Specifically, if an untenured teacher becomes demoralized due to the policy, then the marginal cost of effort increases. As the marginal cost of effort increases, the teacher will need to re-optimize in order to return to equilibrium where marginal cost equals expected marginal benefit. In order to reoptimize, the teacher would reduce effort levels. Hence the period after the layoff announcement would show lower teacher effort. Note that the signaling and demoralization effects can occur concurrently and the magnitude of each may affect teacher behavior heterogeneously.

One potentially confounding factor in the analysis of demoralization in this setting is that the principal's may have tipped off the teachers if they were planning on retaining them. In the RPS policy, at the time of the layoff announcement, it was publicly stated that principals would be in control of teacher retention decisions. As such, if a principal knew that she wanted to retain a certain teacher, she could informally tell the teacher that her job is secure. In essence, the principal would tip off the teacher to her retention decision before the end of the school year. The conceptual framework for the principal tip-off effect starts with the signaling framework established earlier in this section. In particular, the teacher still learns of the layoffs and there is a drop in the probability of retention, which leads to higher effort, or a demoralization, which lowers effort. Then at some point between the layoff announcement and the actual layoffs, the principal tips off the teachers she wants to retain. After the teacher receives the tip-off, the probability of rehire increases toward one, and equals one if the teacher fully believes the principal's tip-off. Note that this is a higher probability of retention than the teacher would have in a regular period, because the principal would not ordinarily explicitly tell the teacher whether she would be retained before the end of the year. So, if a principal tip-off effect results in an initial increase of a teacher's effort, then after the teacher is tipped off her effort subsequently drops. However, if a teacher does not believe the principal's tip-off, there would not be the subsequent dropoff in teacher effort towards the end of the year. Additionally, there will not be a principal tip-off effect if principals are consistently communicating with the untenured teacher their desire to retain said teacher. If this is true across years, not just in the year of the layoffs, then the pattern of effort in the year of the layoffs should reflect the trend in prior years.

Finally, when looking at teachers' effort in response to the layoff announcements, there is potential for heterogeneous responses. I consider four dimensions that may affect how a teacher

responds to the layoff announcement. First, schools with a culture of allowing for absences may lead to untenured teachers affected by the policy taking more absences. If this is the case, one would expect to see untenured teachers at schools with more average annual teacher absences take more absences after the layoffs are announced than they would have if they taught in a school with a culture of not taking absences. Next, teachers of different subjects may adjust his or her level of effort heterogeneously in response to the policy. If a teacher teaches a subject that is generally considered hard to fill – for instance high school math or science – there is less incentive for the teacher to apply more effort to retain his or her job because they have a higher likelihood of being retained in their same positions.²³ On the other hand, for teachers of easierto-fill subjects, there is more incentive for them to increase effort in order to retain their position. Next, the teacher's experience may factor into their response. Assuming principals work under a Bayesian updating of beliefs about individual teacher's quality, then teachers with less experience could use effort as a signal more effectively than teachers with more experience. For example, if a teacher is in his fourth year, the principal has been able to observe each of the years the teacher has been in the district and has a pretty good idea of the teacher's quality. However, with fewer time periods of observation, the principal would have less information and would have to rely on noisy signals a bit more. Thus, there is potential for less experienced teachers to have larger effort response to the layoff announcement. Lastly, the teacher's students' performance may have a factor in the intensity the teacher's response is to the layoffs. In particular, if a teacher is a high-quality teacher and she believes the principal knows this, then she may not respond to the layoff announcement. However, if the teacher is of low quality or

²³ I do not have specific information on the difficulty of RPS to fill specific subjects. However, I do have anecdotal evidence that positions that are generally harder to fill are also harder to fill in RPS. Specifically, while discussing this research with a current special education teacher, I learned that RPS has a vacant opening in special education five years after the layoffs.

near the marginal quality for retention, there is potential for the teacher to increase effort after the layoff announcement as a way to signal that she is of higher quality and increase her likelihood of being retained. Or conversely, lower-quality teachers may be more prone to demoralization as discussed earlier in the literature review section. Therefore, in my empirical analyses I examine if there are any heterogeneous responses to the layoff policy.

2.4 Data

This study utilizes data from two sources, the Illinois State Board of Education (ISBE) and the Rockford Public School District (RPS). Data on teacher absences and student test scores – linked to teachers – are collected from the RPS. Teacher demographic information [including gender, race, salary, grade(s) taught, and education level] on teachers and principals over six fiscal years, 2007 through 2012, are collected from the ISBE. Each fiscal year runs from July to June (e.g., the 2007 fiscal year runs from July 2006 through June 2007). So, my data range from July 2006 through June 2012. However, for this study, all data after the layoff year (2010) are dropped from the sample because periods after 2010 can be seen as partially treated if higher effort teachers were retained at a higher rate or if there were lingering effects on effort from the layoffs. Thus, my sample data range from July 2006 through June 2010.

Unfortunately, this dataset does not have tenure status for the teachers, so that variable must be imputed. This variable generation is straightforward since teachers are only eligible for tenure after four years of experience and, while there is a teacher evaluation for tenure, in conversations with RPS teachers I found that virtually all teachers pass the evaluation. Using ISBE's data on teacher experience, any teacher with four years or fewer of in-district experience is assigned a value of one in the new variable; everyone else is assigned a value of zero. This

method does leave open the possibility of some teachers with less than four years of in-district experience to be misclassified as untenured when they are not.²⁴ The misclassification is not much of a concern since this is likely a rare occurrence and any such cases would only lead to underreported effects.

Since teacher effort is not directly measurable, I must use a proxy. There have been a few variables suggested as proxies in the existing literature. For example, Duflo, et al. (2015), use a proxy based on unannounced spot checks of teachers, classroom observations, and surveys of administrators. However, the most common proxy used in the literature is teacher attendance or absences (Ahn, 2013; Jacob, 2013; Gershenson, 2014).²⁵ Following these studies, I use teacher attendance to proxy for teacher effort.

One potential source of concern when using teacher absence data is the results may be biased by the number of school days in a period. Specifically, some time periods may have more or fewer school days due to holidays or breaks from school. There are three steps that I take to adjust for this. First, I drop the summer months when the majority of teachers are on break. Secondly, I use percent of school days absent as a robustness check and the results are qualitatively the same. Finally, I add in controls for number of school days within the period.

The structure of sick and personal days in the RPS is outlined in the collective bargaining agreement (CBA) between the RPS and the Rockford Education Association (REA) - the local teachers' union (Rockford Education Association Inc. and Rockford Board of Education, 2011). Within the CBA, teachers are allowed up to 12 days of sick or personal leave per school year.

²⁴ As a robustness check, I redefined my dummy variable using the state-level experience and total experience (i.e., combined in-state and out-of-state experience). The results of these robustness checks are reported in Appendix Table 2.A2 and show that the results are not sensitive to type of experience used to define tenure.

 $^{^{25}}$ Most of the literature uses teacher absences instead of teacher attendance, but I prefer teacher attendance because the empirical interpretations are simple to put into effort terms since attendance and effort are positively related – as effort goes up, attendance increases.

Teachers with over 20 years experience are allotted an extra three sick days per school year. The above maximums are only if the previous year's supply of sick days is exhausted; any teacher's unused sick leave accumulates without bound. The district also has a process to reimburse teachers for unused sick days upon retirement. Upon retirement a teacher gets their daily pay applied to 20 percent of the sum of unused sick days minus 105. For example, if a teacher did not use a sick day for 20 years (i.e., 240 sick days), she would get a payment equal to her daily rate multiplied with 27 days [(240-105)*0.2]. Thus, this teacher would receive the equivalent of 27 days paid at her retirement. While this is a nice benefit for the teachers, the present discounted value (PDV) of this dollar amount is likely not enough to forego taking a sick day when needed. Additionally, on average, the treatment group, untenured teachers, are relatively young, thus the PDV is even lower. Thus it is possible that the maximum number of sick days increase with more experience. I adjust for this by including controls for teacher experience and limit the control group for my main analysis to only tenured teachers with 10 years of experience or fewer (henceforth referred to as young tenured teachers).

In order to identify the different quality teachers, value-added measurements (VAM) are used. However, teacher VAM score data is not calculated by the RPS. Since I was able to get teacher-linked student performance data, I am able to calculate the teacher VAMs using the dynamic ordinary least squares (DOLS) estimator (DOLS has also been referred to as OLS-lag) (Guarino, et al., 2014). To calculate DOLS, the following equation is used:

(2.1)
$$A_{ig} = \tau_g + \lambda A_{i,g-1} + \boldsymbol{E}_{ig}\beta + \boldsymbol{X}_{ig}\gamma + c_i + e_{ig}\beta$$

In this equation, A_{ig} is student *i*'s test score in grade g, $A_{i,g-1}$ is student *i*'s test score in grade g - 1 (i.e., the one-year lag test score), X_{ig} is a vector of student observable characteristics, c_i is student-level fixed effects, and e_{ig} is the idiosyncratic error term. The

teacher VAM comes from the $E_{ig}\beta$ term. In particular, E_{ig} is a vector of teacher dummy variables that take on the value of one if student *i* had the teacher in grade *g*. When this equation is estimated using ordinary least squares (OLS), the resulting coefficient estimates, $\hat{\beta}$, are the VAM estimates. From this, I sort the teachers into terciles based on their value-added scores.

2.5 Empirical Strategy

In order to correctly identify the effect of the policy on teacher effort, I utilize a difference-in-difference model. The first difference is in untenured teachers over time. Since the policy was only in effect in 2010, the years prior to 2010 provide a baseline for which to compare. The second difference comes from the fact that only untenured teachers were directly affected by the policy. It is possible that there is a spillover effect on tenured teacher effort during the restructuring. Figure 2.1 shows that when looking at teacher attendance over time, there is a sharp downturn in untenured absences after the policy announcement, but there is a similar downturn in all tenured teacher absences. Figure 2.1 shows that after the restructuring was announced, the drop in young tenured teachers' absences is similar to the decline in all tenured teachers' absences. My difference-in-difference identification strategy leads to the following baseline equation:

(2.2) $Z_{it} = \beta_0 + \beta_1 Untenured_{it} + \beta_2 Untenured_{it} * Policy_t + X_{it}\gamma + \mu_i + \delta_t + e_{it}$ Z_{it} is the number of school days teacher *i* was absent in time period *t*. Untenured is a dummy variable that equals one if a teacher has fewer than five years of in-district experience.²⁶ Policy is a dummy variable that takes the value of one for the period from when the restructuring was

²⁶ I also use in-state and total experience to create the untenured dummy variable as a robustness check and find that the results are qualitatively similar. This robustness check can be found in Appendix Table 2.A2.

announced (March 23, 2010) to the end of the school year when the restructuring occurred. Additionally, alternative versions of this model replace the policy dummy with dummies for months and weeks in 2010 to allow for pinpointing when the effects of the policy occurred. X_{it} is a vector of teacher demographic variables that change over time. μ_i and δ_t are teacher and time fixed effects (with the time fixed effects being the period of time from March 23 until the end of the school year, month, or week depending on the specification). In this analysis, the coefficient of interest in this equation is β_2 .

My initial method of estimation is Ordinary Least Squares (OLS). However, my main dependent variable, teacher absences, is a count variable with large masses at zero. Thus there is concern that the OLS estimates could underestimate the true effect. Thus, I also estimate using Poisson quasi-maximum likelihood estimation (QMLE) as developed by Hausman, et al. (1984). The fixed effects Poisson QMLE estimation may be a better fit because it does not allow negative values of the conditional expectation of the dependant variable (which is good since the dependant variable in this study is strictly non-negative). Additionally, Wooldridge (1999) shows that even if a data set doesn't follow a Poisson distribution, the Poisson QMLE yields consistent results as long as we have the correct specification of the conditional mean.

In addition to the above estimation of teacher effort, I also analyze at four areas of possible heterogeneity, school-level absence culture – as split into terciles, untenured teacher experience level, subject taught, and teacher performance level. The potential effects from each of these heterogeneous responses are discussed in the conceptual framework section.

2.6 Results

2.6.1 Effect of Layoff Announcement on Teacher Absences

To get a sense of how on average teachers responded to the layoff announcement, I first look at the average number and percent of teacher absences by tenure status over time. Table 2.1 shows that from March 23rd, when the layoffs were publicly announced, until the end of the school year, on average untenured teachers decreased their number of absences by about 0.36 compared to the prior years' average.²⁷ Tenured teachers have a decline in absences that almost matches the decrease of untenured teachers, but young tenured teachers' absences decline less than the untenured teachers. The results in Table 2.1 are similar if looking at the teachers' number of absences or percent of school days absent. This is important because in 2010 the Rockford Public School District (RPS) held its annual spring break in April while in the previous years, spring break occurred in March. Since the results are qualitatively similar, this suggests that a spring break effect is not driving the level results. However, to make sure any of my results are not being driven by a Spring Break Effect, I control for number of school days in my analyses.

The initial results from Table 2.1 show no statistically significant effect of the layoff announcement on teacher absences, but it does not control for experience, time, number of school days, or teacher fixed effects. Table 2.2 reports the results when controlling for these effects. When looking at the percent of days absent during the layoff period and the number of days absent, there is no statistically significant effect, but the coefficients are generally negative. When the number of days absent analysis is extended to use a Poisson Quasi-Maximum Likelihood estimation to control for the count dependent variable, the coefficients are a bit larger

²⁷ The regressions compares the March-23rd-to-end-of-school-year period in 2010 to that same time period in the prior years.

in size than the OLS estimation of the level of absences, but are still not statistically different from zero.

2.6.2 Effect of Layoff Announcement on Teacher Absences by Month, Week, and Day

While there is no apparent effect on teacher effort when looking at the entire layoff period, it is possible that there was a more temporary shock to teacher effort that would be apparent at the month-by-month, week-by-week, or day-by-day level. Table 2.3 looks at the effect of the layoff announcement on teacher effort at a month-by-month level. In this table, the effect of the policy is not statistically different from zero. This suggests that if there was a behavioral response, it was not sustained at a statistically noticeable level for an entire month.

Drilling down further to see if there was a shorter-term response, I next look at the weekby-week effect. Table 2.4 shows the week-by-week analysis when time fixed effects, experience controls, and teacher fixed effects are included. Across all versions of the analysis, there is a decline in absences the week immediately following the announcement.²⁸ Column [3] shows the results when experience dummies, week fixed effects, teacher fixed effects, and number of school days in the week are included. In this column, an untenured teacher took, on average, 0.08 fewer absences in the week after the layoffs were announced than they would have in that same week in prior years or if they were tenured. This suggests that the announcement of the layoffs caused a short-term boost to teacher effort, but in the following weeks, the effects are not statistically different from zero. One could question whether this effect is driven by a change in when Spring Break took place in 2010. In 2010, the Spring Break took place two weeks after the week of the layoff announcement. So, the negative effect on teacher absences comes in the week before Spring Break. In general, teachers are more likely to take time off right before or after

²⁸ These results are robust to using percent of school days absent. This robustness check is in Table 2.10.

Spring Break in order to extend their vacation time. However, since I find a negative effect the week before Spring Break, one hypothesis is that the layoff announcement lead teachers to reconsider getting a head start on their vacations. Finally, in the last week of the school year, untenured teachers took, on average, approximately 0.03 more absences than they would have if they were tenured or in a prior year.

In the final part of my main analysis, I study teachers' behavioral response at the day-byday level. Table 2.5 shows the day-by-day response of the teachers starting with the day of the layoff announcement. In Column [3], it is clear that teachers were more likely to take an absence the Friday of the week of the layoff announcement (March 26) and less likely to take an absence on the Wednesday and Thursday the week after the layoffs. (March 31 and April 1). One interpretation of these results is that teachers had an initial demoralization from the policy that led to them taking a three-day weekend. However, after that, they came back the following week and applied more effort. One concern with the results from March 31st and April 1st is that the week immediately following them was Spring Break for the RPS. So, teachers may take fewer absences knowing they will have the following week off. However, this theory is not supported in the placebo test shown in Table 2.11, which I describe in more detail later.

2.6.3 Effect of Layoff Announcement on Teacher Absences by Subject Taught, Experience Level, and Value-Added Tercile

There is potential for heterogeneous response of teachers to the layoff announcement. First, untenured teachers in schools where absences are more prevalent on an annual basis may be more likely to take more absences after the policy announcement. For instance, if a school has a culture where taking absences is common, then one could surmise that any demotivating policy would lead to affected teachers taking more absences in that school than other schools. However, Table 2.6 shows that after splitting schools into terciles based on the average annual absences for all teachers, the school tercile does not have a statistically significant effect on untenured teacher absences after the layoff announcement.²⁹ This suggests that school absences culture did not play a large role in the teacher's decision to adjust effort.

The next area teachers may differ in their response based on their subject taught. Teachers in harder to fill positions, like science and math, may feel less need to change effort in order to signal quality since they will likely be retained anyway. Table 2.7 shows that across subjects, the only statistically significant change in how teachers responded to the layoffs is for science teachers when experience dummies, time fixed effects, and teacher fixed effects are included. In particular, science teachers took, on average, 1.248 fewer absences during the layoff period than they would have in prior years or if they were tenured. This is the opposite of what is expected, in that the hard to fill subject of science actually had teachers increasing effort levels. However, this effect is only statistically significant at the 10% level.

A second area for potential heterogeneous response is teacher experience. In particular, principals likely have more uncertainty over first-year teachers' quality, so the first-year teachers' signal through effort holds more weight. Additionally, there is a greater marginal benefit for fourth-year teachers to be retained since they are up for tenure in their fifth year. However, the results in Table 2.8 show no statistically significant effects across the different levels of experience. This suggests that teachers did not significantly differ in their response by their level of experience.

²⁹ In addition to the regressions reported in Table 2.6, I also looked that the average number of teacher absences for each week at a school-level. I did not see any specific school with disproportionately large spikes or valleys.

The last area of heterogeneity I look into is teacher quality as proxied by value added measures (VAM). Table 2.9 shows how teachers in different VAM terciles respond to the layoff announcement. Untenured teachers in the bottom reading VAM tercile took, on average, 6.352 more absences during the layoff announcement period (March 23, 2010 to the end of the school year) than they would have in a prior year or if they were tenured. A similar size magnitude coefficient is found looking at the untenured teachers in the bottom mathematics VAM tercile, but this estimate is not statistically different from zero. These results suggest that the lowest value-added teachers were more likely to lower their effort in response to the layoff announcement. However, the estimates are not precise, due to the smaller sample size.

2.6.4 Testing the Principal Tip-Off Hypothesis

The main results found in the previous tables suggest that teachers, in general, take fewer absences after the restructuring is announced. However, there is the potential for principals to inform certain teachers that they will be retained considerably before the actual layoffs occurred at the end of the school year. A tip-off effect would lead to teachers having more job security and feeling more comfortable taking time off which, in turn, would lead to a higher number of absences.

In order to test if there is a principal tip-off effect, I look at whether teachers that ultimately returned to their same school behaved differently than the other untenured teachers. Table 2.10 shows the results of this test. While the coefficients for the variables that indicate an untenured teacher returned to the same school the following year are positive, none of them are statistically different from zero. Thus, there may have been a principal tip-off effect, but there is not enough evidence to completely confirm it. Additionally, looking at the week-by-week results in Table 2.4, there is an increase in absences in the final week of the school year for untenured teachers. This does support a principal tip-off effect would theoretically lead to either more absences or a similar level of year-end absences as in prior years because teachers have would have increased job security from the principal's reassurance of the teacher's job. While this does support a principal tip-off, these results do not rule out a motivational effect such as the one discussed in further detail in section 2.6.2.

2.6.5 Placebo and Robustness Tests of Main Results

With the use of a difference-in-difference identification strategy, one may be concerned that the previous results are being driven by an unobserved factor that occurred over the same period as the policy being analyzed. To assuage this concern, I perform a placebo test. In the placebo test, I use only the data through the 2009 fiscal year. I then use March 23, 2009, through the end of the school year as the new "treated" period. If any significant effect is found, it would suggest that there is an underlying difference in my treatment and control groups that are driving the results instead of the restructuring policy. Tables 2.11 and 2.12 show the results from the placebo test. The first column reported is the main effects shown in Tables 2.4 and 2.5, column [3]. For the week-by-week absences regressions (Table 2.11), the placebo coefficients for the week following the layoff announcement are not statistically significant. The day-by-day regression placebo is a little trickier. In 2009 in RPS, the week that included March 23rd was the district's Spring Break week. So, a direct comparison of that week is not feasible. However, I do look at the week before Spring Break and the week of March 29th for comparison to the main results. In Table 2.12, I find that in the days leading up to Spring Break, teachers took more days off. This is completely opposite of the negative effects found for March 31st and April 1st (i.e.

the days leading up to the 2010 RPS Spring Break) in the main results. This suggests that the effect of Spring Break is not driving my results and may actually be leading to lower effects than the teachers otherwise would have had. If we instead focus on comparing the week of March 29th between the main results and the placebo, we find that there is no statistically significant effect in the placebo during that week.

In addition to the placebo test, I perform two robustness checks to evaluate my choice of using number of absences as my main dependent variable and young untenured teachers as my control group. In Table 2.11, the coefficients, when percent of school days absent are used as a dependent variable instead of number of days absent, are similarly signed, large in magnitude, and similarly statistically significant. This suggests that my choice of dependent variable is not driving my results and that the spring break timing is not driving my results. A second robustness check examines whether the results are being driven by the choice of using only young tenured teachers as the control group. When the control group is expanded to include all tenured teachers, the coefficients are very similar to the coefficients from when just young tenured teachers are used as a control group. This suggests that my results are robust to control group specification.

2.7 Conclusions

This study measures the impact on teacher effort of a district-wide decision to give all untenured teachers layoff notices by using a difference-in-difference identification strategy on Rockford Public School District (RPS) data. Using ordinary least squares and Poisson quasimaximum likelihood estimation, I find that the teachers affected by the policy did not change their behavior in a statistically significant way in the aggregated layoff period. However, I do

find that there is a temporary effect of fewer absences taken in the week after the layoffs were announced. Specifically, the decline in absences appears to occur in days at the end of the week immediately following the announcement and before RPS' Spring Break. There is some marginal evidence that science teachers took fewer absences during the layoff period, but years of experience does not appear to affect the quantity of absences taken. Additionally, there is evidence that supports that teachers in the bottom VAM tercile took more absences in the layoff period, but these estimates are subject to small sample size bias.

I propose a hypothesis for the result of increased effort (proxied for by absences). In particular, since teachers knew that principals were in control of rehire decisions, the untenured teachers affected by the layoffs may take fewer absences in order to signal they are of higher quality. However, this increase in teacher effort is not sustained over the entire layoff period. So, while the untenured could initially attempt to signal quality of higher effort, this strategy is abandoned in later weeks and months.

For the teachers in the lowest VAM tercile, they may have felt demoralized by the layoff announcement and taken more absences in response. There is some evidence of this hypothesis in the school board minutes that show several teachers arguing about the unjustness in the manner the layoffs were structured. Anecdotally, I have conversed with a few teachers and principals that were in the RPS when the layoffs occurred and the common narrative supports the demoralization hypothesis.

I present an alternative hypothesis that principals tipped off teachers they planned on rehiring, leading to more job security for those teachers and, in turn, leading to the teachers taking more time off. However, the results from the empirical test of the tip-off hypothesis are unclear. When looking at the impact of how teachers who were ultimately retained in the same

school changed their attendance behavior, there is no statistically significant evidence that it is different than the non-retained untenured teachers. So, while there may have been a principal tipoff effect, it was not strong enough to provide a statistically significant adjustment in untenured teacher's effort levels. Chapter 3

Capitalization of Charter Schools into Residential Property Values
3.1 Introduction

The charter school movement began about twenty years ago and was driven by the belief that privately run and publicly financed schools could be superior to traditional public schools. Proponents argue that charters can adapt more smoothly in times of financial hardship than traditional schools (e.g. by reducing non-unionized labor force or changing administrative policies). They also argue that charters are leaders in methodological innovations in education. On the other hand, opponents argue that charters are able to restrict admission to make them look better than they are and that they divert necessary resources from public schools. While existing research has generally shown charter effectiveness to be mixed (e.g. Angrist, Pathak and Walters, 2013; Angrist et al., 2012; Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011; Imberman, 2011b; Hoxby and Murarka, 2009; Bifulco and Ladd, 2006; Sass, 2006; Bettinger, 2005), the impacts of these schools on the wider economy is not well known. In this paper we attempt to establish the extent to which charter schools impact residential property markets by examining how charter penetration rates in a community are capitalized into surrounding home prices using data in Los Angeles County (LA County), California. Understanding whether housing markets are responsive to charter availability is important given the increasing prevalence of charter schools across the country. Indeed, California has seen significant growth in the number of charter schools since they were authorized in 1992; the overall number of charters has increased from 299 in 2000 to 912 in 2010, with 242 of those in LA County alone. This is the highest number of charter schools in any county in the U.S.³⁰

While there is also a substantial literature relating housing values and school characteristics (e.g. Imberman and Lovenheim, forthcoming; Gibbons, Machin and Silva, 2011; Bayer, Ferreira and McMillan, 2007; Kane, Riegg and Staiger, 2006; Figlio and Lucas, 2004;

³⁰ California Charter Schools Association, accessed via www.calcharters.org.

Gibbons and Machin, 2003; Black, 1999), only Buerger (2014) in an unpublished working paper specifically considers home owners' valuation of charter schools. To identify the impact of charters on housing prices, we use data on single-family home sales from 2008-2011, obtained from Los Angeles County Assessor's Office. We estimate the impacts of both the number of charters and the share of public enrollment in charters within various distances of a property up to two miles. To account for endogenous charter locations and changes in the geographic distribution of sales we include census block fixed effects along with a set of housing and school characteristics to account for the non-random location of charter schools. Month-by-year fixed-effects account for any general changes to the education and housing markets over time in LA County.³¹ Thus, our identification comes from houses sold in the same census block at different times as charters open, close, expand and shrink. As a result, we note that our study does not identify how *existing* charter enrollment affects housing prices but rather how contemporaneous changes in charter enrollment and the number of charters affect housing prices in localized areas, specifically within census blocks.

Overall, our results suggest that neither the increase in the number of charter schools nor the expansion in charter enrollment relative to public school enrollment – our proxy for the availability of charter school slots to local residents – is capitalized into housing prices on average. This holds both for Los Angeles Unified School District (LAUSD) and other parts of Los Angeles County. It also holds for both startup charters – new schools that begin as charters – and conversion charters – public schools that convert to charter status, though we caution that very few schools convert during our sample period. Further, we find no evidence that capitalization varies with income level, minority population, or achievement levels of the local public elementary school.

³¹ We acknowledge, nonetheless, that since we do not have neighborhood controls that vary over time, our model does not account for changes in neighborhoods independent of changes in local schools that may affect charter penetration. We discuss this issue in more detail in the empirical strategy section below.

However, we do find that when we count charters located only within the household's school district's boundaries and exclude LAUSD there is a significant *negative* effect of additional nearby charter schools on housing prices. This restriction is reasonable as students who reside within the charter's authorizing school district (which is almost always the district they are located in) have admissions priority, thus generating a link between these schooling options and local district boundaries. A potential explanation for this finding is that opening a nearby charter school reduces the value of a local community school, thus weakening the link between the availability of local schooling as a public good and house prices.

3.2 Charter Schools Background

Charter schools are public schools that are tuition-free and managed by an independent operator. Typically they are open to any student wishing to attend, regardless of where they live, though some schools give preference to students who reside nearby. Many schools require an application, and those that are in high demand will often have a waitlist. Charters are typically governed by parents, teachers, members of the local community, or a private company and are reviewed for renewal every few years by an authorizer, usually the state or a local school district. In California, charters are funded through a mix of block grants and a state-based funding formula that provides funding at the same per-pupil rate to all charters of a given grade level across the state.³² There is substantial heterogeneity across schools in the way they are managed, their goals, their targeted student population, and level of autonomy from the local school system.

³² "Charter Schools FAQ Section 3," California Department of Education, accessed http://www.cde.ca.gov/sp/cs/re/qandasec3mar04.asp.

An important distinction to recognize among charter schools is that they are either brand new schools – startup charters – or were previously a traditional public school that switches to a charter model – conversion charter. According to the California Charter Schools Association, there are many reasons why traditional schools decide to convert to charter status, but above all is the appeal of increased flexibility and autonomy. Conversion charters must satisfy the same legal requirements and processes as startup charter schools. This involves submitting a charter petition establishing features such as the school's goals, finances, and governance plan, as well as obtaining signatures of at least fifty percent of the permanent teachers currently employed at the school.³³ However, California law does require that conversions give priority to students in the school's district and many districts, including Los Angeles Unified, give priority to students in a local catchment area. Typically startup charters do not have catchment areas, but if they are over-subscribed they are also required to give priority to students who reside in the authorizing school district and may choose to give priority to those in the local school zone if the neighborhood school has high rates of economic disadvantage.

As of the 2010-2011 school year, conversion charters represented 16 percent of California's charter schools, enrolling about 25 percent of all charter school students.³⁴ Charter school facilities vary with type of charter, with some building brand new structures, renting available spaces in churches, community centers, or commercial buildings, or occupying a previously traditionally run public school campus.³⁵ When a school converts to charter status, it usually remains in the same building and retains teachers, staff, and students. In contrast, startup

³³ "School Conversion," California Charter Schools Association, accessed via www.calcharters.org/starting/conversion/.

³⁴ "Conversion Charter Schools: A Closer Look," California Charter Schools Association, accessed via www.calcharters.org/2012/04/conversion-charter-schools-a-closer-look.html.

³⁵ California Charter Schools Association, accessed via www.calcharters.org.

charters need to recruit a student body because parents have the option to enroll their child in the charter or in the assigned public school.

Another important distinction between types of charter schools that has drawn interest recently is the role of larger charter management organizations (CMOs). CMOs are non-profits that operate multiple charter schools and charters within an organization are able to pool management and resources in order to gain economies of scale, a benefit often shared by schools within a traditional public school district. Evidence of the impacts of these types of charters on student outcomes suggest that effectiveness varies substantially across CMOs and students (Furgeson, et al., 2012; Angrist, et al., 2012). Another heterogeneous distinction between charter schools is whether a charter has a waiting list. Recent work using oversubscription lotteries has indicated that waitlist charters perform better than local public schools but are unable to assess the impacts of non-waitlist charters (Angrist, Pathak and Walters, 2013; Angrist et al., 2012; Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011; Hoxby and Murarka, 2009). Unfortunately, while it would be interesting to see whether housing prices respond differently to these two ways charters vary, we do not have data on whether charters are operated by CMOs or have waitlists.

3.3 Theory of Charter Impacts on Housing Prices

The theory behind the relationship between housing prices and local school quality predicts that, due to the close link between residential location and the school attended via attendance zones, higher quality schooling will generally lead to an increase in housing prices, though the extent of this increase depends on a number of factors (Black and Machin, 2011; Rosen, 1974). This relationship has been well established through empirical analyses (Gibbons, Machin and Silva, 2013; Bayer, Ferreira and McMillan, 2007; Kane, Reigg and Staiger, 2006;

Figlio and Lucas, 2004; Downes and Zabel, 2002; Black, 1999). However, since charter schools do not typically have attendance zones and typically students may attend a charter regardless of their location of residence, the theoretical link between charter schools and housing prices is ambiguous.

Despite a less obvious link between charter schools and housing prices, economic theory suggests homeowners may respond to charters in a neighborhood for a few reasons. First, charters provide an option value. Even if a child does not attend a charter school, the availability of charters nearby may make a location more attractive for parents. Since charters rarely offer busing, travel distance is especially important if transport costs are expensive as is the case in Los Angeles County where there is limited public transportation, heavy traffic congestion and high gas prices. Further, as previously mentioned, in California oversubscribed charters much give priority to students who reside in the school district containing the charter which could increase the option value to living in the district.

Second, charters may have an indirect effect on housing prices if they affect the performance of local public schools. Evidence on how charters affect local public schools is mixed. While Booker, Gilpatric, Gronberg and Jansen (2008), Bifulco and Ladd (2006), and Sass (2006) find positive effects of charters on nearby public schools, Imberman (2011a) finds negative effects. Thus it is unclear how this mechanism might influence housing prices.

Third, the public may value the direct infrastructure and community improvements charters sometimes provide. Indeed, Cellini, Ferreira and Rothstein (2010) show that housing prices respond to non-charter public school facility investments. While many charters rent or use donated space, some build their own facilities or convert abandoned properties for use as schools. Even those that rent will often fill up vacant properties in locations like strip malls

(Imberman, 2011a). Thus the additional economic activity generated by the charters may influence local housing prices.

Another theory is that charter schools may serve to break the connection between local public schools and housing prices. In so doing we might expect additional charters (and more school choice options more broadly) to lead to increased housing prices where existing schools are low performing as these locations would have artificially low housing values due to the poor school quality. Alternatively, in high performing areas, additional charters may actually *reduce* housing prices as the availability of nearby charters weakens a key benefit of being zoned to a high-performing school if, through attending charters, high school quality becomes available to households outside the attendance zone (Nechyba, 2003). Another possibility, however, is that by severing this link, the availability of having a public school option at all, irrespective of school quality, is less valuable. The public good of a local school provides less utility and thus, without a commensurate reduction in property taxes, lowers the value of living near that school.

The theories outlined above indicate that it is unclear how charter schools may affect housing prices as some economic effects may be positive and some may be negative. As such, understanding the overall effect on local property markets is necessarily an empirical question. We should also note that while it may be tempting to interpret housing price responses as measures of how much people value charters, the complexity of the underlying processes makes it difficult to do this. In fact the theories described above of how charter schools may sever the link between local public schools and property values highlight that the effects could be showing something entirely different from valuation.

3.4 Previous Literature

Most of the existing literature on charter schools focuses on the effect of charters on student achievement. Early research that relies on panel data methods have found mixed results, with some researchers finding insignificant or significant negative impacts of attending a charter school on student test scores (Imberman, 2011b; Hanushek, Kain, Rivkin and Branch, 2007; Bifulco and Ladd, 2006; Sass 2006; Zimmer and Buddin, 2006), and others finding positive impacts (Booker, Gilpatric, Gronberg and Jansen, 2008; Hoxby and Rockoff, 2004). More recent research employing random lotteries (Angrist, Pathak and Walters, 2013; Angrist et al., 2012; Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011; Hoxby and Murarka, 2009) and natural experiments (Abulkadiroglu et al., 2014) have found large positive effects. Some research has also recognized the distinction between conversion and startup charters and suggests there is a differential impact on performance across the two types (Sass, 2006; Buddin and Zimmer, 2005; Zimmer and Buddin, 2009).

There are two studies in particular that are similar to ours. First, Chakrabarti and Roy (2010) try to use the impact of charter schools on enrollment in private schools as a proxy for how much parents prefer charters to other schooling options. They find modest declines in private school enrollment when charters locate nearby. Second, in an unpublished working paper Buerger (2014) looks at differences in housing prices across school districts in New York due to charter penetration and finds positive effects. His identification relies on differences in charter penetration across school districts and census-tract fixed effects.

Nonetheless, our paper is distinct from Buerger (2014) in a few key ways. First, the focus on differences across districts, while useful in areas with many school districts, is less relevant to areas like Los Angeles that are dominated by a large central core district. Indeed, most charter

schools tend to locate in urban core areas dominated by large urban districts. Thus, our analysis allows for identification of charter impacts within these urbanized areas. Second, Buerger looks at the impacts on housing prices from the entry of the first charter school into the district. In our analysis, we look at capitalization of marginal changes in charter penetration using multiple charter penetration measures. Third, our inclusion of census-block fixed-effects instead of the geographically larger census-tract fixed effects allows us to account for more potential sources of time-invariant unobserved characteristics.

A separate branch of literature focuses on the relationship between housing prices and school characteristics. There is ample evidence from previous work that housing prices are responsive to test score differences across schools.³⁶ Both Black (1999) and Bayer, Ferreira and McMillan (2007) estimate regression discontinuity models across school zone boundaries to identify how school-average test scores are capitalized into housing prices. Figlio and Lucas (2004) examine the effect of the release of "school report card" data in Florida on property values. These report cards rated schools from A to F based on average performance on statewide exams. All three studies find sizable, positive impacts of higher school test scores on home values, suggesting that parents place significant value on this school quality measure. Gibbons, Machin and Silva (2013) find similar results in England using boundary discontinuities using test score gains. On the other hand, Imberman and Lovenheim (forthcoming) find little impact of the release of teacher and school value-added information on housing prices in Los Angeles.

Several studies have considered the effects of other school characteristics such as student demographics, per-pupil spending, and pupil-teacher ratio, on housing prices. In the footsteps of Oates' (1969) seminal paper, which uses per pupil spending and pupil-teacher ratio as measures of school quality, much of this research has found positive relationships between similar

³⁶ For a comprehensive review see Black and Machin (2011).

measures and housing prices (Bradbury, Mayer and Case, 2001; Bogart and Cromwell, 1997; Weimer and Wolkoff, 2001). Clapp, Nanda and Ross (2008), using panel data from Connecticut, find that an increase in the percentage of Hispanic students has a negative effect on housing prices. Using data from Chicago, Downes and Zabel (2002) find that households do not capitalize per-pupil expenditures.

Bogart and Cromwell (2000) exploit school redistricting in Ohio and find that disruption of neighborhood schools - in terms of student demographics, changes in transportation services, and geographic location within the neighborhood - reduces house values by nearly 10 percent. Reback (2005) analyzes the effect of adoption of a public school choice program in Minnesota to estimate the capitalization effects related to changes in school district revenues, as districts' state revenues depend on enrollment. He finds that a one percentage point increase in outgoing transfer rates is associated with an increase in house prices of about 1.7 percent.

Our analysis builds off the approaches of these studies, by estimating the impact of charter schools on local housing prices while carefully accounting for selection of charters into neighborhoods. In particular, our baseline specification includes census block fixed effects to account for unobserved heterogeneity across local neighborhoods in the propensity for charters to open or close nearby.

3.5 Data

Our home price data come from the Los Angeles County Assessor's Office (LACAO). The data contain the most recent sale price of every home in Los Angeles County as of October 2011. In addition to Los Angeles Unified School District (LAUSD), the second largest district in the country, the data encompasses 75 other school districts. Since our data is based on most

recent sales, to avoid endogenous selection into the sample and small sample sizes in early years, we restrict our data to include only residential sales that occurred between September 1, 2008 and September 30, 2011. From LACAO, we also obtained parcel-specific property maps, which we overlay with school zone maps from 2002, which is the most recent year such data is available for the whole county.³⁷ The data also include home and property characteristics, such as the number of bedrooms, the number of bathrooms, units on the property, square footage, and the year the structure was built.

We drop all properties with sale prices above \$1.5 million in order to avoid results being driven by home price outliers. Further, about 25 percent of the residential properties in the dataset do not have a sale price listed. Usually, these are property transfers between relatives or inheritances. Hence, we limit our sample to those sales that have "document reason codes" of "A," which denotes that it is a "good transfer" of property. We also drop all properties with more than either eight bedrooms or eight bathrooms.

The charter school data is from the California Department of Education. We rely on two measures of charter school penetration: the counts of the number of charter schools within a specified distance from a home and the percentage of total enrollment in the public sector attributable to charter schools within a specified distance from a home. For the former measure, we calculate the distance between each charter and the home, and count the number of charters falling within a specified distance. For the latter measure, we use enrollment figures for all public schools in Los Angeles County from the Common Core of Data, managed by the Institute of Education Sciences at the U.S. Department of Education. An explanation for why we choose these variables and our specified distances is provided in the empirical strategy section below.

³⁷ The 2002 LA County maps come from the Los Angeles County eGIS portal at

http://egis3.lacounty.gov/dataportal/. The maps were created using a variety of sources and thus may not match precisely to actual school zones.

We combine these data with school-by-academic year data on Academic Performance Index (API) scores, API rank, school average racial composition, percent on free and reduced price lunch, percent disabled, percent gifted and talented, average parental education levels and enrollment. The API score is California's summary index of school test score performance. These covariates, which are available through the California Department of Education, control for the differences in charter school penetration that are correlated with underlying demographic trends in each school.

Our main analytic sample consists of 158,211 house sales occurring from September, 2008 through September, 2011. Of these, 65,170 are sales of homes zoned to an elementary school in LAUSD and 93,041 are sales of homes zoned to an elementary school in another school district in LA County. Table 3.1 provides information on the types of charter and public schools that operate in LA County over our sample period. Panel A provides schools by grade level. Charters are more common for middle and high schools but still account for a substantial portion of elementary schools at 9 percent. Conversion charters in particular are common for elementary schools but not middle and high schools. Panel B shows that over the time period of our study, the percent of schools that are charters grows from 7.7 percent in 2008 to 11.7 percent in 2011. Table 3.2 and 3.3 provide sample means and standard deviations at the property level for several of the variables we include in our regressions. In Table 3.2 we see that properties in Los Angeles County have an average sale price of \$383,546 and tend to be of modest size, averaging around 3 bedrooms, 2 bathrooms and 1600 square feet. We also have a ranking of the quality of the structures on the property which will be useful for conducting validity tests. The property is given a rating on a scale of 1 to 12.5 by LACAO assessors, where a rating of 12.5 is the highest assessed quality. Not surprisingly, the average quality of a property in LA County is

close to the midway point on this scale at 6.45. For charter penetration, the number of charters in each distance ring increases as we go further out, primarily due to the larger amount of land area in larger distance rings. When we look at charters as a percentage of total public school enrollments, the rates are relatively constant across distance rings at 5 - 6 percent.

We note that our data covers some periods of abnormal rigidity in the Los Angeles housing market due to the housing collapse of 2008 and the Great Recession. Figure 3.1 shows the Case-Shiller House Price Index for the Greater Los Angeles area from 2008 through 2011.³⁸ Even though housing prices in Los Angeles fell dramatically until May 2009, afterwards they had begun to rebound, increasing by 11 percent through July 2010. The prices fell slightly thereafter until the end of our data in September 2011. Thus, the housing market had been in recovery for most of our sample period. Even so, we may be worried that market rigidities would continue to limit capitalization. To address this we provide results in the online appendix that vary by year of sale and show that our estimates are similar to baseline in later years of the sample when the market had more fully recovered.³⁹

In panel A of Table 3.3 we provide information on the characteristics for the elementary, middle, and high schools to which each property is zoned. Panel B provides a comparison with charters at each grade level within 1 mile of the property. For elementary and middle schools, the characteristics of charters are pretty similar to those of the zoned school in terms of enrollment, API score and demographics. For high schools, however, there are some differences. Charter high schools tend to be substantially smaller (1,140 students versus 2,002) but lower performing as measured by API score. Zoned and charter high schools are demographically similar, though high school charters tend to have fewer gifted students.

³⁸ Acquired from http://us.spindices.com/indices/real-estate/sp-case-shiller-ca-los-angeles-home-price-index.

³⁹ The appendix can be found at http://www.msu.edu/~imberman/appendix_imberman_naretta_orouke_2015.pdf.

3.6 Empirical Strategy

Our identification strategy relies on variation across households and over time within a census block in the number of charters within various distance radii. To achieve this, in addition to controls for characteristics of the local elementary school and property characteristics, we include census block fixed-effects along with month-of-sale fixed-effects. Including census block fixed-effects allows us to compare the sale prices of properties that are geographically very close by; the mean land area for census blocks in LA County is 108,322 square feet with a median of 19,283 square feet. While it may be preferable to use repeated sales on the same property, this is not possible with our data as we only have sale price information for the most recent sale. Even if we did have repeated sales, given the short time frame, restricting to those types of households would create a selected sample as a disproportionate number of those properties may be distressed, in fast changing neighborhoods, or houses that are often "flipped."

We believe that multiple sales within census blocks provide a reasonably small enough geographic area to closely mimic repeated sales for specific properties while avoiding the potential selection issues generated by using repeated sales. For example, in our final estimation sample the median census block in LA County has three sales during the study period with a mean of 3.9. Figure 3.2 provides a histogram of the distribution of sales within census blocks, conditional on having any sales, over the study period. While our econometric strategy identifies the effect of charter penetration only from blocks with more than one sale, a substantial number of census blocks provide this identification. There are 29,512 blocks with at least two sales and of those, 14,494 blocks have at least four sales and 7,387 blocks have at least six sales. Further, of all blocks with at least one sale, 73 percent have multiple sales, providing wide geographic variation in blocks that contribute to identification. Finally, we conduct an ANOVA analysis of

property characteristics to assess the within and between census block variance. In our estimation sample only 39 percent of the variance in house size and 20 percent of the variance in housing quality is within census block, along with less than half of the variation in bedrooms and bathrooms.⁴⁰ These results suggest that different houses within a block have largely similar characteristics.

By including census block fixed effects, our identification strategy assumes that there are no changes in neighborhood conditions over time that are correlated both with housing prices and charter penetration. Of course, housing prices are increasing in general in Los Angeles during our analysis period as is the number of charter schools. Hence, to account for general changes in house prices related to overall market conditions, we include year-by-month indicators in all of our regression models.

Even with census-block fixed effect and year-by-month fixed effects, it is possible there are factors changing locally that could bias our estimates. Of primary concern is the possibility that charters select into neighborhoods where the local public school is under-performing and the poor quality of the school is reflected in lower housing prices. Ideally, we would be able to at least control for changes in neighborhood characteristics as we do for school characteristics and housing supply. Unfortunately, the data available to us for this is very limited. To our knowledge, only the American Communities Survey (ACS) provides neighborhood data at a small enough geographic level (e.g. census tract) to be relevant for this analysis. However, the ACS only provides five-year estimates at the census tract level as estimates based on smaller periods of time are too imprecise. As a result, the ACS data does not provide temporal variation in neighborhood characteristics over our three-year time period and any data on neighborhood

⁴⁰ An ANOVA using the residuals from regressions of the characteristics on month-by-year indicators provides similar results.

characteristics would be absorbed by the census-block fixed-effects. Thus, we assume that selection of charter location is unrelated to time-varying neighborhood characteristics that are themselves not captured in our housing and school characteristics controls. While we cannot test this assumption directly, we do attempt to address it indirectly by testing whether our observable measures of housing characteristics change when more charters move in and by testing whether charter penetration can be explained by prior changes in house prices. If time varying neighborhood characteristics are correlated with prior house prices and the types of houses put on the market then we should expect to see some impact on these observables, and indeed we do not find evidence for this. Nonetheless, while we do not have temporal variation in neighborhood variables, we do have such variation for local elementary school characteristics. Thus in Table 3.A1 of the online appendix, we look at how charter entry relates to public school characteristics when we condition on school fixed- Without school fixed-effects the estimates show that charters tend to locate in the zones of elementary schools with fewer minorities, more gifted students, more English language learners and more disabled students. When school fixed-effects are added some characteristics are statistically significant, but importantly they are all economically small. The largest statistically significant coefficient is on percent of black residents in the public school zone, but this coefficient is still rather small. For a one charter increase in the school zone, there would need to be an increase in percent black by of 84 percentage points. Given this pattern and the general shift in the coefficients towards zero as the school fixed-effects are added, these results suggest that lower levels of geographic fixed-effects, specifically census-block effects, should reduce these correlations further to the point where they are negligible.

Another difficulty in this analysis is deciding how to measure charter penetration. There are two key factors here. First, there is the question as to whether the important factor is the existence of a charter school as a whole or the relative size of a charter school. Arguably, while the former is the most visible aspect of the school to the wider public (people in the neighborhood know that a school exists but may be uncertain as to how large it is), the latter is a potentially better indicator of the supply constraints on a family that wishes to send a child to the charter. The second issue is that it is unclear how far from the charter a household must be before we can be confident that the household should not care about the charter's existence. To deal with both of these issues we follow the prior literature on the effects of charter schools on public schools (Imberman, 2011a, Booker et al., 2008; Bifulco and Ladd, 2006; Sass, 2006). The analyses in these studies estimate the effects of charter schools on traditional public schools within concentric rings of various distances. Since it is not obvious whether what matters is relative enrollment in charters or the number of charters they estimate the effects of both charter counts and enrollment in the charters as a share of total enrollment.

We use measures of charter penetration equal to (a) the number of charters and (b) the share of all public school enrollments in charters in concentric rings between 0 and 0.5 miles, 0.5 and 1 miles, 1 and 1.5 miles and 1.5 and 2 miles from a property. We focus our attention on charters within relatively short distances of properties due to the urbanicity and size of school zones in LA County. The mean elementary school zone in LA County has an area of 3.2 square miles. With this area, if school zones were circular, the radius of the average zone would be 1.0 miles. The median school zone has an area of 0.8 square miles translating into a radius of 0.5 miles. Hence, given the size of school zones in LA County, these are reasonable distances within which to measure the effect of charters. Indeed, in a large Southwestern city that is less densely

populated than Los Angeles, Imberman (2011a) shows that charters only impact enrollment of public schools within 2 miles of the charter. Further, in an analysis of charter applicants in Boston, Walters (2014) finds that 40 percent of applicants apply to the closest charter school while a further 22 percent apply to the second closest. While we do not have data on who actually applies to or attends charters, we note that in LA County the median property is 1.35 miles from the nearest charter while the second closest charter 2.18 miles away. Since these measures include all properties, it is likely that the average distances for charter attendees are substantially smaller. Based on these factors, we believe that 2 miles is a reasonable maximum distance, though we also check distances between 2 and 5 miles in the online appendix.

Our baseline model estimates the impact of charter penetration on the log of the sales price of property i in census block s at time t as

(3.1) Ln(SalePrice_{ist}) =
$$\alpha$$
 + Charter_{it} β + X_{it} Γ + H_i Φ + λ _t + γ _s + ε _{it}

where *Charter* is a vector of charter penetration variables calculated as the number of charters or the share of public school enrollment in charters between 0 and 0.5 miles, 0.5 and 1 mile, 1 and 1.5 miles, and 1.5 and 2 miles from the property. The β coefficients can be interpreted as jointly identifying a house price gradient that captures the differential valuation of charter penetration by homeowners over distance. **X** is a vector of school-by-year observables, where the school is the elementary school to which the property is zoned. **H** is a vector of house-specific characteristics, such as the number of bedrooms, the number of bathrooms, age, quality and square footage. The model also includes month-by-year fixed effects (λ_t) to control for common time trends and census block fixed effects (γ_s) to control for time-invariant neighborhood quality and quality of the locally zoned school.⁴¹ We cluster standard errors at the school zone level to account for correlation between prices of properties in the same census block. An adjustment to this model also restricts to charter schools within school-district boundaries. This is relevant since, as previously mentioned, California requires oversubscribed charters to give admissions priority to within-district students.

We expand the baseline model to account for heterogeneous effects on housing price by disaggregating our charter penetration variables by type of charter: conversion or startup. In this model, the charter penetration vector is split into two:

(3.2) $Ln(P_{ist}) = \alpha + StartupCharter_{it}\beta_1 + ConversionCharter_{it}\beta_2 + Convers$

$$\mathbf{X}_{it}\mathbf{\Gamma} + \mathbf{H}_{i}\mathbf{\Phi} + \lambda_{t} + \gamma_{s} + \varepsilon_{it}$$

In this set-up, the β_1 coefficients will provide a gradient for startup charters and the β_2 coefficients will provide a gradient for conversion charters. We include the same controls as in equation (1). As mentioned above, we would expect to find differing valuation of these two types of charters if homeowners place different weights on the inputs of each type; conversion charters often remain in the same building, with the same student body and staff, and adopting new operating styles while startup charters are often in rental spaces, tend to be smaller than conversions and traditional public schools, and need to recruit students and staff in addition to operating under a new management style.⁴²

⁴¹ The baseline model excludes school-zone fixed effects since most census blocks do not straddle school zones. Nonetheless, inclusion of school-zone fixed effects has a negligible impact on the results.

⁴² The fact that conversions usually maintain the same attendance zone after converting suggests the potential for using a difference-in-differences approach to assessing the impacts of these schools on housing prices. Unfortunately, only five schools in LA County convert to charter status during our study period making the estimates from this type of analysis too imprecise.

3.7 Results

3.7.1 Effect of Charter Penetration on Housing Prices

Table 3.4 provides the baseline results of our analysis using variations of equation (1) and the sample of homes sold across all of LA County. The table includes two panels, one for each charter measure, overall numbers of charters and percentage of total enrollment attributed to charters. Each specification in the table includes month-by-year time dummies, housing controls – square footage, number of bedrooms, number of bathrooms, and quality – and controls for the locally zoned elementary school – enrollment, API score, school demographics, percentage disabled, gifted, free or reduced price lunch eligible, and English language learners. All standard errors are clustered at the school-zone level, where the school is the elementary school to which a property was zoned in 2002.

In columns (i) and (iv) of Table 3.4, we regress the log of the house price on charter counts and the share of public school enrollment in charters within half mile diameter rings, respectively, without geographic fixed effects. The estimates suggest that there is a positive relationship that strengthens as the distance from the property increases. However, in columns (ii) and (v), we include elementary school-zone fixed effects to account for characteristics of the locally zoned school. In these models the patterns differ depending on how we measure charter penetration. When using charter counts, the results indicate that charters negatively impact housing prices, becoming more negative the closer charters are to the property. The coefficient on the zero to half mile radius charter measure indicates that an additional charter is associated with a statistically significant 3.5 log point decrease in the sale price. When using enrollment share, however, only 1 - 1.5 miles is significant.

However, we may still be concerned that there are endogenous differences within school zones, but across neighborhoods, that affect both housing prices and charter penetration. Thus in columns (iii) and (vi) we provide our preferred estimates that replace school-zone fixed effects with census-block fixed-effects. In this model, estimates are all statistically insignificant and small. The largest estimate in column (iii) suggests, when taken at face value, that an additional charter school increases housing prices between 1 and 1.5 miles away by 0.2 percent, with smaller values for other distances. For the enrollment share measure, all of the values are negative, insignificant, and economically small with a 10 percentage point (pp) increase in charter share reducing housing prices by less than 0.2 percent at all distance levels. To provide additional context, if we focus on charter penetration within 0.5 miles of the property, the 95% confidence interval for the impact of an additional charter is [-2%, 1%] while for a 10 pp increase in charter enrollment share it is [0.4%, -0.4%].

One potentially important issue in interpreting the estimated effect of charter penetration is that as the distance increases, the area in which the charter could locate increases. This is not a substantial concern when focusing on share of enrollment, but it does indicate that there may be more variation in the number of charters in farther rings making comparing the estimated effects of charter penetration at different distances difficult. To address this we also provide estimates using charter penetration within the full 2 mile radius around the property in Panel B. The results are similar to those in Panel A and show no impact of charters on housing prices when we include census block fixed-effects. It is also interesting to note that the standard errors decrease when we add census block (or school) fixed-effects. This is another indicator that there is substantial identifying power within blocks and that including between-block variation adds uninformative noise to the analysis.

Table 3.5 provides results for our preferred model that includes census-block fixedeffects when we split the sample by whether the properties are within the boundaries of LAUSD, which is the largest district in LA County, or all other school districts in the county. We may suspect there are different property effects for the two samples because LAUSD covers the main urban core of the county, and recent evidence suggests that urban charters are more effective than suburban charters (Angrist, Pathak and Walters, 2011). Our results, however, provide little evidence that house price effects vary via this location difference. Only one estimate – for charter counts in LAUSD from 1 - 1.5 miles – is statistically significant.

Table 3.6 provides the results for equation (2), splitting the charter penetration variable by charter type – conversion and startup – for homes in all of LA County. As in our regression split by school district, we focus on our preferred model with census block fixed-effects, zoned elementary school controls, and housing controls. As in the pooled model, none of the coefficients are statistically significant and the magnitudes and signs of the estimates do not reveal a consistent relationship between charter counts or charter enrollment rates and sale price for either charter type.

In Table 3.7 we provide estimates that look at how charters affect house prices when we restrict the charters included in the count and enrollment share variables to those that are located in the same school district as the household. In California, within-district students get priority for charter enrollment and so there may be a stronger link with housing prices for these charters than those outside the district. Since LAUSD is especially large with most properties located far from district boundaries, when we estimate this model for LAUSD the estimates are little changed from baseline. Hence in Table 3.7 we only provide estimates using the districts in LA County outside LAUSD. These estimates are the only ones in this paper that provide a consistent

indicator of a charter impact on housing prices. Intriguingly, this estimated effect is negative. An additional charter school within 2 miles reduces house prices by 1.9 percent while a 10 percentage point increase in charter share of enrollment within 0.5 miles reduces prices by 1.2 percent. This analysis provides some evidence that charters weaken the link between public schools and housing prices.

We build on this analysis further by testing whether we see larger effects in areas with higher quality schools. Tables 3.A2 and 3.A3 in the online appendix provide estimates that are allowed to differ by terciles of income and school API score counting all charters, and counting only within-district charters, respectively. While there are some marginally significant estimates in the low income schools in Table 3.A3, overall the evidence for a pattern across school types is weak. However, one possibility is that the district quality is what matters. In Table 3.A4 we investigate this by extending the model in Table 3.7 to allow for different estimates by tercile of district API score. Here a clearer pattern emerges. In fact, the estimates suggest that there is a small increase in property values for high performing districts but a reduction for low performing districts. However, the relationship remains weak with only one estimate for bottom tercile schools significant at the 5% level. It is unclear why such a pattern emerges, but one possibility is that the low achieving districts that are competing with LAUSD, which is also low performing (12th percentile API), benefit from a premium over LAUSD that is weakened by charters. Or it could be that the relationship between housing prices and school quality are more sensitive to charters in low performing districts. Nonetheless, it is unclear to what extent this restriction to within district charters should matter. While district students get priority, this is only relevant if charters are over-subscribed. Hence, given the null results when we do not make this restriction

we think it is best to consider these estimates to be a bound on the potential negative effect of charters.

3.7.2 Testing for Endogenous Charter Location

A consistent estimate of the relationship between charter penetration and housing prices rests on the assumption that the variation in charter penetration is exogenous conditional on the included controls and, most important, the census block fixed-effects. As a test of this, we regress our limited set of housing characteristics on charter penetration variables and census block fixed effects. Ideally we would like to test the relationship between charter penetration and local neighborhood characteristics. However, including census block fixed-effects precludes such an analysis as we do not have access to time-varying neighborhood characteristics. Thus, we must rely on characteristics of the specific households that can be acquired from the property sales data.

Table 3.8 presents results that estimate whether charter penetration is related to square footage, the number of bedrooms, the number of bathrooms, and the quality of the structures on the property as measured by the county assessor. We find no statistically significant relationship between the numbers of charters in any radius ring and square footage, the number of bedrooms, or the number of bathrooms. For quality, only the estimate for charters between 1.5 and 2 miles is statistically significant and only at the 10% level. For charter seats as a percentage of all public education seats, no estimate is statistically significant.

In a second analysis, we regress the log of house price on charter penetration within a half mile of the home in twelve month lag and lead intervals up to three years before the home was sold and three years after the home was sold. For example, the 12 month lag measure

corresponds to charters within a half mile of the property that were in operation 12 months prior to the home's sale. The purpose of this analysis is to test for pre-existing trends and to see if there are any anticipatory or delayed impacts of charter openings. Thus, a clear pattern of higher prices from charters in operation after the house sale would be evidence of either anticipatory effects or preexisting trends in housing prices, the latter of which would invalidate the identification strategy. A pattern of higher prices from charters in operation prior to the home sale would indicate that housing prices are affected by charters but with a delay, potentially due to short-term price stickiness.

Table 3.9 provides the impacts of lags and leads, which show little evidence of responses to charter penetration. Of the two significant coefficients, one is for the 12 month lead in charter enrollment percentage that suggests an increase in enrollment rates of 10 percentage points within a half-mile of the property 12 months following the sale of the home increases the sale price by 0.3%. While this could be indicative of a pre-existing trend, the other estimates indicate this is not likely to be the case. First, estimates for charter penetration 24 and 36 months after the sale show no impact. Second, there is no similar impact when measuring penetration using the number of charters. The other significant coefficient is for the 36 month lead in number of charters, suggesting an additional charter school within a half-mile of the property 36 months following the sale of the home increases the sale price by 1.9 percent. However, if this were indicative of an anticipatory response or pre-existing trend, we would expect to find significant impacts from charter penetration 24 months and 12 months after the sale, as well. Thus, while there are a couple estimates that indicate anticipatory responses or pre-trends, the bulk of the evidence in Table 3.7 argues against such a pattern. Further, we note that the results in the table

also provide little indication of a delayed response since there is no significant impact from the number of charters open or the charter enrollment rates 12, 24 or 36 months prior to the sale.

Finally, in Table 3.10, we test the concern that the addition (or closure) of charter schools may generate sample selection by inducing some people to enter or stay out of the housing market. To do this we regress the number of annual sales in a census block on charter penetration near the block centroid. Further, even though we only have price data for the most recent sale of a property, we can see the dates for the three most recent sales. Thus in the second column we repeat the analysis using the three most recent sales of properties in the sales counts. The results show little impact of charter share of enrollment on housing sales. There is also no significant relationship between charter counts and sale counts within 1 mile of the centroid. Nonetheless, there is a statistically significant but economically small relationship between sales counts and the number of charters one to two miles from the centroid. The estimates suggest that, after conditioning on census block fixed-effects, a new charter opening one to two miles from the block centroid is related to an increase of 0.1 to 0.2 sales in a year. To put this in perspective it would take 5 to 10 new charter openings in a year to generate an additional sale. Given that the average number of charters in that distance range from properties is 1.9, we believe this impact is too small to substantially affect our estimates.

3.7.3 Effect of Charter Penetration on Housing Prices: Heterogeneity and Specification Checks

In the online appendix we provide a series of analyses to look at impacts of charters when we allow the characteristics of the charters, local neighborhoods, and local public schools to vary. First, in Table 3.A5 we provide different estimates by the grade level of the charter. Thus

we split charter penetration measures into four categories – elementary, middle, high and multilevel schools. We see little evidence of differential impacts on housing prices by the level of the charter school at any distance up to two miles from the property. Only one estimate out of 32 is statistically significant at the 5% level. Three more are significant at the 10% level, but show no clear pattern and differ in sign.

In Table 3.A6 we interact the charter penetration measures with the year of the property sale. Since the housing market in Los Angeles had undergone substantial declines just prior to our study, we may be concerned that the lack of capitalization is due to abnormal rigidities in the market, though we note that the significant effects when we restrict to within-district charters suggests this is not the case. Nonetheless, to address this, we focus on the estimates for 2010 and 2011, well after the market had started its recovery. As with our main results, we find no statistically significant impacts of charter penetration at any distance within 2 miles of a property in 2010 or 2011. In fact only one estimate out of the 32 shown is statistically significant - 1 to 1.5 miles in 2008.

In Table 3.A7 we provide evidence on whether the overall mean charter impacts may be hiding heterogeneous effects between neighborhoods with high performing and low performing schools by interacting the charter penetration variables with both the distance from the property and quartiles of household income (across all properties in the data) in the Census tract, the zoned elementary school's API score, percent minority enrollment in the zoned elementary school, and minority enrollment in the census tract. Only five estimates out of 128 are statistically significant at the 10% level (1 estimate at the 1% level) and do not show a clear pattern. Thus we see little indication our pooled estimates hiding heterogeneous impacts amongst

these characteristics. Thus further indicates that the overall null results are not due to differential impacts from weak and strong schools canceling each other out.

Finally, in Table 3.A8 we provide estimates under different specifications and sample restrictions. Through all of these specification and sample checks, no estimates are statistically significant. These checks include using sale price levels rather than log sale prices, splitting the sample by the number of bedrooms, keeping properties with more than 8 bedrooms in the regression, dropping large (5000 square feet or larger) properties, dropping multi-unit properties, and limiting to the summer months of June, July and August as families with children are more likely to move during this period between school-years. Further we show that adding a fifth distance ring of 2 to 5 miles does not change the estimates, nor is the estimate on the added ring significant and adding in school fixed-effects (in addition to census block fixed effects) has little impact on the baseline estimates.

3.8 Conclusions

Research has previously shown close links between school quality and property prices. This has been explained as a capitalization of both the quality and capital stock of schooling into local property values given that typically students are required to attend a specific local public school. Hence, properties zoned to schools and districts with higher performance and more resources have seen higher values, all else equal. Charter schools have the potential to weaken this relationship. Students can typically attend a charter regardless of where they reside, thus making the local school potentially less important to residency decisions. Given that enrollment in charter schools has been increasing across the country over the past twenty years and, if present trends continue, is likely to increase further, the breaking of the link between housing

prices and school quality can have implications for local public finance as well as socioeconomic diversity across schools.

In this study we directly estimate how charter schools affect local property values in Los Angeles County. We also expand our analysis to separate our measures of charter penetration by urbanicity, charter type, and grade level of the school along with wealth of the local neighborhood and the achievement levels of the local elementary school. Our approach follows the work other researchers have done relating school characteristics to housing prices, and carefully accounts for the correlation between neighborhood characteristics and housing prices by including census block fixed effects. This method allows us to estimate the impacts of charters net of any time-invariant differences between local neighborhoods and, by extension, local public schools. Using data from the Los Angeles County Assessor's Office on property sale prices from 2008 through 2011, our estimates show that there is very little impact of charters on home prices on average. The results are not sensitive to sample selection or model specification, nor do we find differential impacts by whether a charter is a startup or conversion, whether the property is in the primary urban school district in the area, Los Angeles Unified School District, by the grade level of the charter, by the income level of the neighborhood, or by test scores in the zoned elementary school. However, given that in California over-subscribed charters must provide priority enrollment to students within the local school district, we also estimate a model that restricts to charters located in the same school district as the property. In this case, which we consider a negative lower-bound impact as it is not clear whether such a restriction is appropriate, we find some evidence that housing prices actually fall by 2 percent for each additional charter within two miles. Since evidence of differential impacts by school quality is weak and, at best, negatively related to income and performance, this suggests that perhaps

charter schools weaken the capitalization of schooling as a public good into property values rather than the capitalization of school quality in particular.

APPENDICES

Appendix A

Figures for Chapter 1



FIGURE 1.1 - District Performance and Race in Illinois in 2010



FIGURE 1.2 - Trends in Teacher Retention Rates



FIGURE 1.3 - Rockford Public School District (RPS) versus Synthetic RPS

Appendix B

Tables for Chapter
					Rock	kford						I	Rest of	Illinois	1		
				Ι	Experier	ice Lev	el				Experience Level						
			Young Tenured			Untenured			Young Tenured			Untenured					
		Initial	Retain	Switch	Leave	Initial	Retain	Switch	Leave	Initial	Retain	Switch	Leave	Initial	Retain	Switch	Leave
2006-2007	Number of Teachers	517	417	69	31	542	376	95	71	41552	35726	3341	2485	47003	36943	6017	4043
2007-2008	Percent (%)		80.66	13.35	6.00		69.37	17.53	13.10		85.98	8.04	5.98		78.60	12.80	8.60
2007-2008	Number of Teachers	502	424	63	15	656	451	143	62	43251	37562	3264	2425	49881	38696	6139	5046
2008-2009	Percent (%)		84.46	12.55	2.99		68.75	21.80	9.45		86.85	7.55	5.61		77.58	12.31	10.12
2008-2009	Number of Teachers	480	436	26	18	672	506	95	71	44115	39724	2550	1841	50796	40725	5360	4711
2009-2010	Percent (%)		90.83	5.42	3.75		75.30	14.14	10.57		90.05	5.78	4.17		80.17	10.55	9.27
2009-2010	Number of Teachers	553	483	51	19	555	355	121	79	46878	41167	3808	1903	46487	34884	6311	5292
2010-2011	Percent (%)		87.34	9.22	3.44		63.96	21.80	14.23		87.82	8.12	4.06		75.04	13.58	11.38

 TABLE 1.1 - Teacher Mobility in the Rockford Public School District and All Other Illinois Districts

Notes: The first listed year refers to the fiscal year used to calculate the initial number of teachers. The second listed year refers to the fiscal year that we observe whether a teacher returns to his/her same position or not. The highlighted rows indicate the year of the mass teacher layoffs. The calculation for the retention variable is if a teacher was ever seen returning to the same school.

		Pre-2010 Mean	2010 Mean	Difference	Diff-in-Diff	Triple Difference
RPS	Untenured	0.7016	0.6005	-0.1011		
				(0.0351)		
	Young Tenured	0.8506	0.8523	0.0017	-0.1028	
				(0.0289)	(0.0329)	
Other Illinois Districts	Untenured	0.7953	0.7652	-0.0301		
				(0.0040)		
	Young Tenured	0.8809	0.8947	0.0137	-0.0438	-0.0590
				(0.0041)	(0.0047)	(0.0331)
Other Illinois Districts - No Chicago	Untenured	0.8246	0.7838	-0.0408		
				(0.0040)		
	Young Tenured	0.9156	0.9167	0.0010	-0.0418	-0.0610
				(0.0025)	(0.0041)	(0.0328)
Synthetic Control	Untenured	0.5840	0.6355	0.0515		
				[0.0084]		
	Young Tenured	0.7265	0.7937	0.0672	-0.0157	-0.0871
	-			[0.0063]	[0.0105]	[0.0372]

TABLE 1.2 - Teacher Retention Rates

Notes: School-clustered standard errors are reported in parentheses. Teacher-clustered standard errors reported in brackets.

	Dependent Variable: Dummy for Teacher Returning to Same School						
	О	DLS	L	ogit	OLS w/ District FE		
Untenured Teacher in RPS in 2010	-0.0575* (0.0330)		-0.0406* (0.0247)		-0.0614* (0.0330)		
Untenured Teacher in RPS	-0.0648*** (0.0174)		-0.0319** (0.0161)		-0.0544*** (0.0173)		
First-Year Teacher in RPS in 2010		0.0202 (0.0690)	, ,	0.00938 (0.0353)	、	0.0156 (0.0103)	
Second-Year Teacher in RPS in 2010		-0.0151		-0.00538 (0.0369)		-0.0276^{**}	
Third-Year Teacher in RPS in 2010		-0.189***		-0.158***		-0.179***	
Fourth-Year Teacher in RPS in 2010		-0.113** (0.0511)		-0.122*		-0.111*** (0.00860)	
First-Year Teacher in RPS		(0.0311) -0.163***		(0.0638) -0.0951***		(0.00869) -0.141***	
Second-Year Teacher in RPS		(0.0347) -0.0827***		(0.0298) -0.0486**		(0.0167) -0.0665***	
Third-Year Teacher in RPS		(0.0256) -0.0173		(0.0239) -0.000746		(0.00828) -0.0177*	
Fourth-Year Teacher in RPS		(0.0252) 0.0334 (0.0275)		(0.0188) 0.0392 (0.0246)		(0.0107) 0.0332^{***} (0.00413)	
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	
District Fixed Effects	No	No	No	No	Yes	Yes	
Main Effects Included	Yes	Yes	Yes	Yes	Yes	Yes	
Total Number of Observations	268,279	268,279	268,279	268,279	268,279	268,279	
Number of Teachers	100,446	100,446	100,446	100,446	100,446	100,446	
Number of Untenured Teachers	68,742	68,742	68,742	68,742	68,742	68,742	

TABLE 1.3 - Impact of Layoffs on Untenured Teacher Retention Probability

Notes: School-clustered standard errors are in parentheses. Marginal effects for Logit regressions are reported. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

		Teacher Outcom	me
	Same School	Left District	Switched Schools
Untenured RPS Teacher in 2010	-0.0388**	0.0165	0.0222*
	(0.0170)	(0.0124)	(0.0114)
Untenured RPS Teacher	-0.0358**	0.0236**	0.0121
	(0.0142)	(0.0118)	(0.0094)
Year Fixed Effects		Yes	
Total Number of Observations		268,279	
Number of Teachers		100,446	
Number of Untenured Teachers		68,742	

TABLE 1.4 - Multinomial Logit Estimation of Teacher Outcomes

Notes: School-clustered standard errors are in parentheses. In the main regression, Same Position is the omitted category and then is backed out from the results. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

		i cucifici v u	iue iluueu	in teasare () III	(I) and I cach	er outcomt			
	OLS				Logit				
	Standardized		Standardized		Standardized		Standardized		
	One-Ye	ar VAM	Two-Y	ear VAM	One-Year VAM		Two-Year VAM		
	Reading	Math	Reading	Math	Reading	Math	Reading	Math	
	Panel A: De	ependent Vari	iable: <i>Dumn</i>	ıy for Teacher .	Returning to S	ame School			
Untenured Teacher	0.0925*	0.0405	0.0155	-0.0150	0.110**	0.0706	0.0205	0.0566	
	(0.0471)	(0.0343)	(0.0407)	(0.0373)	(0.0502)	(0.0464)	(0.138)	(0.124)	
Untenured Teacher in									
2010	-0.240***	-0.200***	-0.0918	-0.0603	-0.252***	-0.165**	-0.0285	-0.0521	
	(0.0877)	(0.0595)	(0.0900)	(0.0680)	(0.0896)	(0.0762)	(0.158)	(0.138)	
	Panel B: Dependent Variable: Dummy for Teacher Leaving the District								
Untenured Teacher	0.000364	0.00322	-0.0205	0.000257	0.0524	-0.00372	-0.496***	0.00107***	
	(0.0140)	(0.00716)	(0.0178)	(0.00191)	(0.0681)	(0.0429)	(0.0938)	(0.000209)	
Untenured Teacher in					. ,			, , ,	
2010	0.117**	0.101***	0.0631	0.0613**	0.0280	0.0496	0.495***	0.00657	
	(0.0502)	(0.0287)	(0.0593)	(0.0290)	(0.0751)	(0.0528)	(0.101)	(0.0357)	
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Dummy Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Variables for Teacher									
Experience									
Controls for Main Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Total Number of	600	600	338	338	600	600	338	338	
Observations									
Number of Teachers	302	302	208	208	302	302	208	208	
Number of Untenured	194	194	136	136	194	194	136	136	
Teachers									

TABLE 1.5 - Teacher Value-Added Measure (Value-Added Measure (Value-Ad	VAM) and Teacher Outcomes
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Notes: School-clustered standard errors are in parentheses. Teacher observable variables are included in the regressions but not reported. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

	Dependen <i>Reading</i>	t Variable: Z-Score	Dependen Math 2	t Variable: Z-Score	
Percent of Teachers Untenured in					
2010*Year After Layoffs	-0.000601	-0.00191	-0.000762	0.00115	
	(0.00118)	(0.00153)	(0.00118)	(0.00124)	
Percent of Teachers Untenured in					
2010*2 Years After Layoffs	0.000756	-1.59e-06	-0.00363**	-0.00166*	
	(0.00228)	(0.00245)	(0.00174)	(0.000902)	
Year Fixed Effects	Yes	Yes	Yes	Yes	
School Fixed Effects	Yes	Yes	Yes	Yes	
Control for Main Effects	Yes	Yes	Yes	Yes	
Student Fixed Effects	Yes	No	Yes	No	
Lag of Student Test Score	No	Yes	No	Yes	
Total Number of Observations	35,324	35,324	35,344	35,344	
Number of Individual Students	16,327	16,327	16,381	16,381	

TABLE 1.6 - Impact of Layoffs on Student Test Scores - Intent to Treat

		Panel A	A: First-Stage					
	Dependent Variables: Percent of Teachers Leaving the School in							
		2010 (interacted	d with year dumm	ies)				
Percent of Teachers			·					
Untenured in 2010*Year								
After Layoffs	0.181***	0.176***	0.180***	0.176***				
	(0.00474)	(0.00408)	(0.00470)	(0.00403)				
Percent of Teachers								
Untenured in 2010*2 Years								
After Layoffs	0.256***	0.242***	0.258***	0.244***				
	(0.00527)	(0.00470)	(0.00517)	(0.00463)				
		Panel B: Second-Stage						
	Dependent Variable: Dependent Variable: Mat							
	Reading Z-Score Score							
Percent of Teachers Leaving the School in 2010*Year								
After Layoffs	0.000242	-0.00878**	-0.00172	0.00885***				
0 00	(0.00380)	(0.00402)	(0.00318)	(0.00327)				
Percent of Teachers Leaving the School in 2010*2 Years	` ,	· · · ·	, ,					
After Layoffs	0.00411	0.00140	-0.0131***	-0.00527**				
	(0.00412)	(0.00306)	(0.00247)	(0.00242)				
Year Fixed Effects	Yes	Yes	Yes	Yes				
School Fixed Effects	Yes	Yes	Yes	Yes				
Control for Main Effects	Yes	Yes	Yes	Yes				
Student Fixed Effects	Yes	No	Yes	No				
Lag of Student Test Score	No	Yes	No	Yes				
Total Number of								
Observations	35,319	35,319	35,436	35,436				
Number of Individual								
Students	16,327	16,327	16,386	16,386				

TABLE 1.7 - Impact of Layoffs on Student Test Scores - Two-Stage Least Squares

	Dependent V	Variable: Dumm Same S	y for Teacher chool	Returning to
	OLS Triple	e Difference	Syntheti	c Control
Untenured Teacher in RPS in 2010	-0.0575*		-0.0838**	
	(0.0330)		(0.0372)	
Untenured Teacher in RPS	-0.0648***		-0.00981	
	(0.0174)		(0.0188)	
First-Year Teacher in RPS in 2010		0.0202		-0.132*
		(0.0690)		(0.0728)
Second-Year Teacher in RPS in 2010		-0.0151		-0.0419
		(0.0698)		(0.0701)
Third-Year Teacher in RPS in 2010		-0.189***		-0.213***
		(0.0522)		(0.0573)
Fourth-Year Teacher in RPS in 2010		-0.113**		-0.0770
		(0.0511)		(0.0565)
First-Year Teacher in RPS		-0.163***		0.0569*
		(0.0347)		(0.0313)
Second-Year Teacher in RPS		-0.0827***		-0.0104
		(0.0256)		(0.0264)
Third-Year Teacher in RPS		-0.0173		0.0366
		(0.0252)		(0.0271)
Fourth-Year Teacher in RPS		0.0334		0.0471*
		(0.0275)		(0.0270)
Voor Eined Effects	Ves	Ves	Ves	Ves
Tear Fixea Effects	103	105	105	105
Total Number of Observations	268,279	268,279	118,536	118,536
Number of Teachers	100,446	100,446	62,875	62,875
Number of Untenured Teachers	68,742	68,742	32,511	32,511

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Notes: For Synthetic control, teacher-clustered standard errors are in parentheses. For main results, school-clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

	Dependent Variable: Dummy for Teacher Returning to Same School							
	М. ¹	D 1/	D.00. D.00		Diff-in-	Diff - No		
11	Main	Kesults	Diff-in-Diff	- Only RPS	Ienured	Teachers		
Untenurea Teacher in								
RPS in 2010	-0.0575*		-0.0961***		-0.0711**			
M 5 <i>m</i> 2010	(0.0330)		(0.0323)		(0.0349)			
First-Year	(0.0000)		(0.0020)		(0.02.15)			
Teacher in								
RPS in 2010		0.0202		-0.0341		0.00993		
		(0.0690)		(0.0678)		(0.0667)		
Second-Year								
Teacher in								
RPS in 2010		-0.0151		-0.0899		-0.0259		
		(0.0698)		(0.0702)		(0.0614)		
Third-Year								
Teacher in		0 100***		0 011***		0 001 ****		
RPS in 2010		-0.189***		-0.211***		-0.201***		
		(0.0522)		(0.0481)		(0.0545)		
Fourth-Year Togohon in								
PDS in 2010		0 112**		0 125**		0 107**		
KF 5 <i>ln</i> 2010		(0.0511)		(0.0524)		(0.0588)		
Voar Fired		(0.0311)		(0.0324)		(0.0300)		
Fffects	Ves	Ves	Ves	Ves	Ves	Ves		
District	105	105	105	105	105	105		
Fixed Effects	No	No	No	No	No	No		
Main Effects								
Included	Yes	Yes	Yes	Yes	Yes	Yes		
Total								
Number of								
Observations	268,278	268,278	3,072	3,072	148,704	148,704		
Number of								
Teachers	100,446	100,446	1,293	1,293	68,742	68,742		
Number of								
Untenured								
Teachers	68,742	68,742	873	873	68,742	68,742		

	Dependent Variable: Dummy for Teacher Returning t Same School		
	Triple Diff	ference OLS	
Untenured Teacher in RPS in 2009	0.0243 (0.0395)		
Untenured Teacher in RPS in 2008		0.0350 (0.0394)	
Year Fixed Effects	Yes	Yes	
District Fixed Effects	No	No	
Main Effects Included	Yes	Yes	
<i>Total Number of Observations</i> <i>Number of Teachers</i>	201,646 91,548	133,308 79,470	
Number of Untenured Teachers	60,275	48,925	

TABLE 1.10 - Impact of Placebo Layoffs on Untenured Teacher Retention

	Dependent Variable: Dummy for Teacher Returning to Same School		
	OLS	Logit	
Untenured Teacher in RPS in 2008	-0.0538	-0.00575	
	(0.0453)	(0.0372)	
Untenured Teacher in RPS in 2009	-0.0211	0.0362	
	(0.0426)	(0.0257)	
Untenured Teacher in RPS in 2010	-0.0870**	-0.0318	
	(0.0406)	(0.0324)	
Untenured Teacher in RPS in 2011	-0.157***	-0.140***	
	(0.0451)	(0.0477)	
Year Fixed Effects	Yes	Yes	
Total Number of Observations	328,402	328,402	
Number of Individual Teachers	177,110	177,110	
Number of Untenured Teachers	74,523	74,523	

TABLE 1.11 - Impact of Layoffs on Untenured Teacher Retention - Event Study

Notes: Teacher-clustered standard errors are in parentheses. Marginal effects for Logit regressions are reported. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

	Reading Z-Score	Math Z-Score
Percent of Teachers Untenured in 2008	-0.00240	0.00384
	(0.00157)	(0.00263)
Percent of Teachers Untenured in 2008*One Year		
After	-0.00363**	0.00344
	(0.00165)	(0.00329)
Year Fixed Effects	Yes	Yes
School Fixed Effects	Yes	Yes
Student Fixed Effects	Yes	Yes
Total Number of Observations	12,766	12,801
Number of Individual Students	8,775	8,805

TABLE 1.12 - Impact of Placebo Layoffs on Student Test Scores

	Dependent	Dependent	Dependent	Dependent	Dependent Variable:	Dependent Variable:
	Variable:	Variable:	Variable:	Variable:	Average	Average
	Percent of	Percent of	Percent of	Percent of		Lagged
	Male	Black Stord and a	Hispanic	Low-Income	Reading Z-	Math Z-
	Students	Students	Students	Students	Score	Score
Percent of	4 17- 06	0.000224	0.00100	0 002(0***	0.0154	0.0101
Untenurea	4.1/e-00	(0.000324)	(0.00108)	(0.00260^{****})	(0.0134)	(0.0191)
Darcout of	(0.000311)	(0.000794)	(0.000704)	(0.000928)	(0.0113)	(0.0123)
Ferceni oj Untenured						
Teachars in						
2010*Year of						
the Lavoffs	-0.000104	0.000494	-0.000387	-0.00136**	-0.00652	-0.00842
ine Eujojjis	(0.000288)	(0.000510)	(0.000458)	(0.000524)	(0.0104)	(0.0115)
Percent of	(0.000200)	(0.0000010)	(0.000.00)	(0.00002.)	(0.010.)	(0.0110)
Untenured						
Teachers in						
2010*Year						
After Layoffs	-6.72e-05	-6.80e-05	-0.000515	-0.000663	-0.00657	-0.00345
	(0.000340)	(0.000731)	(0.000548)	(0.000776)	(0.0230)	(0.0190)
Percent of						
Untenured						
Teachers in						
2010*2 Years						
After Layoffs	0.000202	-1.41e-05	-0.000190	-0.00158	-0.0121	0.00505
	(0.000410)	(0.000911)	(0.000763)	(0.00118)	(0.0213)	(0.0188)
Year Fixed	V	N Z	X 7	V	N Z	V
Effects	Yes	Yes	Yes	Yes	Yes	Yes
School Fixed	V	Var	Var	V	Var	V
Effects	Yes	res	res	res	res	res
1 otal Number						
0j Observations	242	242	242	242	101	101
Number of	243	243	243	243	101	101
Individual						
Schools	47	47	47	47	40	40

TABLE 1.13 - Impact of Layoffs on Student Observables

Appendix C

Figures for Chapter 2



FIGURE 2.1 - Average Teacher Absences from March 23rd to the End of the School Year

Appendix D

Tables for Chapter 2

	Panel A: Average Percent of School Days Absent During Layoff Period [†]				
	Pre-2010 Mean	2010 Mean	Difference	Diff-in-Diff	
Untenured	4.1671	3.4031	-0.7639*		
			(0.3977)		
Tenured	4.4644	3.7225	-0.7419*	-0.0221	
			(0.4020)	(0.5647)	
Young Tenured	4.6758	4.2278	-0.4479	-0.3160	
			(0.7802)	(0.8743)	
	Panel B: Average N	lumber of School I	Days Absent During	g Layoff Period [†]	
	Pre-2010 Mean	2010 Mean	Difference	Diff-in-Diff	
Untenured	1.8189	1.4634	-0.3556**		
			(0.1715)		
Tenured	1.9471	1.6007	-0.3464**	-0.0092	
			(0.1735)	(0.2436)	
Young Tenured	2.0363	1.8180	-0.2183	-0.1373	
			(0.3358)	(0.3765)	

 TABLE 2.1 - Difference-in-Difference Averages

Notes: Teacher-clustered standard errors are reported in parentheses. *†The layoff period is the time between the layoffs were announced (March 23rd) and the end of the school year (June 30).*

	[1]	[2]	[3]		
Panel A - OLS - Percent of Days Absent Over Layoff Period †					
Untenured Teacher*Layoffs Announced	-0.00934	0.351	-0.223		
	(0.746)	(0.712)	(1.968)		
Panel B - OLS - Number of Days Abse	nt Over Layoff	Period †			
Untenured Teacher*Layoffs Announced	-0.00364	0.153	-0.0862		
	(0.321)	(0.307)	(0.850)		
Panel C - POMLE - Number of Days Absent ††					
Untenured Teacher*Layoffs Announced	0.428	-0.206	-0.206		
	(0.332)	(0.393)	(0.393)		
Year Fixed Effects	Yes	Yes	Yes		
Main Effects Included	Yes	Yes	Yes		
Teacher Fixed Effects	No	No	Yes		
Linear Control Variable for Experience	Yes	No	No		
Dummy Control Variables for Experience	No	Yes	Yes		
Control for Number of School Days	Yes	Yes	Yes		
Total Number of Observations	2,012	2,012	2,012		
Number of Individual Teachers	1,584	1,584	1,584		

TABLE 2.2 - Effect of Layoff Announcement on Teacher Absences

Notes: Teacher-clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level. †The layoff period is the time between the layoffs were announced (March 23rd) and the end of the school year (June 30). ††PQMLE regressions all include teacher fixed effects and have fewer observations. The total number of observations for the PQMLE regressions is 658 and the number of individual teachers is 316.

	Dependent Variable: Number of Days Absent by Month			
	[1]	[2]	[3]	
Untenured*March 2010	0.0359	-0.0700	-0.0409	
	(0.0932)	(0.0962)	(0.102)	
Untenured*April 2010	-0.0227	-0.128	-0.111	
	(0.0780)	(0.0801)	(0.0916)	
Untenured*May 2010	0.0471	-0.0564	-0.00986	
	(0.0979)	(0.0994)	(0.107)	
Month and Year Fixed Effects	Yes	Yes	Yes	
Teacher Fixed Effects	No	No	Yes	
Linear Control Variable for Experience	Yes	No	No	
Dummy Control Variables for Experience	No	Yes	Yes	
Control for Number of School Days	Yes	Yes	Yes	
Total Number of Observations	39,985	39,985	39,985	
Number of Individual Teachers	1,799	1,799	1,799	

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Dependent Variable: Number of Days Absent			
]	[2]	[3]	
00131	-0.0255	-0.0170	
.0248)	(0.0256)	(0.0268)	
.0649***	-0.0917***	-0.0832***	
.0203)	(0.0208)	(0.0223)	
Spi	ring Break Wee	k	
1	0		
0.0133	-0.0401	-0.0316	
0.0266)	(0.0271)	(0.0283)	
0149	-0.0119	-0.00333	
.0272)	(0.0276)	(0.0288)	
0627**	0.0359	0.0445	
.0316)	(0.0321)	(0.0332)	
00926	-0.0175	-0.00901	
.0286)	(0.0290)	(0.0303)	
0.0178	-0.0447*	-0.0361	
.0259)	(0.0263)	(0.0278)	
0486***	0.0219	0.0303*	
.0149)	(0.0154)	(0.0175)	
es	Yes	Yes	
0	No	Yes	
es	No	No	
0	Yes	Yes	
es	Yes	Yes	
57.720	167.720	167.720	
799	1.799	1.799	
	00131 .0248) .0649*** .0203) Sp: .0133 .0266) 0149 .0272) 0627** .0316) 00926 .0286) .0178 .0259) 0486*** .0149) es o es o es o es	$\begin{array}{c c c c c c c c c c c c c c c c c c c $	

TABLE 2.4 - Effect of Lavoff Announcement on Teach	her Absences by	Week
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	Dependent Variable: Number of Days Absent			
	[1]	[2]	[3]	
Untenured*March 23	-0.0105	-0.0175**	-0.0165*	
	(0.00841)	(0.00849)	(0.00864)	
Untenured*March 24	-0.00277	-0.00967	-0.00874	
	(0.0112)	(0.0113)	(0.0114)	
Untenured*March 25	0.00356	-0.00334	-0.00241	
	(0.0108)	(0.0110)	(0.0111)	
Untenured*March 26	0.0489***	0.0416***	0.0426***	
	(0.0107)	(0.0108)	(0.0110)	
Untenured*March 29	-0.000956	-0.00785	-0.00693	
	(0.0115)	(0.0116)	(0.0118)	
Untenured*March 30	0.00930	0.00276	0.00362	
	(0.00897)	(0.00908)	(0.00922)	
Untenured*March 31	-0.0120**	-0.0190***	-0.0180***	
	(0.00605)	(0.00624)	(0.00647)	
Untenured*April 1	-0.0232***	-0.0301***	-0.0292***	
	(0.00464)	(0.00487)	(0.00514)	
Day and Year Fixed Effects	Yes	Yes	Yes	
Teacher Fixed Effects	No	No	Yes	
Linear Control Variable for Experience	Yes	No	No	
Dummy Control Variables for Experience	No	Yes	Yes	
Control for Number of School Days	Yes	Yes	Yes	
Total Number of Observations	763,633	763,633	763,633	
Number of Individual Teachers	1,809	1,809	1,809	

 TABLE 2.5 - Effect of Layoff Announcement on Teacher Absences by Day

	Dependent Variable: Number of Days Absent			
	[1]	[1]	[1]	
Bottom School Tercile Untenured Teacher in				
2010	0.141	0.168	0.359	
	(0.162)	(0.164)	(0.247)	
Second School Tercile Untenured Teacher in				
2010	0.0442	0.0466	0.00780	
	(0.0523)	(0.0529)	(0.0754)	
Top School Tercile Untenured Teacher in 2010	0.000403	0.000333	0.00329	
	(0.00730)	(0.00762)	(0.0100)	
Annual Fixed Effects	Yes	Yes	Yes	
School Fixed Effects	Yes	Yes	Yes	
Teacher Fixed Effects	Yes	Yes	Yes	
Linear Control Variable for Experience	Yes	No	Yes	
Dummy Control Variables for Experience	No	Yes	No	
Control for Number of School Days	Yes	Yes	Yes	
Total Number of Observations	3,481	3,481	3,481	
Number of Individual Teachers	2,594	2,594	2,594	

TABLE 2.6 - Effect of Layoff Announcement on Teacher Absences by Tercile of School by **Annual Teacher Absences** 1 CD

Notes: Teacher-clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level. No constant is included in these regressions.

	Dependent Variable: Number of School Absent		
	[1]	[2]	[3]
Untenured Math Teacher*Layoff Period	0.0681	0.114	0.527
	(0.617)	(0.618)	(1.861)
Untenured Science Teacher*Layoff Period	-0.182	-0.157	-1.248*
	(0.437)	(0.450)	(0.664)
Untenured Reading Teacher*Layoff Period	0.274	0.319	0.511
	(0.474)	(0.466)	(0.940)
Untenured Self-Contained Elementary			
Teacher*Layoff Period	1.445	1.506	0.923
	(0.999)	(0.990)	(2.692)
Untenured Other Subject Teacher*Layoff Period	-0.229	-0.161	-0.536
	(0.304)	(0.294)	(0.770)
Annual Fixed Effects	Yes	Yes	Yes
Main Effects Included	Yes	Yes	Yes
Teacher Fixed Effects	No	No	Yes
Linear Control Variable for Experience	Yes	No	No
Dummy Control Variables for Experience	No	Yes	Yes
Control for Number of School Days	Yes	Yes	Yes
Total Number of Observations	2,012	2,012	2,012
Number of Individual Teachers	1,584	1,584	1,584

TADLE 2.7 - Effect of Lavon Announcement of Teacher Absences by Subject Taugh	TABLE 2.7 ·	- Effect of Lave	off Announcement	on Teacher	Absences by	y Subject Taught
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	Dependen	Dependent Variable: Number of Days			
	Absent [1]	[2]	[3]		
First-Year Teacher*Layoffs Announced	-0.209	-0.122	0.306		
	(0.324)	(0.314)	(3.403)		
Second-Year Teacher*Layoffs Announced	0.332	0.417	0.855		
	(0.412)	(0.405)	(0.929)		
Third-Year Teacher*Layoffs Announced	0.210	0.297	-0.000328		
	(0.606)	(0.601)	(1.328)		
Fourth-Year Teacher*Layoffs Announced	0.0196	0.0976	-1.065		
	(0.406)	(0.398)	(0.949)		
Annual Fixed Effects	Yes	Yes	Yes		
Teacher Fixed Effects	No	No	Yes		
Linear Control Variable for Experience	Yes	No	No		
Dummy Control Variables for Experience	No	Yes	Yes		
Control for Number of School Days	Yes	Yes	Yes		
Total Number of Observations	2,012	2,012	2,012		
Number of Individual Teachers	1,584	1,584	1,584		

 TABLE 2.8 - Effect of Layoff Announcement on Teacher Absences by Teacher Experience

	Dependent V	ariable: Number of Days Absent
	[1]	[2]
<i>Reading VAM Tercile 1 Untenured Teacher in Year of Layoffs</i>	6.352**	
<i>Reading VAM Tercile 2 Untenured Teacher in Year of Layoffs</i>	(2.516) 0.269 (5.656)	
<i>Reading VAM Tercile 3 Untenured Teacher in Year of Layoffs</i>	0.135	
Math VAM Tercile 1 Untenured Teacher in Year of Layoffs		4.903 (4.327)
Math VAM Tercile 2 Untenured Teacher in Year of Layoffs		-16.55
Math VAM Tercile 3 Untenured Teacher in Year of Layoffs		(12.27) -4.493 (7,200)
Annual Fixed Effects	Yes	Yes
Teacher Fixed Effects	Yes	Yes
Linear Control Variable for Experience	No	No
Dummy Control Variables for Experience	Yes	Yes
Control for Number of School Days	Yes	Yes
Total Number of Observations	233	234
Number of Individual Teachers	196	197

TABLE 2.9 - Effect of Layoff Announcement on Teacher Absences by Value-Added Tercile

Notes: Teacher-clustered standard errors are in parentheses. ***, **, and * *indicate statistical significance at the 1%, 5%, and 10% level. No constant is included in these regressions.*

	Depende Number	nt Variabl of Days A	e: bsent
	[1]	[2]	[3]
Panel A - Ordinary Least Squares			
Untenured Teacher*Layoffs Announced	-0.165	0.0529	-0.535
	(0.357)	(0.349)	(0.979)
Untenured Teacher in Same Position the Following Year*Layoffs	0.105	0.0392	0.194
	(0.370)	(0.375)	(1.172)
Panel B - Poisson Quasi-Maximum Likelihood E	stimation		
Untenured Teacher*Layoffs Announced	0.178	-0.387	-0.387
	(0.325)	(0.414)	(0.414)
Untenured Teacher in Same Position the Following Year*Layoffs	0.313	0.0887	0.0887
	(0.531)	(0.502)	(0.502)
Annual Fixed Effects	Yes	Yes	Yes
Main Effects Included	Yes	Yes	Yes
Teacher Fixed Effects	No	No	Yes
Linear Control Variable for Experience	Yes	No	No
Dummy Control Variables for Experience	No	Yes	Yes
Control for Number of School Days	Yes	Yes	Yes
Total Number of Observations	2,012	2,012	2,012
Number of Individual Teachers	1,584	1,584	1,584

TABLE 2.10 - Estimation of Tip-Off Effect on Teacher Attendance

	Depende	nt Variable: 1	Number of Day	s Absent
	Main Results	Percent of School Days Absent	All Tenured Teacher Control	2009 Placebo
Untenured*Week of Layoff Announcement	-0.0170	0.0915	-0.00515	Spring Break
Untenured*Week 1 of April 2010	(0.0268) -0.0832*** (0.0223)	(0.566) -2.090*** (0.527)	(0.0240) -0.0744*** (0.0196)	Week -0.0227 (0.0252)
Untenured*Week 2 of April 2010	Sp	0.0404 (0.0311)		
Untenured*Week 3 of April 2010	-0.0316 (0.0283)	-0.711 (0.583)	-0.0171 (0.0263)	0.0423 (0.0307)
Untenured*Week 4 of April 2010	-0.00333	-0.146 (0.594)	0.0104	0.0741**
Untenured*Week 1 of May 2010	0.0445	0.810	0.0489	0.0177
Untenured*Week 2 of May 2010	(0.0332) -0.00901 (0.0202)	-0.260	0.000765	(0.0294) 0.0326 (0.0202)
Untenured*Week 3 of May 2010	-0.0361	-0.885	-0.0270	(0.0293) 0.0268 (0.0265)
Untenured*Week 4 of May 2010	(0.0278) 0.0303* (0.0175)	(0.384) 0.782 (0.935)	(0.0253) 0.0378** (0.0150)	(0.0203) 0.0727*** (0.0207)
Week and Year Fixed Effects	Yes	Yes	Yes	Yes
Teacher Fixed Effects	Yes	No	No	No
Linear Control Variable for Experience	No	No	No	No
Dummy Control Variables for Experience	Yes	Yes	Yes	Yes
Control for Number of School Days	Yes	Yes	Yes	Yes
Total Number of Observations	167,720	167,720	305,188	127,379
Number of Individual Teachers	1,799	1,799	2,832	1,654

TABLE 2.11 - Effect of Layoff Announcement on Teacher Absences by Week - Placebo and Robustness

	Dependent Var	riable: Number of Days
	Main Results	Absent 2009 Placebo
Untenured*March 16	Walli Results	-0.00385
omenarea maren 10		(0.0103)
Untenured*March 17		0.00290
		(0.00290)
Untenured*March 18		0.0200**
		(0.00969)
Untenured*March 19		0.00742
		(0.00712)
Untenured*March 20		0.0200**
		(0.00851)
Untenured*March 23	-0.0165*	(0.00001)
	(0.00864)	
Untenured*March 24	-0.00874	
	(0.0114)	
Untenured*March 25	-0.00241	
	(0.0111)	
Untenured*March 26	0.0426***	
	(0.0110)	
Untenured*March 29	-0.00693	0.0141
	(0.0118)	(0.00862)
Untenured*March 30	0.00362	0.00334
	(0.00922)	(0.00909)
Untenured*March 31	-0.0180***	0.00237
	(0.00647)	(0.00774)
Untenured*April 1	-0.0292***	-0.00172
	(0.00514)	(0.00782)
Day and Year Fixed Effects	Yes	Yes
Teacher Fixed Effects	No	Yes
Linear Control Variable for Experience	Yes	No
Dummy Control Variables for Experience	No	Yes
Control for Number of School Days	Yes	Yes
Total Number of Observations	763,633	763,633
Number of Individual Teachers	1,809	1,809

 TABLE 2.12 - Effect of Layoff Announcement on Teacher Absences by Day - Placebo

Appendix E

Figures for Chapter 3



FIGURE 3.1 - Case-Shiller House Price Index for Greater Los Angeles

FIGURE 3.2 - Distribution of House Sales by Census Block During Sample Period Conditional on Census Block Having Any Sales



Appendix F

Tables for Chapter 3

A. Schools by Grade Level						
	Elementary	Middle	High	Multiple Levels		
Non-charter public schools	1,196	243	390	68		
Total charter schools	113	48	88	35		
% Charter Schools	8.6%	16.5%	18.4%	34.0%		
		1	10	2		
Conversion charters	21	1	10	3		
Start-up charters	92	47	78	32		
	B. Schools by Years	of Operation				
	Non-Charter			% Charter		
	Public	Conversion	Start-up	Schools		
2008	1,743	19	127	7.7%		
2009	1,758	23	147	8.8%		
2010	1,777	24	181	10.3%		
2011	1,809	26	213	11.7%		

TABLE 3.1 - Schools in LA County

Note: Schools included in panel A are those open and active at any point September 2008 through September 2011. Data obtained from California Department of Education.

Property Characteristics	
Sale price	383,546
	(247,685)
# of Beds	2.98
	(1.05)
# of Baths	2.11
	(0.92)
Square Footage	1,573
	(718)
Quality	6.45
	(1.25)
Number of Charters	
0 - 0.5 miles	0.16
	(0.54)
0.5 - 1 mile	0.47
	(1.11)
1 - 1.5 miles	0.78
	(1.59)
1.5 - 2 miles	1.06
	(2.04)
Charters as percentage of enrollment	
0 - 0.5 miles	0.05
	(0.18)
0.5 - 1 mile	0.06
	(0.15)
1 - 1.5 miles	0.06
	(0.13)
1.5 - 2 miles	0.06
	(0.12)
Observations	158,211

TABLE 3.2 - Summary	v Statistics	of Propertie	es with	Sale	Prices
	Dualistics	or i roperti		Daic.	

Notes: Summary statistics are means for sales from September 2008 through September 2011. Property sample excludes homes with a sale price exceeding \$1.5 million, and a bedroom or bathroom count in excess of eight. Homes are divided into the "LAUSD" or "Rest of LA County" samples via the location of the elementary school to which the property is zoned. Standard deviations in parentheses.

	A: Characteristics of zoned school		B: Characteristics of charters within 1 mile (enrollment			
					weighted)	
	Elementary	Middle	High	Elementary	Middle	High
Enrollment	440.5	1,197.4	2,002.6	443.0	1,121.3	1,140.5
	(165.6)	(488.0)	(680.6)	(138.0)	(435.7)	(814.3)
API Score	805.6	746.1	707.0	800.1	744.7	663.7
	(73.6)	(90.3)	(88.2)	(64.5)	(93.0)	(112.4)
% Black	10.9	10.1	11.2	10.6	10.3	11.1
	(14.4)	(11.9)	(13.3)	(13.2)	(11.6)	(12.6)
% Hispanic	58.2	62.9	60.1	62.0	63.6	65.2
	(28.5)	(24.7)	(24.8)	(25.7)	(25.2)	(23.9)
% Asian	7.1	7.0	7.6	7.2	7.7	5.9
	(12.6)	(12.0)	(12.3)	(12.0)	(13.2)	(11.5)
% Disabled	11.4	11.4	10.3	11.8	11.1	10.6
	(4.5)	(2.8)	(3.1)	(4.1)	(2.7)	(12.2)
% Gifted	8.4	13.8	11.4	7.5	12.6	7.3
	(7.3)	(9.7)	(8.8)	(5.4)	(8.6)	(7.0)
% Free or Reduced Price	64.7	66.9	55.9	68.7	67.5	61.2
Lunch	(30.0)	(25.7)	(26.9)	(26.6)	(26.6)	(24.1)
% English Language	28.1	19.8	18.0	30.3	20.2	20.8
Learner	(17.2)	(11.5)	(10.5)	(15.2)	(11.7)	(12.2)
Observations	158,211	127,558	141,212	136,546	81,204	83,079
for api score:	158,211	127,174	140,866	136,536	80,686	80,979

TABLE 3.3 - Summary Statistics - Schools Near Properties with Sale Prices

Notes: Summary statistics are means for sales from September 2008 through September 2011. Sample excludes homes with a sale price exceeding \$1.5 million, and a bedroom or bathroom count in excess of eight. School zones are based on 2002 zoning. See text for details on how to access school zone maps. Standard deviations in parentheses.

			LA COL	inty			
				Charter s	eats as perc	entage of	
	Number of charters			enrollment			
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	
A. Distance gradient							
0 - 0.5 miles	-0.00725	-0.0353***	-0.00543	0.0741*	0.00648	-0.00134	
	(0.0131)	(0.00800)	(0.00827)	(0.0438)	(0.0249)	(0.0194)	
0.5 - 1 mile	0.00858	-0.0253***	0.000950	0.117**	-0.0166	-0.0128	
	(0.00748)	(0.00609)	(0.00476)	(0.0564)	(0.0270)	(0.0195)	
1 - 1.5 miles	0.0252***	-0.0149***	0.00223	0.140**	-0.0442*	-0.0123	
	(0.00578)	(0.00387)	(0.00313)	(0.0616)	(0.0268)	(0.0239)	
1.5 - 2 miles	0.0239***	-0.00460	-0.00110	0.120	-0.0217	-0.00470	
	(0.00494)	(0.00309)	(0.00279)	(0.0770)	(0.0340)	(0.0255)	
B. Condensed 0-2	· · · · · ·	· · · ·			× /		
miles							
0 - 2 miles	0.0193***	-0.0101***	-9.80e-05	0.328***	-0.0301	-0.00750	
	(0.00321)	(0.00255)	(0.00207)	(0.112)	(0.0609)	(0.0544)	
Observations	158.211	158.211	158.211	158.211	158.211	158.211	
Housing							
Characteristics	Y	Y	Y	Y	Y	Y	
School							
Characteristics	Y	Y	Y	Y	Y	Y	
School Fixed-Effects	Ν	Y	Ν	Ν	Y	Ν	
Census Block Fixed-							
Effects	Ν	Ν	Y	Ν	Ν	Y	

 TABLE 3.4 - Effect of Charters on Log Sale Prices for Los Angeles County

Sample includes property sales from April 2009 through September, 2011. The independent variable denotes either the number of charters in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Housing characteristics include number of bedrooms, bathrooms, square footage, and quality. School characteristics include API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels for elementary school zoned to the property in 2002. All regressions include month-by-year fixed-effects. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.
	LAUSD		Re	est of LA County
_	Number of	Charter seats as percentage	Number of	Charter seats as percentage
	charters	of enrollment	charters	of enrollment
0 - 0.5 miles	0.000504	0.00923	-0.0143	-0.0220
	(0.00970)	(0.0236)	(0.0169)	(0.0373)
0.5 - 1 mile	0.00591	-0.00620	-0.00447	-0.0114
	(0.00559)	(0.0278)	(0.00869)	(0.0241)
1 - 1.5 miles	0.00712**	-0.00607	-0.00225	0.000579
	(0.00349)	(0.0314)	(0.00632)	(0.0351)
1.5 - 2 miles	0.00233	-0.0130	-0.00375	0.0259
	(0.00342)	(0.0408)	(0.00470)	(0.0206)
Observations	65,170	65,170	93,041	93,041
R-squared	0.83	0.83	0.91	0.91
Housing Characteristics	Y	Y	Y	Y
School Characteristics	Y	Y	Y	Y
School Fixed-Effects	Ν	Ν	Ν	Ν
Census Block Fixed- Effects	Y	Y	Y	Y

TABLE 3.5 - Effect of Charters on Log Sale Prices for Los Angeles County by School District

Notes: See Table 3.4 for a description of baseline sample and controls. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	LA County		
_		Charter seats as percentage of	
_	Number of charters	enrollment	
Start-up charters			
0 - 0.5 miles	-0.00450	0.00389	
	(0.00952)	(0.0226)	
0.5 - 1 mile	0.00253	0.000917	
	(0.00537)	(0.0241)	
1 - 1.5 miles	0.00341	0.00269	
	(0.00357)	(0.0240)	
1.5 - 2 miles	-0.00110	0.0273	
	(0.00299)	(0.0234)	
Conversion charters			
0 - 0.5 miles	-0.0137	-0.0226	
	(0.0133)	(0.0362)	
0.5 - 1 mile	-0.0110	-0.0432	
	(0.0103)	(0.0311)	
1 - 1.5 miles	-0.00664	-0.0397	
	(0.00854)	(0.0456)	
1.5 - 2 miles	-0.00225	-0.0506	
	(0.00788)	(0.0513)	
Observations	158,211	158,211	
R-squared	0.881	0.881	
Housing Characteristics	Y	Y	
School Characteristics	Y	Y	
School Fixed-Effects	Ν	Ν	
Census Block Fixed-Effects	Y	Y	

TABLE 3.6 - Effect of Charters on Log Sale Prices by Charter Type

Notes: See Table 3.4 for a description of baseline sample and controls. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	Number of charters	Charter seats as percentage of enrollment
0 - 0.5 miles	-0.0387	-0.118***
	(0.0284)	(0.0431)
0.5 - 1 mile	-0.0123	-0.0786*
	(0.0149)	(0.0409)
1 - 1.5 miles	-0.0178*	-0.0758*
	(0.0098)	(0.0433)
1.5 - 2 miles	-0.0186	-0.0670
	(0.0156)	(0.0541)
B. Condensed 0-2 miles		
0 - 2 miles	-0.0192**	-0.0292
	(0.0090)	(0.1540)
Observations	93,041	93,041
Housing Characteristics	Y	Y
School Characteristics	Y	Y
School Fixed-Effects	Ν	Ν
Census Block Fixed-Effects	Y	Y

TABLE 3.7 - Effect of Charters Within the Home's School District on Log Sale Prices for Los Angeles County Excluding LAUSD

Sample includes property sales from April 2009 through September, 2011. The independent variable denotes either the number of charters within the home's zoned school district in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Housing characteristics include number of bedrooms, bathrooms, square footage, and quality. School characteristics include API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels for elementary school zoned to the property in 2002. All regressions include month-by-year fixed-effects. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	LA County			
Number of charters	Square footage	# of Beds	# of Baths	Quality
0 - 0.5 miles	3.899	0.0147	0.0186	0.0124
	(14.0)	(0.0247)	(0.0192)	(0.0172)
0.5 - 1 mile	-6.593	-0.0112	-0.0183	-0.0096
	(7.4)	(0.0147)	(0.0112)	(0.0098)
1 - 1.5 miles	4.954	0.0116	0.0045	-0.0086
	(5.0)	(0.0098)	(0.0074)	(0.0066)
1.5 - 2 miles	-2.357	-0.0015	0.0001	-0.0120*
	(4.8)	(0.0097)	(0.007)	(0.0065)
Observations	158,211	158,211	158,211	158,211
Charter seats as percentage of	Square footage	# of Beds	# of Baths	Quality
enrollment				-
0 - 0.5 miles	-22.710	-0.005	0.023	0.025
	(37.3)	(0.048)	(0.040)	(0.046)
0.5 - 1 mile	25.590	0.011	0.023	-0.037
	(40.6)	(0.062)	(0.050)	(0.05)
1 - 1.5 miles	28.490	0.011	-0.009	-0.021
	(39.5)	(0.057)	(0.051)	(0.050)
1.5 - 2 miles	-23.360	-0.044	-0.046	0.000
	(43.2)	(0.052)	(0.052)	(0.049)
Observations	158,211	158,211	158,211	158,211
R-squared	0.67	0.55	0.59	0.80
School Fixed-Effects	Ν	Ν	Ν	Ν
Census Block Fixed-Effects	Y	Y	Y	Y

TABLE 3.8 - Impacts of Charters on Exogenous Observables

Notes: See Table 3.4 for a description of baseline sample. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	LA	County
	Number of charters	Charter seats as percentage of enrollment
36 months prior to sale	-0.0005	-0.001
-	(0.0100)	(0.027)
24 months prior to sale	-0.0039	-0.017
-	(0.0093)	(0.031)
12 months prior to sale	-0.0055	-0.008
-	(0.0089)	(0.030)
Time of sale	-0.0010	0.006
	(0.0088)	(0.031)
12 months after sale	0.0038	0.033**
	(0.0054)	(0.015)
24 months after sale	-0.0082	-0.019
	(0.0080)	(0.025)
36 months after sale	0.0190*	0.013
	(0.0102)	(0.037)
Observations	158,211	158,211
R-squared	0.88	0.88

TABLE 3.9 - Effect of Lags and Leads of Charter Penetration

Notes: See Table 3.4 for a description of baseline sample and controls. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	Quantity of House Sales within Census Block				
	Counting Most Recent		Counting	Counting Three Most	
	Hou	ise Sale	Recent H	House Sales	
		Charter seats		Charter	
		as		seats as	
	Number	percentage	Number	percentage	
	of	of	of	of	
	Charters	enrollment	Charters	enrollment	
0 - 0.5 miles	0.0828	0.354	0.0901	0.346	
	(0.168)	(0.414)	(0.174)	(0.417)	
0.5 - 1 mile	0.119	0.346	0.130	0.391	
	(0.110)	(0.533)	(0.110)	(0.537)	
1 - 1.5 miles	0.139*	0.581	0.171**	0.801	
	(0.0776)	(0.647)	(0.0794)	(0.721)	
1.5 - 2 miles	0.0972*	-0.0104	0.121**	0.120	
	(0.0547)	(0.607)	(0.0554)	(0.659)	
Observations	87,683	87,683	87,683	87,683	
Census Tract Fixed-					
Effects	Ν	Ν	Ν	Ν	
Census Block Fixed-					
Effects	Y	Y	Y	Y	

TABLE 3.10 - Relationship Between Charter Penetration and the Number of
Annual House Sales in Census Block

Sample includes property sales from September 2008 through September, 2011. The independent variable denotes either the number of charters in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

Appendix G

Supplemental Figures for Chapter 1



FIGURE 1.A1 - Trends in Teachers Switching Schools



FIGURE 1.A2 - Trends in Teachers Leaving the District

Appendix H

Supplemental Tables for Chapter 1

	Reading		Math	
	Single-Lag VAM	Double-Lag VAM	Single-Lag VAM	Double-Lag VAM
2007-2008	-0.1680	-	-0.0786	-
2008-2009	-0.1808	-0.3569	-0.0979	-0.6404
2009-2010	-0.1831	-0.3723	-0.0936	-0.6497
2010-2011	-0.2332	-0.4050	-0.1167	-0.6577
2011-2012	-0.2293	-0.4009	-0.1205	-0.6666

 TABLE 1.A1 - Average RPS Teacher Value-Added (VAM) Scores Over Time

1	Tobability			
	Dependent Varia Returnin	Dependent Variable: Dummy for Teacher Returning to Same School		
	OLS	Logit		
First Year Superintendant	-0.0100*	-0.00995*		
	(0.00535)	(0.00521)		
Year Fixed Effects	Yes	Yes		
Total Number of Observations	125,843	125,843		
Number of Untenured Teachers	55,123	55,123		

TABLE 1.A2 - Effect of First-Year Superintendant on Untenured Teachers Retention Probability

Notes: Teacher-clustered standard errors are in parentheses. Marginal effects for Logit regressions are reported. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

	Dependent Variable: Dummy for Teacher Being Rehired in Same Position OLS w/		
	OLS	Logit	District FE
Untenured Female Teacher in RPS in 2010	0.0420	0.0516	0.0477
	(0.0754)	(0.0694)	(0.0757)
Untenured White Teacher in RPS in 2010	-0.0573	-0.0284	-0.0600
	(0.0688)	(0.0630)	(0.0689)
Untenured Teacher Log Salary in RPS in 2010	-0.115	-0.0844	-0.0966
	(0.151)	(0.0734)	(0.151)
Year Fixed Effects	Yes	Yes	Yes
District Fixed Effects	No	No	Yes
Main Effects Included	Yes	Yes	Yes
Total Number of Observations	267,054	267,054	267,054
Number of Teachers	99,957	99,957	99,957
Number of Untenured Teachers	68,296	68,296	68,296

TABLE 1.A3 - Impact of Layoffs on Untenured Teacher Retention Probability - Teacher Demographics

Notes: Teacher-clustered standard errors are in parentheses. Marginal effects for Logit regressions are reported. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

	Teacher Outcome		
	Same School	Left District	Switched Schools
First-Year Teacher			
in RPS in 2010	0.0055	0.0103	-0.0159
	(0.0361)	(0.0236)	(0.0250)
Second-Year Teacher			
in RPS in 2010	-0.0056	-0.0042	0.0098
	(0.0379)	(0.0271)	(0.0258)
Third-Year Teacher			
in RPS in 2010	-0.1255***	0.0643***	0.0612***
	(0.0311)	(0.0230)	(0.0207)
Fourth-Year Teacher			
in RPS in 2010	-0.0969**	0.0434	0.0535**
	(0.0421)	(0.0361)	(0.0259)
First-Year Teacher in RPS	-0.0861***	0.0494***	0.0367***
	(0.0189)	(0.0146)	(0.0123)
Second-Year Teacher in RPS	-0.0517***	0.0409***	0.0108
	(0.0181)	(0.0144)	(0.0122)
Third-Year Teacher in RPS	-0.0058	-0.0004	0.0062
	(0.0217)	(0.0178)	(0.0145)
Fourth-Year Teacher in RPS	0.0462	-0.0347	-0.0115
	(0.0301)	(0.0264)	(0.0190)
Year Fixed Effects		Yes	
Total Number of Observations		268,279	
Number of Teachers		100,446	
Number of Untenured Teachers		68,742	

TABLE 1.A4 - Multinomial Logit Estimation of Teacher Outcomes – Experience Heterogeneity

Notes: School-clustered standard errors are in parentheses. In the main regression, Same School is the omitted category and then is backed out from the results. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

	Untenured Leavers in 2010	New School in 2011	New to RPS in 2011			
	N=80	N=296	N=142			
Salary	\$43,536.18	\$43,916.86	\$41,656.63			
District Experience	2.34	1.89	1.00			
Total Experience	6.55	4.24	3.43			
Female	0.71	0.76	0.73			
White	0.83	0.82	0.84			
Black	0.10	0.06	0.04			
Hispanic	0.05	0.08	0.09			
Have Advanced	0.41	0.45	0.42			
Degree						
Selective	0.04	0.03	0.04			
Baccalaureate College						
Reading Value-	-0.18	-0.28	-0.32			
Added [†]						
Math Value-Added [†]	-0.10	-0.12	-0.14			

TABLE 1.A5 - Summary Statistics of Leaving Teachers in the Year of the Layoffs and New Hires the Following Year

 $^{+}$ The sample sizes for the subsets of teachers with value-added scores are Leavers = 17, New School = 44, and New to RPS = 18.

District	Percent
Byron	25.60%
City of Chicago	20.80%
Galesburg	0.80%
North Boone	1.80%
Plano	0.90%
Triad	25.80%
Wauconda	1.40%
Waukegan	1.10%
Other Illinois Districts (each between 0.1 and 0.6%)	21.80%
Total	100.00%

 TABLE 1.A6 - Composition of RPS Synthetic Control

Appendix I

Supplemental Tables for Chapter 2

v	Dependent Variable: Number of Days Absent Over Layoff Period		
	[1]	[2]	[3]
Panel A - OLS - H	Percent of Days Ab	sent Over Layoff Per	riod †
Untenured Teacher*Layoffs			
Announced	0.179	0.120	0.622
	(0.682)	(0.666)	(0.855)
Untenured Teacher*Layoffs			
Rumored	-0.768***	-0.870**	-0.642
	(0.296)	(0.340)	(0.473)
Panel B - OLS - N	lumber of Days Ab	sent Over Layoff Pe	riod †
Untenured Teacher*Layoffs			
Announced	0.488	0.319	0.741
	(0.319)	(0.341)	(0.639)
Untenured Teacher*Layoffs			
Rumored	-1.107***	-1.321***	-1.129**
	(0.378)	(0.413)	(0.520)
Panel C - J	PQMLE - Number	of Days Absent	
Untenured Teacher*Layoffs	0.000**	0.154	0.154
Announced	0.392**	0.154	0.154
	(0.188)	(0.209)	(0.209)
Untenured Teacher*Layoffs	0.0459	0.102*	0.102*
Rumorea	0.0438	-0.192*	-0.192*
	(0.0783)	(0.116) V	(0.116)
Annual Fixed Effects	Yes	Yes	Yes
Main Effects Included	Yes	Yes	Yes
Teacher Fixed Effects	No	No	Yes
Linear Control Variable for Experience	Yes	No	No
Dummy Control Variables for Experience	No	Yes	Yes
Total Number of Observations	5,518	5,518	5,518
Number of Individual Teachers	1,790	1,790	1,790

Notes: Teacher-clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level. †The layoff period is the time between the layoffs were announced (March 23rd) and the end of the school year (June 30). ††PQMLE regressions all include teacher fixed effects and have fewer observations. The total number of observations for the PQMLE regressions is 4,616 and the number of individual teachers is 1,420.

	Dependent	Variable: Number	r of Days Absent
	[1]	[2]	[3]
Panel A - Main Results - Dis	trict Experience	e Definition of Te	nure
Untenured Teacher*Layoffs Announced	0.0302	0.166	-0.150
	(0.239)	(0.252)	(0.680)
Panel B - State Exp	erience Definiti	on of Tenure	
Untenured Teacher*Layoffs Announced	0.0752	0.216	-0.877
	(0.299)	(0.302)	(1.021)
Panel C - Total Exp	erience Definiti	on of Tenure	
Untenured Teacher*Layoffs Announced	-0.00404	0.138	-0.687
	(0.307)	(0.320)	(0.966)
Annual Fixed Effects	Yes	Yes	Yes
Main Effects Included	Yes	Yes	Yes
Teacher Fixed Effects	No	No	Yes
Linear Control Variable for Experience	Yes	No	No
Dummy Control Variables for Experience	No	Yes	Yes
Total Number of Observations	2,012	2,012	2,012
Number of Individual Teachers	1,584	1,584	1,584

TABLE 2.A2	- Robustness	of Main	Results to	Tenure S	pecification
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Notes: Teacher-clustered standard errors are in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level.

Appendix J

Supplemental Tables for Chapter 3

	Characteristics	
	Count of Open Charters	Count of Open Charters
	within Local School Zone	within Local School Zone
Enrollment	-0.0001	-0.0001
	(0.0001)	(0.0003)
API Score	-0.0010	0.0006
	(0.0007)	(0.0006)
Percent Black	-0.0030	0.0118**
	(0.0036)	(0.0053)
Percent Native American	0.0363	-0.0004
	(0.0346)	(0.0037)
Percent Asian	-0.0138***	0.0041
	(0.0025)	(0.0030)
Percent Filipino	-0.0204**	-0.0093
	(0.0095)	(0.0069)
Percent Hispanic	-0.00920***	0.0019
	(0.0027)	(0.0027)
Percent Pacific Islander	-0.0340*	-0.0167
	(0.0201)	(0.0137)
Percent Gifted	0.0479***	0.0031
	(0.0082)	(0.0025)
Percent Free or Reduced Lunch	-0.0006	0.00340*
	(0.0020)	(0.0018)
Percent ELL	0.00680***	-0.0007
	(0.0024)	(0.0016)
Percent Disabled	0.00603*	0.00520**
	(0.0035)	(0.0022)
Percent HS Graduate	-0.0043	0.0001
	(0.0027)	(0.0007)
Percent Bachelors Degree	-0.0015	0.00216*
	(0.0049)	(0.0012)
Percent Graduate School	-0.0034	-0.0040
	(0.0049)	(0.0034)
Observations	5,858	5,858
R-squared	0.126	0.974
School Fixed-Effects	Ν	Y
Census Block Fixed-Effects	Ν	Ν

TABLE 3.A1 - Relationship Between Charters in a School Zone and Elementary School
Characteristics

Notes: See Table 3.4 for a description of baseline sample. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

		Litesbj		
	Median Household	Income in	API Score of Zoned	Elementary
	Census Tra	act	School	
		Charter		Charter
		seats as %		seats as %
		of		of
	Number of charters	enrollment	Number of charters	enrollment
0 - 0.5 miles x	-0.0216	0.0788	-0.00899	-0.00894
Tercile 1	(0.0297)	(0.107)	(0.0260)	(0.0608)
0 - 0.5 miles x	0.00447	-0.0713	-0.0266	-0.0440
Tercile 2	(0.0366)	(0.0802)	(0.0204)	(0.0378)
0 - 0.5 miles x	-0.00412	-0.0401	0.0432*	0.0222
Tercile 3	(0.0183)	(0.0304)	(0.0236)	(0.0647)
0.5 - 1 mile x	-0.0221*	0.0349	-0.0101	0.0294
Tercile 1	(0.0133)	(0.0590)	(0.0131)	(0.0352)
0.5 - 1 mile x	-5.21e-05	0.0222	-0.00150	0.00373
Tercile 2	(0.0186)	(0.0526)	(0.00952)	(0.0536)
0.5 - 1 mile x	0.0119	-0.0146	-0.00128	-0.0299
Tercile 3	(0.0126)	(0.0379)	(0.0131)	(0.0458)
1 - 1.5 miles x	0.00495	0.0608	-0.00461	0.00164
Tercile 1	(0.0125)	(0.0941)	(0.00937)	(0.0440)
1 - 1.5 miles x	-0.0154	-0.0164	0.00475	0.0273
Tercile 2	(0.0109)	(0.0554)	(0.00823)	(0.0631)
1 - 1.5 miles x	-0.00496	0.0127	-0.0162	0.0267
Tercile 3	(0.0108)	(0.0572)	(0.00985)	(0.0572)
1.5 - 2 miles x	-0.00514	0.0439	-0.00279	0.0284
Tercile 1	(0.00843)	(0.0537)	(0.00720)	(0.0356)
1.5 - 2 miles x	-0.000721	0.0520	-0.00480	-0.00102
Tercile 2	(0.00998)	(0.0542)	(0.00620)	(0.0382)
1.5 - 2 miles x	0.00205	0.00948	0.00592	0.0817**
Tercile 3	(0.00771)	(0.0301)	(0.00814)	(0.0362)
Observations	93 041	93 041	93 041	93 041
R-squared	0.881	0 880	0.880	0 880
Housing	0.001	0.000	0.000	0.000
Characteristics	V	V	V	V
School	1	1	1	1
Characteristics	V	V	V	V
School Fixed	I	1	1	1
Effects	N	N	N	N
Census Rlock	T A	1 N	1 N	1 N
Fixed-Effects	V	V	V	V
I IACU-LIIUUIS	1	1	1	1

TABLE 3.A2 - Heterogeneity by Neighborhood Income and Public School API (Excluding LAUSD)

Notes: See Table 3.4 for a description of baseline sample. API scores are from the year of sale for the school that was zoned to the property in 2002. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	Median Household Income in Census		API Score of Zoned Elementary School	
	Number of charters	Charter seats as % of	Number of charters	Charter seats as % of
0 0.5 miles r				
0 - 0.5 miles x	-0.0700°	-0.080	-0.044	-0.084
$\frac{1}{2} = \frac{1}{2} = \frac{1}$	(0.039)	(0.096)	(0.032)	(0.033)
0 - 0.5 miles x	-0.008	-0.130^{*}	-0.0330^{+}	-0.188^{++}
1 erche 2	(0.055)	(0.090)	(0.031)	(0.095)
0 - 0.5 miles x	-0.002	-0.001	(0.0323)	-0.027
1 erche 5	(0.031)	(0.052)	(0.018)	(0.096)
0.5 - 1 mile x	-0.0391*	-0.063	-0.019	-0.0/1
lercile l	(0.021)	(0.102)	(0.016)	(0.058)
0.5 - 1 mile x	-0.010	-0.126	-0.010	-0.035
lercile 2	(0.026)	(0.143)	(0.016)	(0.094)
0.5 - 1 mile x	0.016	-0.02/	0.000	-0.079
Tercile 3	(0.022)	(0.049)	(0.026)	(0.091)
1 - 1.5 miles x	-0.012	-0.092	-0.019	-0.0/4
Tercile 1	(0.019)	(0.073)	(0.016)	(0.061)
1 - 1.5 miles x	-0.061	-0.081	-0.012	-0.056
Tercile 2	(0.051)	(0.108)	(0.020)	(0.074)
1 - 1.5 miles x	-0.029	-0.083	-0.016	-0.131*
Tercile 3	(0.037)	(0.080)	(0.030)	(0.068)
1.5 - 2 miles x	-0.022	-0.103	-0.012	-0.062
Tercile 1	(0.027)	(0.074)	(0.026)	(0.101)
1.5 - 2 miles x	0.024	0.140	-0.005	-0.054
Tercile 2	(0.022)	(0.087)	(0.018)	(0.052)
1.5 - 2 miles x	0.006	-0.060	0.029	0.039
Tercile 3	(0.031)	(0.095)	(0.029)	(0.082)
Observations	93,041	93,041	93,041	93,041
R-squared	0.88	0.88	0.88	0.88
Housing				
Characteristics	Y	Y	Y	Y
School				
Characteristics	Y	Y	Y	Y
School Fixed-				
Effects	Ν	Ν	Ν	Ν
Census Block				
Fixed-Effects	Y	Y	Y	Y

TABLE 3.A3 - Heterogeneity by Neighborhood Income and Public School API using Counts of
Charters in Home School District (Excluding LAUSD)

Notes: See Table 3.4 for a description of baseline sample. API scores are from the year of sale for the school that was zoned to the property in 2002. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	API Score of School District		
		Charter seats as percentage of	
	Number of charters	enrollment	
	(iii)	(iv)	
0 - 0.5 miles x Tercile 1	-0.0459*	-0.111**	
	(0.026)	(0.055)	
0 - 0.5 miles x Tercile 2	-0.034	-0.039	
	(0.060)	(0.117)	
0 - 0.5 miles x Tercile 3	0.0293**	0.006	
	(0.013)	(0.070)	
0.5 - 1 mile x Tercile 1	-0.0235*	-0.063	
	(0.014)	(0.059)	
0.5 - 1 mile x Tercile 2	-0.005	-0.069	
	(0.027)	(0.073)	
0.5 - 1 mile x Tercile 3	0.031	0.060	
	(0.027)	(0.130)	
1 - 1.5 miles x Tercile 1	-0.017	-0.108*	
	(0.011)	(0.062)	
1 - 1.5 miles x Tercile 2	-0.027	-0.096	
	(0.025)	(0.064)	
1 - 1.5 miles x Tercile 3	0.013	0.113*	
	(0.022)	(0.068)	
1.5 - 2 miles x Tercile 1	-0.020	-0.098	
	(0.020)	(0.075)	
1.5 - 2 miles x Tercile 2	0.031	0.042	
	(0.021)	(0.048)	
1.5 - 2 miles x Tercile 3	0.010	0.090	
	(0.060)	(0.154)	
Observations	93,041	93,041	
R-squared	0.88	0.88	
Housing Characteristics	Y	Y	
School Characteristics	Y	Y	
School Fixed-Effects	Ν	Ν	
Census Block Fixed-			
Effects	Y	Y	

TABLE 3.A4 - Heterogeneity by Public School District API using Charters in Home School District (Excluding LAUSD)

Notes: See Table 3.4 for a description of baseline sample. API scores are from the year of sale for the district of the elementary school that was zoned to the property in 2002. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	LA	County
	Number of charters	Charter seats as percentage of enrollment
Elementary - high school		
0 - 0.5 miles	0.00454	0.0161
	(0.0190)	(0.0229)
0.5 - 1 mile	0.00448	0.0282**
	(0.0109)	(0.0134)
1 - 1.5 miles	-0.00673	0.0126
	(0.0102)	(0.0122)
1.5 - 2 miles	-0.00706	-0.00861
	(0.00984)	(0.0126)
Middle school		
0 - 0.5 miles	-0.00538	0.00362
	(0.0248)	(0.0264)
0.5 - 1 mile	0.0204*	-0.00197
	(0.0118)	(0.0155)
1 - 1.5 miles	0.0161	0.0104
	(0.0101)	(0.0158)
1.5 - 2 miles	0.0110	-0.00336
	(0.00744)	(0.0189)
High school		
0 - 0.5 miles	0.00184	-0.00533
	(0.0115)	(0.0194)
0.5 - 1 mile	0.00515	-0.000962
	(0.00735)	(0.0128)
1 - 1.5 miles	-0.00250	0.0130
	(0.00541)	(0.0140)
1.5 - 2 miles	-0.00362	0.00529
	(0.00489)	(0.0121)
Elementary school		
0 - 0.5 miles	-0.0173	-0.0162
	(0.0132)	(0.0226)
0.5 - 1 mile	-0.0159*	-0.0418*
	(0.00861)	(0.0251)
1 - 1.5 miles	0.00152	-0.00958
	(0.00636)	(0.0356)
1.5 - 2 miles	-0.00457	0.00664
	(0.00559)	(0.0305)
Observations	158,211	158,211
R-squared	0.881	0.881
Housing Characteristics	Y	Y
School Characteristics	Y	Y
Census Block Fixed-Effects	Y	Y

TABLE 3.A5 - Effect of Charters on Log Sale Prices by Charter Grade Levels

Notes: See Table 3.4 for a description of baseline sample and controls. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	LA County					
_	Number of charters	Charter seats as percentage of enrollment				
0 - 0.5 miles x 2008	-0.00164	-0.00106				
	(0.0144)	(0.0273)				
0 - 0.5 miles x 2009	-0.00305	0.00532				
	(0.00957)	(0.0220)				
0 - 0.5 miles x 2010	-0.000975	0.0132				
	(0.00855)	(0.0200)				
0 - 0.5 miles x 2011	-0.0106	-0.0258				
	(0.00955)	(0.0235)				
0.5 - 1 mile x 2008	0.0102	0.00924				
	(0.00809)	(0.0312)				
0.5 - 1 mile x 2009	-0.00327	-0.0304				
	(0.00581)	(0.0213)				
0.5 - 1 mile x 2010	0.00127	-0.0116				
	(0.00489)	(0.0228)				
0.5 - 1 mile x 2011	0.00244	-0.00873				
	(0.00563)	(0.0238)				
1 - 1.5 miles x 2008	0.0141***	0.0135				
	(0.00472)	(0.0324)				
1 - 1.5 miles x 2009	0.00109	-0.0318				
	(0.00372)	(0.0272)				
1 - 1.5 miles x 2010	0.00116	-0.00301				
	(0.00318)	(0.0307)				
1 - 1.5 miles x 2011	0.00346	-0.0109				
	(0.00350)	(0.0313)				
1.5 - 2 miles x 2008	0.00166	0.0461				
	(0.00474)	(0.0394)				
1.5 - 2 miles x 2009	-0.00204	-0.0137				
	(0.00354)	(0.0294)				
1.5 - 2 miles x 2010	-0.000481	-0.0141				
	(0.00322)	(0.0312)				
1.5 - 2 miles x 2011	-0.000747	0.000509				
	(0.00305)	(0.0299)				
Observations	158,211	158,211				
R-squared	0.881	0.881				
Housing Characteristics	Y	Y				
School Characteristics	Y	Y				
Census Block Fixed-Effects	Y	Y				

Notes: See Table 3.4 for a description of baseline sample. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

				LA C	ounty				
	Median Househo Census T	ld Income in Fract	API Score o Elementary	f Zoned School	Percent Minority Schoo	in Elementary ol	Percent Minority in Census Tract		
		Charter	-	Charter		Charter		Charter	
		seats as		seats as		seats as		seats as	
		percenta		percenta		percenta		percenta	
		ge of		ge of		ge of		ge of	
	Number of	enrollme	Number of	enrollme	Number of	enrollme	Number of	enrollme	
	charters	nt	charters	nt	charters	nt	charters	nt	
-	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	
0 - 0.5 miles	0.011	0.079	0.001	-0.007	-0.026	-0.047	-0.032	-0.065	
x Quartile 1	(0.014)	(0.053)	(0.012)	(0.032)	(0.040)	(0.042)	(0.050)	(0.051)	
0 - 0.5 miles	-0.0530***	-0.068	-0.006	0.003	-0.001	0.027	-0.020	-0.055	
x Quartile 2	(0.017)	(0.056)	(0.012)	(0.028)	(0.019)	(0.039)	(0.020)	(0.036)	
0 - 0.5 miles	0.024	0.004	-0.015	-0.012	-0.001	0.022	-0.007	0.0812*	
x Quartile 3	(0.018)	(0.035)	(0.017)	(0.039)	(0.012)	(0.031)	(0.016)	(0.049)	
0 - 0.5 miles	-0.026	-0.052	0.017	0.000	-0.002	-0.005	0.004	0.019	
x Quartile 4	(0.032)	(0.042)	(0.026)	(0.042)	(0.011)	(0.032)	(0.012)	(0.034)	
0.5 - 1 mile	-0.002	0.039	-0.005	-0.006	0.003	0.001	0.013	-0.020	
x Quartile 1	(0.008)	(0.049)	(0.007)	(0.034)	(0.014)	(0.037)	(0.024)	(0.041)	
0.5 - 1 mile	0.000	0.029	-0.002	-0.027	-0.002	-0.027	-0.009	0.019	
x Quartile 2	(0.010)	(0.048)	(0.006)	(0.036)	(0.009)	(0.027)	(0.011)	(0.043)	
0.5 - 1 mile	-0.006	-0.069	0.008	0.028	-0.004	-0.033	-0.016	0.026	
x Quartile 3	(0.010)	(0.071)	(0.007)	(0.044)	(0.006)	(0.041)	(0.011)	(0.052)	
0.5 - 1 mile	0.007	-0.007	0.000	-0.017	0.000	0.034	0.002	-0.027	
x Quartile 4	(0.012)	(0.032)	(0.011)	(0.033)	(0.006)	(0.048)	(0.007)	(0.059)	
1 - 1.5 miles	0.005	0.021	0.005	-0.022	-0.008	-0.008	-0.012	-0.052	
x Quartile 1	(0.005)	(0.073)	(0.004)	(0.037)	(0.010)	(0.031)	(0.014)	(0.039)	
1 - 1.5 miles	0.0163**	0.097	0.00790*	0.032	-0.005	-0.022	0.003	0.034	
x Quartile 2	(0.008)	(0.083)	(0.005)	(0.044)	(0.007)	(0.045)	(0.010)	(0.065)	
1 - 1.5 miles	-0.007	-0.049	0.001	0.006	0.008	-0.001	0.007	0.074	
x Quartile 3	(0.008)	(0.046)	(0.006)	(0.035)	(0.006)	(0.050)	(0.010)	(0.047)	
1 - 1.5 miles	-0.015	-0.012	-0.009	-0.016	0.00694*	0.043	0.005	0.008	
x Quartile 4	(0.011)	(0.038)	(0.008)	(0.038)	(0.004)	(0.056)	(0.004)	(0.066)	
1.5 - 2 miles	-0.002	-0.055	-0.002	0.021	0.002	-0.013	-0.005	-0.034	
x Quartile 1	(0.004)	(0.097)	(0.003)	(0.039)	(0.009)	(0.043)	(0.013)	(0.057)	
1.5 - 2 miles	-0.005	0.006	-0.003	-0.016	-0.003	-0.023	0.003	-0.009	
x Quartile 2	(0.007)	(0.084)	(0.005)	(0.044)	(0.007)	(0.039)	(0.008)	(0.049)	

 TABLE 3.A7 - Heterogeneity by Neighborhood Income, Public School API, and Percent Minority

	Table 3.A7 (cont'd)										
1.5 - 2 miles	0.005	0.035	0.000	0.002	-0.004	0.002	-0.012	0.013			
x Quartile 3	(0.006)	(0.052)	(0.005)	(0.040)	(0.004)	(0.056)	(0.008)	(0.046)			
1.5 - 2 miles	-0.004	-0.026	-0.004	-0.030	-0.002	-0.007	0.000	0.062			
x Quartile 4	(0.007)	(0.042)	(0.007)	(0.055)	(0.003)	(0.070)	(0.003)	(0.070)			
Observation											
S	158,211	158,211	158,211	158,211	158,211	158,211	158,211	158,211			
R-squared	0.85	0.85	0.85	0.85	0.85	0.85	0.85	0.85			
Housing											
Characteristi											
cs	Y	Y	Y	Y	Y	Y	Y	Y			
School											
Characteristi											
cs	Y	Y	Y	Y	Y	Y	Y	Y			
School											
Fixed-											
Effects	Ν	Ν	Ν	Ν	Ν	Ν	Ν	Ν			
Census											
Block											
Fixed-											
Effects	Y	Y	Y	Y	Y	Y	Y	Y			

Notes: See Table 3.4 for a description of baseline sample. API scores are from the year of sale for the school that was zoned to the property in 2002. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

	1110		leet of chart	cits on hog b		peemeation	Cheens		
				Include	Drop				
		Limit to 0-		Properties	Properties				
	Use sale	2	Limit to 3+	with > 8	w/>5000	Drop	Summer	Add 2-5	Include
	levels	Bedrooms	Bedrooms	Bedrooms	\mathbf{sf}	Multi-Unit	Only	mile ring	School FE
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)
				A. N	umber of chart	ters			
0 - 0.5 miles	-2181	-0.0043	-0.0019	-0.0050	-0.0061	-0.0029	0.0133	-0.0068	-0.0048
	(2422)	(0.0147)	(0.0109)	(0.0087)	(0.0083)	(0.0087)	(0.0262)	(0.0084)	(0.0083)
0.5 - 1 mile	565	0.0008	0.0067	0.0001	0.0009	-0.0016	0.0074	-0.0004	0.0016
	(1383)	(0.0075)	(0.0061)	(0.0049)	(0.0048)	(0.0048)	(0.0133)	(0.0049)	(0.0048)
1 - 1.5 miles	-272	0.0065	-0.0026	0.0040	0.0018	0.0026	0.0021	0.0011	0.0029
	(921.8)	(0.0056)	(0.004)	(0.0034)	(0.0031)	(0.0034)	(0.01020)	(0.0032)	(0.0031)
1.5 - 2 miles	-5	0.0063	-0.0050	-0.0021	-0.0013	0.0011	-0.0048	-0.0018	-0.0007
	(875.6)	(0.0059)	(0.0036)	(0.0032)	(0.0028)	(0.003)	(0.0092)	(0.0029)	(0.0028)
2 - 5 miles			, ,	. ,			. ,	0.0009	, í
								(0.0008)	
			E	B. Charter seat	s as percentage	e of enrollmen	t		
0 - 0.5 miles	-3049	0.007	0.006	0.001	-0.002	-0.005	0.053	-0.002	-0.002
	(7929)	(0.037)	(0.025)	(0.023)	(0.019)	(0.021)	(0.06)	(0.019)	(0.02)
0.5 - 1 mile	-2389	0.022	-0.003	-0.009	-0.013	-0.019	0.047	-0.014	-0.012
	(9215)	(0.048)	(0.022)	(0.021)	(0.020)	(0.020)	(0.057)	(0.019)	(0.020)
1 - 1.5 miles	-4876	0.039	-0.026	-0.004	-0.019	-0.010	0.034	-0.013	-0.013
	(9456)	(0.049)	(0.029)	(0.025)	(0.022)	(0.024)	(0.058)	(0.024)	(0.024)
1.5 - 2 miles	3330	0.028	0.001	-0.013	-0.009	0.002	-0.001	-0.003	-0.006
	(9264)	(0.066)	(0.027)	(0.03)	(0.026)	(0.026)	(0.040)	(0.026)	(0.026)
2 - 5 miles								0.096	
								(0.082)	
Observations	158,211	49,432	108,779	159,906	157,783	151,797	42,962	158,211	158,211
R-squared	0.90	0.89	0.91	0.87	0.88	0.89	0.93	0.88	0.88

TABLE 3.A8 - Effect of Charters on Log Sale Prices - Specification Checks

Note: The data cover sales from September 2008 through September 2011. All regressions control for the following: month by year fixed effects; census block fixed effects; housing characteristic controls - number of bedrooms, bathrooms, square footage, and quality; school characteristics - API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels. Standard errors clustered at the school level are in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

					LA County				
	N	umber of charte	rs	Charter seats	s as percentage c	of enrollment	Charter Pen	etration Variable	es Excluded
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)
A. Distance gradient									
0 - 0.5 miles	-0.00725	-0.0353***	-0.00543	0.0741*	0.00648	-0.00134			
	(0.0131)	(0.00800)	(0.00827)	(0.0438)	(0.0249)	(0.0194)			
0.5 - 1 mile	0.00858	-0.0253***	0.000950	0.117**	-0.0166	-0.0128			
	(0.00748)	(0.00609)	(0.00476)	(0.0564)	(0.0270)	(0.0195)			
1 - 1.5 miles	0.0252***	-0.0149***	0.00223	0.140**	-0.0442*	-0.0123			
	(0.00578)	(0.00387)	(0.00313)	(0.0616)	(0.0268)	(0.0239)			
1.5 - 2 miles	0.0239***	-0.00460	-0.00110	0.120	-0.0217	-0.00470			
	(0.00494)	(0.00309)	(0.00279)	(0.0770)	(0.0340)	(0.0255)			
Housing									
Characteristics									
Number of			0.00 75 0#	0.0440	0.0054	0.00-104	0.044.6444		0.00 - 10+
Bathrooms	-0.0406***	-0.0369***	0.00750*	-0.0413***	-0.0371***	0.00748*	-0.0416***	-0.0370***	0.00748*
	(0.00643)	(0.00381)	(0.00418)	(0.00666)	(0.00382)	(0.00418)	(0.00670)	(0.00383)	(0.00418)
Number of	0.00(22	0 0 5 1 2 * * *	0.025(***	0.00107	0.0510***	0.025(***	0.000074	0 0 - 1 1 * * *	0.005(***
Bedrooms	0.00632	0.0513^{***}	0.0356***	0.0018/	0.0512^{***}	0.0356***	0.000854	0.0511^{***}	0.0356***
Course Freder	(0.00664)	(0.00327)	(0.00289)	(0.00/43)	(0.00327)	(0.00289)	(0.00752)	(0.00327)	(0.00289)
Square Feet of	0 000258***	0 000222***	0.000220***	0 000260***	0 000222***	0 000220***	0 000270***	0 000222***	0.000220***
House	$(1.29e_{-}05)$	$(7.88e_{-}06)$	$(7.57e_{-}06)$	$(1.34e_{-}05)$	$(7.90e_{-}06)$	(7.58e-06)	$(1.36e_{-}05)$	$(7.89e_{-}06)$	(7.58e-06)
Quality of Housing	(1.2)0-03)	(7.886-00)	(7.576-00)	(1.540-05)	(7.900-00)	(7.580-00)	(1.500-05)	(7.870-00)	(7.580-00)
Materials	0.00715	0.00569	0.0105***	-0.00123	0.00619	0.0105***	-0.00187	0.00617	0.0105***
Waterfals	(0.00791)	(0.0050)	(0.00380)	(0.00123)	(0.0001)	(0.00380)	(0.00187)	(0.00017)	(0.0103)
Local School	(0.007)1)	(0.00120)	(0.00500)	(0.00010)	(0.00101)	(0.00500)	(0.000000)	(0.00102)	(0.00500)
Characteristics									
Number of Students									
Enrolled in Local									
School	-0.000576***	2.36e-05	-5.56e-05	-0.000559***	2.73e-05	-5.57e-05	-0.000560***	2.56e-05	-5.57e-05
	(9.23e-05)	(6.59e-05)	(4.76e-05)	(9.48e-05)	(7.24e-05)	(4.72e-05)	(9.75e-05)	(7.31e-05)	(4.71e-05)
Number of Students									
enrolled in Private									
School	0.00197***	0.000575***	0.000143	0.00190***	0.000590***	0.000143	0.00192***	0.000588***	0.000144
	(0.000325)	(0.000179)	(0.000437)	(0.000327)	(0.000180)	(0.000437)	(0.000326)	(0.000180)	(0.000436)
Academic									
Performance Index									
(API) Growth	0.00114**	-0.000102	-0.000197**	0.00121***	-0.000100	-0.000196*	0.00121***	-9.99e-05	-0.000196*
	(0.000452)	(9.78e-05)	(0.000100)	(0.000460)	(9.96e-05)	(0.000100)	(0.000452)	(9.99e-05)	(0.000100)
Lag of API	3.77e-05	0.000186**	6.78e-05	-2.88e-05	0.000200**	6.87e-05	-6.48e-05	0.000203**	6.93e-05
	(0.000388)	(9.41e-05)	(9.27e-05)	(0.000419)	(9.65e-05)	(9.26e-05)	(0.000417)	(9.67e-05)	(9.27e-05)

TABLE 3.A9 - Effect of Charters on Log Sale Prices for Los Angeles County - All Controls Shown

TABLE 3.A9 (cont'd)									
Double Lag of API	0.00173***	8.95e-05	0.000153	0.00163***	8.56e-05	0.000155	0.00163***	8.31e-05	0.000153
	(0.000477)	(9.40e-05)	(9.54e-05)	(0.000489)	(9.21e-05)	(9.58e-05)	(0.000495)	(9.17e-05)	(9.56e-05)
Percent Black	0.000800	-0.00340*	-0.00152	0.00173	-0.00280	-0.00155*	0.00261	-0.00269	-0.00152
	(0.00164)	(0.00199)	(0.000934)	(0.00165)	(0.00189)	(0.000934)	(0.00161)	(0.00190)	(0.000935)
Percent American									
Indian	-0.0493**	0.000368	-0.00125	-0.0563**	-0.00168	-0.00132	-0.0549**	-0.00157	-0.00128
	(0.0220)	(0.00425)	(0.00393)	(0.0224)	(0.00445)	(0.00393)	(0.0226)	(0.00448)	(0.00393)
Percent Asian	0.00203**	0.00151	0.000215	0.00241***	0.00136	0.000198	0.00228**	0.00141	0.000207
	(0.000892)	(0.00199)	(0.000884)	(0.000883)	(0.00223)	(0.000885)	(0.000891)	(0.00225)	(0.000886)
Percent Filipino	-0.00199	0.00135	0.00178	-0.00234	0.00116	0.00176	-0.00266	0.00122	0.00178
	(0.00291)	(0.00182)	(0.00142)	(0.00291)	(0.00180)	(0.00142)	(0.00295)	(0.00179)	(0.00143)
Percent Hispanic	0.00663***	-0.00273**	-0.00121	0.00648***	-0.00264**	-0.00122	0.00633***	-0.00255*	-0.00120
	(0.00131)	(0.00139)	(0.000851)	(0.00132)	(0.00133)	(0.000847)	(0.00133)	(0.00133)	(0.000849)
Percent Pacific									
Islander	0.0311**	0.000137	-0.000251	0.0267**	-0.000726	-0.000257	0.0221*	-0.000591	-0.000242
	(0.0138)	(0.00355)	(0.00374)	(0.0136)	(0.00360)	(0.00374)	(0.0131)	(0.00361)	(0.00374)
Percent Gifted	0.00203	0.000416	4.41e-05	0.00222	0.000311	3.74e-05	0.00327**	0.000291	4.29e-05
	(0.00156)	(0.000593)	(0.000615)	(0.00157)	(0.000567)	(0.000615)	(0.00155)	(0.000563)	(0.000615)
Percent									
Free/Reduced	0.000046	0.000050	0.000116	0.001.40	0.000252	0.000115	0.00100#	0.000262	0.000114
Lunch	0.000946	-0.000273	-0.000116	0.00142	-0.000372	-0.000115	0.00190*	-0.000362	-0.000114
	(0.00117)	(0.0003/3)	(0.000308)	(0.00117)	(0.000395)	(0.000308)	(0.00115)	(0.000395)	(0.000307)
Percent English	0 0022 4***	0.000201	0.0001.50	0.0040(***	0.000545	0.000174	0.00420***	0.000540	0.0001(2
Language Learners	0.00334^{***}	0.000391	-0.000159	0.00406^{***}	0.000545	-0.000164	0.00428***	0.000549	-0.000163
D (D'111	(0.00109)	(0.000742)	(0.000394)	(0.00115)	(0.000785)	(0.000389)	(0.00119)	(0.000790)	(0.000390)
Percent Disabled	0.00993^{***}	-0.00195^{*}	-0.00143*	0.00965***	-0.00221^{*}	-0.00141*	0.0105***	-0.00226*	-0.00143^{*}
D (11, 10, 1, 1	(0.00207)	(0.00112)	(0.000797)	(0.00274)	(0.00119)	(0.000799)	(0.00279)	(0.00120)	(0.000798)
Creductor	0.00705***	0.000774	7.04×05	0.00640***	0.000002	9 5 4 a 0 5	0.00601***	0.000010	7.07 . 05
Graduates	(0.00/93)	(0.000774)	(0.000622)	$(0.00049)^{-1}$	(0.000892)	-8.346-03	$(0.00001^{-0.00})$	(0.000919)	-7.976-03
Daraant Callaga	(0.00149)	(0.000000)	(0.000022)	(0.00155)	(0.000033)	(0.000021)	(0.00155)	(0.000038)	(0.000022)
Graduates	0 0103***	0.000101	-0.000152	0 0100***	0.000264	-0.000161	0.0187***	0.000309	-0.000153
Oraduates	(0.0173)	(0.000101)	(0.000673)	(0.01)0	(0.000204)	(0.000673)	(0.0137)	(0.00030)	(0.000133)
Percent Graduate	(0.00237)	(0.000727)	(0.000075)	(0.00243)	(0.000700)	(0.000075)	(0.00240)	(0.000795)	(0.000072)
School Graduates	0.0143***	-0.000143	-0.000235	0.0143***	-0.000170	-0.000222	0.0149***	-0.000190	-0.000230
Senoor Graduates	(0.00221)	(0.000113)	(0.000233)	(0.00228)	(0.000998)	(0.000222)	(0.00231)	(0.0001)0	(0.000230)
B Condensed 0-2	(0.00221)	(0.000)00)	(0.000723)	(0.00220)	(0.000))0)	(0.000721)	(0.00251)	(0.00101)	(0.000722)
miles									
0 - 2 miles	0.0193***	-0.0101***	-9.80e-05	0.328***	-0.0301	-0.00750			
	(0.00321)	(0.00255)	(0.00207)	(0.112)	(0.0609)	(0.0544)			
Observations	158 211	158 211	158 211	158 211	158 211	158 211	158 211	158 211	158 211
	100,211	120,211	100,211	120,211	100,211	100,211	120,211	100,211	120,211

	TABLE 3.A9 (cont'd)								
Housing									
Characteristics School	Y	Y	Y	Y	Y	Y	Ŷ	Y	Y
Characteristics School Fixed-	Y	Y	Y	Y	Y	Y	Y	Y	Y
Effects Census Block	Ν	Y	Ν	Ν	Y	Ν	Ν	Y	Ν
Fixed-Effects	Ν	Ν	Y	Ν	Ν	Y	Ν	Ν	Y

Sample includes property sales from April 2009 through September, 2011. The independent variable denotes either the number of charters in operation or the share of enrollment in operating charters as of the sale date in various distance rings from the property. Housing characteristics include number of bedrooms, bathrooms, square footage, and quality. School characteristics include API levels overall, lags and second lags of overall API scores, % of students of each race, % free lunch, % gifted, % English language learners, % disabled, and parent education levels for elementary school zoned to the property in 2002. All regressions include month-by-year fixed-effects. Robust standard errors clustered by elementary school zone in 2002 in parentheses. *, **, and *** denote significance at the 10%, 5% and 1% levels, respectively.

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