THREE ESSAYS IN THE ECONOMICS OF EDUCATION

By

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ABSTRACT

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This dissertation consists of three self-contained chapters. The first chapter investigates the effects of school closings in Michigan on student achievement. Many school districts across the country are shutting schools, but school closing policies remain a very controversial issue. This study investigates the effects of school closing policies on student achievement by examining over 200 school closings in Michigan. Relative to the previous literature, the analysis uses a broader set of school closings to thoroughly investigate heterogeneity in treatment effects based on the performance level of the closed school. The results indicate that, on average, school closings in Michigan did no persistent harm to the achievement of displaced students. Moreover, students displaced from relatively low-performing schools experience achievement gains. The displacement of students and teachers creates modest negative spillover effects on the receiving schools, however. Hence, the closing of low-performing schools may generate some achievement gains for displaced students, but not without imposing spillover effects on a large number of students in receiving schools.

The second chapter examines the effects of a shortened school year policy on student achievement. Changing the length of the school year has dramatic potential effects for student achievement, but the magnitude of these effects will depend on the extent to which parents and teachers respond to the policy change. This study examines student achievement in Hawaiian public schools, which furloughed teachers on 17 Fridays during the 2009-2010 school year. This policy was well-publicized in advance, allowing time for parents and teachers to adjust their behavior. Using multiple specifications and identification strategies, the study finds negative effects from the school furlough policy on student achievement in el-

ementary school. The analysis finds no effects on achievement in middle and high school, however.

The final chapter, co-authored with Seth Gershenson and Michael Hayes, looks at teacher grade reassignments in elementary schools. While recent research suggests that grade-level reassignments play an important role in fostering student achievement, the literature on teacher turnover and attrition has largely ignored the reassignment of teachers within schools. We seek to fill this gap using teacher-level micro data from Michigan to document the prevalence and distribution of grade-level reassignments across different types of schools and teachers. We find that inexperienced teachers and teachers who are new to their school are less likely to switch grades. The results also suggest that the disruptions associated with within-school teaching reassignments are inequitably distributed across schools and students. Urban schools, schools with higher attrition rates, and schools with higher concentrations of minorities have significantly higher rates of grade switching.

To Jennifer.

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CHAPTER 1

THE EFFECT OF SCHOOL CLOSINGS ON STUDENT ACHIEVEMENT

1.1 Introduction

Over 1800 public schools were shut in the United States after the 2008-2009 academic year alone (Common Core of Data, 2011). School closings have become common nationwide, and urban centers such as Chicago, Detroit, Kansas City, New Orleans, Oakland, Philadelphia, and Pittsburgh have all recently closed schools. In addition, as policy discussions increasingly focus on high-stakes accountability, some policymakers have suggested shutting the lowest-performing schools and shifting students to higher-performing schools as a way to increase student achievement. Community leaders and teachers unions often vehemently oppose these school closings, however. In fact, during the recent teacher strike in Chicago, the president of the Chicago Teachers Union described the district's desire to shut schools with excess capacity as the "big elephant in the room" (Lah and Botelho, 2012). Given this controversy, understanding how school closings influence student achievement is essential for policymakers, because the extent to which districts should utilize closing policies depends crucially on the effect of closings on student achievement.

Theoretically, the effect of shutting schools on student achievement is ambiguous. On one hand, school closings may cause harm to students, because the closings disrupt peer and teacher networks. This disruption may affect the displaced students who are forced to change schools as well as students at the receiving schools who experience an influx of new students and teachers. On the other hand, being displaced from low-performing schools may expose students to higher-quality peer groups and teachers, generating achievement gains. Hence, if students are systematically moved to higher-quality schools, the net effect of the displacement could very well be positive. Which of these effects dominates and under what circumstances is an open empirical question.

This paper provides evidence on the nature of these effects by examining school closings in Michigan. Michigan provides an excellent setting for examining school closings because a large number of schools have shut in the past decade. Using statewide student-level micro-data to follow students after displacement, the study estimates the effects of school closings on both displaced students and students in nearby receiving schools. Because schools may be selected to close on the basis of their past test scores, the analysis examines the achievement trajectories of these schools prior to closure. By documenting the magnitude of the dip in test scores prior to closure, the analysis generates plausible bounds on the effect of closing schools. This bounding approach does not deliver point identified estimates, but generates policy relevant conclusions while relying on less restrictive assumptions than an approach that attempted to match closed schools to a control group of schools on the basis of past test scores. In addition, the current study examines a wide range of school closings and hence is better able than prior studies to estimate heterogeneous effects based on the performance level of the closed school. Identifying this heterogeneity is key for extrapolating these results to other settings. In particular, understanding whether districts should adopt policies of closing particularly low-performing schools will depend on the effects that closing low-performing schools generates on the achievement of both displaced students and students in the receiving schools.

The results indicate that school closings in Michigan did no persistent harm to the achievement of displaced students. For reading, students experience no significant change in test scores at the time of displacement. For mathematics, students in closed schools are falling behind their peers in the district prior to closure, and this dip prior to displacement is not the result of formal school closing announcements. Student achievement in mathematics remains low in the first year in their new school, but improves markedly thereafter. In the second year following displacement, student test scores in mathematics are substantially higher than they were in the year prior to being displaced. This result suggests plausible bounds on the effect of school closings on student achievement. If the drop in test scores prior to closure is driven by a multiple period transitory shock, then the results indicate no long term effect of school closings on student achievement. If instead the drop prior to closing represents a declining trend in student achievement at the closed school, displacement has a positive impact on mathematics achievement for displaced students. In either case, school closings create modest negative spillover effects onto students in receiving schools, however, and these effects persist for multiple years. All of these results are robust to controlling for district-wide time trends and selective mobility of students out of schools prior to closure.

Intuitively, the effect of displacement varies based on the performance level of the closed school. In mathematics, students displaced from relatively low-performing schools experience gains in achievement compared to their prior performance at the closed school. In addition, the estimated effects on receiving schools vary with respect to the performance level of the closed schools. If students are displaced from relatively low-performing schools, the spillover effects are larger in magnitude.

These results imply that districts forced to close schools due to changing demographics or financial problems do no persistent harm to the achievement of displaced students, and the spillover effects onto students in receiving schools are modest in magnitude. In addition, displaced students experience improvements in achievement if they are displaced from schools that are low-performing relative to nearby schools. Hence, school closings can be effective in raising the achievement of students in low-performing schools while imposing only modest negative spillover effects. However, a large scale policy to close low-performing schools will fail to improve average achievement district-wide because any gains from displaced students will be offset by achievement losses for students in receiving schools.

The paper proceeds as follows. Section 2.2 discusses the relevant literature, Section 1.3 describes the context and institutional details surrounding school closings in Michigan, and Section 1.4 outlines a conceptual model of how school closings can be expected to affect student achievement. Section 2.3 then discusses the data used in the analysis and Section

2.5 presents the empirical specification and results. Section 2.6 concludes, discussing policy implications of the results.

1.2 Literature Review

The qualitative literature on school closings documents concern from both teachers and administrators that students displaced from closed schools would suffer from the displacement (Lipman and Person, 2007; Steiner, 2009; Kirshner, Gaertner, and Pozzoboni, 2010). For example, Kirshner et al. (2010) investigate the closing of one large urban high school. In addition to documenting achievement losses, they report roughly 40% of students surveyed reported that they felt a sense of loss or that friendships and relationships were disrupted by the displacement. Whether this sort of disruption generates persistent achievement effects across a wide range of school closings is an empirical question.

The few quantitative studies to investigate school closing policies in particular districts have found mixed results, however.[1] Sacerdote (2012) examines the achievement of students forced to leave school due to Hurricane Katrina. His results indicate that students experience temporary sharp declines in test scores following displacement, but make up substantial ground thereafter and in many cases experience long-run achievement gains as a result of the displacement. The circumstances faced by Katrina evacuees are unique, however, and it is impossible to understand whether these results are driven by changes in family and residential circumstances due to the hurricane. In Chicago Public Schools, De la Torre and Gwynne (2009) evaluate school closings aimed at chronically low-performing schools, and find that the closings led to transitory drops in test scores.[2] The most comprehensive

[1]A related literature that explores the effects of school turnarounds and reconstitutions (i.e., replacing school staff without shifting students to other schools) finds mixed results for student outcomes (Gill, Zimmer, Christman, and Blanc, 2007; Hess, Jr., 2003; Brady, 2003; Malen, Croninger, Muncey, and Redmond-Jones, 2002).

[2]Ongoing work by Barrow, Park, and Schanzenbach (2012) investigates a similar set of school closings from Chicago and finds persistent drops in test scores for displaced students.

published study to date is Engberg, Gill, Zamarro, and Zimmer (2012), which investigates the closing of approximately 20 schools in an anonymous urban school district. The authors find that displaced students are harmed substantially, but these effects can be mitigated by sending displaced students to higher quality schools. Due to data limitations, however, they are unable to examine the achievement trajectory of students in closed schools prior to displacement. In addition, the policy investigated by Engberg et al. (2012) displaced 25% of the students in the school district in the same year. Because this large upheaval affected the majority of students in the district either directly or through spillover effects, it may be difficult to apply these results to other settings.

This disagreement about the effects of school closing policies is likely due to the fact that these results pertain to specific school closing policies, and not to broad-base closing policies such as those investigated in the present analysis. By examining a larger variety of school closings, the current study seeks to add to this existing literature in three ways. First, the study examines a broader set of closings than these previous studies, and uses this large data set to investigate heterogeneity of school closing effects on the basis of school performance. Second, by using statewide micro-data, the analysis is able to account for students who leave the district after a school closing. This allows the analysis to be robust to non-random selection of students leaving the school district after a school closing. Last, the study pays particular attention to the role of teachers in school closing policies. In many school closings in Michigan, teachers are retained in the district after displacement. This generates additional spillover effects in a possibly distinct set of receiving schools.

The effect of shifting students from one school to another has also been studied in a variety of other contexts. For instance, a large literature documents achievement losses for students who change schools voluntarily or as part of a structural transition from elementary to middle school.[3] As well, the school choice literature uses random lottery admissions to

[3] Recent prominent papers in the voluntary student mobility literature include Hanushek, Kain, and Rivkin (2004a), Xu, Hannaway, and D'Souza (2009), and Loeb and Valant (2011). See Rockoff and Lockwood (2010) for an examination of student mobility from elementary

examine the effect of being admitted to school choice programs on a variety of student outcomes, but students who apply to school choice programs are a select sample and hence different from students displaced by school closings.[4] The literatures on desegregation and peer effects also investigate the effect of shifting students from one school to another.[5] All of these policies differ from school closings in that they do not include the mobility of teachers that is typically generated by a school closing. In addition, the policy environments are much different. For instance, the peer effects generated by Katrina evacuees in Houston, as studied by Imberman et al. (2012), are likely different than those generated by closing schools and shifting students to another school within the same school district.

1.3 School Closings in Michigan

The current study examines the closing of 246 elementary and middle schools in Michigan between 2006 and 2009.[6] School closings in Michigan are driven almost exclusively by declining district enrollments. In addition to well-documented statewide population declines, an increase in school choice policies has led to further enrollment declines in some districts.[7] Figure D.1 displays the location of closed elementary and middle schools in Michigan between 2006 and 2009.[8] As can be seen, the Detroit Metropolitan Area had a large number of school closings over this time period. This includes many schools in the city of Detroit, but also

to middle school.

[4]Prominent examples of the school choice literature include Rouse (1998), Cullen, Jacob, and Levitt (2005), Abdulkadiroğlu, Angrist, Dynarski, Kane, and Pathak (2011) and Deming (2011).

[5]See Guryan (2004) or Reber (2010) for studies on the effects of desegregation policies and student outcomes. Imberman, Kugler, and Sacerdote (2012) and Angrist and Lang (2004) are prominent examples of studies that use exogenous movement of students to estimate the magnitude and structure of peer effects.

[6]This paper will adopt the convention of referring to academic years by the spring, i.e., the 2005-2006 academic year will be referred to as 2006.

[7]Toma, Zimmer, and Jones (2006) find that in Michigan around 80% of students who enroll in charter schools were previously enrolled in traditional public schools.

[8]These 246 closed schools include 18 charter schools.

schools in suburban areas. In addition, other urban centers such as Flint, Saginaw, and Grand Rapids have closed schools recently. Hence, while a number of rural districts closed schools between 2006 and 2009, the analysis presented below is primarily representative of urban and suburban school closings.

While some districts make plans for school closures years in advance, the vast majority of districts do not decide on which school to close until the spring of the last year the school is open. The choice of which school to close is very complex, and the schools chosen for closure are often not the lowest-performing schools in the district. In addition to test scores, district officials examine factors such as school condition, enrollment, and location when making their decision. Nonetheless, because district officials take into account test scores when making their decision, closed schools are on average lower performing than neighboring schools.

After closure, students are assigned to a new school within the district. While parents can choose to move or have their children utilize a school choice program, the shutting of a school does not alter parents' choice set. Displaced teachers also move within the district after a school closes. In fact, the fraction of teachers leaving the teaching profession is no larger after a closing than in a normal year.[9] While the majority of displaced students are usually transferred to the same nearby schools, displaced teachers can be shuffled throughout the district. Hence, schools that receive displaced teachers may be quite different from students in schools that receive displaced students, and both groups of schools may be affected by the closing.

[9] A small number of districts have placement policies based on teacher preferences, but the majority of displaced teachers are reassigned using standard district transfer policies that weigh school needs and administrator preferences in addition to the teacher's own stated preferences.

1.4 Conceptual Model

Consider the following stylized model of student achievement:

$$Y_{ist} = X_{it}\beta + M_{it}\alpha_{it} + \psi_{st} + \mu_i + e_{ist} \tag{1.1}$$

Here, Y_{ist} represents the achievement of student i in school s at time t. X_{it} is a vector of student observable characteristics, ψ_{st} is a school quality component of student achievement, μ_i represents fixed student-level influences, and e_{ist} is an idiosyncratic error. Given that student mobility may affect student achievement for more than one year, M_{it} is a vector of indicators indicating that the student moved schools k years ago. In particular, $M_{it}\alpha_{it} = \sum_k \alpha_{it,k} m_{i,t-k}$ where $m_{i,t-k}$ is an indicator that the student moved schools in year $t-k$. Note that α_{it} varies both across students and within students over time. This allows for the fact that different students respond heterogeneously to moving schools, and the same student may respond differently to different moves. For example, students may experience less disruption when moving to new schools with many of their previous classmates or teachers.

For ease of exposition, assume that mobility has no effect on X_{it} and future values of m_{it}. This rules out scenarios where being displaced by a closing makes the student more likely to either move schools in the future or be placed in programs such as special education.[10] Letting T_{itk} be an indicator that the student was displaced due to a school closing k years ago, the causal effect of moving schools due to a school closing can be written as follows:

$$E[Y_{ist}|T_{itk}=1] - E[Y_{i,s',t}|T_{itk}=0] = \alpha_{it,k} + (E[\psi_{st}|T_{itk}=1] - E[\psi_{s',t}|T_{itk}=0]) \tag{1.2}$$

Similar to the model of student mobility discussed in Hanushek et al. (2004a), this effect consists of two components. The first component represents the direct effect of changing schools on student achievement. Again, this effect may be heterogeneous among students

[10] The model can be readily extended to incorporate these effects, but their inclusion does not change the conclusions of the model.

depending on the circumstances surrounding the displacement. In addition to this direct effect, students will on average experience a change in school quality.

Note that the quality of the receiving school may change due to the influx of new peers and teachers. Hence, school closings may affect students in receiving schools as well as displaced students. In particular, consider the following decomposition of school quality:

$$\psi_{st} = (F_{st}^P \Pi^P + F_{st}^T \Pi^T) + SQ_{st} + u_{st} \tag{1.3}$$

The first group of terms consists of F_{st}^P and F_{st}^T, which represent the fraction of students and teachers in a school who are new to the school, respectively. This captures that new students and teachers in the school may cause disruption to the learning environment. As with student mobility, disruption to the school environment may affect schools dynamically. Therefore, $F_{st}^P \Pi^P = \sum_j \pi_j^P f_{s,t-j}^P$ and $F_{st}^T \Pi^T = \sum_j \pi_j^T f_{s,t-j}^T$, where f_{st}^P and f_{st}^T represent the fraction of students and teachers at school s in year t, respectively. SQ_{st} captures both school-level factors that are fixed in the school over time such as school facilities and administration, as well as aspects of school quality that may fluctuate within a school over time, such as the quality of the peer and teacher composition in the school. u_{st} is a school*year level error term.

In this framework, the change in school quality following a displacement can be written as follows:

$$\begin{aligned}\psi_{st} - \psi_{s',t-k} = {} & (F_{st}^P \Pi^P - F_{s',t-k}^P \Pi^P) + (F_{st}^T \Pi^T - F_{s',t-k}^T \Pi^T) \\ & + (SQ_{st} - SQ_{s',t-k}) + (u_{st} - u_{s',t-k})\end{aligned} \tag{1.4}$$

The first two terms reflect that the receiving school experience disruption due to the influx of new students and teachers. Hence, it is possible that students who might have been expected to move to a much better school as a result of displacement do not actually experience this improvement in school quality because the influx of teachers and students from the closed school reduced the quality of the receiving school. The last two terms will be important in the empirical analysis. In particular, consider two cases. On one hand, if student achievement improved after displacement because students were displaced to higher-quality schools, then $SQ_{st} > SQ_{s',t-k}$, which represents a causal effect of school closings on displaced students. On the other hand, if student achievement increased because $u_{st} > u_{s',t-k}$, this would not represent a causal increase in student achievement. For example, if schools were selected to be closed on the basis of their latest test scores, displaced students would have particularly low draws of $u_{s',t-k}$, leading $u_{st} - u_{s',t-k}$ to be positive. This issue is discussed in greater detail in Section 2.5.

Students in receiving schools also experience a change in school quality due to the disruption from new students and teachers and the change in peer and teacher quality. This effect may differ from that experienced by displaced students, however, because students at receiving schools do not experience changes in some aspects of school quality. For example, a school's curriculum is likely not changed by the influx of new students. Hence, the displacement of students would expose them to potentially better curriculum without changing the curriculum experienced by students who were already attending the receiving school.

While being displaced by a school closing is a type of student mobility, it is important to note that the effect of moving schools due to a school closing is distinct from effects that have been estimated in other literatures on student mobility. In particular, these effects are likely to differ for at least three reasons.

First, the circumstances surrounding mobility may be different between displaced students and other movers. This will result in $\alpha_{it,k}$ being different on average for students displaced by closings compared to other forms of student mobility. For instance, students in closed schools often attend school post-displacement with many of their previous classmates. If the mobility effect is due to disruption in student peer networks as a result of the move, one might expect student achievement to be lower in the case of students who move by themselves when compared to students displaced by school closings. In addition, the discussion of the impending closure may create disruption in the school prior to closure.

This disruption in student achievement would not occur prior to most other student moves and might alter the effect of the move on student achievement.

Next, as discussed in the voluntary mobility literature, different types of student moves will result in different changes in school quality. In the context of the current model, this implies that $(SQ_{st} - SQ_{s',t-k})$ is different depending on the setting of the study. These differences in school quality may potentially be very different for school closings compared to other forms of student mobility because students are typically reassigned after closings. With other forms of student mobility, students are often explicitly making the decision to change schools, implying that they may be more likely to make their schooling decisions on the basis of school quality.

Last, the inflow of students following school closings is typically very large. Hence, F^P_{stj} and F^T_{stj} are greater in the context of school closings compared to other types of mobility. In addition, because the influx of new students and teachers into the receiving schools will often be much larger than influxes of other movers, school closings have the potential to fundamentally alter the quality of the receiving schools. In the context of the model, this implies that changes in SQ_{st} are potentially much larger for receiving schools after closings than typically would occur in the case of other forms of student mobility.

1.5 Data

This study uses student-level administrative data maintained by the Center for Educational Performance and Information (CEPI) and the Michigan Department of Education (MDE). A detailed description of the data can be found in an online data appendix.[11] Briefly, the sample includes all students in grades 3-8 over the 2006-2010 academic years. In total, this represents 1,252,101 students and 3,416,174 student*year observations. Of these students, 39,205 were displaced by a school closing, with some students being displaced multiple times.

[11] Online material can be found on the author's website at https://sites.google.com/site/quentinbrummet.

Student achievement scores are taken from the Michigan Educational Assessment Program (MEAP) exams, which are administered to students in grades 3-8 in early to mid-October. Therefore, displaced students usually take their last test in the closed school prior to the formal announcement of the school closing. The fall administration of the MEAP slightly changes the interpretation of the results. MDE designs the tests so that test scores in year t are the result of instruction in year $t-1$ (Michigan Department of Education, 2005). Nonetheless, it is possible that extensive MEAP preparation prior to taking the tests may affect MEAP scores. If test scores represent knowledge gained the current year, interventions should have an immediate impact. However, if test scores reflect knowledge gained the previous year, interventions should have a lagged effect on student achievement. The current study takes the approach of assuming that achievement in year t is the result of instruction in year t. This contrasts with the approach taken by Hoxby (2000), who assumes achievement in year t is not affected by school inputs in year t. As will be seen later, treating achievement in year t as being the result of instruction in year $t-1$ only strengthens the results.

Table A.1 presents descriptive statistics for the sample used in the analysis. The sample includes significant portions of minority and disadvantaged students. In particular, 42.9% of the sample is eligible for free or reduced-price lunch and 24.1% of the sample is either black or Hispanic. Over 1.2% of the sample attend a school that will close that year, corresponding to over 40,000 observations. In addition, 7.0% of the sample attends school in a district that closed a school the previous year, and may themselves be affected by closings due to spillover effects. Due to the limited time frame of the study, there are fewer observations in the sample for students who are many years away from closure. Nonetheless, over 10,000 student*year observations exist on students three or more years after displacement. In addition, there is substantial variation in the fraction of students at a school that were just displaced by a school closing. Most students in the sample do not attend school with any displaced students, but in some schools up to 74.0% of students were previously displaced by a school closing.[12]

Table A.2 presents descriptive statistics for students in closed schools in Michigan and compares them to all students in Michigan and students within their district. Compared to other schools in their district, closed schools enroll almost 100 fewer students. This is not an artifact of different grade configurations between open and closed schools, because closed schools have lower enrollment even comparing among schools with K-5 grade configurations. This evidence supports statements by district officials, who often cite declining enrollments as one of the key factors in deciding which schools to close. While students in closed schools are similar in terms of gender and Limited English Proficiency status compared to other students in the state, districts that close schools have much different demographics than districts that do not. Almost 50 percent of students displaced by school closings attend schools in urban areas. Compared to the average student in Michigan, these students are more likely to be African-American, participate in special education programs, and be economically disadvantaged as measured by eligibility for free or reduced-price lunch. These differences are largely due to differences between districts that close many schools and districts that do not. In terms of student achievement, students in closed schools tend to perform 0.4–0.5 standard deviations worse than the state average, but also a little over 0.1 standard deviations worse than students within their district. Hence, while the majority of poor performance by students in closed schools can be attributable to district-level differences, the fact that students in closed schools perform worse than peers in their own district supports anecdotal evidence from district officials that school performance is considered when making their decision of which school to close.

Table A.3 displays statistics on the movement of students and teachers after closings. For the majority of school closings, the median student in that closed school attends a school less than 1.5 miles away from their previous school after being displaced. However, for some

[12] These instances where many students were previously displaced are the result of school consolidations.

closings the median student attends school almost five miles away from their previous school after displacement. As a result of remaining in the same school district, students often attend the same receiving school after closure. As can be seen, the fraction of students attending the modal receiving school after closure is often less than the fraction of teachers attending the modal receiving school, underscoring the point that displaced teachers are often more widely dispersed after closure than displaced students. Table A.3 also shows that for the majority of school closings, non-trivial fractions of students either leave the school district or attend a charter school after displacement. This highlights one advantage to using state-wide administrative data to study school closings – it is much easier for parents to move out of district in response to school closings than to leave the Michigan public and charter school system.

1.6 Empirical Analysis

Given the complex decision process for deciding which school to close, school closings are not likely to be randomly assigned conditional on student fixed effects and observable characteristics. Most importantly, district administrators often take student test scores into account when deciding what school to close, creating a pattern where student test scores in schools decline prior to closure. This dip could be due to either a systematic decline in student test scores prior to closure or a transitory decline in test scores, similar to the "Ashenfelter Dip" that has been observed in job training programs (Ashenfelter, 1978; Ashenfelter and Card, 1985). If the dip is transitory, it may lead conventional program evaluation techniques to overestimate the impact of school closings due to mean reversion in student test scores (Chay, McEwan, and Urquiola, 2005). Note that there could also be a causal effect of school closings on student achievement prior to closure. Closed schools are often in danger of being closed for years before they are eventually closed, which could lower student test scores through additional disruption in the schools.

Because of these concerns, it is important to consider the pattern of test scores prior to closure when investigating school closings. Consider the following model:

$$Y_{ist} = \sum_{k=-2}^{3} \delta_k T_{itk} + \sum_{j=1}^{3} 1[T_{itj} = 0](\pi^P_{j,l} f^P_{s,t-j+1} + \pi^T_{j,l} f^T_{s,t-j+1})$$
$$+ X_{ist}\beta + \mu_i + \theta_{gdt} + \epsilon_{ist} \tag{1.5}$$

The treatment specification for displaced students mirrors that found in research on displaced workers and includes dummy variables indicating how many years the student is from displacement (Jacobson, LaLonde, and Sullivan, 1993; Stevens, 1997). Specifically, for $k < 0$, T_{itk} is a dummy variable indicating that the student attends a school that will close in $|k|$ years. If k is positive, T_{itk} takes a value of 1 if the student was displaced by a school closing k years ago.[13]

The variables f^P_{st} and f^T_{st} represent the fraction of students and teachers at school s in year t who were displaced j years ago, respectively. Note that these variables can also be defined at the grade level as opposed to the school level. School-level disruption measures are preferred for two reasons. First, they capture potentially important spillover effects across grade levels, which would bias the results if f^P_{st} and f^T_{st} were defined at the grade level. In addition, the fall administration of the MEAP means that multiple teachers may be responsible for student achievement in year t, implying that grade level f^P_{st} and f^T_{st} measure would miss spillover effects across grade levels.[14] Both f^P_{st} and f^T_{st} are interacted with an indicator for whether the student was displaced k years ago.[15] Y_{ist} represents the achievement of student i in school s, year t. X_{ist} is a vector of student controls containing free or reduced-price lunch eligibility, Limited English Proficiency status, and special education

[13] For example, if a school closes after the 2008 academic year, students attending that school in 2007 have $T_{i,2007,-1} = 1, T_{i,2008,0} = 1$, and $T_{i,2009,1} = 1$.

[14] Results using grade-level variation are qualitatively similar, and can be found in Online Appendix Table C.1.

[15] If a student moves into a school for a reason other than being displaced by a closing, they are counted as a student at the receiving school. The results are not sensitive to changes in this definition.

status.[16] All test scores are standardized within grade and year to have mean 0 and variance 1. μ_i and θ_{gdt} are student and grade*district*year fixed effects, respectively.

While school closings are potentially endogenous, the dynamic specification allows the researcher to identify separate effects of school closings based on what assumptions are made about the decline in test scores prior to displacement. If the drop in achievement prior to closure is caused by the closing itself or is transitory in nature, then the effect of school closings on student achievement k years after closure is δ_k. To the extent that drops in test scores represent a systematic decline in the quality of the closed schools, the effect of the school closure is at least as large as $\delta_k - \delta_0$. In particular, under the assumption that student achievement would have remained the same had the school not closed, the effect of displacement is exactly $\delta_k - \delta_0$. If the school was trending downwards, then $\delta_k - \delta_0$ is a lower bound for the effect of the displacement on student achievement.

As shown in Table A.2, districts that closed many schools are much different than the average district in the state. The inclusion of grade*district*year fixed effects makes the analysis robust to any district-level time trends that may differ between districts that did or did not close schools. In addition, because a given test is administered to students in a particular year*grade, this analysis compares students displaced by closure only to their peers within the district who took the same test.[17] These fixed effects makes the estimation computationally demanding, however. As a result, the analysis uses a computational algorithm developed by Guimarães and Portugal (2010).[18] Standard errors are adjusted for

[16]Student mobility history, average school demographics, and average rates of student mobility in the school are excluded as they are possible outcomes from school closings. The results are not sensitive to the inclusion of these variables.

[17]While not shown here, results are qualitatively similar using district*year fixed effects as opposed to grade*district*year fixed effects. Note that the inclusion of district*grade*year fixed effects also that this makes the specifications similar to much of the school closing literature using district-level administrative data, which often includes either year or year*grade fixed effects.

[18]Briefly, this algorithm alternates between estimating the coefficients on the variables of interest and the fixed effects, holding one set of parameters fixed while estimating the other set.

clustering at the district level.[19]

Equation (1.5) is similar to value-added models used to estimate teacher quality, which often condition on lagged student outcomes. However, recent simulation evidence by Guarino, Reckase, and Wooldridge (2011) shows that the decision of whether to condition on a lagged test score or student fixed effect should depend largely on the mechanism for assigning students to treatment. In the context of estimating teacher effects, estimators that condition on lagged test scores often perform well if students are tracked into classrooms on the basis of past test scores. However, the relevant assignment mechanism in the current study is how students are assigned to schools, which is likely to be based primarily on relatively time-invariant characteristics of students such as residential location. As such, a student fixed effects approach is preferred for the current study.

Given these concerns, the analysis is broken up into three parts. Section 1.6.1 estimates the average effect of school closings in Michigan, and Section 1.6.2 examines possible explanations for the patterns observed in the data. Last, Section 1.6.3 examines heterogeneity based on the performance level of the closed school.

1.6.1 Baseline Analysis

Table A.4 contains estimates of the specification shown in Equation (1.5). For mathematics, student test scores drop in the years prior to closure, stay low in the first year at their new school, and then improve in the years after closure. In particular, students perform 0.061 standard deviations worse during the last year the school is open compared to three years prior to closure. Student performance is even lower in the year after closure, and drops to 0.074 standard deviations below their performance three years prior.[20] Student achievement

[19] Adjusting for clustering at the district level is preferred since that is the level on which the decision to close a school is made. In practice, standard errors clustered at the district level are larger than those clustered at the school level, and hence the standard errors reported in the analysis can be viewed as conservative.

[20] Given that there are five years of data and six treatment indicators, it is clear that different cohorts of displaced students identify different subsets of the δ_k coefficients. If

improves thereafter and, two years after closure, is similar to levels two years prior to closure and significantly better than in the last year in the closed school.[21] Note again that because the MEAP is given during the fall, much of the poor student performance in the first year after displacement could be the result of knowledge that was gained the year before. Hence, the estimated coefficient for one year after displacement may overstate any potential negative effects from the displacement.

As discussed previously, this analysis provides plausible bounds for the effect of school closings on the achievement of displaced students. If the drop in student test scores prior to closure is transitory in nature, school closings have short-run negative effects on displaced students, but no long-run effects. Hence, at the very least the results imply that school closings in Michigan had no sustained negative impacts on displaced students. To the extent that drops in mathematics scores prior to displacement represent a declining trend not caused by the school closing itself, the effect of the closing is positive, and over the long run student achievement in mathematics improved as a result of the displacement. Section 1.6.2 will return to this and present evidence about the nature of the dip in student performance prior to displacement.

For reading scores, the pattern is less clear. On the whole, the estimated achievement trajectories are similar for mathematics and reading. Student achievement drops prior to displacement and in the first year at the new school, but improves moderately thereafter. There are some important differences between reading and mathematics, however. In particular, the dip in test scores prior to displacement is not significant, and the results are not estimated with enough precision to rule out that student achievement does not change after displacement.[22] Hence, while these estimates are imprecise, the results indicate the

the composition of school closings differs substantially from year to year, then the dynamic pattern of the pooled results will not accurately reflect the true effect of school closings. Appendix F.1 displays the results of specifications like those shown in Section 1.6.1, but where treatment is defined separately for each cohort. The results are qualitatively similar.

[21] The t statistic for a Wald test testing that mathematics achievement two years after displacement is the same as mathematics achievement the year prior to displacement is 1.86.

[22] The t statistic for a Wald test testing that reading achievement two years after displace-

potential for only modest effects of displacement on student achievement in reading.

Both Sacerdote (2012) and De la Torre and Gwynne (2009) similarly find fadeout in displacement effects, but these patterns contrast with those presented in Engberg et al. (2012), who find sustained detrimental effects from displacement. There are multiple possible explanations for this difference in results. For one, these studies only analyze data from single school districts and therefore are unable to account for dynamic attrition of students out of districts after closure. If the students selecting out of the district were students whose achievement was trending upwards, estimates will be biased towards estimating overly negative effects of school closings. In addition, it is possible that displaced students experienced different changes in school quality between the two settings. The only other study to attempt to estimate heterogeneity in closing effects on the basis of the performance level of the closed school is Engberg et al. (2012), who use a measure of school quality developed by the school district, which is difficult to extrapolate to other settings. This point is returned to in Section 1.6.3, which attempts to estimate this heterogeneity in school closing effects.[23] Also, Engberg et al. (2012) examine a policy that displaced 25% of students in the school district in a single year. Hence, the disruption studied in this district may not be representative of school closing policies where a smaller fraction of students in the district were displaced. Whether these explanations account for the differences in results is a question for future research.

Students in schools that receive the displaced students are also affected by the closing. Table A.5 displays results from specifications as described by Equation (1.5), with the fraction of displaced students in the receiving school also interacted with treatment for displaced students.[24] Both displaced students and students previously attending the receiving

ment is the same as reading achievement the year prior to displacement is -0.87.

[23]In addition, the model estimated by Engberg et al. (2012) does not include student fixed effects, but controls instead for student's baseline test scores. As explained in Todd and Wolpin (2003), if the baseline scores measure student achievement with error, the estimated model under-adjusts for student fixed effects. The resulting omitted-variables bias is likely to generate overly negative estimates of displacement effects.

[24]Refer to Online Appendix Table C.3 for full estimates of this specification, where the

schools are negatively affected by the influx of new students. To interpret the size of these coefficients, consider that the standard deviation of the fraction of displaced students in a receiving school is roughly 0.035, so the -0.218 coefficient for mathematics represents a 0.008 standard deviation decline in student achievement for a one standard deviation increase in the fraction of students in the school who were just displaced by a school closure. Likewise, the -0.280 coefficient on reading can be interpreted as a 0.010 standard deviation drop in student achievement for each standard deviation increase in the fraction of displaced students in the school. These results could potentially be biased if students were sent to schools that were trending up or down prior to receiving the displaced students. While not shown here, leads of the f_{st}^{P} and f_{st}^{T} variables are close to zero, providing support for the assumption that students were not dynamically sorted into receiving schools.[25]

These figures are of the same magnitude as those reported in Hanushek et al. (2004a) for the fraction of students who voluntarily move into the school. While this estimate is very small, it is important to note that, unlike voluntary moves studied in Hanushek et al. (2004a), school closings affect concentrated geographic areas, and most receiving schools are likely to experience a much larger than one standard deviation increase in the fraction of displaced students when nearby schools close. In fact, the median displaced student attends a school in which roughly 20% of the student body was just displaced the previous year. In this case, student achievement at the receiving school declines by roughly 0.044 standard deviations in math and 0.056 standard deviations in reading as a result of the influx of new students.[26]

main treatment specification for displaced students is estimated for those students attending receiving schools with mean levels of fraction of displaced students and teachers in their receiving school after displacement.

[25]Online Appendix Table C.2 contains the estimates of regressions including these leads of the treatment variables.

[26]One might expect that larger schools have more resources to shift around, and hence are better able to accommodate the same percentage change in new students compared to smaller schools. If this were the case, then there would be heterogeneity in spillover effects with regards to school size. This does not appear to be the case, however, as the spillover effects do not vary with the size of the receiving school.

Receiving schools are also affected by an influx of displaced teachers. Similar to previous results, both displaced students and students at the receiving school are affected similarly by the disruption. In particular, the fraction of displaced teachers in a receiving school tends to have a negligible negative effect in the first year, and is significantly larger in magnitude two or more years afterwards for both mathematics and reading. This pattern could be explained by the fall administration of the MEAP, as instruction in year t may not have its full impact until year $t + 1$. The implied negative impact is very small, however. The median school receiving displaced teachers has 16.7% of its teaching force composed of displaced teachers.[27] This implies that for students in these receiving schools, the drop in mathematics test scores due to the new teachers would be small in first year and rise to 0.014 the year after. The effect on reading scores is similar. In the first year there is a negligible 0.005 drop in test scores, but the year after student achievement is 0.022 lower than before the influx of new teachers. While these estimates are smaller in magnitude than those presented previously for the fraction of displaced students entering a school, they are not statistically different.

1.6.2 Possible Explanations for Changes in Student Achievement

The interpretation of the results may change substantially depending on the explanation of the dip in mathematics test scores prior to displacement. One possible explanation is that announcements of school closings caused a drop in student and teacher morale which drove the decrease in test scores prior to closure. This is unlikely though, because the vast majority of closings in Michigan were announced during the spring of the last year the school was open, after students had taken the MEAP exams.[28] In particular, these closings contain over 90% of all students who were displaced. This implies that the drop in

[27] The median displaced student attends a school where 5.3% of the teachers were displaced one year prior.

[28] Data on date of announcement of school closings were obtained from internet searches of newspaper articles and personal communication with district officials. There is substantial missing data on when school closings were announced, however, corresponding to 19.5% of displaced students.

student achievement prior to closure is not driven by disruption resulting from the formal announcement that the building would be closed.[29] This does not rule out the possibility that the discussion of impending closure caused disruption in the learning environment prior to the formal announcement of the closure, but it is unlikely that the formal announcements of the school closings are driving the decline in test scores prior to closure.[30]

Another possible explanation is that students were selectively moving out of schools prior to closure. Because the treatment specification in Equation (1.5) defines all students attending schools prior to closure as treated, students switching into or out of schools prior to closure help to identify the model, implying that dynamic selection of these students may bias the results. In addition, the analysis above ignores other student mobility patterns that may be correlated with school closings. To address both these concerns, Table A.6 displays results based on specifications such as that shown in Equation (1.5), with a separate treatment specification estimated both for students who move schools voluntarily and for students who move out of schools prior to the closing. The estimated mathematics trajectory for displaced students is very similar to that shown in Table A.4, implying that the drop in test scores prior to displacement is not driven by selective mobility out of schools prior to closing. For reading, the drop in student achievement appears to be slightly larger than in Table A.4. Nonetheless, students still recover substantially in the two to three years after displacement and the results are not statistically different from those presented previously. In addition, achievement trajectories for displaced students and voluntary movers are statistically different in both mathematics and reading. This underscores the fact that moving schools voluntarily is

[29] Online Appendix Table C.4 contains results of specifications estimated just on the set of school closings not announced in advance.

[30] Another possible way to investigate whether the anticipation of the closing had a negative effect is to examine whether displacement effects were much less for students in the highest grade at the school. This is highly suggestive however, as prior work has documented achievement losses for students who change schools due to grade progressions (Rockoff and Lockwood, 2010). Nonetheless, as documented in Online Appendix Table C.5, results for students in their terminal grade at the time of closure are qualitatively very similar to students in younger grades.

a different treatment from being displaced by a school closing. Note also that voluntary movers experience a decline in test scores in the last year in their old school, suggesting that voluntary moves are endogenous to student achievement.

Given these results, it is unlikely that either formal announcements of closings or selective mobility of students out of closed schools drove the drop in mathematics scores prior to displacement. This still leaves open the possibility that the drop was the result of a transitory shock in student test scores, and schools were selected to be closed on the basis of these negative shocks. However, the fact that the shock is present for multiple periods rules out a story that the school simply had one bad year and students rebounded afterwards due to simple mean reversion.[31] One explanation that cannot be ruled out is that discussion of the impending closure harmed closed schools relative to other schools in the district. While this is possible, a potentially more likely explanation is that district administrators shut schools that were systematically losing ground prior to closure. If this latter explanation is true, the increase in mathematics scores after displacement is not simply a statistical artifact but a causal increase in student test scores. In either case, the substantive conclusions for reading scores are unchanged. There is no detectable dip in achievement prior to displacement and student achievement does not change significantly after displacement. These results are unchanged accounting for selective mobility of students out of schools prior to closure and do not appear to be driven by formal announcements of school closings.

One potential explanation for the increase in student achievement after displacement is that school closings occurred as part of a district-wide plan to upgrade facilities, as previous research has found that upgrading facilities can produce moderate student achievement gains (Cellini, Ferreira, and Rothstein, 2010). Given the current fiscal environment in Michigan, however, very few of the school closings currently studied are a result of these sorts of plans.[32]

[31] In addition, if the fall MEAP test does measure knowledge gained in prior periods as it was designed, this implies that the shock would have needed to be present for three years in a row to explain the results.

[32] Districts are classified as upgrading facilities if they have obtained a qualified bond to

In fact, only a little over ten percent of the displaced students considered in the analysis attended school in a district that was upgrading facilities, and results are qualitatively similar when limiting the analysis to just those districts without such plans.[33] One might also think that by closing schools, districts are able to devote more expenditures towards students as opposed to building maintenance. However, results available from the author suggest that the distribution of district-level per-pupil expenditures do not change significantly when districts close buildings.[34]

One might also worry that the increase in test scores after closure was driven entirely by selective attrition of displaced students from the Michigan public school system. While the analysis is robust to attrition based on time-constant factors, dynamic attrition of displaced students out of the Michigan public and charter school system would bias the results. Only 2.25% of students displaced by closure leave Michigan public schooling compared to 2.11% of all other students, however.[35] As previously documented in Table A.3, it is much more likely for students to either leave the school district or attend a charter school after displacement. Hence, while it is possible for students to leave the state or attend private schools after closure, these figures suggest that bias resulting from dynamic selection of students moving out of Michigan or to private schools is likely not a first-order concern for the current analysis.[36]

The next section will examine another possible explanation for the increase in mathematics achievement after displacement. In particular, it will examine the role of school performance in the effect of school closings on student achievement and whether increases

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renovate or construct a new elementary or middle school over the previous five years. These data is publicly available from the Michigan Department of Treasury and can be found at https://treas-secure.state.mi.us/apps/findschoolbondelectinfo.asp.

[33]The estimates of these regressions are available in Online Appendix Table C.6.

[34]Expenditure information is publicly available from Bulletin 1014, which is published by the Center for Educational Performance and Information and can be found at http://www.michigan.gov/mde/0,1607,7-140--21514--,00.html.

[35]The corresponding figure for students in receiving schools is 2.70%.

[36]Data from the 2010 Private School Universe Survey indicate that on the order of $6-7\%$ of Michigan students attend private schools. This is roughly half the size of the fraction of students who utilize school choice programs such as charter schools or inter-district choice.

in school performance can explain the estimated gains in mathematics after displacement.

1.6.3 Heterogeneity by Performance of the Closed School

As discussed in Section 1.4, the effect of school closings on student achievement can be expected to vary based on the performance of closed schools relative to nearby receiving schools. Hence, estimating heterogeneity in the effects of school closings with respect to the performance of the closed school is essential to evaluating the effects of a potential school closing policy. It is important to note that there are two possible effects of interest. One effect of interest is the effect of moving from a low- to high-performing school. This is the question that is answered if students are randomly assigned to receiving schools after displacement. However, a question of greater policy interest is the effect of closing a low-performing school, taking into account that districts cannot forcibly assign students to receiving schools after displacement.[37] Because no school closing policy can force students to attend their assigned school after displacement, the effect of shutting a low-performing school will include the location and schooling choices that are made by parents after the closing. Hence this study investigates heterogeneity in effects based on the difference in performance between the closed school and nearby schools.

The current study uses the average of 4th and 7th grade proficiency rates from 2000-2005 as a measure of school performance.[38] This contrasts with Engberg et al. (2012), who use a value-added index generated by the anonymous school district to measure the quality of the closed school. Proficiency rates are preferred for the current study because value-added measures attempt to partial out aspects of school performance that are out of the school's

[37] Engberg et al. (2012) instrument for the quality of school that a student attends post-displacement with the quality of the school to which the student was assigned to attend, which estimates the effect of school quality on student achievement for students who attend their assigned school after displacement.

[38] To ensure comparability across years, these measures are normalized within year to have mean zero and standard deviation one. As a frame of reference, in the current sample one standard deviation in proficiency rates corresponds to roughly a 0.4–0.5 standard deviation difference in student test scores.

control, including peer effects.[39] The effect of shutting a low-performing school will depend on all components of school performance, however. Average school proficiency measures capture peer inputs in addition to school and teacher quality. This is important because the effect of shutting schools will depend on these changes in peer composition.[40]

As with the main results presented in Section 1.6.1, the specification examines the achievement trajectory of displaced students prior to closure. In particular, it interacts the treatment variables from Equation (1.5) with the difference in school performance between the closed school and nearby schools that remained open:

$$
\begin{aligned}
Y_{ist} = & \sum_{k=-2}^{3} \delta_k T_{itk} + \sum_{j=1}^{3} 1[T_{itj} = 0](\pi_j^P f_{s,t-j}^P + \pi_j^T f_{s,t-j}^T) \\
& + \sum_{k=-2}^{3} \gamma_k T_{itk} \Delta_{is,t-k}^3 + \sum_{j=1}^{3} 1[T_{itj} = 0](\pi_j^P f_{s,t-j}^P \bar{\Delta}_{s,t-j}^{3,P} + \pi_j^T f_{s,t-j}^T \bar{\Delta}_{s,t-j}^{3,T}) \\
& + X_{ist}\beta + \mu_i + \theta_{dgt} + \epsilon_{ist} \qquad (1.6)
\end{aligned}
$$

where $\Delta_{ist}^3 = P_{i,s',t} - \bar{P}_{i,s',t}^3$ is the difference between the performance of the closed school and average performance of all schools within three miles of the closed school. $\bar{\Delta}_{s,t}^{3,P}$ and $\bar{\Delta}_{s,t}^{3,T}$ represent school-level averages of $\Delta_{i,s,t}^3$ for displaced students and teachers in a receiving school, respectively.[41] Note that the coefficients of the model are only identified using variation across different school closings and do not use within-closing variation of which

[39]Depending on the specification of the value-added model, school quality may be defined to include peer quality or teacher quality in addition to fixed school-specific components. Nonetheless, as the estimates in the current study are estimated using within-student variation, any differences in the results when using value-added compared to average performance measures would more likely be due to differences in the measurement of peer and teacher quality than differences in student ability.

[40]While average proficiency rates are preferred for the current study, it is possible to construct out-of-sample value-added measures for the sample of schools used in the analysis. Results from specifications as shown in Equation (1.6) using these value-added measures are available in Appendix Table F.3. The results are qualitatively similar.

[41]One could also interact $P_{i,s',t-1}$ and $\bar{P}_{i,s',t-1}^3$ separately with the treatment. This produces qualitatively similar results to what is presented in Table A.7, and hence the current specification is preferred for ease of exposition.

schools students attend after displacement. Hence, the estimates reflect long-run locational choices of parents that are arguably exogenous conditional on student fixed effects, and are not biased by parental decisions about where to send their children after displacement.

In the current sample, the average displaced student attends a school that is 0.66 standard deviations below the state average in terms of school performance the year prior to displacement, and 0.30 standard deviations below the average of schools within three miles. There is substantial heterogeneity in performance of the closed school, however. Figure D.2 shows the distribution of standardized math proficiency statewide, and plots separate densities for both closed and open schools. The density for schools that were closed is shaded black and the density for schools that remained open is shaded white. Examining Figure D.2, it is clear that the average closed school was below average for the state, but there is substantial variation in the performance level of closed schools and in some cases closed schools were above the state average.

Table A.7 displays three achievement trajectories based on estimates of Equation (1.6).[42] The first column displays the achievement trajectory for students who attended a closed school that was 0.5 standard deviations worse in terms of school performance than nearby schools. The middle column displays the same trajectory for students who attended a closed school of the same quality as nearby schools, and the final column contains students who attended a closed school that was 0.5 standard deviations better than nearby schools. The results show that students who attended poor-performing closed schools experience sharp increases in test scores after displacement, and two or more years after displacement are performing better than they ever did in the closed school. Students displaced from schools of the same quality as nearby schools experience a drop in achievement prior to closure and into their first year after displacement, but recover and perform no differently two or three years after displacement than they did two or three years prior to displacement. Students displaced from higher-performing schools are hurt by displacement, though, and never recover

[42] The parameter estimates of Equation (1.6) can be found in Appendix Table F.2.

in their new school. Hence, while shutting low-performing schools generates achievement gains, shutting high-performing schools does persistent harm to student achievement.[43]

For reading, the pattern is again less clear. While students are trending similarly prior to displacement, there is no clear pattern in achievement after displacement. This can be explained by a story that mathematics scores are more heavily influenced by factors within school control, while reading scores are more heavily influenced by other factors such as parental involvement. Both mathematics and reading trajectories are plotted out in Figure D.3 and Figure D.4, respectively. Prior to displacement students in these three groups have similar achievement trajectories. The year after displacement, students displaced from low-performing schools experience slight increases in mathematics scores relative to their peers. Two or more years after displacement, these students continue to improve and are performing significantly better in mathematics than they ever did in the closed school. For reading, there is no distinct pattern between the three lines, again suggesting that reading scores may be less influenced by school-level factors.

The relative performance of the closed school affects not only the magnitude of the effects on displaced students, but also the magnitude of the effects on students in receiving schools. Figures D.5 and D.6 plot out the differential impacts of closed school performance for schools that contain 20% displaced students, while Figures D.7 and D.8 plot out trajectories for schools that contain 20% displaced teachers. Unsurprisingly, spillover effects are larger in magnitude if displaced students come from relatively low-performing schools. For displaced teachers, heterogeneous effects for mathematics are significant at the .10 level in the first year after teachers are displaced, but become imprecise thereafter. These results are consistent with a story that spillover effects from displaced students are due to negative peer effects from displaced students, while spillover effects from displaced teachers are due to disruption

[43]Since the policy idea of shutting down failing schools is often targeted at schools in urban settings, it is important to examine the effects of shutting down low-performing schools in these areas. Online Appendix Table C.7 contains the results of specifications such as shown in Table A.7, limiting the sample to only Detroit Public Schools.

as the school accommodates the new teachers.[44] Another possible explanation is that the disruption is due to teachers who are not accustomed to dealing with new students. If this were the case, one might expect the lowest grade at the new school to not experience as much disruption, because the teachers in these grades are accustomed to accommodating new students. While not shown here, spillover effects do not vary between the lowest grade and older grades in receiving schools.[45]

Given the estimates presented above, it is unlikely a policy to systematically close low-performing schools would be able to generate substantial achievement gains without imposing widespread spillover effects on receiving schools. For instance, consider a hypothetical policy that shut a school that was 0.5 standard deviations below the average for nearby schools. A simple back-of-the-envelope calculation based on the above estimates would suggest that student achievement three years after displacement will be 0.116 standard deviations higher on average than it was during their last year in the closed school. However, spillover effects may mitigate many of these gains. For instance, suppose that the closed school was half the size of the nearby schools and the displaced students were distributed evenly over four different schools, resulting in a relatively small 11% of the receiving school population being new to the school. Given the estimates presented above, students at the receiving school would have their achievement lowered by around 0.02 standard deviations. If displaced teachers were to be retained within the district, this would be even larger. In addition, the spillover effects will be larger due to the influx of low-performing peers. In this example, this implies that the spillover effects will be roughly .01 larger in magnitude due to the low-performing peers. Hence, even in this optimistic scenario, displaced students would only experience a positive gain in achievement of 0.096 compared to a 0.03 decline for students

[44] An alternative explanation for the negative spillover effects on receiving schools is that receiving schools were near capacity and had difficulty accommodating the new students who entered the school. As documented in Online Appendix Table C.8, the results are qualitatively similar when controlling for capacity of the school, using the maximum enrollment in the school since 1990 as a crude proxy for capacity.

[45] These results may be found in Online Appendix Table C.9

in receiving schools. While this loss is smaller in magnitude than the gain in achievement for displaced students, students in receiving schools constitute a much larger segment of the district population. Hence, average achievement district-wide will not improve.

This calculation does not rule out all justifications for school closing policies. First, the results from Section 1.6.1 suggest that even when there are negative effects from closings, they tend to be modest. Hence, if there are large financial gains from shutting down schools then closings may be an effective cost-saving tool for school district administrators to undertake. As well, the above calculation focuses on average district-wide student achievement. It may be that districts wish to maximize the minimum level achievement in the district, in which case school closing policies may be justified because they have the potential to substantially raise achievement for the subset of students in very low-performing schools. Nonetheless, this analysis does point to the limitations of targeted school closing policies as a large-scale policy tool to improve student achievement. Any policy will have to balance the costs of achievement losses at receiving schools against any potential achievement gains for displaced students.

1.7 Conclusion

Given the prevalence of school closings, understanding the effects of shutting schools on student achievement is crucial for policymakers. The results show no significant change in reading scores in the two to three years before and after displacement. For mathematics, students in closed schools are falling behind their counterparts in the district prior to displacement. After closure, students continue to perform at a low level in the first year in a new school but improve significantly within two to three years after displacement. In both reading and mathematics, the influx of new students and teachers negatively impacts students in receiving schools, but the losses tend to be modest. The results vary based on the performance of the closed school. If the closed school was low-performing relative to nearby schools, displaced students perform relatively better after displacement and students in receiving schools perform relatively worse.

These results have important implications for policymakers. First, the fact that school closings in Michigan did no persistent harm to student achievement suggests that school closings may be an effective policy tool for districts that need to cut costs. In addition, because students displaced from low-performing schools experience increases in test scores after closure, there is the potential for increases in achievement for displaced students by shutting low-performing schools. This increase in achievement comes at the expense of additional disruption due to the influx of previously low-performing new students in receiving schools, however. Moreover, these spillover effects are even larger if teachers from the low-performing school are reassigned within district. Hence, while closing low-performing schools may generate some achievement gains for displaced students, it is unlikely that these policies can improve average student achievement district-wide.

There are other potential consequences associated with school closing policies that are outside the scope of the current study. In addition to the impact on the the achievement of students immediately affected by the policy, school closings will have longer-run impacts that are outside the context of the current analysis. In particular, school closings have lasting effects on quality of schools that future cohorts of students attend. Closings may also fundamentally alter the way in which students are distributed throughout the district, changing the structure of peer effects. In addition, closings create potentially important effects on teacher labor markets. When schools close, teachers are shuffled throughout the district, often on the basis of seniority. This reshuffling changes both teacher-school matches and the structure of teacher peer effects, both of which have the potential to significantly affect student achievement (Jackson and Bruegmann, 2009; Jackson, forthcoming). Hence, the welfare effects of closing schools may go beyond immediate achievement effects on the displaced students and receiving schools. While these issues are still very much unresolved, understanding the immediate impacts on student achievement due to school closings provides policymakers with valuable information to consider when weighing the costs and benefits of shutting down schools.

CHAPTER 2

THE EFFECT OF FOUR-DAY SCHOOL WEEKS ON STUDENT ACHIEVEMENT: EVIDENCE FROM HAWAIIAN SCHOOL FURLOUGH DAYS

2.1 Introduction

Changing the length of the school year is one of the most natural policy levers for affecting student achievement. Because changing how much time students spend in school has such dramatic potential consequences, policies that alter school year length garner much attention. On one hand, many observers have called for longer school years to improve student achievement, citing the fact that American schools have on average shorter school years than similar developed countries (Lee and Barro, 2001). On the other hand, some school districts across the country have shortened their school years to ease budget deficits, generating much controversy. Because these policies may have profound effects on student learning, understanding the relationship between length of school year and student achievement is imperative for policymakers. While less instructional time theoretically harms student achievement, teachers may respond by covering more material in the school day or parents may respond by obtaining after-school tutors. These behavioral responses may work to offset the negative effects of lost instructional time, and the magnitude of their effects is an empirical question.

The few quasi-experimental studies to examine the effect of school year length in modern educational settings use variations in instructional time that may not have been entirely salient to parents and teachers. The current study fills this void by investigating a school furlough day policy in Hawaii, which reduced the 2009-2010 school year for all public school students from 180 to 163 instructional days. This policy was well-publicized in advance and very salient to parents and teachers. In fact, while the furlough days were originally intended to be in place for two years, the government reached a deal with the teachers union and a coalition of banks to eliminate furlough days for the 2010-2011 academic year. While charter schools saw reductions in funding, they were not mandated to take the furlough days and the majority of charter schools took no school furlough days.

The analysis uses two identification strategies to isolate the effect of the furlough days on student achievement in Hawaiian public schools. First, the study uses variation within furloughed public schools over time in an "interrupted time series" (ITS) design, comparing student performance in academic year 2010 to the trend in test scores before and after the furlough day policy. This approach is unable to control for test-period effects that affected all studentsi in 2010, however. Hence, the study also performs a "difference in difference" (DD) analysis, using unfurloughed charter schools as a control group. This design is able to control for test-period effects, but charter schools are an imperfect control group because they saw reductions in funding in the 2010 school year. Hence, the DD analysis is biased towards finding positive effects of the furlough days on student achievement.

The results indicate that school furlough days had negative effects on mathematics achievement in elementary school. The magnitude of these effects is well in line with prior literature, indicating that reductions in school year length hurt student achievement even in a scenario where the policy is well-publicized in advance and salient to teachers and parents. The results for middle and high school indicate that there were no negative impacts from the school furlough days, however. This may indicate that teachers in these grade levels had an easier time adjusting the content of their material or that students in these grade levels are less susceptible to educational interventions. All of these results are robust to a number of different specifications, and unobserved test-period effects are likely not large enough to substantially alter the results.

The remainder of the paper is organized as follows. Section 2.2 discusses the relevant literature on school year length, Section 2.3 discusses sources of data used for the analysis, and Section 2.4 discusses the estimation strategies employed in this analysis. Section 2.5 then presents the results, and Section 2.6 concludes and discusses potential policy implications of the results.

2.2 Literature Review

A large literature correlates school year length and student achievement,[1] but only a few studies use quasi-experimental variation to examine the effects of school year length. Marcotte (2007) and Marcotte and Hemelt (2008) use variation in the amount of snow days taken by schools in Maryland to identify the impact of instructional time. Their results indicate that students who took exams in years with relatively more snow days performed significantly worse on mathematics exams. In addition, Marcotte and Hemelt (2008) show that more than half the schools that failed to meet Adequate Yearly Progress (AYP) in a given year would have met AYP had they been open for all scheduled school days. While snow days provide credibly exogenous variation, the reduction in instructional time may not be entirely salient to parents and teachers. Moreover, even if the reduction is salient, snow days are not announced in advance and hence there is less time for parents and teachers to adjust their behavior.

Sims (2008) uses variation induced by a 2001 policy change in Wisconsin that forced schools to start the school year after Labor Day, and finds that reductions in classroom time lead to lower average math scores for 4th graders, but have no effect on average for reading scores. Hansen (2011) further explores using both law changes and snow days to examine the effect of school year length on student achievement, and finds similar results to both Marcotte and Hemelt (2008) and Sims (2008). While potentially more salient than snow days, changes in school start dates change the number of instructional days before an exam while often leaving total instructional time unaltered. Hence, the results of such studies may be difficult to extrapolate to scenarios where a policy to shorten the school year was announced in advance.

[1] For examples, see Card and Krueger (1992), Eide and Showalter (1998), Lee and Barro (2001), Woessmann (2003), or Eren and Millimet (2007).

Pischke (2007) examines shortened school years in West Germany from 1966-1967, where students were exposed to school years that were about 1/3 shorter than the typical year. He finds that the short school years led to increased grade retention among the affected students, but had no effect on later labor market outcomes. While this policy was very salient to both parents and teachers, it is hard to extrapolate the results of this analysis to modern educational settings.

A related literature examines four-day school weeks where instructional time was extended on the four days in which school was in session. In particular, Anderson and Walker (2012) and Sagness and Salzman (1993) investigate four-day school week policies in rural Colorado and Idaho, respectively. They both find that student achievement improved after the implementation of the four-day school week policy. Other related work investigates the effects of multi-track year-round calendars, where students in different tracks attend school year round, but at least one track is always on break.[2] For example, Graves (2010) and Graves (2011) find negative effects of these calendars on student achievement, while McMullen and Rouse (2012) find no impact on student achievement.[3] While these studies investigate similar policies to Hawaiian school furlough days, the policies they investigate alter the distribution of instructional time in a much different manner than furlough days. Hence, their results are not directly comparable to the current study.

2.3 Data

The study uses school characteristics from the Common Core of Data (CCD) and proficiency rates from the Hawaii State Assessment (HSA) for years 2007-2012.[4] The HSA is a computer

[2] For a survey of this literature, see Cooper, Valentine, Charlton, and Melson (2003).

[3] Another strand of literature uses discontinuities in assignment to estimate the causal effect of mandatory summer school for low-performing students. See Jacob and Lefgren (2004) or Matsudaira (2008) for prominent papers in this literature.

[4] HSA data is available for years prior to 2007, but is not used for the current study because the HSA was redesigned in 2007 and hence scores prior to 2007 are not directly comparable to scores from 2007 onwards.

adaptive test administered to all students in grades 3-8 and grade 10, and results are made publicly available by the Hawaii Department of Education.[5] The current study will not explicitly examine scores from grade 3 because they are the product of multiple years of instruction instead of a single year. The exams are administered every spring between late March and early April, and 13 of the 17 furlough days in 2010 occurred prior to the administration of the HSA exams. Hence, the reduction in instructional days prior to the administration of the exam was roughly 10%.

To assess the validity of the HSA exams, Table B.1 compares proficiency rates from the HSA to the commonly used proficiency rates from the National Assessment of Educational Progress (NAEP).[6] While the proficiency results for the tests are very different in terms of levels, both tests show increases in proficiency rates over time. In fact, the correlation between the HSA and NAEP scores in mathematics is 0.945. While this calculation is based only on six data points, it does give some weight to the validity of the HSA results, indicating that the increase in test scores over the sample period was most likely due to an increase in content knowledge and not a lowering of standards.

In total, the sample consists of 31 charter schools and 260 traditional public schools in Hawaii.[7] Table B.2 displays descriptive statistics for the sample of schools used in the analysis, broken apart by time period and number of furlough days that the school took in 2010.[8] Note that 22 of the 31 charter schools in Hawaii took zero furlough days, and only 2 charter schools took the entire 17 furlough days that were taken by Hawaiian public schools.[9] As

[5]Students in grades 4, 8, and 10 take a science exam as well. Students in grades 3 and 4 who are in the Hawaiian Language Immersion Program can take the exam in the Hawaiian language. HSA results can be found at http://doe.k12.hi.us/myschool/sat-hcps-terranova.htm.

[6]NAEP scores can be found at http://nces.ed.gov/nationsreportcard.

[7]In 2008, a larger than average number of schools did not report testing data. Because data from year 2008 is not central to the identification of the effects of school furlough days, this issue is not considered a first-order concern. Results available from the author show results with year 2008 excluded, which are qualitatively similar.

[8]Refer to Appendix G.1 for descriptive statistics for the entire sample.

[9]Data on number of furlough days taken by charter schools was collected through phone

seen in Table B.2, schools that took less than 17 furlough days look slightly different in terms of demographic characteristics. These schools serve similar populations in terms of fraction eligible for free/reduced-price lunch compared to their furloughed counterparts. However, they are lower performing in terms of mathematics proficiency, enroll fewer Asian/Pacific Islander students, and tend to have lower pupil-teacher ratios than their furloughed counterparts. While these groups of schools are very different, Table B.2 also shows that there were only slight changes in the demographic makeup of these schools after the school furlough days, particularly for furloughed schools. In particular, the racial and free/reduced-price lunch composition of the schools remained relatively flat, and public schools experienced only modest enrollment declines. Hence, it does not appear that the furlough policy caused a dramatic shift in the composition of Hawaiian public schools.

2.4 Empirical Model

In order to estimate the effect of Hawaii's furlough days on student achievement, one must estimate what the counterfactual achievement of Hawaiian students would have been in the absence of school furlough days. Consider the following model where student achievement in school i, grade g, year t can be written as:

$$Y_{igt} = \tau T_{it} + u_{igt} \tag{2.1}$$

where Y_{igt} is the fraction of children proficient in year t, T_{it} is an indicator that the school was "treated" (i.e., took school furlough days), and u_{igt} is an error term. Let $u_{igt} = \xi_{igt} + \epsilon_{igt}$ be a decomposition of u_{igt}, where by construction $Cov(T_{it}, \xi_{igt}) \neq 0$ and $Cov(T_{it}, \epsilon_{igt}) = 0$. Hence,

$$E[Y_{igt}|T_{it} = 1] - E[Y_{igt}|T_{it} = 0] = \tau + \Big[E(\xi_{igt}|T_{it} = 1) - E(\xi_{igt}|T_{it} = 0)\Big] \tag{2.2}$$

As previously stated, the current study uses two different designs to estimate τ. First, an ITS design uses trends in student achievement in other years to construct an estimate

conversations with charter school officials.

of what student achievement in Hawaiian public schools would have been in the absence of school furlough days. In practice, this restricts the model so that $\xi_{igt} = \mu_{ig} + f(t)$, and estimates the following equation on the subset of schools that experienced school furlough days:

$$Y_{igt} = f(t) + \tau \cdot 1[t = 2010] + X_{it}\beta + \mu_{ig} + e_{igt} \tag{2.3}$$

The clear drawback to this approach is that identification relies on the functional form specification of $f(\cdot)$.

The second strategy uses a DD design, and compares furloughed schools to unfurloughed schools in the same year. This implies that $\xi_{igt} = \mu_{ig} + \gamma_t$, and hence there is allowed to be a separate test-period effect for each year. In particular, consider the following model where $FDays_{it}$ is the number of furlough days the school took divided by 17:

$$Y_{igt} = \gamma_t + \tau FDays_{it} + X_{it}\beta + \mu_{ig} + \epsilon_{igt} \tag{2.4}$$

This approach allows for test-period effects, and does not rely on the functional form of $f(\cdot)$ for identification. Identification instead relies on variation in student performance between schools that did and did not take furlough days. The resulting estimates will be robust to any time-constant unobserved heterogeneity that may be correlated with $FDays_{it}$.

While this approach allows for test-period effects, it maintains the assumption that $E(\gamma_t|T_{it} = 0) = E(\gamma_t|T_{it} = 1)$. This assumption is violated in the current scenario because charter school funding cuts in 2010 likely harmed student achievement. Hence, the estimated effect of the furlough policy is expected to be biased towards finding positive effects of the furlough days, and the magnitude of the bias will depend on the magnitude of the negative effect of the funding cuts. Section 2.5 returns to this issue further, and discusses the potential magnitude of effects from charter school funding cuts. This specific problem is not a problem for the ITS analysis because it compares the set of schools that took furlough days to their projected performance. Hence, the two designs are viewed as complementary.

2.5 Results

Prior to placing restrictions on the empirical model, Figures E.1-E.4 display test score trajectories over the sample period. Schools are divided into groups based on the number of furlough days that they took in the 2009-2010 academic year. Figure E.1 shows the trajectory of scores for the HSA mathematics exam in grades 4 and 5. Schools that took furlough days perform below their trend in year 2010, indicating that there may have been a negative effect from the furlough days. In addition, unfurloughed schools made up ground in 2010 compared to 2009, again indicating the potential for a negative effect from the furlough days. Nonetheless, it is noteworthy that furloughed and unfurloughed schools followed roughly similar trajectories over the time period, lending support to the idea that unfurloughed charter schools serve as a comparable control group for the DD analysis. Figure E.2 shows the trajectory of reading scores for grades 4-5. As can be seen, there is almost no distinguishable deviation from trend in achievement in year 2010. Figures E.3 and E.4 show the same graphs for grades 6-10. Neither math nor reading scores for furloughed schools dip in 2010. Hence, these graphs present suggestive evidence that the furlough day policy negatively impacted mathematics achievement for students in elementary school, but effects on students in grades 6-10 were likely small in magnitude.

Figures E.5-E.8 display the trajectory of school composition variables over the time period. As can be seen by examining Figure E.5, unfurloughed schools experienced increases in enrollment in 2010 relative to their previous trajectory. If this increase represents selective mobility of students from one school to another, the results will be biased. Students who selected out of furloughed schools were likely relatively high-achieving, however. If this is the case, selective mobility leads to overly-negative estimates of school furlough days on student achievement and any negative effects from the furlough days would tend to be overstated. Hence, the analysis can be viewed as a conservative estimate of the impact of school furlough days.

Figure E.6 shows the trajectory of pupil-teacher ratios over the time period, which increase only slightly in the 2010 academic year. Hence, increases in pupil-teacher ratio are an unlikely explanation for the results shown below.[10] Figures E.7 and E.8 also show that the racial composition of schools did not change in year 2010. Note that the CCD changed it's racial categorization of students in the 2011 academic year and hence 2011 is not comparable to other years.[11] While this recoding affects the descriptive pattern of student composition in Hawaiian schools, it is not a concern for the empirical analysis presented below.

2.5.1 Grades 4–5

Table B.3 displays the results of regressions based on the specification shown in Equation (2.3) for grades 4 and 5.[12] These regressions are estimated only on the subset of schools that were furloughed.[13] Column (1) presents results setting $f(t) = \delta_1 t$, and column (2) includes controls for fraction of students eligible for free or reduced-price lunch, African-American, Hispanic, Asian/Pacific Islander, and American Indian. Note that columns including controls have fewer observations because controls are not available for 2012. Column (3) examines whether the trend in test scores changed after year 2010 by setting $f(t) = \delta_1 t + \delta_2 t \cdot 1[t \geq 2010]$. If the trend differs before and after 2010, this reflects either a persistent effect from the school furlough days or misspecification of $f(t)$. Column (4) sets $f(t) = \delta_1 t + \delta_3 t^2$. The quadratic specification allows for the most flexible functional form of $f(t)$ while not

[10] In particular, pupil-teacher ratios in charter schools went up by only 0.3 percent in 2010. Even given the large effects of class size on student achievement found in studies such as Krueger (1999) and Angrist and Lavy (1999), this difference is not large enough to predict sizeable drops in student achievement.

[11] Starting in 2011, the CCD categorized multiracial students as a separate racial category. This had the effect of causing both white and Asian/Pacific Islander populations to decline. The current study imputes multiracial students to a single race category using data on the schools past populations of students, but this procedure still leaves the rates incomparable.

[12] While not shown here, all results are robust to setting the dependent variable equal to $log[Y_{it}/(1 - Y_{it})]$ or using a fractional probit specification as in Papke and Wooldridge (2008).

[13] Refer to Appendix Table G.2 for results by individual grade level. The results indicate that there was no effect of the furlough days on achievement in third grade.

overfitting the data, and hence is the most preferred specification out of the four columns.

The results are very consistent across all specifications. In particular, the estimates indicate a roughly 4-6 percentage point drop in the fraction of students proficient as a result of the furlough days. Given that proficiency in 2010 was roughly 50%, a 4-6 percentage point drop represents a 8-12% decline in test scores for an almost 10% reduction in school days.[14] This result is well in line with previous work by Marcotte and Hemelt (2008), Sims (2008), and Hansen (2011), who all find similar declines for elementary school. Also, as shown in column (3), there is little difference in the trend of scores pre- and post-2010. This indicates that achievement continues along the pre-2010 trend line in the years following the furlough policy. Taken literally, this would indicate that there was no persistent effect of the furlough days. However, as discussed previously, any misspecification in $f(t)$ would bias these results. Hence, this result is only suggestive evidence that the negative effects of the furlough days were transitory. As shown in Panel B, the results for reading are different from the math results, and indicate that furlough days had no significant effect on student achievement in reading. This result fits with a story that reading achievement is less susceptible to educational interventions than mathematics achievement.

Table B.4 displays the results from the DD specifications, based on specifications as shown in Equation (2.4).[15] Columns (1)-(3) present results with year fixed effects, columns (4)-(6) present results with group-specific trends, and columns (7)-(8) present results with school-specific trends. In addition, columns (2) and (5) include a lead of the treatment variable while columns (3), (6), and (8) include control variables for racial and free or reduced-price lunch eligible composition in the school. In contrast to the results presented in Table B.3, the results are always positive and typically statistically insignificant. Note that the estimates of leads of the furlough day policy in columns (2) and (5) indicate that furloughed schools had a

[14] It is difficult to convincingly map these figures into student standard deviation units, but a rough conversion can be obtained if one assumes that underlying test scores are normal and the policy change resulted in a mean shift to the underlying normal distribution. Under these assumptions, the effect was in the range of 0.126-0.151 student standard deviations.

[15] Refer to Appendix Table G.3 for DD results by individual grade level.

spike in performance relative to unfurloughed schools in the year prior to the furlough policy. While this result is not statistically significant, it gives some indication that the estimates are potentially upward biased. Columns (7) and (8) present the preferred specifications, where school-specific trends are included in the empirical model. The estimates are very imprecise, and the lower bounds of the 95% confidence intervals are -0.039 and -0.060, respectively. As discussed before, these results are biased upwards and the magnitude of the bias will depend on the magnitude of the effect of charter school funding cuts. While evidence on the relationship between school expenditures and student performance is mixed,[16] the confidence intervals presented above are very wide and even small negative effects from funding cuts would put the DD estimates in line with those presented in Table B.3. Nonetheless, the fact that public schools did not fall behind their charter counterparts in 2010 indicates that test-period effects are likely not substantially biasing the results presented in Table B.3 towards zero.

Taken as a whole, these results indicate that students in elementary school were negatively affected by the shortened school year. They also present suggestive evidence that this effect was transitory and student achievement recovered in the years following the furlough policy. Hence, while Hawaiian school furlough days represent a much different policy from those studied by previous literature, the effects of the policy on students in grades 4-5 were very similar to what was has been found in other settings.

2.5.2 Grades 6–10

Table B.5 displays ITS results for students in grades 6-10. For math scores, students in grades 6-10 perform at or above their trend in math scores in 2010, contrasting with the results shown for grades 4-5 in Table B.3. The trend line fits the achievement trajectories of furloughed schools quite well, and hence it is unlikely that test-period effects are substantially

[16] Hanushek (2003) and Krueger (2003) provide surveys of the literature on the relationship between school resources and student achievement.

biasing these estimates. In fact, the largest average observed deviation from trend in a given year is 0.001, which is much smaller in magnitude than the difference between the grade 4-5 and grade 6-10 results. While the trend of math test scores appears to increase after 2010, this trend in test scores became steeper after 2010 and hence is unlikely to be the result of persistent effects from the furlough day policy. As with the results for students in grades 4-5, reading scores change very little in year 2010 relative to their trend, with the exception of column (3), which is heavily influenced by one unusual year of reading scores in 2012. The result of no effect from the furlough days contrasts with results in prior studies, which find significant effects of shortened instructional time on achievement in eighth grade.

Table B.6 presents the results of DD specifications such as those shown in Table B.4, but estimated for grades 6-10. As shown for grades 4-5, the results for reading are not statistically different from zero. However, the DD estimates for math are consistently positive and statistically significant. Note also that, unlike the results presented for grades 4-5, the estimates of leads of the furlough policy presented in columns (2) and (5) are extremely close to zero. This result indicates for grades 6-10, charter schools performed no differently the year prior to the policy relative to their prior trajectory. The lower bounds of the 95% confidence intervals for the preferred estimates presented in columns (7) and (8) are -0.002 and -0.003, respectively. For these confidence intervals to overlap with the results presented in Table B.3, the effect of the expenditure cuts on charter school students would need to be roughly 0.05 percentage points. While an effect of this magnitude is theoretically plausible,[17] it is hard to reconcile this large of an effect with the trend in charter school achievement observed over the sample period. To examine this, Figure E.9 displays the per-

[17] For instance, Papke (2005) finds that a 10% increase in funding led to between a 1-2 percentage point increase in proficiency rates. Applying these results to the current setting, this implies that the observed decrease in proficiency rates would have been on the order of .025 to .050 percentage points. While taken literally, the high end of this estimate implies that the 95% confidence interval overlaps with some of the point estimates shown in Table B.3, this calculation makes clear that the DD estimates are difficult to reconcile with prior studies.

pupil funding of charter schools in Hawaii over the sample period. Note that charter schools experienced consistent cuts in funding over this time period, not just a single cut in 2010. In addition, Figure E.1 shows that charter schools were not systematically falling behind their counterparts in traditional public schools over the time period.[18] Because charter schools neither experienced a single drop in funding in 2010 nor fell steadily behind their furloughed counterparts over the time period, it is unlikely that funding cuts had a large enough negative effect on student achievement to explain the difference between the grade 4-5 and grade 6-10 results.

For the grade 6-10 results to align with previous literature, two facts must be true. First, an unobserved positive shock would need to have affected all test scores in 2010, and the positive test-period effect would have to be larger than any observed deviation from trend. Second, unfurloughed charter schools would need to have had large drops in achievement from funding cuts. This is unlikely because charter schools saw similar drops in funding in many other years while not falling behind regular public schools in terms of student achievement. Hence, the most plausible explanation is that students in middle and high school were less affected by the change in instructional time.

2.5.3 Heterogeneity by School Characteristics

As discussed previously, parents and teachers may have offset some of the damage of the furlough days by changing lesson plans or by offering out-of school tutoring. If this were the case, one might expect the effect of furlough days to vary based on the composition of the school. To explore this possibility, Table B.7 contains the results of ITS specifications such as those in column (4) of Table B.3 broken apart by quartiles of both percent of students eligible for free or reduced-price lunch and average achievement in 2007. Panels A and B present results for grades 4-5, while Panels C and D present results for grades 6-10. For

[18] Also note that the results are qualitatively similar if the model conditions on charter school funding while assuming that public school funding remained unchanged.

students in grades 4-5, the effects appear to differ by school-level achievement, with higher-achieving schools being less affected by the policy. These differences are slightly imprecise, however. In particular, the analysis rejects the hypothesis that the sum of the effects in the first two quartiles is different from the sum of the effects in the second two quartiles, but just fails to reject the hypothesis that all estimated effects are identical.[19] The results do not differ by the fraction of students eligible for free or reduced-price lunch, however, and none of the estimated effects are statistically different from one another.[20] These results indicate that parents and teachers in high-achieving schools may have been able to offset some of the negative impacts of the furlough days.

The results from grades 6-10 display little heterogeneity by school composition, however. In particular, the estimated effects of the furlough policy are almost identical across both quartiles of achievement and quartiles of free or reduced-price lunch eligibility. On one hand, this result could reflect that teachers in these grades are better able to adjust to the shortened school year and therefore students in all types of schools were unaffected by the policy. On the other hand, these results could simply reflect that there was little effect of the furlough days on students in these grades, and hence teachers and parents did not need to adjust their behavior. This explanation fits with a story that students in these grades are less susceptible to interventions, and hence were not harmed by the shortened school year. Which of these explanations is more relevant is a question for future research.

2.6 Conclusion

Hawaii's school furlough days present an excellent opportunity to study the effects of school year length on student achievement. This policy decreased the length of the school year for all public school students by 17 days and was announced in advance, allowing parents and teachers time to adjust their behavior. The results indicate no detectable effect of

[19] The p-values for these two tests are 0.037 and 0.105, respectively.

[20] The p-value for a test that all effects are identical is 0.849.

the furlough day on reading achievement. For mathematics achievement, the shortened school year negatively impacted achievement in elementary school, and the analysis presents suggestive evidence that high-performing schools were less affected by the policy. This latter evidence highlights the potential for responses on the part of parents and teachers to offset some of the damage of diminished instructional time. The analysis indicates no effects in middle and high school, however. This result may be explained by differences in the teaching environment in middle and high school, or differences in the receptiveness of these students to educational interventions. All of these results are robust to multiple identification strategies and specification checks.

The total effect of shortening the school year goes beyond effects on student learning. School year length may profoundly affect parental labor supply, teacher classroom behavior, or even crime rates as shown in Jacob and Lefgren (2003). Understanding how school year length relates to these outcomes is important for future research. Nonetheless, understanding what effect a highly publicized and controversial shortened school year policy had on student achievement is a major step towards understanding the effects of school year length.

CHAPTER 3

THE FREQUENCY AND CORRELATES OF TEACHERS GRADE-LEVEL REASSIGNMENTS: EVIDENCE FROM MICHIGAN

3.1 Introduction

Teacher turnover, whether measured by attrition from the profession or mobility across schools, can disrupt the functioning of schools in a myriad of ways. For example, high rates of teacher turnover may reduce instructional quality, destabilize schools, and disrupt schools curriculums and course offerings (Shields, Humphrey, Wechsler, Riehl, Tiffany-Morales, Woodworth, Young, and Price, 2001). Within-school teaching reassignments (i.e., grade-level and subject changes) and initial classroom assignments have similar consequences, as recent research suggests that teachers returns to experience are greater when experience is accrued in the same grade and that the composition of teachers initial classroom assignments significantly impacts subsequent mobility decisions (Ost, 2011; Feng, 2010). This evidence has led observers such as Jacob and Rockoff (2011) to argue that principals should think carefully about how to best allocate teachers to grades and subjects, as such decisions are typically non-controversial yet may have substantial effects on student achievement.

However, the large literature on teacher turnover generally ignores the within-school sorting of teachers into grade levels. This is a glaring omission, as student achievement is affected not only by the number of teachers new to the school, but also by the number of teachers who are teaching in a new assignment. In addition, within-school rates of grade switching are similar in magnitude to both rates of attrition from the profession and mobility across schools. Well-documented higher rates of teacher turnover in low-performing and impoverished schools suggest that such schools may necessarily experience more shuffling of teachers across grade levels and subjects.[1] An inequitable distribution of within-school turnover in teaching assignments presents an additional challenge that students, teachers, and administrators in disadvantaged schools must overcome. The current study contributes to the literature on teacher turnover by investigating the teacher- and school-level predictors of grade switching and the relationship between grade switching and other types of teacher turnover.

We use rich administrative panel data on the universe of self-contained kindergarten through fifth-grade Michigan public school teachers during the 2003-04 through 2008-09 school years. These data are well suited for the analysis, as Michigan is home to a large demographically and socioeconomically diverse student population, the panel nature of the data allows individual teachers to be tracked over several years, and the large sample size provides sufficient power with which to identify the predictors of grade switching. In addition, we verify that the phenomenon of grade switching is not unique to Michigan by showing that rates of grade switching and other types of teacher turnover in the nationally representative Schools and Staffing Survey (SASS) are similar to those in Michigan.

We find that grade switching is more prevalent in schools in urban areas, schools serving minority student populations, and schools with higher attrition rates. In addition, less experienced teachers switch grades more often, particularly those teachers who are new to their school. Grade switching strongly predicts future grade switching, but not other types of turnover. Interestingly, there is significantly less grade switching in charter schools and no relationship between grade switching and schools academic performance. These results imply that in addition to higher rates of teacher turnover, urban schools with high concentrations of minority students also experience significantly higher rates of grade-level reassignments. This is true even after conditioning on school-level turnover rates and suggests that policymakers concerned with problems of teacher turnover in disadvantaged schools should pay similar

[1] For example, see Hanushek, Kain, and Rivkin (2004b) or Lankford, Loeb, and Wyckoff (2002).

attention to the inequitable distribution of grade-level reassignments.

Section 2 proceeds with a brief review of the relevant literature. Section 3 describes the data and methods used to perform the empirical analysis of grade switching. Section 4 presents the results and section 5 concludes with a summary and discussion of the main findings.

3.2 Literature Review

Two studies tangentially touch on the frequency of within-school grade-level reassignments of self-contained classroom teachers. Chingos and West (2011) use administrative data from Florida to show that teachers in tested grades who have low value-added scores are more likely to move to non-tested positions within their current school and exit teaching than their higher-achieving counterparts. Of Floridas 24,475 self-contained tested-grade (4th–8th grade) teachers in 2001-02, 84% remained in a tested-grade classroom the following year and 52% remained in a similar position seven years later (excluding retirements). These figures represent a lower bound for the percentage of tested-grade teachers who changed grades, however, as the analysis does not consider switches between tested grades (and does not consider teachers initially teaching in non-tested grades). Notably, 7% of the 2001-02 cohort taught in self-contained, non-tested classrooms the following year. Male, Hispanic, and experienced teachers were more likely to transition from tested to non-tested grades.

Ost (2011) is primarily concerned with identifying grade-specific returns to teaching experience. Ost shows that of North Carolina teachers who taught in self-contained classrooms in consecutive years between 1995 and 2007, about 70% remained in the same grade, most reassignments were to an adjacent grade, moving to higher and lower grades was equally likely, and that within-school grade switching is weakly related to students current performance.

More generally, the current study is related to the literatures on teacher attrition and teacher sorting, as grade-level reassignments are a type of teacher turnover. Guarino, Santibanez, and Daley (2006) thoroughly review the literature on teacher attrition and mobility, which generally finds higher rates of teacher turnover in urban and low-performing schools, schools comprised of low-income or minority students, and amongst inexperienced, female, white, and highly-credentialed teachers. Loeb, Darling-Hammond, and Luczak (2005) find good working conditions and higher pay to be associated with reduced rates of turnover. However, the existing literature largely ignores within-school changes in teaching assignments as a type of teacher turnover.

An exception to this critique is Boyd, Lankford, Loeb, and Wyckoff (2008), who investigate the effect of a newly-implemented fourth-grade test in New York State on fourth-grade teacher turnover, where turnover is defined as either leaving the fourth grade or leaving the public-school system. Using administrative data, the authors find that teachers were more likely to remain in the newly-tested fourth grade and that this change was primarily driven by a decrease in the grade-level reassignments of fourth-grade teachers (as opposed to attrition from the profession). The Boyd et al. (2008) study exemplifies the potential importance of grade-level reassignments in operationalized definitions of teacher turnover.

Similarly, Cohen-Vogel (2011) investigates the extent to which principals in ten Florida elementary schools use student-performance data to reassign teachers across grade levels and make staffing decisions more generally. The author presents qualitative evidence that principals felt free to make grade-level reassignments and typically considered both teachers preferences and performance when making such decisions. Cohen-Vogel and Osborne-Lampkin (2007) analyze the collective-bargaining agreements of 66 Florida school districts and find that while teachers seniority matters, administrators retain a reasonable amount of discretion in changing teachers assignments. Furthermore, the authors find that collective-bargaining agreements are not more rigid in low-performing, poor, or minority schools.

We contribute to the existing literature by examining patterns in the frequency of grade switching and documenting the distribution of grade switching across different types of schools and teachers. We further test whether grade switching predicts other types of teacher turnover and compare the predictors of grade switching to those of other types of teacher turnover. By providing a more comprehensive picture of the distributions of different types of teacher turnover, the current analysis furthers our understanding of the functioning of teacher labor markets and the equity of teacher quality.

3.3 Data and Methods

The primary analysis examines teachers grade-level assignments in the Registry of Educational Personnel (REP) database, an administrative employee-level panel data set maintained by Michigans Center for Educational Performance and Information (CEPI). The REP contains information on all public-education employees in Michigan, but the sample is restricted to teachers in self-contained classrooms for the 2003-04 through 2008-09 school years. This leaves a final sample of 113,978 observations on 33,390 unique teachers.

We augment the REP data with publicly available school- and district-level information from two additional sources. First, data on school type, student demographics, and locale are taken from the National Center for Education Statistics Common Core of Data. Second, we use grade-level math proficiency rates, publicly provided by the Michigan Department of Education.[2] Proficiency rates indicate the fraction of the schools students who tested as proficient on the Michigan Education Assessment Program (MEAP) standardized test.[3] Proficiency rates increased statewide throughout the sample period. To ensure comparability across years and grades, we standardize proficiency rates to have a mean of zero and a standard deviation of one within each year and grade. Because the external validity of state-level analyses is always a concern, we also compute a variety of teacher turnover rates using data from the nationally representative 1999 and 2003 Schools and Staffing Surveys (SASS) and corresponding Teacher Follow-Up Surveys (TFS).[4] The SASS and TFS are conducted by the U.S. National Center for Education Statistics (NCES) and are publicly

[2]See: http://www.michigan.gov/mde/0,4615,7-140-22709_31168_31530---,00.html.

[3]See Michigan Department of Education (2005) for an overview of the MEAP tests.

[4]We are unable to use the 2007 SASS, as the 2008 TFS does not record current grade.

available.[5] The SASS is a nationally-representative random sample of approximately 43,000 elementary and secondary public school teachers in each survey year. The TFS follows up with approximately 5,300 randomly sampled SASS respondents the following year to see if and where they are still teaching. The SASS-TFS analysis is restricted to regular full-time kindergarten through fifth-grade self-contained classroom teachers who were surveyed by the TFS. The final sample excludes special education teachers and teachers who taught more than one grade in the SASS survey year. These restrictions yield samples of 763 and 1,069 teacher observations for the 1999-00 and 2003-04 school years, respectively.

In the empirics we identify the predictors of grade switching and other types of turnover in Michigan by estimating logit models of the form

$$Pr(y_{igst} = 1|\cdot) = \Lambda\left(\beta_0 + \beta_1 x_{it} + \beta_2 z_{st} + \eta_g + \tau_t\right) \tag{3.1}$$

where y is a binary indicator of teacher turnover between years t and $t+1$, i indexes teachers, g indexes grades, s indexes schools, x is a vector of possibly time-varying teacher characteristics, z is a vector of possibly time-varying school characteristics, η is a grade fixed effect (FE), and τ is a year FE. Extensions of equation (3.1) are considered that add school, district, or grade-by-year FE to the model. The parameters of equation (3.1) are estimated by maximum likelihood, from which we compute average partial effects (APE) of each covariate on the conditional probability of turnover.[6] Subsequent analyses report APE standard errors that are adjusted for clustering at the district level. Adjusting for clustering at the district level makes statistical inference robust to the presence of correlation within districts and schools, as schools are nested in districts.[7]

[5]See http://nces.ed.gov/surveys/sass/dataproducts.asp.

[6]See Wooldridge (2010) for the definitions and estimation of APE for both continuous and discrete explanatory variables in binary choice models such as logit.

[7]See Angrist and Pischke (2009, p. 319) for a discussion of clustering at the highest level. Technically, two-way standard errors that allow for correlation within both teachers and districts are appropriate, as teacher sometimes change districts (Cameron, Gelbach, and Miller, 2008). Changing districts is rare in the current data, however, and estimated two-way standard errors are marginally smaller than one-way district clustered standard errors in the

3.4 Analysis

3.4.1 The Prevalence and Distribution of Grade Switching

Table C.1 displays the prevalence of teacher grade reassignments in relation to other types of teacher turnover. In particular, the first panel of table C.1 describes the overall and year-specific turnover of Michigans self-contained kindergarten through fifth-grade classroom teachers between 2003-04 and 2008-09. Of teachers remaining in a self-contained classroom in the same school the following year, 76% remained in the same grade. The percentage of teachers who changed grades but remained in a self-contained classroom in the same school is 6.7%. Notably, this figure is similar in magnitude to two commonly-used measures of turnover: the percentages who changed schools (6.1%) and who exited the Michigan public school system (5.3%). The frequency of within-school grade-level reassignments in Michigan is fairly constant across years, ranging from 6% to 7%. Not reported in table C.1 is the finding that grade switch rates are also similar across grades, ranging from 5% to 8%, and that most grade changes are to an adjacent grade.[8] Similarly, other types of turnover are fairly constant over time and across grades. The second panel of table C.1 reports similar average turnover rates derived from nationally representative SASS data. The SASS data suggest that grade switching is a national phenomenon and that national rates of teacher turnover, including grade switching, are similar in magnitude to those observed in Michigan. Table C.2 examines the distribution of switching across individual teachers in Michigan. Grade switching is not driven by a small number of serial switchers, as 63% of grade-level reassignments were experienced by teachers who changed grades only once and another 24% of reassignments involved teachers who changed grades twice. Overall, a non-trivial 18%

current application. Thus we report the more conservative (i.e., larger) district-clustered standard errors.

[8] Markov transition matrices reported in appendix table H.1 show that teachers are most likely to move to adjacent grades and that moving to lower grades is slightly more common. These patterns are similar to those documented by Ost (2011) using data from North Carolina and do not vary by year.

of Michigans self-contained classroom elementary-school teachers experienced at least one grade-level reassignment between the 2003-04 and 2008-09 school years.

3.4.2 The Correlates of Grade Switching

Table C.3 documents the unadjusted differences in average teacher and school characteristics included by the number of teacher switches. The first column reports overall averages for the six-year sample. Each subsequent column of table C.3 reports the same average characteristics separately by the number of times that teachers changed grades between 2003-04 and 2008-09. A few differences emerge between the teachers who never changed grades and those who did. For example, non-switchers were more likely to hold a Masters degree and to have ten or more years of teaching experience. Similarly, teachers who experienced no grade-level reassignments were less likely to be in urban and charter schools, and were more likely to be in the highest-performing (fourth-quartile of proficiency rates) schools.[9] In addition, teachers who switch grades are more likely to teach in urban schools and schools with high proportions of students eligible for free or reduced-price lunch. The following section examines these correlates in more detail, by conditioning on teacher and school observables and focusing on teacher-years as the unit of analysis.

Table table C.4 reports logit-model APE of teacher and school characteristics on the probability that self-contained classroom teachers changed grades, but remained in the same school the following year. Column 1 of table C.4 contains a baseline specification that conditions on teacher and school characteristics, grade taught, and a full set of year dummies. Teachers race and education are not significant predictors of grade switching. A small, marginally significant effect of gender is found, suggesting that female teachers are about half of a percentage point more likely to be reassigned than male teachers. Teachers age and

[9] The practice of looping (i.e., teachers changing grades in lockstep with a student cohort) may cause certain schools to have multiple teachers who repeatedly change grades. However, we find no evidence of systematic looping in Michigan, and do not believe that this drives the results.

experience are strongly statistically significant predictors of grade switching, although only the experience effects are practically significant: teachers with two to nine years of experience are between one and two percentage points less likely to change grades than new teachers, while those with ten or more years of experience are more than three percentage points less likely to do so. Teachers in urban schools are significantly more likely to experience grade-level reassignments than their counterparts in rural and suburban schools. The most important predictor of grade switching in column 1 is charter-school status, as teachers in charter schools are more than four percentage points less likely to change grades than teachers in traditional public schools, and this difference is strongly statistically significant.

We include school-level attrition from the school, district or Michigan public education in the model to test whether grade switching is more common in schools with high attrition rates, or if grade switching is a substitute for other types of teacher turnover. The former may result from principals shuffling teachers grade-level assignments in an effort to fill the vacancies created by teacher attrition when the availability of external replacements is limited. Alternatively, the latter would occur if dissatisfied teachers change (or are asked to change) grades before changing schools or leaving the profession to see if a different grade provides a better fit. Attritions positive and statistically significant APE suggests that grade switching is more common in high-attrition schools, although the difference is practically small.[10]

An interesting non-finding regards the relationship between school achievement levels and grade switching: the grade-level indicators of schools math-proficiency quartiles are individually and jointly insignificant. In fact, the APE appear to be precisely-estimated zeros, which suggest that grade switching is not concentrated in either high- or low-performing schools. This non-finding is robust to instead measuring school quality with Michigan's School Report Card grades (Michigan Department of Education, 2007).

Relative to fifth-grade teachers and the omitted group of kindergarten teachers, the

[10]The results on teacher- and school-level predictors of grade switching are qualitatively similar when controlling for higher-order polynomials of turnover or quartiles of turnover.

results in column 1 of table C.4 show that first, second, third, and fourth-grade teachers are between 1 and 2 percentage points more likely to change grades and that these differences are strongly statistically significant. This is likely because in the sample, kindergarten and fifth-grade teachers have only one adjacent grade switch to, and the majority of grade-level reassignments are to an adjacent grade. Of course, the result for fifth-grade teachers may change if the analysis was extended to include sixth-grade teachers. The year dummies, which omit 2003-04 as the base year, indicate no clear trends in the frequency of grade switching.

Column 2 of table C.4 adds grade-by-year fixed effects (i.e., grade-year interaction terms) to the baseline specification of column 1. Doing so is potentially important, as the years studied in the current analysis witnessed the implementation of high-stakes tests in certain grades associated with the 2001 No Child Left Behind Act. As a result, pressures to change grades potentially varied systematically by grade-year during this time period. However, adding grade-by-year fixed effects (FE) does not change the estimated effects of the teacher or school characteristics in a meaningful way; nor does this addition explain a significant amount of variation in grade switching, as evidenced by the nearly identical pseudo-R2 in columns 1 and 2.

Columns 3 and 4 of table C.4 add district and school FE to the baseline specification, respectively. The estimates in columns 3 and 4 are nearly identical to one another, suggesting that variation across schools is less important than variation across districts. The district and school FE do explain a nontrivial amount of variation in within-school grade-level reassignments, as the pseudo-R2 increases by a factor of 3 when district FE are added to the baseline model and by a factor of 4 with the addition of school FE.[11]

With two exceptions, the estimates in columns 3 and 4 are remarkably similar to those

[11] It is also reasonable to condition on principal FE. Unfortunately, reliable principal data is missing for more than 20% of school-year observations, so we do not report these results. However, estimates of such models using observations for which principal data are available yield qualitatively similar results to those that include school FE.

of the baseline specification in column 1, suggesting that the main results are not driven by unobserved differences between districts. First, when looking at within-district (or within-school) variation, teachers holding a masters degree become significantly less likely to change grades. This result is interesting despite the relatively small effect size, as it suggests that some combination of the distribution of teachers and the functioning of internal (within-school) teacher labor markets vary by unobserved district characteristics. Second, the estimated APEs of the schools black population and school-level attrition rate lose statistical significance, which is likely the result of these variables exhibiting little within-district variation between 2003 and 2008.

Finally, we attempt to better understand the relationship between teachers experience and grade switching, as experience is consistently one of the most important predictors of grade switching in columns 1 through 4 of table C.4. We distinguish between total teaching experience and tenure in the current school, as the predictive ability of experience may vary by type of experience. Such differences may arise because teachers within-school seniority or relationships with school administrators matter more than general teaching experience, or because teachers with substantial teaching experience have identified their preferred grade level, regardless of their tenure at their current school.

Specifically, the model estimated in column 5 of table C.4 adds a "new to school" indicator to the school-FE specification of column 4.[12] The results suggest that all else equal, teachers in their first year in a school are about one percentage point more likely to change grades than teachers with more school-specific experience. The overall effect of experience diminishes slightly, but remains strongly statistically significant for teachers with more than ten years of experience; estimated effects of the other teacher and school characteristics are unchanged. This basic finding is robust to either removing the school FE or replacing them with district FE.

[12] We lose one year of data in the process, as the administrative data records total experience in the school district, but not in any particular school.

3.4.3 The Relationship between Grade Switching and Other Types of Teacher Turnover

This section considers the relationship between grade switching and other types of teacher turnover. We begin by testing grade switchings ability to predict future teacher turnover, as grade switching may be indicative of teachers unease in the classroom or schools instability. In table C.5 we extend table C.4's baseline specification (column 1) to include a year-specific count of teachers previous grade-level reassignments and examine the effect of previous grade changes on the probability of remaining in the same teaching assignment, changing grades, changing schools, and leaving Michigan public education in four separate logit models.[13] As in column 5 of table C.4, one year of data is lost when creating this variable.

Column 1 of table C.5 shows that past switches significantly lower the probability of teachers remaining in the same grade and school in consecutive years. Column 2 suggests that most of this decrease is due to the 5 percentage point increase in the probability of changing grades associated with each past switch. However, columns 3 and 4 of table C.5 suggest that grade switching does not strongly predict other sorts of teacher turnover.

It is also of interest to compare the predictors of grade switching reported in table C.4 to those of other types of teacher turnover, as the optimal design and targeting of policies aimed at decreasing teacher turnover may vary by turnover type. Accordingly, columns 1 and 2 of table C.6 report logit-model APE of select covariates on the probability of changing schools and of leaving teaching, respectively. The specifications estimated in table C.6 are otherwise identical to the baseline specification of column 1 in table C.4. Again, adding district or school FE to the logit models estimated in table 6 does not change the qualitative results.

A comparison of the estimated APE in table C.6 to those in column 1 of table C.4 yields several similarities: teachers experience and the racial makeup of schools similarly

[13] We estimate separate logits rather than a multinomial logit (MNL) to avoid making the strong Independence of Irrelevant Alternatives assumption, though MNL results are qualitatively similar.

influence the probability of all three types of teacher turnover. There are some notable differences, however, especially among the school characteristics. For example, teachers in charter schools are significantly less likely to change grades or schools, but are more likely to leave the teaching profession. Similarly, school performance (as measured by grade-level proficiency quartiles) is not associated with changing grades or leaving the profession, but is significantly negatively correlated with changing schools. This may be reflective of teachers systematically moving from lower- to higher-performing schools throughout as their careers progress.

3.5 Discussion

This study examines the frequency and predictors of teachers within-school grade-level reassignments using rich administrative data from Michigan between 2003-04 and 2007-08. This time period witnessed a nontrivial number of such grade changes, as in any given year about 7% of the states self-contained kindergarten through fifth-grade teachers experienced a grade-level reassignment. The phenomenon of grade switching is not unique to Michigan, as a similar rate of about 9% is found in nationally representative data over a similar time period. In Michigan, and nationally, the rate of grade switching is similar in magnitude to rates of attrition from the profession and mobility across schools.

Grade switching is more common in schools with high attrition rates, which may be the result of principals responses to vacancies created by teachers departures. Urban schools with higher fractions of minority students are found to have higher levels of grade switching, even conditional on the amount of teacher turnover in the school. While grade switching does not appear to vary by schools achievement levels, charter schools have significantly fewer grade-level reassignments than their traditional public school counterparts. This suggests that charter school principals may be following the advice of researchers such as Jacob and Rockoff (2011) by minimizing grade-level reassignments.

Teachers with more experience, both overall and in the current school, are found to be significantly less likely to change grades, as are teachers in charter schools. The negative correlation between grade switching and teachers experience likely results from some combination of experienced teachers having relatively more input in their teaching assignments and having learned which grade(s) they are most comfortable in. Importantly, grade switching predicts future grade switching but no other types of teacher turnover, suggesting that grade switching is not an early indicator of teachers dissatisfaction with the profession.

A limitation of the current study is its inability to differentiate between teacher- and principal-induced reassignments. While nearly 90% of teacher mobility across schools is at teachers, as opposed to administrators, discretion (Keigher, 2010), it is difficult to determine the corresponding percentage of grade-level or subject reassignments initiated by teachers. Indeed, Cohen-Vogel (2011) suggests that such reassignments are initiated by both teachers and principals. Furthermore, some reassignments may be determined by mutual agreement or compromise. Future iterations of nationally representative surveys of teachers and/or principals might consider adding items that ask the reason for grade and subject reassignments.

Nonetheless, the general finding that grade switching is non-random has several implications for administrators, principals, and policy makers seeking to improve student outcomes. That the predictors of teacher turnover vary by turnover type suggests that principals, policymakers, and analysts must think carefully about the operationalized definition of teacher turnover and recognize grade (and potentially subject) reassignments as a nontrivial type of within-school turnover when devising and implementing teacher-retention programs and investigating the impact of high-stakes accountability on teacher turnover (Boyd et al., 2008).[14]

Finally, the finding that new teachers are more likely to change grades following their first

[14] The frequency and non-random distribution of grade switching may also have implications for value-added modeling. For instance, omitting grade-specific experience from value-added models may contribute to the time instability frequently observed in rankings of estimated teacher effects (e.g., McCaffrey, Sass, Lockwood, and Mihaly (2009).

year in a new school, even after controlling for total years of teaching experience, suggests that the within-school politics of seniority play an important role in determining grade-level reassignments. This is not to say that all schools should adjudicate teaching assignments in the same manner, as the optimal level of grade switching will depend on the specific school and district context. Rather, the results of the current analysis underscore the potential benefits of paying greater attention to teachers grade-level and subject reassignments. The relatively high rates of grade switching observed in Michigan and nationally suggest that there are potentially large gains in student performance to be had by reconsidering the frequency with, and reasons for, which teachers change grades.

APPENDICES

APPENDIX A

TABLES FOR "THE EFFECT OF SCHOOL CLOSINGS ON STUDENT ACHIEVEMENT"

Table A.1: Descriptive Statistics

Variable	Mean	SD	Min	Max
Student Characteristics				
Grade	5.540	1.711	3.000	8.000
Free/Reduced-Price Lunch Eligible	0.429	0.495	0.000	1.000
African-American/Hispanic	0.240	0.427	0.000	1.000
Mathematics Score	0.006	0.998	-4.631	3.381
Reading Score	0.003	0.999	-4.288	2.222
School Characteristics				
School Enrollment	538.5	233.9	0.0	1793.0
2 Years Prior to Closure	0.006	0.076	0.000	1.000
1 Year Prior to Closure	0.010	0.098	0.000	1.000
Year of Closure	0.012	0.107	0.000	1.000
1 Year After Closure	0.009	0.092	0.000	1.000
2 Years After Closure	0.004	0.066	0.000	1.000
3+ Years After Closure	0.003	0.056	0.000	1.000
Fraction of Students Displaced 1 Year Prior	0.006	0.035	0.000	0.740
Fraction of Students Displaced 2 Years Prior	0.003	0.021	0.000	0.613
Fraction of Students Displaced 3+ Years Prior	0.002	0.016	0.000	0.618
Fraction of Teachers Displaced 1 Year Prior	0.007	0.055	0.000	1.000
Fraction of Teachers Displaced 2 Years Prior	0.004	0.042	0.000	1.000
Fraction of Teachers Displaced 3+ Years Prior	0.004	0.044	0.000	1.000
District Characteristics				
District Closed School Previous Year	0.070	0.254	0.000	1.000
Number of Observations	3,416,174			
Number of Students	1,252,101			
Number of Schools	2,926			

The level of observation is the student*year.

Table A.2: Descriptives of Students in Closed Schools

Variable	Students in Closed Schools	District Mean	Entire Sample
Female	0.488	0.497	0.493
African-American	0.536	0.496	0.187
Hispanic	0.053	0.079	0.053
Special Education	0.151	0.137	0.127
Limited English Proficient	0.036	0.066	0.039
Free/Reduced Price Lunch Eligible	0.671	0.651	0.429
School Enrollment (K-5 Schools Only)	287.9	387.4	425.1
School Enrollment	430.8	524.4	538.5
Elementary School*	0.285	0.282	0.272
Rural School	0.032	0.055	0.244
Urban School	0.500	0.662	0.247
Mathematics Score**	-0.506	-0.390	0.006
Reading Score**	-0.436	-0.318	0.003
N	40,141	364,794	3,416,174

*An elementary school is defined as any school with highest grade less than or equal to 5.

**Test scores standardized by grade*year to have mean zero, variance 1.

Table A.3: Student and Teacher Movement after Closings

		Percentile					
	N	10	25	50	75	90	Mean
Fraction of Students Attending a New School District	246	0.015	0.031	0.056	0.104	0.169	0.093
Fraction of Students Attending a Charter School	246	0.000	0.000	0.025	0.077	0.160	0.064
Median Distance to New School for Student	239	0.441	0.825	1.370	2.043	4.846	2.117
Fraction of Students Attending the Modal Receiving School	246	0.197	0.275	0.350	0.487	0.632	0.389
Fraction of Teachers Attending the Modal Receiving School	246	0.115	0.167	0.250	0.438	0.714	0.339

Each observation represents a single closed school. The modal school is defined separately for students and teachers as the receiving school that receives the largest number of students or teachers from closed schools, respectively.

Table A.4: Average Effects of School Closings on Displaced Students in Michigan

	Math	Reading
2 Years Prior to Closure	-0.004	-0.010
	[0.029]	[0.010]
1 Year Prior to Closure	-0.041***	-0.013
	[0.014]	[0.011]
Year of Closure	-0.061**	-0.026
	[0.030]	[0.017]
1 Year after Closure	-0.074**	-0.053
	[0.033]	[0.033]
2 Years after Closure	-0.016	-0.041
	[0.020]	[0.029]
3+ Years after Closure	-0.010	-0.033
	[0.016]	[0.021]
N	3,416,174	3,416,174

The unit of observation is the student*year. Standard errors are adjusted for clustering at the district level. *** indicates significance at the 0.01 level, ** at the 0.05 level, and * at the 0.1 level.

Table A.5: Effects of School Closings on Receiving Schools

	Math	Reading
Displaced Students:		
Fraction of Displaced Students	-0.246*	-0.266**
	[0.132]	[0.113]
Fraction of Displaced Students (lagged)	-0.113	-0.208**
	[0.103]	[0.083]
Fraction of Displaced Students (twice lagged)	-0.200*	-0.064
	[0.111]	[0.068]
Fraction of Displaced Teachers	-0.071***	-0.035
	[0.019]	[0.030]
Fraction of Displaced Teachers (lagged)	-0.145***	-0.101***
	[0.042]	[0.034]
Fraction of Displaced Teachers (twice lagged)	-0.133***	0.025
	[0.033]	[0.059]
Students At Receiving Schools:		
Fraction of Displaced Students	-0.218*	-0.280**
	[0.112]	[0.133]
Fraction of Displaced Students (lagged)	-0.189***	-0.249
	[0.073]	[0.153]
Fraction of Displaced Students (twice lagged)	-0.184**	-0.257*
	[0.092]	[0.145]
Fraction of Displaced Teachers	0.003	-0.032*
	[0.031]	[0.019]
Fraction of Displaced Teachers (lagged)	-0.082***	-0.134***
	[0.029]	[0.032]
Fraction of Displaced Teachers (twice lagged)	-0.046	-0.114**
	[0.032]	[0.047]
N	3,416,174	3,416,174

The unit of observation is the student*year. Standard errors are adjusted for clustering at the district level. *** indicates significance at the 0.01 level, ** at the 0.05 level, and * at the 0.1 level.

Table A.6: School Closings and Voluntary Student Mobility

	Displaced Students	Voluntary Movers from Closed Schools	Other Voluntary Movers
Panel A: Mathematics Achievement			
2 Years Prior to Move	-0.005	–	0.002
	[0.050]	–	[0.004]
1 Year Prior to Move	-0.046*	0.057***	-0.002
	[0.026]	[0.020]	[0.005]
Year of Move	-0.052	-0.033*	-0.027**
	[0.035]	[0.019]	[0.011]
1 Year after Move	-0.075*	-0.005	-0.036***
	[0.044]	[0.016]	[0.005]
2 Years after Move	-0.022	0.036**	-0.022***
	[0.028]	[0.017]	[0.005]
3+ Years after Move	-0.013	0.058**	-0.015***
	[0.021]	[0.025]	[0.004]
Panel B: Reading Achievement			
2 Years Prior to Move	-0.012	–	-0.002
	[0.035]	–	[0.004]
1 Year Prior to Move	-0.024	0.024*	-0.003
	[0.021]	[0.013]	[0.004]
Year of Move	-0.020	-0.013	-0.026***
	[0.027]	[0.017]	[0.007]
1 Year after Move	-0.072*	0.004	-0.019***
	[0.042]	[0.020]	[0.005]
2 Years after Move	-0.060	-0.010	-0.009**
	[0.039]	[0.023]	[0.004]
3+ Years after Move	-0.036	-0.012	-0.012***
	[0.029]	[0.021]	[0.005]

N=3,416,174. All coefficients for a given dependent variable are estimated in the same regression. Standard errors are adjusted for clustering at the district level. *** indicates significance at the 0.01 level, ** at the 0.05 level, and * at the 0.1 level.

Table A.7: Displaced Student Achievement Trajectories by Change in School Performance

	Closed School .5 s.d. below Nearby Schools	Closed School Same as Nearby Schools	Cloised School .5 s.d. above Nearby Schools
Mathematics:			
2 Years Prior to Closure	-0.018	0.004	0.027
	[0.023]	[0.028]	[0.036]
1 Year Prior to Closure	-0.048***	-0.034**	-0.019
	[0.013]	[0.013]	[0.017]
Year of Closure	-0.073**	-0.048	-0.023
	[0.032]	[0.034]	[0.036]
1 Year After Closure	-0.078**	-0.075**	-0.071**
	[0.038]	[0.035]	[0.035]
2 Years After Closure	0.010	-0.031	-0.073
	[0.056]	[0.023]	[0.048]
3 Years After Closure	0.043**	-0.019	-0.081***
	[0.018]	[0.017]	[0.021]
N		3,257,564	
Reading:			
2 Years Prior to Closure	-0.021	-0.009	0.004
	[0.019]	[0.011]	[0.032]
1 Year Prior to Closure	-0.036***	-0.009	0.019
	[0.013]	[0.015]	[0.021]
Year of Closure	-0.053***	-0.019	0.015
	[0.016]	[0.011]	[0.016]
1 Year After Closure	-0.068**	-0.057*	-0.047
	[0.032]	[0.030]	[0.032]
2 Years After Closure	0.036	-0.054**	-0.144
	[0.087]	[0.027]	[0.115]
3 Years After Closure	-0.045	-0.047**	-0.048
	[0.039]	[0.021]	[0.034]
N		3,292,314	

Reported trajectories based on a linear combination of coefficients shown in Appendix F.2. Standard errors are adjusted for clustering at the district level. *** indicates significance at the 0.01 level, ** at the 0.05 level, and * at the 0.1 level.

APPENDIX B

TABLES FOR "THE EFFECT OF FOUR-DAY SCHOOL WEEKS ON STUDENT ACHIEVEMENT: EVIDENCE FROM HAWAIIAN SCHOOL FURLOUGH DAYS"

Table B.1: Comparison of HSA and NAEP Results

		HSA		NAEP		*Difference*	
Grade	Year	Level	Change	Level	Change	Level	Change
Mathematics:							
4	2007	0.478	-	0.330	-	-0.148	-
4	2009	0.507	0.029	0.370	0.040	-0.137	0.011
4	2011	0.610	0.103	0.400	0.030	-0.210	-0.073
8	2007	0.258	-	0.210	-	-0.048	-
8	2009	0.391	0.133	0.250	0.040	-0.141	-0.093
8	2011	0.539	0.148	0.300	0.050	-0.239	-0.098
Reading:							
4	2007	0.537	-	0.260	-	-0.277	-
4	2009	0.612	0.075	0.260	0.000	-0.352	-0.075
4	2011	0.674	0.062	0.270	0.010	-0.404	-0.052
8	2007	0.599	-	0.200	-	-0.399	-
8	2009	0.663	0.064	0.220	0.020	-0.443	-0.044
8	2011	0.681	0.018	0.260	0.040	-0.421	0.022

Table B.2: Descriptive Statistics

	Less than 17 Furlough Days			17 Furlough Days			
Variable	2007-2009	2010	2011-2012	2007-2009	2010	2011-2012	Total
Math Proficiency	0.313	0.349	0.455	0.426	0.482	0.578	0.474
Reading Proficiency	0.632	0.690	0.700	0.603	0.648	0.695	0.645
Enrollment	280.953	299.000	317.391	686.412	684.749	678.198	652.907
Pupil-Teacher Ratio	12.705	12.883	12.952	15.536	15.608	15.710	15.323
Fraction African-American	0.014	0.013	0.017	0.022	0.021	0.024	0.022
Fraction Hispanic	0.028	0.028	0.027	0.047	0.046	0.049	0.045
Fraction Asian/Pacific Islander	0.705	0.683	0.635	0.730	0.733	0.755	0.731
Fraction Free/Reduced-Price Lunch	0.415	0.428	0.502	0.436	0.470	0.503	0.454
Title I School	0.655	0.690	0.586	0.587	0.615	0.641	0.608
Charter	1.000	1.000	1.000	0.008	0.008	0.008	0.108
N	215	97	210	1782	622	1247	4173

The unit of observation is the school*grade*year, excluding grade 3. Schools that took less than 17 furlough days are grouped with schools that took no furlough days. Refer to Appendix G.1 for full descriptive statistics of sample. CCD data for year 2012 is not available yet.

Table B.3: ITS Estimates – Grades 4 -5

	(1)	(2)	(3)	(4)
Panel A: Mathematics				
Year 2010	-0.056***	-0.051***	-0.078***	-0.044***
	(0.006)	(0.006)	(0.011)	(0.006)
Year	0.041***	0.037***	0.022***	0.016*
	(0.002)	(0.003)	(0.004)	(0.008)
Year*Post2010			0.008	
			(0.008)	
Year Squared				0.004**
				(0.001)
Panel B: Reading				
Year 2010	-0.011*	-0.008	-0.006	-0.005
	(0.005)	(0.005)	(0.011)	(0.005)
Year	0.030***	0.027***	0.022***	0.016*
	(0.002)	(0.002)	(0.003)	(0.007)
Year*Post2010			0.012	
			(0.007)	
Year Squared				0.002*
				(0.001)
Linear Trend	x	x	x	x
Controls		x		
Slope Shift in 2010			x	
Quadratic Trend				x
Observations	2052	1691	2052	2052
Clusters	180	179	180	180
Years	6	5	6	6

Sample restricted to only grades 4 and 5. The unit of observation is the school*grade*year. Year is coded so that the 2007 school year is year 1. Standard errors in parentheses adjusted for clustering at the school level. * denotes significance at the 0.10 level, ** at the 0.05 level, ** at the 0.01 level.

Table B.4: DD Estimates – Grades 4-5

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Mathematics								
(Furlough Days)/17	0.022	0.032	0.027	0.022	0.018	0.010	0.028	0.010
	(0.033)	(0.037)	(0.033)	(0.033)	(0.035)	(0.034)	(0.034)	(0.036)
Lead of (Furlough Days)/17		0.055			0.055			
		(0.031)			(0.031)			
Panel B: Reading								
(Furlough Days)/17	0.016	0.025	0.039	0.014	0.029	0.043	0.033	0.039
	(0.030)	(0.031)	(0.023)	(0.030)	(0.035)	(0.025)	(0.026)	(0.027)
Lead of (Furlough Days)/17		-0.008			-0.009			
		(0.038)			(0.038)			
Group Specific Trends				x	x	x		
School Specific Trends							x	x
Controls			x			x		x
Year FE	x	x	x	x	x	x	x	x
Observations	2228	1839	1823	2228	1839	1823	2228	1823
Clusters	203	201	200	203	201	200	203	200
Years	6	5	5	6	5	5	6	5

Sample restricted to only grades 4 and 5. The unit of observation is the school*grade*year. Standard errors in parentheses adjusted for clustering at the school level. * denotes significance at the 0.10 level, ** at the 0.05 level, ** at the 0.01 level.

Table B.5: ITS Estimates – Grades 6 -10

	(1)	(2)	(3)	(4)
Panel A: Mathematics				
Year 2010	0.003	0.007	0.016	0.012
	(0.006)	(0.006)	(0.011)	(0.006)
Year	0.043***	0.039***	0.033***	0.024**
	(0.002)	(0.003)	(0.004)	(0.008)
Year*Post2010			0.020*	
			(0.008)	
Year Squared				0.003**
				(0.001)
Panel B: Reading				
Year 2010	-0.005	-0.013*	0.036**	-0.011
	(0.006)	(0.006)	(0.012)	(0.006)
Year	0.025***	0.030***	0.040***	0.038***
	(0.002)	(0.002)	(0.003)	(0.006)
Year*Post2010			0.008	
			(0.007)	
Year Squared				-0.002*
				(0.001)
Linear Trend	x	x	x	x
Controls		x		
Slope Shift in 2010			x	
Quadratic Trend				x
Observations	1599	1305	1599	1599
Clusters	185	182	185	185
Years	6	5	6	6

The unit of observation is the school*grade*year. Year is coded so that the 2007 school year is year 1. Standard errors in parentheses adjusted for clustering at the school level. * denotes significance at the 0.10 level, ** at the 0.05 level, ** at the 0.01 level.

Table B.6: DD Estimates – Grades 6-10

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Mathematics								
(Furlough Days)/17	0.030	0.035*	0.037*	0.029	0.035*	0.030*	0.029	0.029
	(0.016)	(0.016)	(0.015)	(0.016)	(0.017)	(0.015)	(0.016)	(0.016)
Lead of (Furlough Days)/17		-0.004			-0.004			
		(0.024)			(0.025)			
Panel B: Reading								
(Furlough Days)/17	-0.020	-0.006	-0.014	-0.023	-0.018	-0.025	-0.017	-0.025
	(0.018)	(0.021)	(0.020)	(0.017)	(0.019)	(0.018)	(0.018)	(0.021)
Lead of (Furlough Days)/17		0.004			0.002			
		(0.017)			(0.018)			
Group Specific Trends				x	x	x		
School Specific Trends							x	x
Controls			x			x		x
Year FE	x	x	x	x	x	x	x	x
Observations	1945	1603	1567	1945	1603	1567	1945	1567
Clusters	213	213	209	213	213	209	213	209
Years	6	5	5	6	5	5	6	5

Sample restricted to only grades 6-10. The unit of observation is the school*grade*year. Standard errors in parentheses adjusted for clustering at the school level. * denotes significance at the 0.10 level, ** at the 0.05 level, ** at the 0.01 level.

Table B.7: Heterogeneity by School Demographics

Panel A: Quartiles of Percent FRL in 2007 (Grades 4-5)

	Quartile			
	(1)	(2)	(3)	(4)
Mathematics	−0.037***	−0.048***	−0.034**	−0.056***
	(0.011)	(0.010)	(0.012)	(0.014)
Reading	−0.037***	−0.000	−0.008	−0.012
	(0.011)	(0.007)	(0.010)	(0.014)

Panel B: Quartiles of Percent Proficient in 2007 (Grades 4-5)

	Quartile			
	(1)	(2)	(3)	(4)
Mathematics	−0.054***	−0.063***	−0.041***	−0.026**
	(0.014)	(0.014)	(0.010)	(0.009)
Reading	−0.054***	−0.021	−0.008	0.009
	(0.014)	(0.012)	(0.008)	(0.008)

Panel C: Quartiles of Percent FRL in 2007 (Grades 6-10)

	Quartile			
	(1)	(2)	(3)	(4)
Mathematics	0.017	0.012	0.022	0.000
	(0.013)	(0.010)	(0.013)	(0.013)
Reading	0.017	−0.017	−0.002	−0.010
	(0.013)	(0.012)	(0.011)	(0.015)

Panel D: Quartiles of Percent Proficient in 2007 (Grades 6-10)

	Quartile			
	(1)	(2)	(3)	(4)
Mathematics	0.018	0.018	−0.020	0.028*
	(0.011)	(0.010)	(0.017)	(0.013)
Reading	0.018	−0.003	−0.032*	−0.020
	(0.011)	(0.010)	(0.015)	(0.010)

Higher quartile schools are schools with higher proportions of free/reduced price lunch eligible students or proficient students, respectively. Dependent variable is school*grade*year math proficiency rate. Estimates are based on specifications such as those presented in column (4) of Table B.3 and Table B.5. Standard errors in parentheses adjusted for clustering at the school level. * denotes significance at the 0.10 level, ** at the 0.05 level, ** at the 0.01 level.

APPENDIX C

TABLES FOR "THE FREQUENCY AND CORRELATES OF TEACHERS GRADE-LEVEL REASSIGNMENTS: EVIDENCE FROM MICHIGAN"

Table C.1: Self-contained Teachers' Assignments the Following Year

Year(s)	Same self-contained grade 1	New self-contained grade 2	Changed school 3	Changed district 4	Left self-contained classroom 5	Left teaching 6	Left MI public education 7	N 8
In Michigan (REP Administrative Data)								
2003/04	76.40%	6.00%	5.80%	0.90%	3.60%	0.50%	6.80%	22,933
2004/05	73.80%	7.00%	6.60%	1.20%	4.10%	1.50%	5.80%	22,250
2005/06	75.80%	6.30%	6.50%	1.00%	3.90%	1.50%	5.10%	23,018
2006/07	75.80%	7.30%	6.30%	0.90%	3.60%	1.30%	4.80%	22,905
2007/08	78.30%	6.80%	5.20%	1.00%	3.10%	1.50%	4.00%	22,872
All	76.00%	6.70%	6.10%	1.00%	3.60%	1.30%	5.30%	113,978
In U.S. (SASS Nationally Representative Sample)								
1999/00	72.70%	8.20%	3.60%	3.00%	6.80%	.	5.60%	760
2003/04	70.80%	9.50%	3.50%	2.50%	8.00%	.	5.60%	1,070
Both	71.70%	8.90%	3.60%	2.70%	7.50%	.	5.60%	1,830

The definitions of columns 1 – 7 are mutually exclusive and sum to 100%. "By year" refers to the initial self-contained year (e.g., the 2003-04 row reports the 2004-05 destination of teachers who were self-contained in 2003-04). SASS means are weighted to account for unequal probabilities of sample selection. SASS sample sizes are unweighted and are rounded to nearest ten.

Table C.2: Incidence of Grade-level Reassignments per Teacher

Switches per teacher	Teacher-year switches	% of switches	Teachers
1	4,761	62.60%	4,761
2	1,834	24.10%	917
3	660	8.70%	220
4	200	2.60%	50
5	150	2.00%	30
Total	7,605	100%	5,978

Michigan REP data. 5,978 teachers constitute 17.9% of the 33,390 teachers who taught in a self-contained kindergarten through fifth-grade classroom in Michigan between 2003-04 and 2007-08.

Table C.3: Average Characteristics by Teacher-year

	All teachers	Switches per teacher		
		0	1	> 1
Teacher Characteristics				
Black	4.00%	3.80%	4.80%	4.50%
Age	43.2	43.9	40.7	40.5
Female	89.60%	89.30%	90.50%	90.20%
Masters degree	51.90%	52.50%	50.20%	48.50%
No prev. experience	4.00%	4.10%	4.10%	3.20%
1 year experience	4.70%	4.50%	5.60%	4.80%
2 years experience	5.10%	4.80%	6.40%	5.70%
3-4 years experience	10.70%	9.90%	13.30%	13.90%
5-9 years experience	25.00%	23.50%	30.50%	31.70%
10+ years experience	50.50%	53.30%	40.30%	40.60%
New to school	34.70%	36.20%	30.50%	25.80%
2nd Year in School	24.50%	24.40%	25.10%	23.60%
3rd Year in School	17.90%	17.40%	19.50%	20.80%
School Characteristics				
Urban	21.10%	20.00%	24.70%	25.80%
Rural	32.40%	33.40%	28.80%	27.30%
Suburban	31.60%	31.10%	33.00%	34.60%
Title 1	37.20%	36.80%	38.70%	38.30%
% free/reduced reduced lunch	14.10%	13.30%	17.10%	16.80%
% black	6.80%	6.90%	6.90%	4.70%
Charter	22.70%	22.50%	23.10%	26.40%
Proficiency Quartile				
1st (lowest)	24.50%	24.60%	24.20%	25.10%
2nd	25.80%	26.00%	25.20%	23.60%
3rd	21.10%	20.00%	24.70%	24.90%
4th (highest)	32.40%	33.40%	28.80%	25.80%
N	113,978	89,354	19,155	5,469

Michigan REP data. Grade-level reassignments refer to the total number of reassignments experienced by each teacher during the six-year period.

Table C.4: Logit Average Partial Effects on Probability of Changing Grades

	1	2	3	4	5
Teacher Characteristics					
Black	0.0014	0.0014	(0.0009)	(0.0028)	(0.0007)
	(0.0045)	(0.0045)	(0.0041)	(0.0042)	(0.0052)
Age	-0.0011***	-0.0011***	-0.0011***	-0.0012***	-0.0012***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Female	0.0057*	0.0057*	0.0055	0.0068*	0.0068
	(0.0034)	(0.0034)	(0.0035)	(0.0036)	(0.0043)
Masters degree	(0.0036)	(0.0036)	-0.0055**	-0.0053**	-0.0078***
	(0.0025)	(0.0025)	(0.0023)	(0.0024)	(0.0027)
1 year experience	(0.0060)	(0.0060)	(0.0062)	(0.0055)	0.0048
	(0.0044)	(0.0044)	(0.0046)	(0.0052)	(0.0064)
2 years exp.	-0.0129***	-0.0129***	-0.0139***	-0.0139**	(0.0070)
	(0.0047)	(0.0047)	(0.0050)	(0.0055)	(0.0071)
3-4 years exp.	-0.0134***	-0.0133***	-0.0128***	-0.0122**	(0.0048)
	(0.0044)	(0.0044)	(0.0047)	(0.0053)	(0.0065)
5-9 years exp.	-0.0170***	-0.0170***	-0.0171***	-0.0171***	(0.0050)
	(0.0046)	(0.0046)	(0.0050)	(0.0053)	(0.0066)
10+ years exp.	-0.0322***	-0.0321***	-0.0337***	-0.0374***	-0.0269***
	(0.0056)	(0.0056)	(0.0059)	(0.0062)	(0.0074)
New to school	.	.	.	.	0.0117***
					(0.0031)
School Characteristics					
Urban	0.0115**	0.0115**	0.0047	.	.
	(0.0055)	(0.0055)	(0.0065)		
Rural	(0.0033)	(0.0033)	(0.0041)	.	.
	(0.0039)	(0.0039)	(0.0049)		
Title 1	0.0016	0.0017	0.0032	0.0072	0.0145**
	(0.0041)	(0.0041)	(0.0042)	(0.0055)	(0.0065)
% free/reduced lunch	(0.0031)	(0.0031)	0.0048	(0.0076)	(0.0011)
	(0.0083)	(0.0083)	(0.0112)	(0.0219)	(0.0240)
% black	0.0261***	0.0262***	(0.0025)	(0.0591)	(0.0706)
	(0.0080)	(0.0080)	(0.0111)	(0.0531)	(0.0676)
Charter	-0.0425***	-0.0424***	.	.	.
	(0.0066)	(0.0066)			
Attrition	0.0238**	0.0237**	0.0143	0.0240	0.0189
	(0.0110)	(0.0110)	(0.0125)	(0.0147)	(0.0188)

Table C.4 (cont'd)

	1	2	3	4	5
Proficiency Quartiles					
2nd	0.0048	0.0048	(0.0017)	(0.0049)	(0.0041)
	(0.0037)	(0.0037)	(0.0037)	(0.0050)	(0.0064)
3rd	0.0025	0.0025	(0.0015)	(0.0055)	(0.0028)
	(0.0041)	(0.0041)	(0.0043)	(0.0056)	(0.0070)
4th (highest)	(0.0007)	(0.0007)	(0.0025)	(0.0057)	(0.0080)
	(0.0048)	(0.0048)	(0.0046)	(0.0063)	(0.0080)
Classroom Characteristics					
First grade	0.0152***	.	0.0165***	0.0163***	0.0207***
	(0.0035)		(0.0035)	(0.0040)	(0.0047)
Second grade	0.0226***	.	0.0241***	0.0243***	0.0284***
	(0.0037)		(0.0037)	(0.0041)	(0.0047)
Third grade	0.0156***	.	0.0167***	0.0161***	0.0189***
	(0.0034)		(0.0035)	(0.0040)	(0.0046)
Fourth grade	0.0119***	.	0.0127***	0.0130***	0.0174***
	(0.0040)		(0.0040)	(0.0046)	(0.0052)
Fifth grade	-0.0074*	.	-0.0074*	(0.0021)	0.0002
	(0.0040)		(0.0040)	(0.0046)	(0.0054)
2005.00	0.0110**	.	0.0115**	0.0121*	0.0017
	(0.0056)		(0.0058)	(0.0063)	(0.0056)
2006.00	0.0045	.	0.0047	0.0051	(0.0072)
	(0.0057)		(0.0058)	(0.0060)	(0.0051)
2007.00	0.0153***	.	0.0161***	0.0173***	0.0056
	(0.0052)		(0.0053)	(0.0056)	(0.0044)
2008.00	0.0113**	.	0.0115**	0.0124**	-
	(0.0053)		(0.0052)	(0.0053)	-
Fixed Effects	None	Grade-year	District	School	School
Log likelihood	-27335	-27327	-25857	-24400	-19520
Pseudo R2	0.0214	0.0217	0.0660	0.0941	0.0996
Districts	692	692	597	598	588
Schools	2158	2158	2029	1603	1513
Observations	113978	113978	110406	100408	77317

Michigan REP data. $***p < 0.01, **p < 0.05, *p < 0.1$. The dependent variable is an indicator of within-school grade switching. Parentheses contain standard errors that are robust to clustering at the district level. Omitted categories include 0 years of prior experience, suburban school, proficiency quartile 1, kindergarten teacher, and 2004. In column 3, each charter school is coded as a unique district and receives its own fixed effect.

Table C.5: Grade Switching as a Predictor of Teacher Turnover

Outcome	Stay Put	Switch Grades	Leave school, district, or self-contained classroom	Leave teaching or state
	1	2	3	4
Prev. Switches	-0.0697***	0.0506***	0.0009	-0.0044*
	(0.0060)	(0.0030)	(0.0030)	(0.0025)
N	91045	91045	91045	91045
Pseudo R-2	0.0391	0.0457	0.0511	0.0748
Log-Likelihood	-48279	-21699	-29611	-20029

Michigan REP data. $***p < 0.01, *p < 0.1$. Parentheses contain standard errors that are robust to clustering at the district level. In addition to the "number of previous switches", the logit models estimated in this table include the same set of covariates as the model estimated in column 1 of table C.4. One year of data was lost in creating the "previous switches" variable. Adding school FE to the model produces qualitatively similar results.

Table C.6: Logit Average Partial Effects on Teacher Turnover

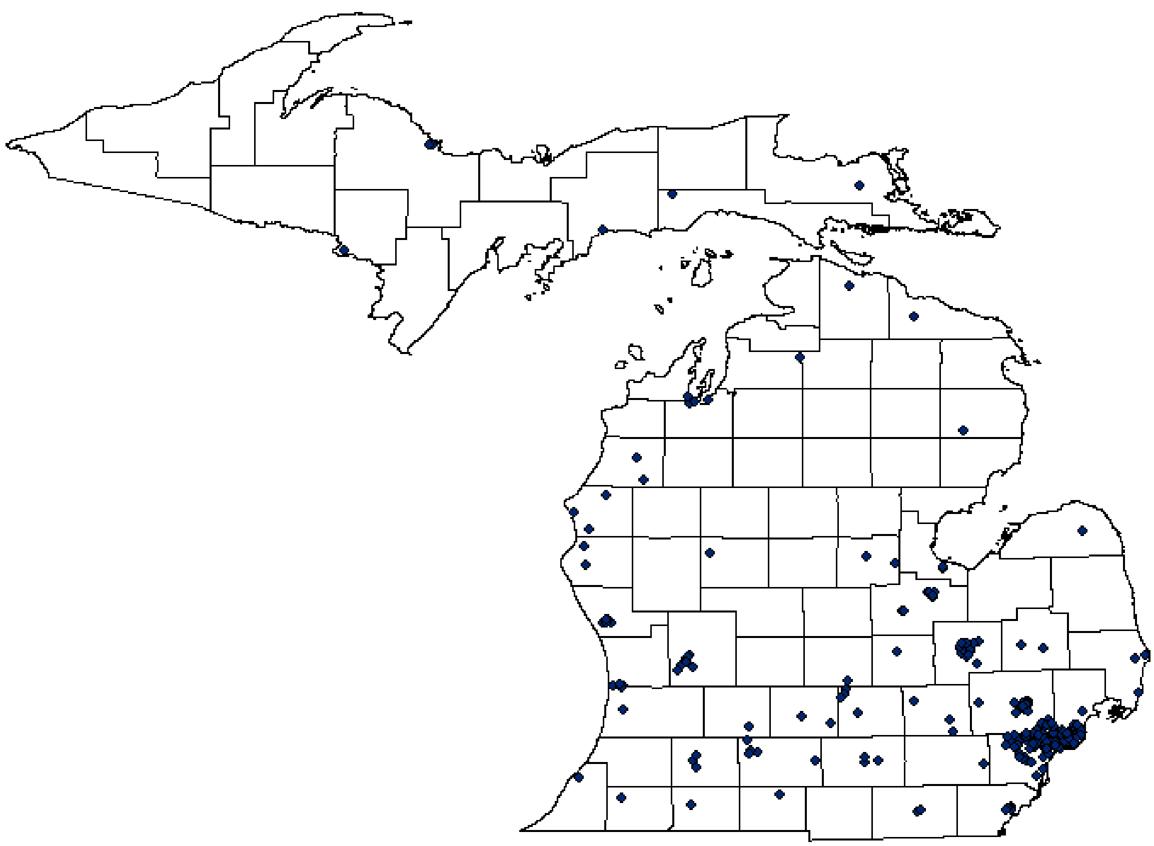
Type of turnover:	Leave school/district	Leave teaching/state
	1	2
Teacher Characteristics		
Masters degree	0.0058*	-0.0113***
	(0.0031)	(0.0017)
1 year experience	-0.0231***	-0.0177***
	(0.0057)	(0.0046)
2 years experience	-0.0344***	-0.0178***
	(0.0070)	(0.0049)
3-4 years exper.	-0.0428***	-0.0359***
	(0.0066)	(0.0051)
5-9 years exper.	-0.0586***	-0.0728***
	(0.0074)	(0.0050)
10+ years exper.	-0.0883***	-0.0606***
	(0.0082)	(0.0049)
School Characteristics		
% free/red. lunch	0.0480***	0.0067
	(0.0146)	(0.0053)
% black	0.0379***	0.0117**
	(0.0102)	(0.0047)
Charter	-0.0551***	0.0620***
	(0.0084)	(0.0040)
Quartile 2	-0.0197***	(0.0042)
	(0.0049)	(0.0026)
Quartile 3	-0.0289***	(0.0033)
	(0.0059)	(0.0026)
Quartile 4 (highest)	-0.0395***	(0.0020)
	(0.0069)	(0.0028)
Pseudo R-squared	0.0490	0.0730
Log Likelihood	-36925	-25643

Michigan REP data. N = 113,978. $***p < 0.01, **p < 0.05, *p < 0.1$. Parentheses contain standard errors that are robust to clustering at the district level. Only selected variables are reported; the specifications estimated in this table are identical to that estimated in column 1 of table 4, less attrition. Adding school FE to the models produces qualitatively similar results.

APPENDIX D

FIGURES FOR "THE EFFECT OF SCHOOL CLOSINGS ON STUDENT ACHIEVEMENT"

Figure D.1: Location of School Closings 2006-2009



For interpretation of the references to color in this and all other figures, the reader is referred to the electronic version of this dissertation.

Figure D.2: Distribution of School Performance Statewide

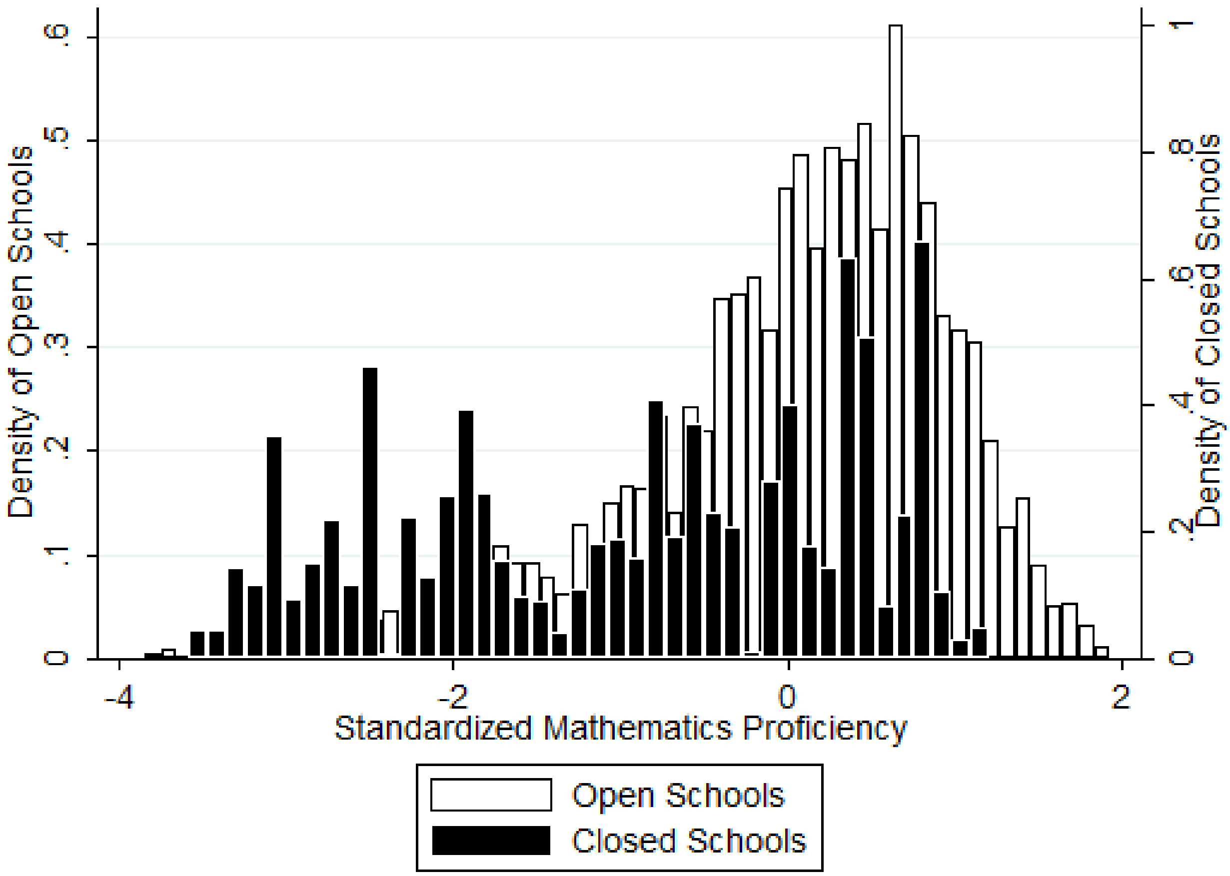


Figure D.3: Achievement Trajectory of Displaced Students by Performance of Closed School – Mathematics

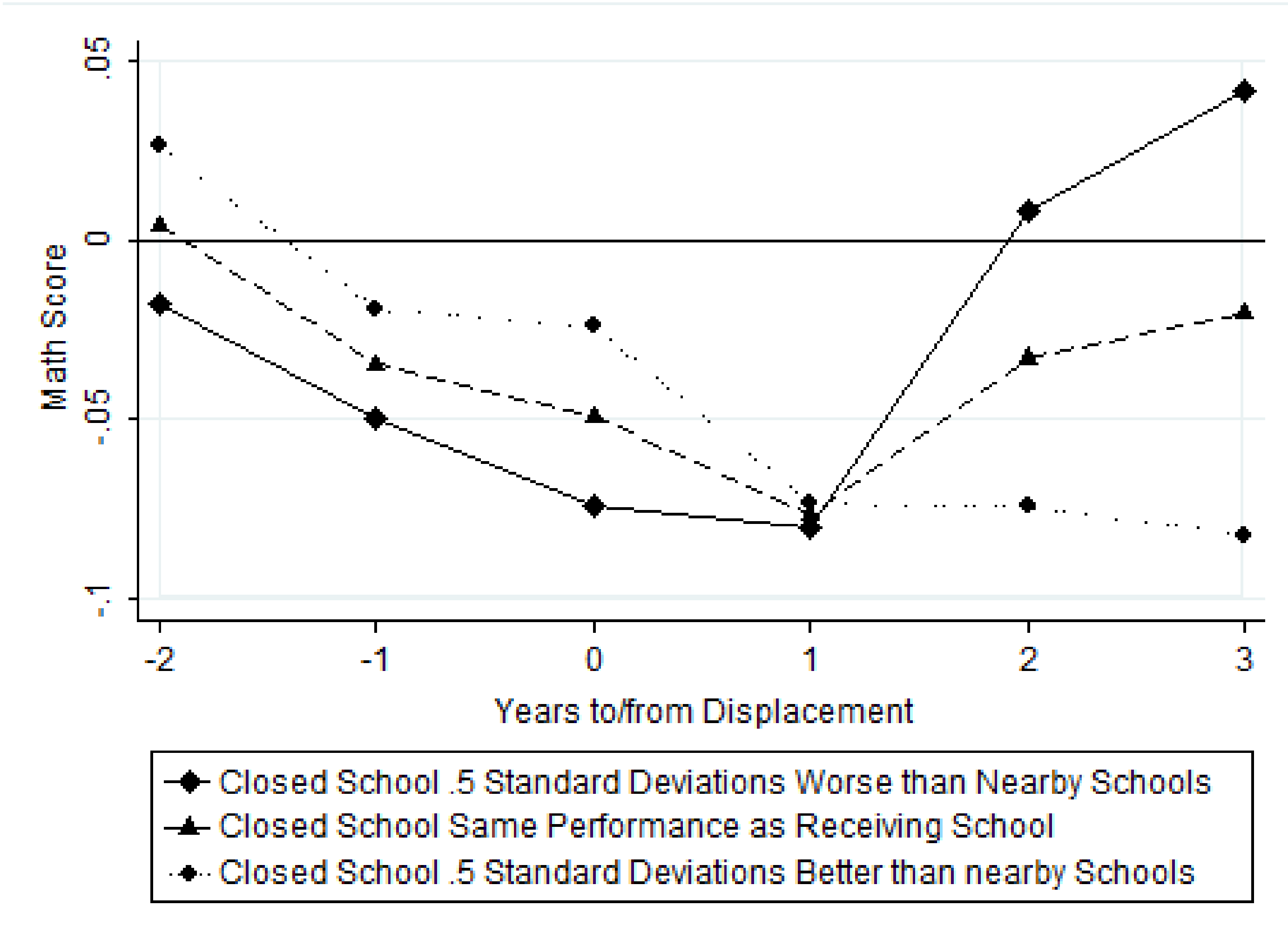


Figure D.4: Achievement Trajectory of Displaced Students by Performance of Closed School – Reading

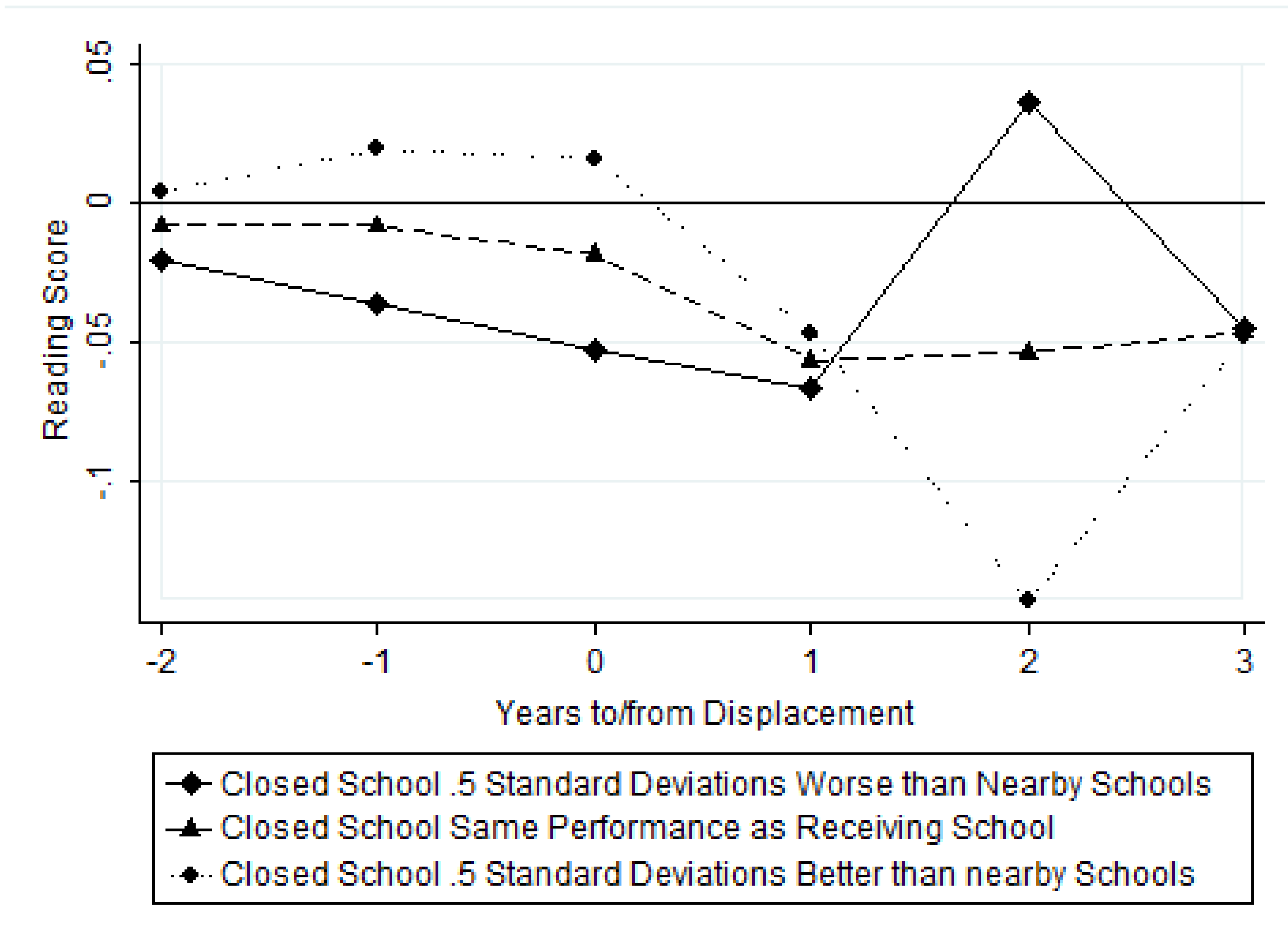


Figure D.5: Performance Trajectory at Schools Receiving Students by Performance of Closed School – Mathematics

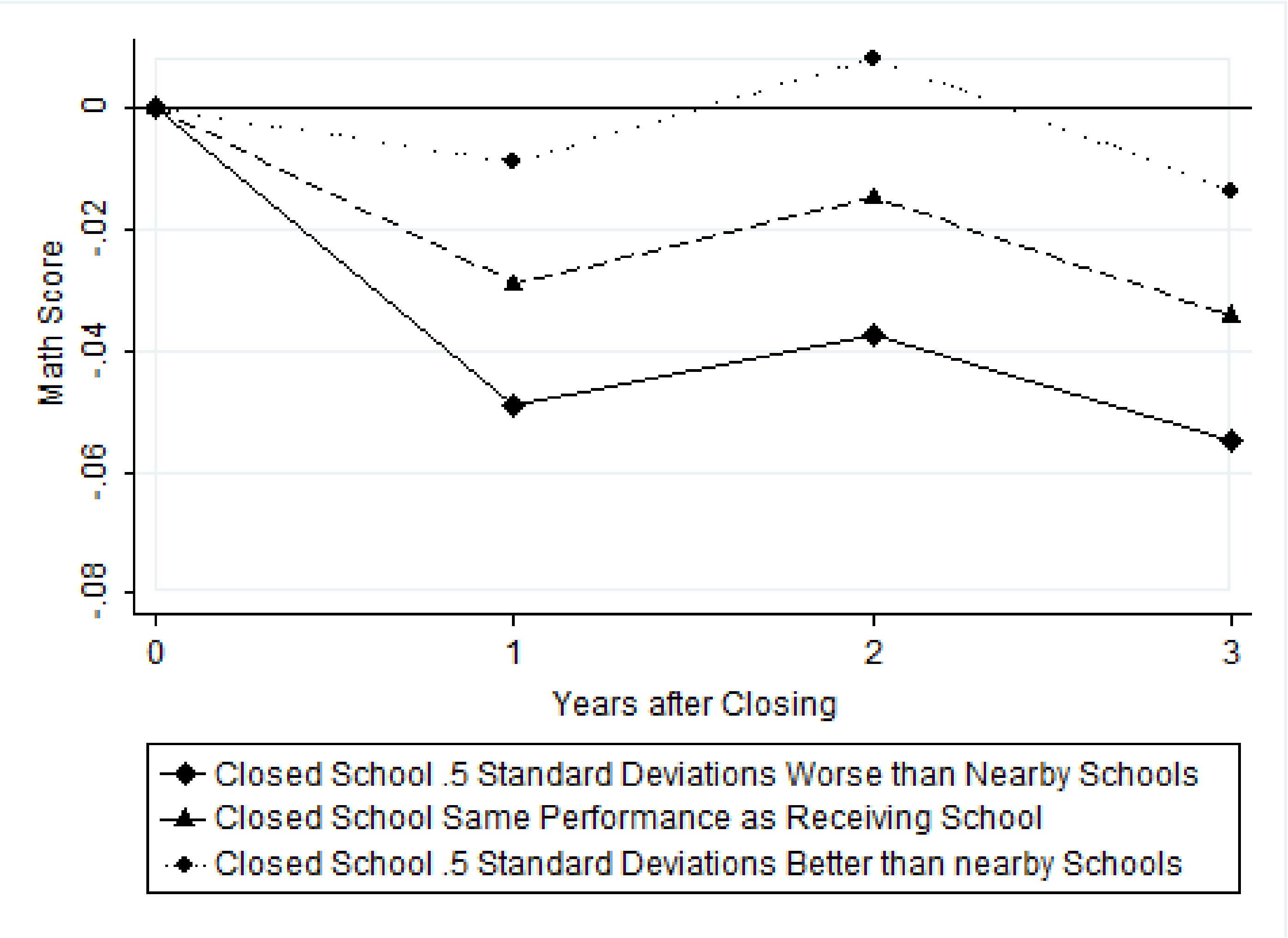


Figure D.6: Performance Trajectory at Schools Receiving Students by Performance of Closed School – Reading

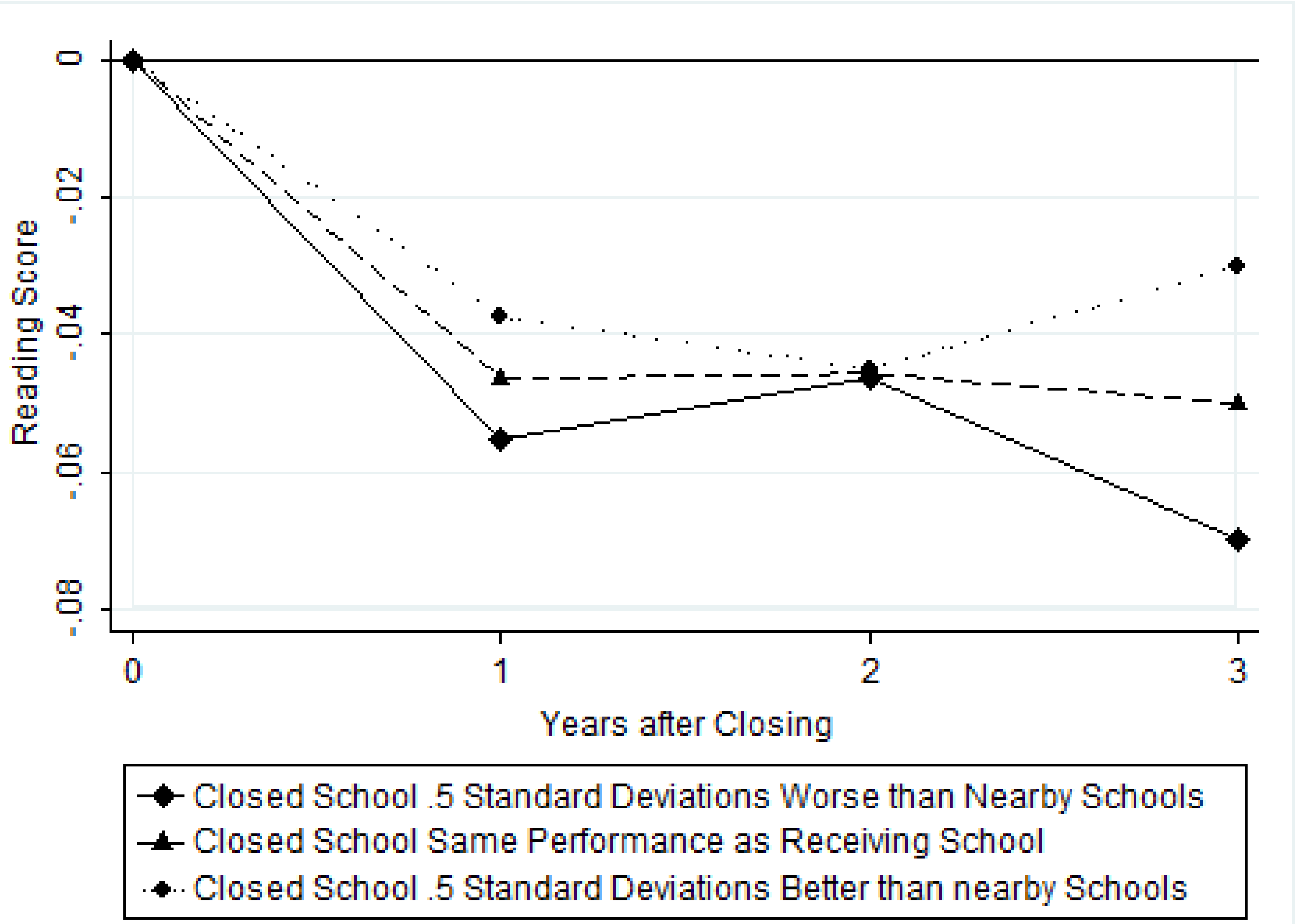


Figure D.7: Performance Trajectory at Schools Receiving Teachers by Performance of Closed School – Mathematics

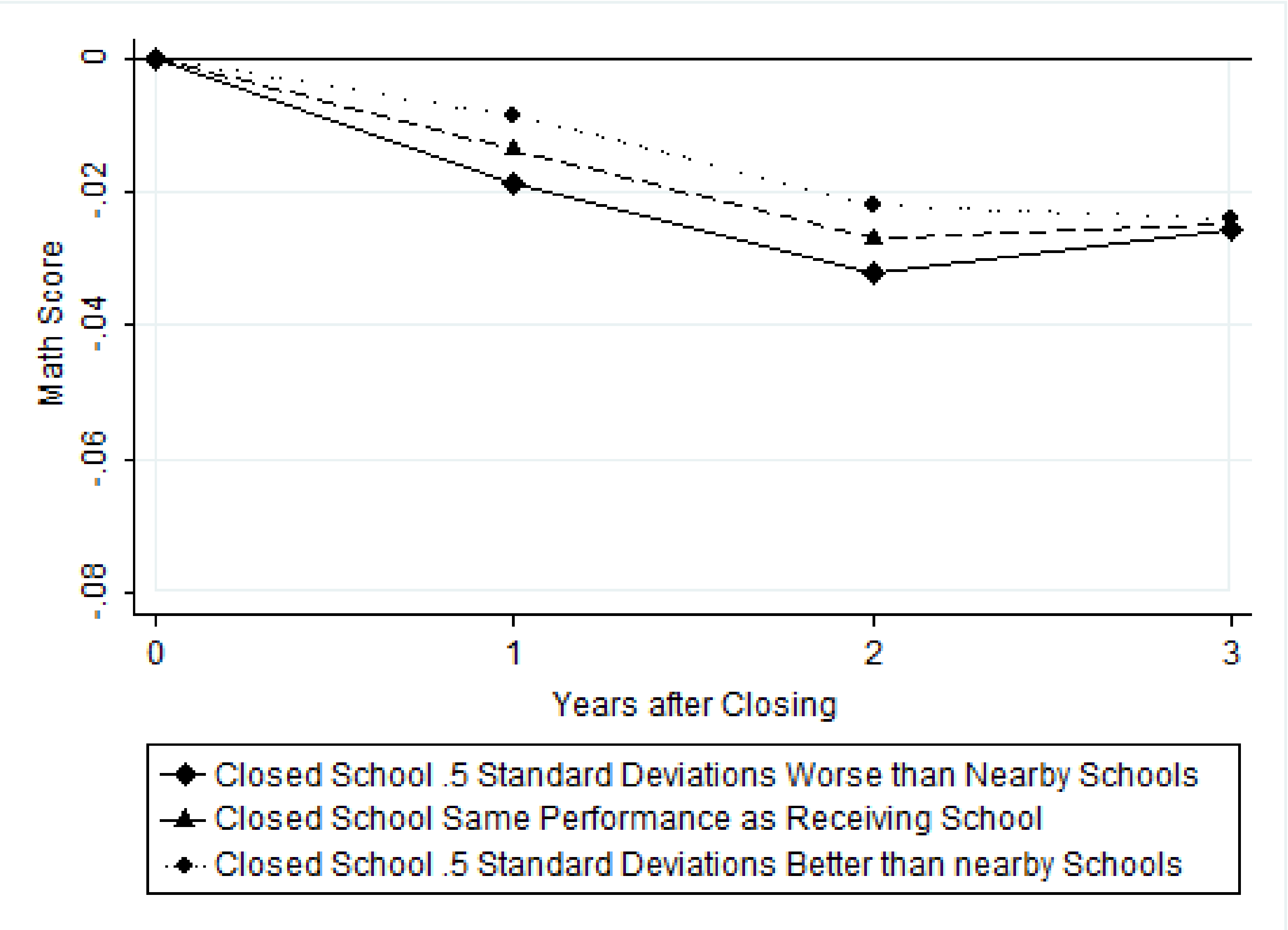
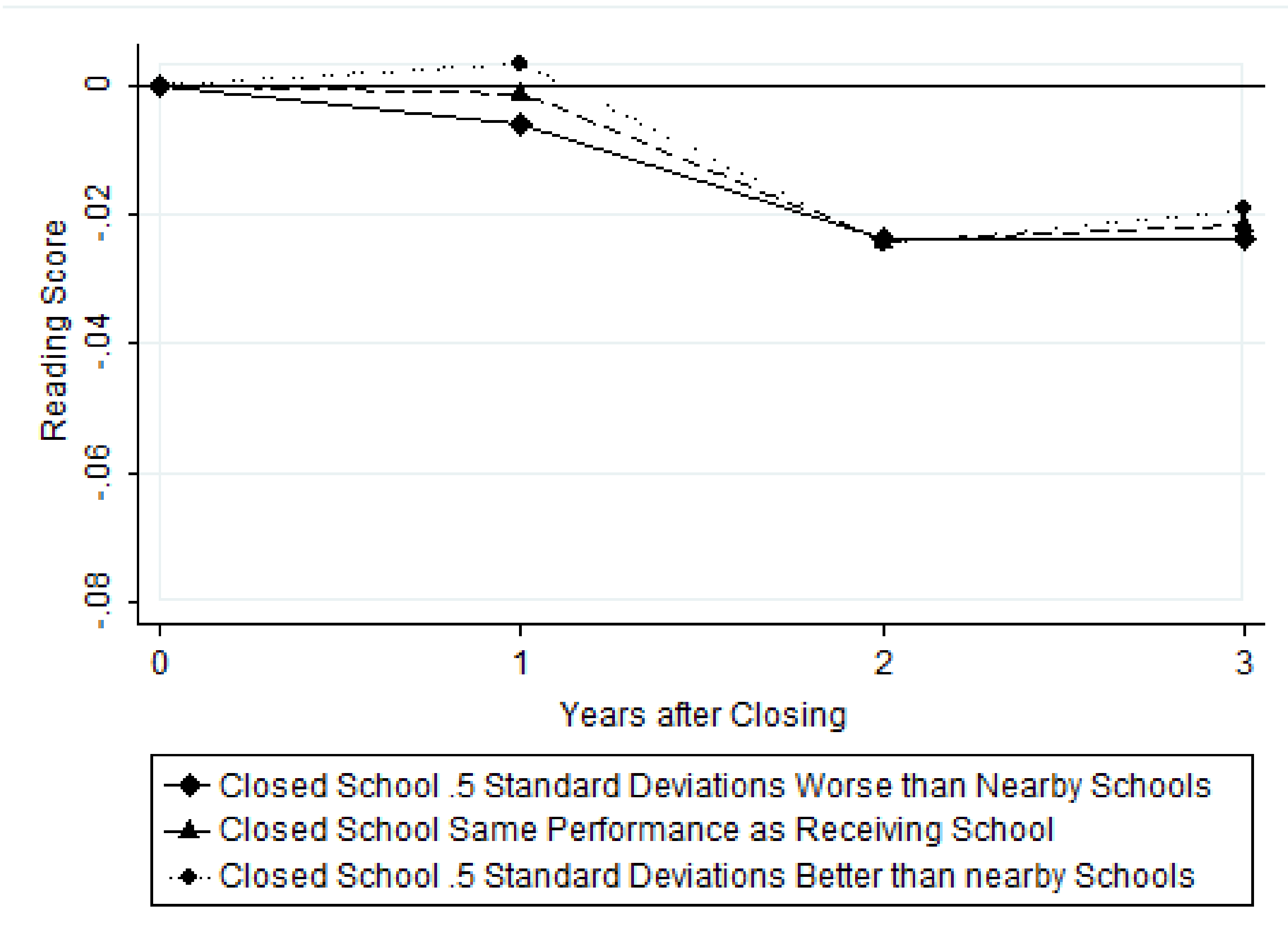


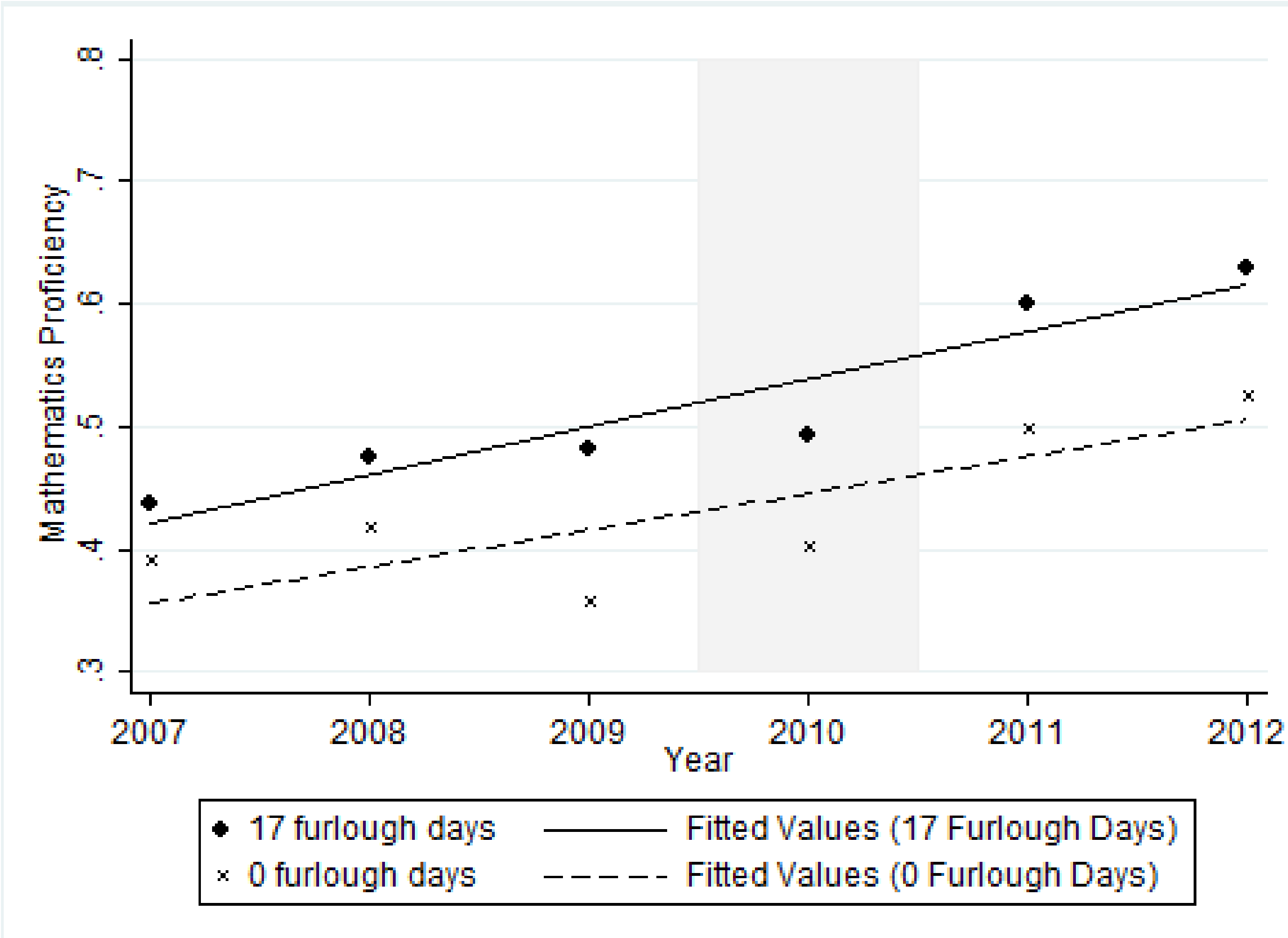
Figure D.8: Performance Trajectory at Schools Receiving Teachers by Performance of Closed School – Reading



APPENDIX E

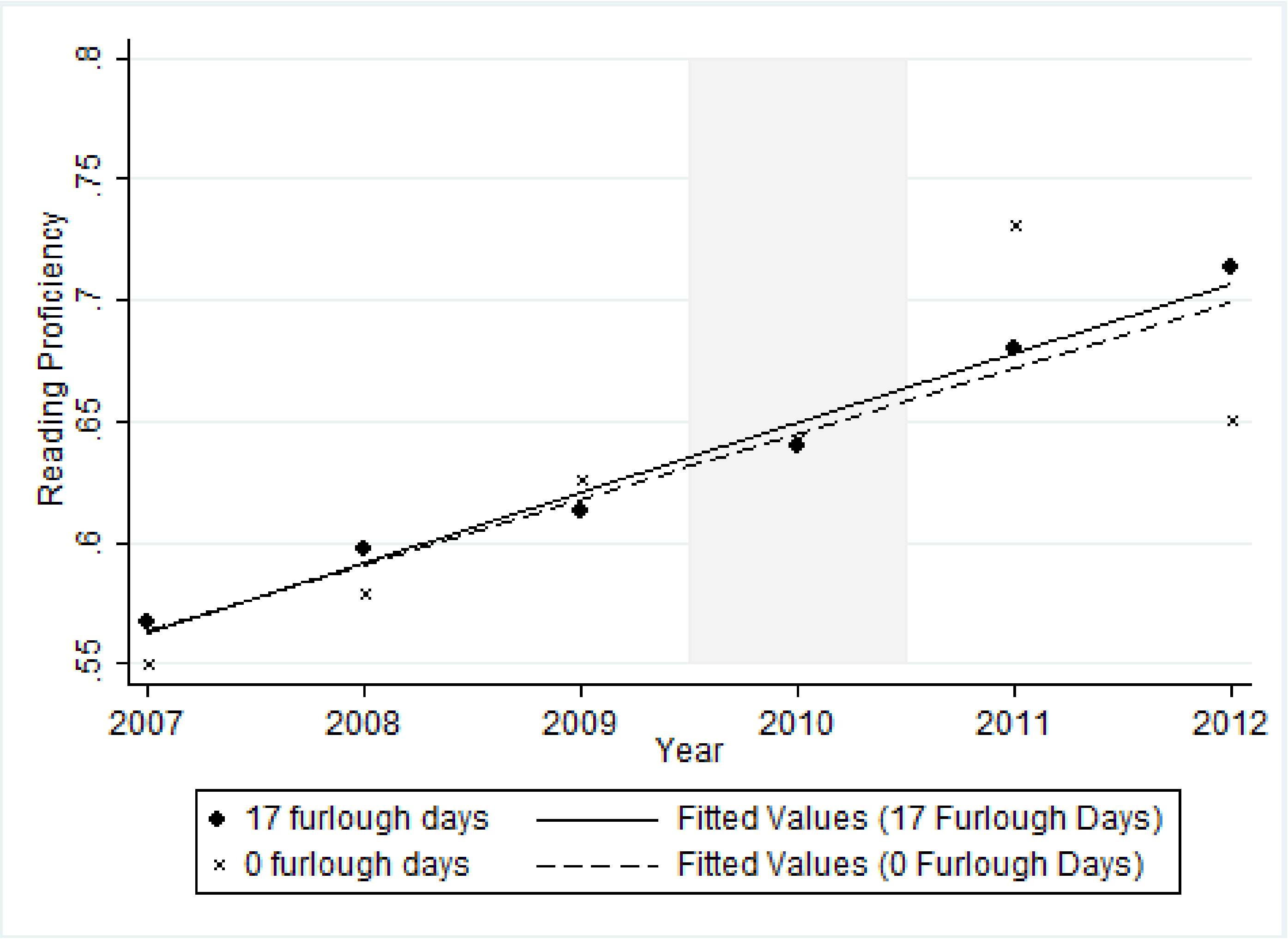
FIGURES FOR "THE EFFECT OF FOUR-DAY SCHOOL WEEKS ON STUDENT ACHIVEMENT: EVIDENCE FROM HAWAIIAN SCHOOL FURLOUGH DAYS"

Figure E.1: Grades 4-5 Mathematics Trajectories



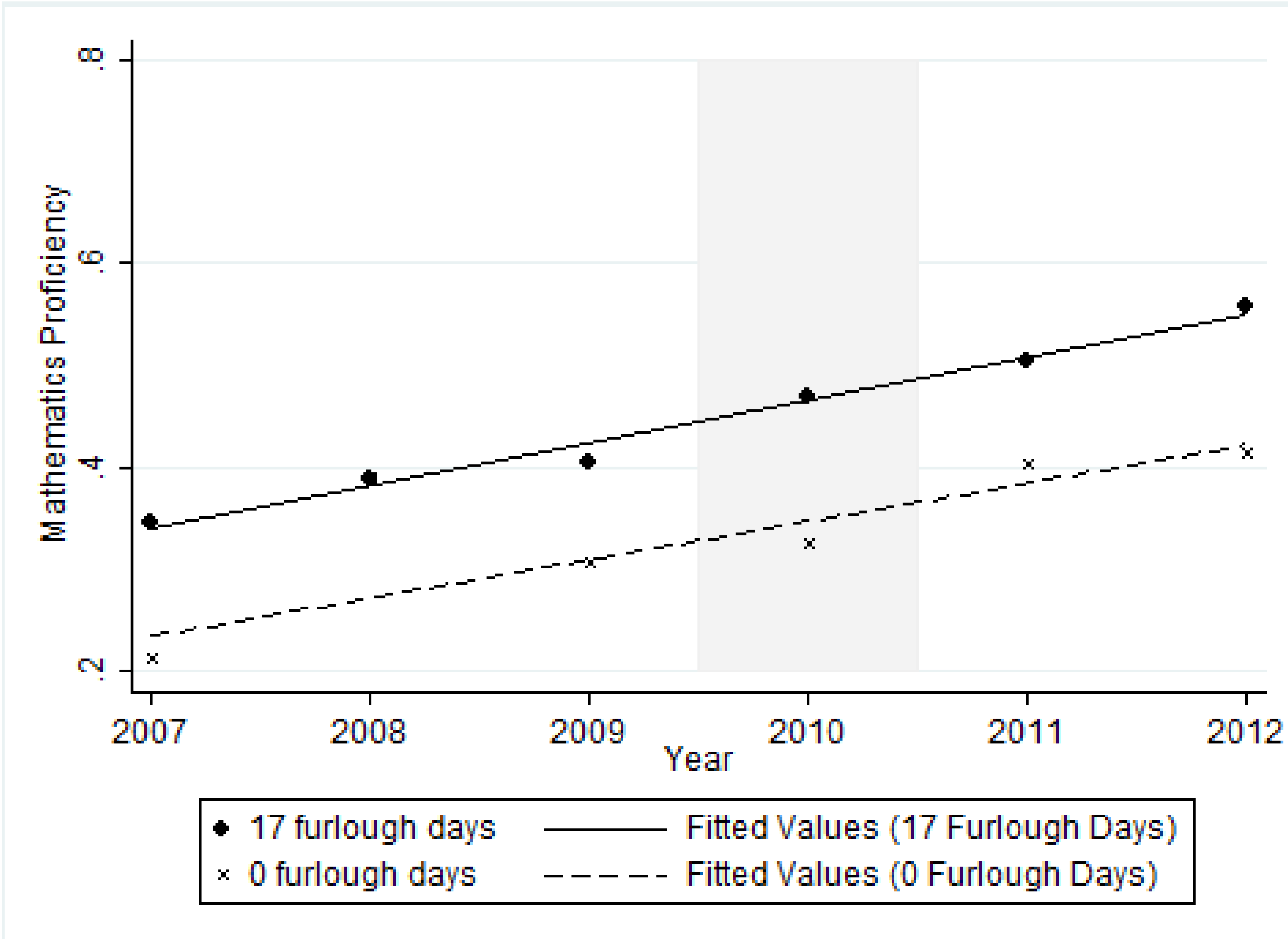
Fitted lines represent linear regressions of average achievement on year.

Figure E.2: Grades 4-5 Reading Trajectories



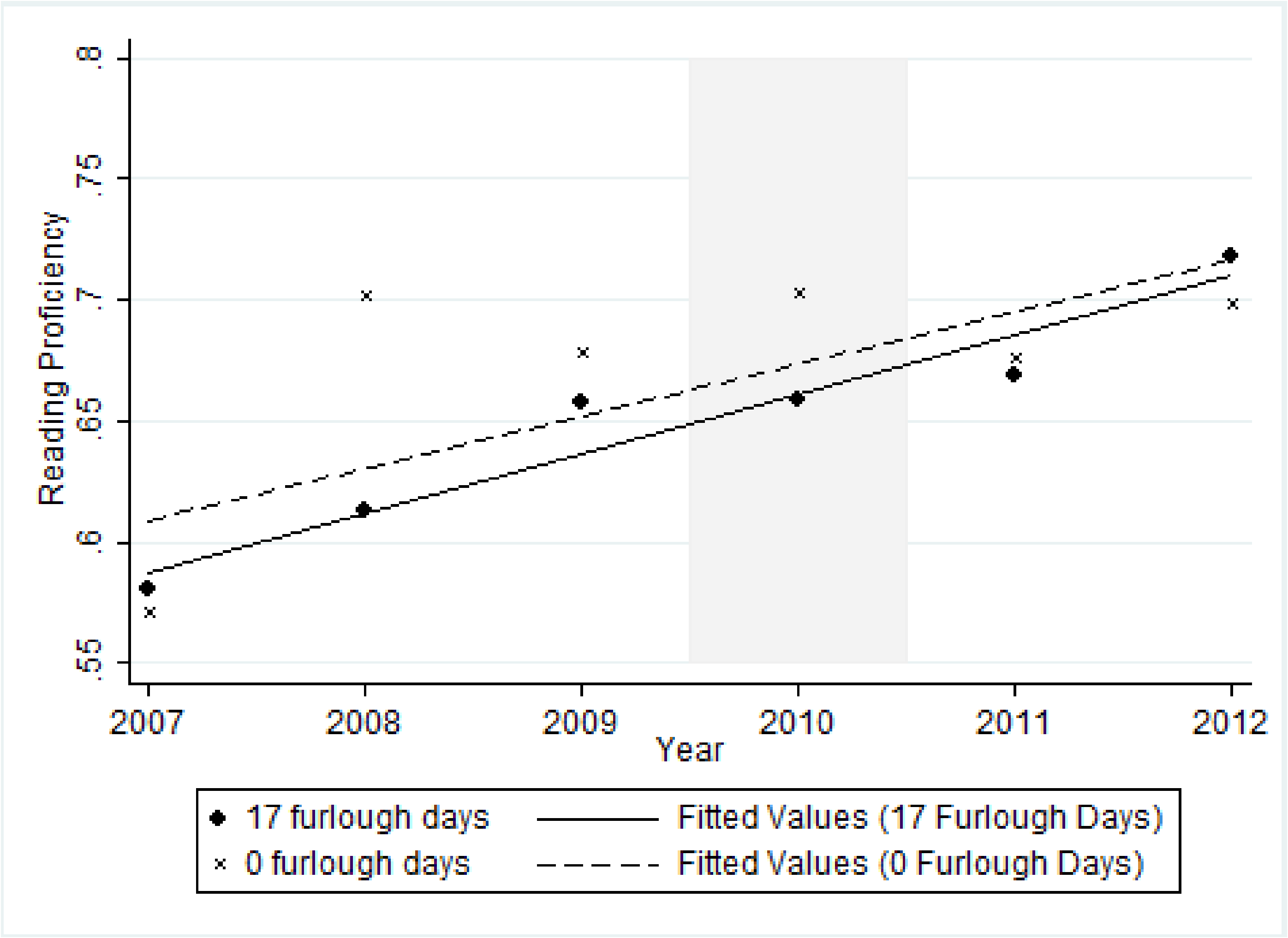
Fitted lines represent linear regressions of average achievement on year.

Figure E.3: Grades 6-10 Mathematics Trajectories



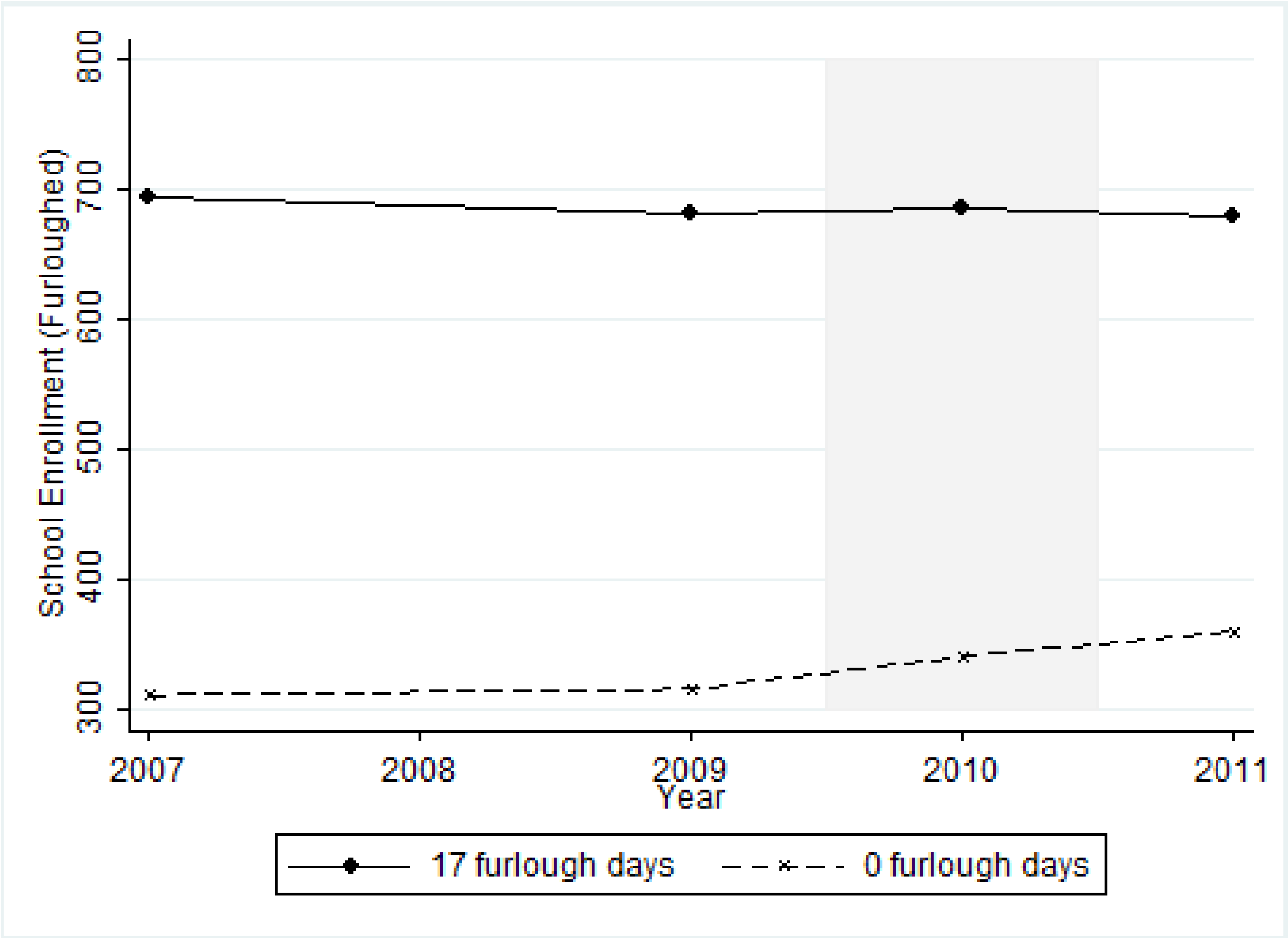
Fitted lines represent linear regressions of average achievement on year.

Figure E.4: Grades 6-10 Reading Trajectories



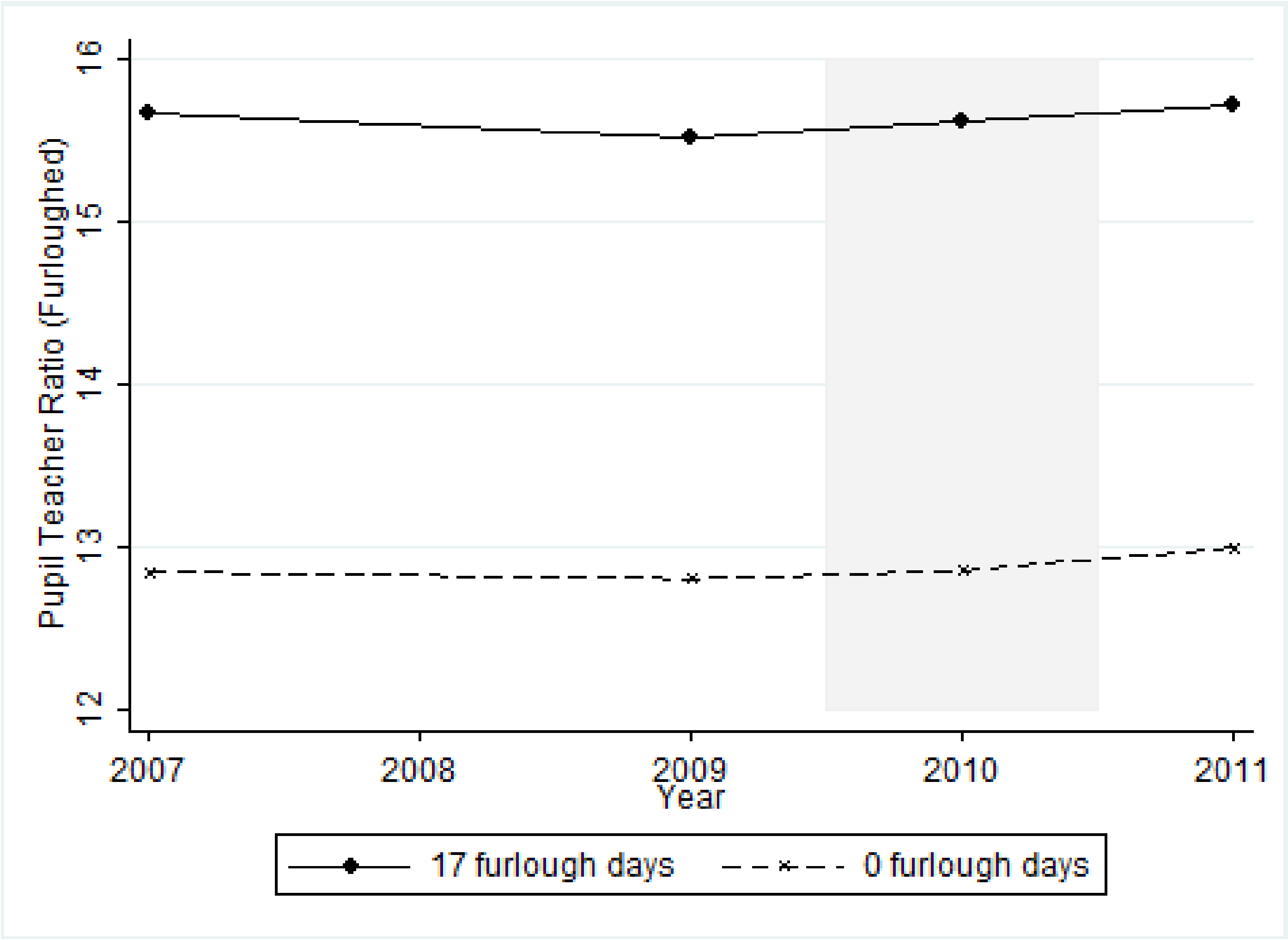
Fitted lines represent linear regressions of average achievement on year.

Figure E.5: Enrollment Trajectory by Furlough Status



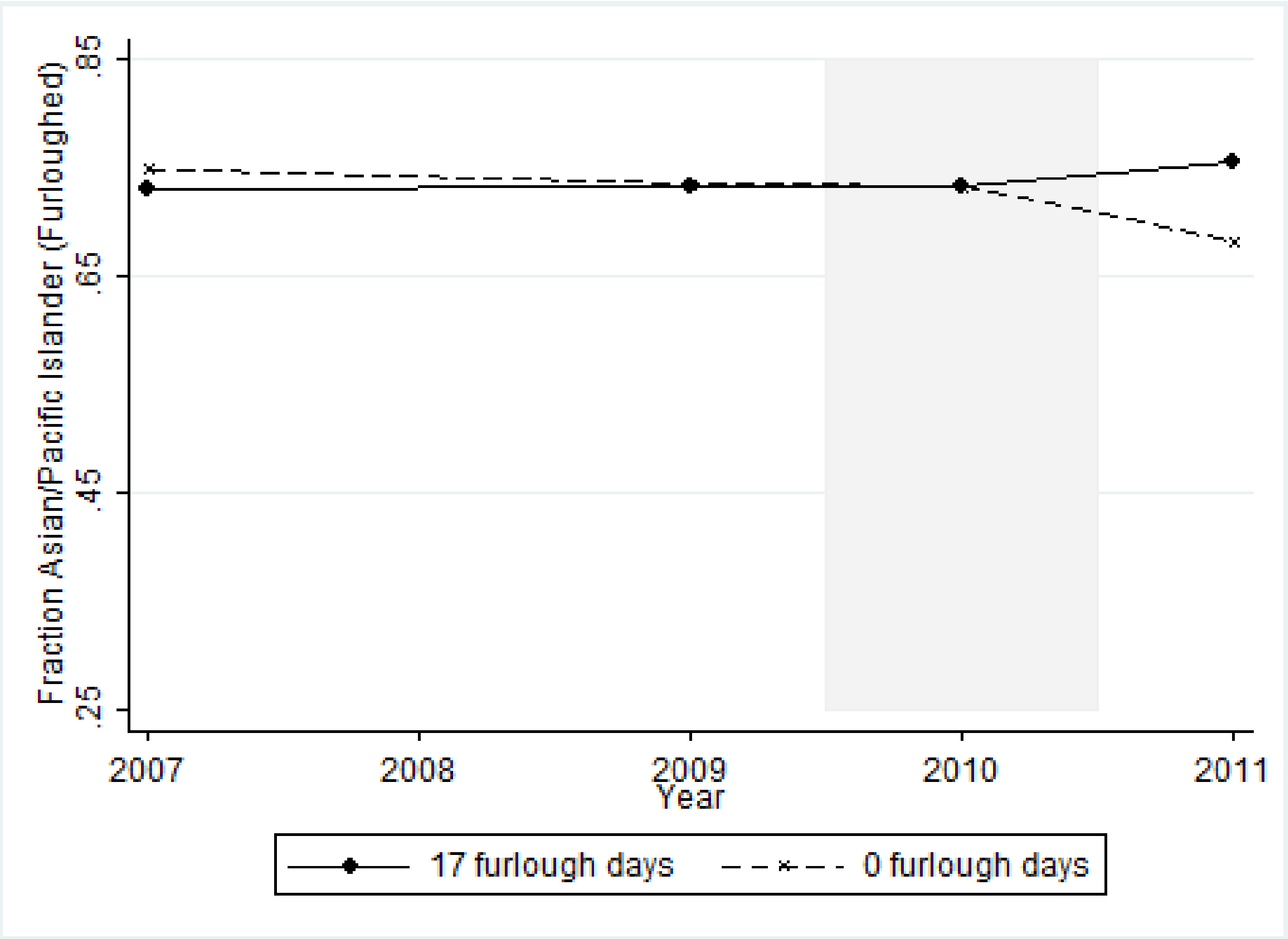
Year 2008 excluded. Racial composition categories in year 2011 are not comparable to previous years.

Figure E.6: Pupil-Teacher Ratio Trajectory by Furlough Status



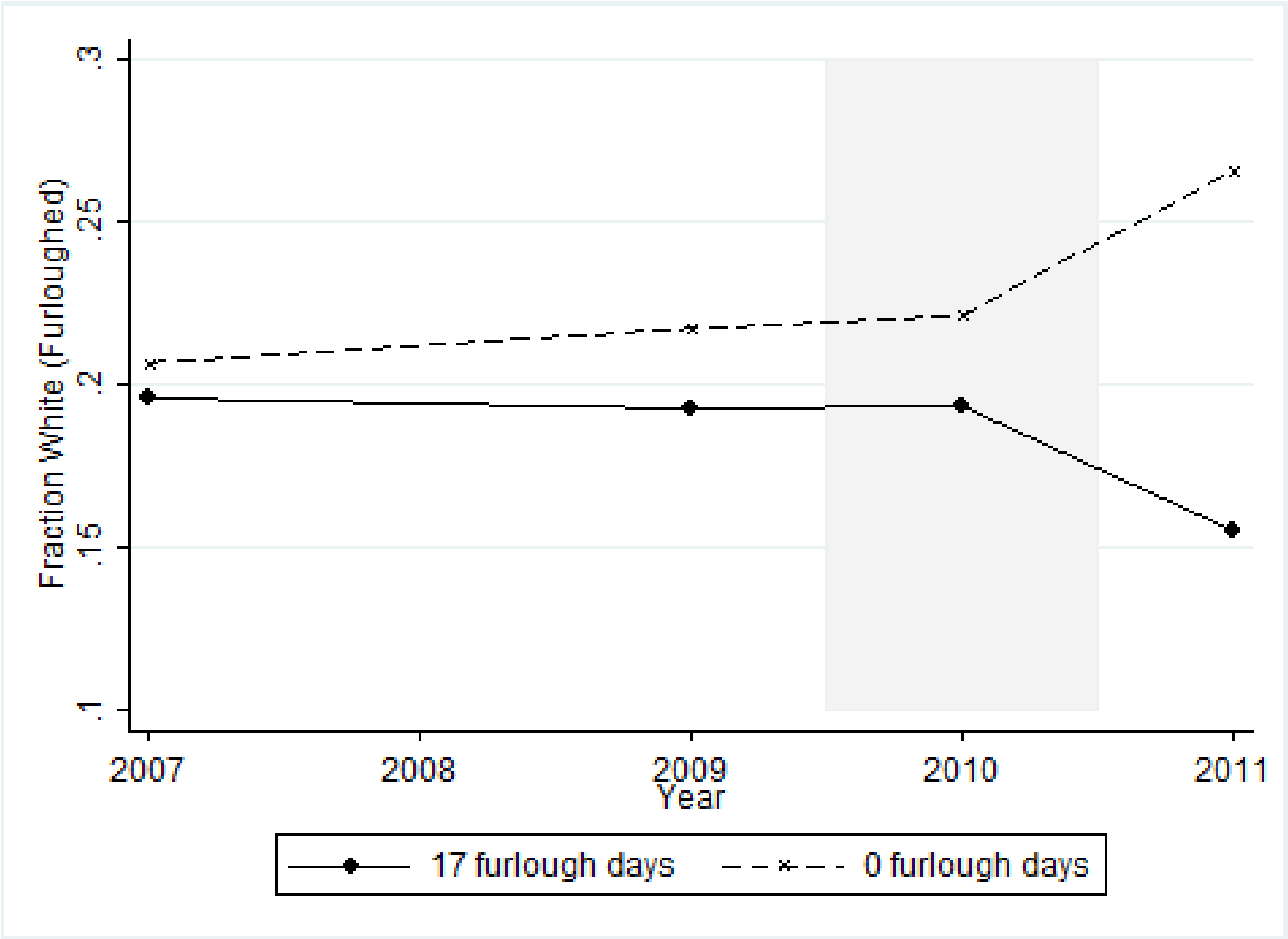
Year 2008 excluded. Racial composition categories in year 2011 are not comparable to previous years.

Figure E.7: Fraction Asian/Pacific Islander Trajectory by Furlough Status



Year 2008 excluded. Racial composition categories in year 2011 are not comparable to previous years.

Figure E.8: Fraction White Trajectory by Furlough Status



Year 2008 excluded. Racial composition categories in year 2011 are not comparable to previous years.

Figure E.9: Per-Pupil Charter School Funding in Hawaii

Data taken from Charter School Administrative Office (CSAO) Testimony to Senate Committee on Ways and Means at http://www.capitol.hawaii.gov/session2012/testimony/Info_Testimony_WAM_01-17-12_CHARTER.pdf.

APPENDIX F

APPENDICES FOR "THE EFFECT OF SCHOOL CLOSINGS ON STUDENT ACHIEVEMENT"

F.1 Baseline Analysis Separate by Closing Cohort

Table F.1 displays the results of specifications like those shown in Table A.4, but where treatment is defined separately for each cohort of displaced students. All coefficients shown are relative to student achievement in 2006.

Table F.1: Effects of School Closings by Closing Cohort

	Year of Displacement			
	2006	2007	2008	2009
Panel A: Mathematics Achievement				
2 Years Prior to Closure				-0.008
				[0.029]
1 Year Prior to Closure			0.012	-0.046***
			[0.037]	[0.018]
Year of Closure		-0.033***	0.004	-0.084***
		[0.011]	[0.021]	[0.032]
1 Year after Closure	-0.035	-0.021	-0.013	-0.095**
	[0.029]	[0.015]	[0.038]	[0.043]
2 Years after Closure	-0.017	0.038*	0.037	
	[0.029]	[0.020]	[0.038]	
3 Years after Closure	-0.023	0.045**		
	[0.026]	[0.018]		
Panel B: Reading Achievement				
2 Years Prior to Closure				-0.012
				[0.008]
1 Year Prior to Closure			-0.010	-0.013
			[0.027]	[0.020]
Year of Closure		-0.027	-0.029	-0.031
		[0.019]	[0.023]	[0.020]
1 Year after Closure	-0.036	-0.023	-0.007	-0.073
	[0.028]	[0.015]	[0.021]	[0.047]
2 Years after Closure	-0.062*	-0.008	0.003	
	[0.033]	[0.024]	[0.019]	
3 Years after Closure	-0.082**	0.018		
	[0.038]	[0.016]		
Number of Displaced Students	9,942	11,063	5,613	13,523
N	3,416,174	3,416,174	3,416,174	3,416,174

The treatment specification is set to 0 for all displaced students except for students displaced following the given academic year. Standard errors are adjusted for clustering at the district level. *** indicates significance at the 0.01 level, ** at the 0.05 level, and * at the 0.1 level.

F.2 Results with Interactions for School Performance Differential

The following table displays estimates of the specification shown in Equation (1.6).

Table F.2: School Closings and School Performance

	Math		Reading	
	Main Effect	Interaction	Main Effect	Interaction
Displaced Students:				
2 Years Prior to Closure	0.004	0.044*	-0.009	0.025
	[0.028]	[0.023]	[0.011]	[0.048]
1 Year Prior to Closure	-0.035***	0.030**	-0.009	0.055***
	[0.013]	[0.014]	[0.015]	[0.021]
Year of Closure	-0.049	0.050***	-0.019*	0.069***
	[0.034]	[0.009]	[0.011]	[0.024]
1 Year After Closure	-0.077**	0.007	-0.057*	0.020
	[0.035]	[0.016]	[0.030]	[0.023]
2 Years After Closure	-0.033	-0.083	-0.054**	-0.179
	[0.022]	[0.094]	[0.027]	[0.197]
3 Years After Closure	-0.020	-0.123***	-0.047**	-0.003
	[0.017]	[0.018]	[0.021]	[0.059]
Students At Receiving Schools:				
Frac. of Disp. Students	-0.145	0.201***	-0.232*	0.090***
	[0.131]	[0.067]	[0.140]	[0.034]
Frac. of Disp. Students (lag)	-0.073	0.230	-0.228	0.006
	[0.063]	[0.158]	[0.148]	[0.094]
Frac. of Disp. Students (2 lags)	-0.171*	0.207	-0.250	0.199
	[0.092]	[0.309]	[0.175]	[0.244]
Frac. of Disp. Teachers	-0.069***	0.051*	-0.006	0.047**
	[0.020]	[0.030]	[0.022]	[0.023]
Frac. of Disp. Teachers (lag)	-0.135***	0.050	-0.120***	-0.004
	[0.063]	[0.061]	[0.045]	[0.022]
Frac. of Disp. Teachers (2 lags)	-0.124***	0.007	-0.108**	0.024
	[0.028]	[0.052]	[0.048]	[0.023]
N	3,257,564		3,292,314	

All coefficients for a given dependent variable are estimated in the same regression. Standard errors are adjusted for clustering at the district level. *** indicates significance at the 0.01 level, ** at the 0.05 level, and * at the 0.1 level.

The following table displays estimates of the specification shown in Equation (1.6), with value-added measures used in place of average school proficiency rates. Value-added measures are constructed by taking the Ordinary Least Squares estimate of ψ from the regression equation $Y_{ist} = X_{ist}\beta + \psi_s + u_{ist}$ using data from years 2001-2005, where X_{ist} includes controls for race, gender, Limited English Proficiency, migrant student status, free or reduced-price lunch status, and lags of both math and reading test scores. These measures are then standardized to have mean zero, standard deviation one.

Table F.3: School Closings and School Performance using Value-Added Measures

	Math		Reading	
	Main Effect	Interaction	Main Effect	Interaction
Displaced Students:				
2 Years Prior to Closure	0.003	0.049***	-0.007	0.021
	[0.027]	[0.016]	[0.011]	[0.014]
1 Year Prior to Closure	-0.035***	0.030**	-0.005	0.034**
	[0.012]	[0.012]	[0.018]	[0.016]
Year of Closure	-0.049*	0.052***	-0.010	0.044*
	[0.028]	[0.009]	[0.018]	[0.025]
1 Year After Closure	-0.077**	0.009	-0.051*	0.022
	[0.036]	[0.015]	[0.031]	[0.021]
2 Years After Closure	-0.031	0.054	-0.052*	-0.032
	[0.022]	[0.147]	[0.029]	[0.150]
3 Years After Closure	-0.014	-0.091***	-0.049**	-0.034
	[0.018]	[0.020]	[0.024]	[0.042]
Students At Receiving Schools:				
Frac. of Disp. Students	-0.153	0.198***	-0.187*	0.112***
	[0.103]	[0.062]	[0.110]	[0.038]
Frac. of Disp. Students (lag)	-0.121**	0.208	-0.215*	0.106*
	[0.056]	[0.135]	[0.123]	[0.064]
Frac. of Disp. Students (2 lags)	-0.216***	0.372**	-0.243**	0.309*
	[0.081]	[0.160]	[0.123]	[0.174]
Frac. of Disp. Teachers	-0.094***	-0.002	-0.019	0.012
	[0.023]	[0.039]	[0.023]	[0.023]
Frac. of Disp. Teachers (lag)	-0.130*	0.044	-0.113**	-0.031
	[0.067]	[0.034]	[0.044]	[0.026]
Frac. of Disp. Teachers (2 lags)	-0.109***	-0.006	-0.101**	-0.005
	[0.034]	[0.043]	[0.044]	[0.025]
N	3,257,564		3,292,314	

All coefficients for a given dependent variable are estimated in the same regression. Standard errors are adjusted for clustering at the district level. *** indicates significance at the 0.01 level, ** at the 0.05 level, and * at the 0.1 level.

APPENDIX G

APPENDICES FOR "THE EFFECT OF FOUR-DAY SCHOOL WEEKS ON STUDENT ACHIVEMENT: EVIDENCE FROM HAWAIIAN SCHOOL FURLOUGH DAYS"

G.1 Descriptive Statisitcs

Table G.1: Descriptive Statistics

Variable	N	Mean	S.D.	Min	Max
Grade	4,173	5.803	1.739	4.000	10.000
Title I School	3,427	0.614	0.487	0.000	1.000
Charter School	4,173	0.137	0.344	0.000	1.000
Enrollment	3,362	595.1	377.3	101	2,639
Pupil-Teacher Ratio	3,393	15.32	2.37	4.33	20.93
Fraction Free/Reduced-Price Lunch Eligible	3,400	0.458	0.209	0.014	0.961
Fraction American Indian	3,412	0.008	0.010	0.000	0.125
Fraction Asian/Pacific Islander	3,402	0.723	0.193	0.062	1.000
Fraction Hispanic	3,402	0.045	0.035	0.000	0.288
Fraction African-American	3,402	0.022	0.036	0.000	0.258
Math Proficiency	4,173	0.474	0.189	0.000	1.000
Reading Proficiency	4,171	0.646	0.158	0.000	1.000
Urban	4,173	0.204	0.403	0.000	1.000
Suburban	4,173	0.311	0.463	0.000	1.000

The unit of observation is the school*grade*year. CCD data for year 2012 are not available yet.

G.2 Grade Level Heterogeneity

This appendix presents results fully broken apart by grade level.

Table G.2: Heterogeneity in Effects by Grade Level – ITS Estimates

	Grade						
	3	4	5	6	7	8	10
Mathematics	0.0127*	-0.0446***	-0.0433***	0.0098	0.0261**	0.0062	0.0088
	(0.0075)	(0.0071)	(0.0077)	(0.0089)	(0.0109)	(0.0114)	(0.0087)
Reading	0.0425***	-0.0076	-0.0117*	-0.0485***	0.0469***	0.0451***	0.0192*
	(0.0067)	(0.0059)	(0.0065)	(0.0077)	(0.0120)	(0.0100)	(0.0108)

Estimates are based on ITS specifications including a quadratic time trend, directly comparable to those presented in column (4) of Table B.3. The unit of observation is the school*grade*year. Standard errors in parentheses adjusted for clustering at the school level. * denotes significance at the 0.10 level, ** at the 0.05 level, ** at the 0.01 level.

Table G.3: Heterogeneity in Effects by Grade Level – DD Estimates

	Grade						
	3	4	5	6	7	8	10
Mathematics	-0.0749	0.0275	0.0280	0.0233	0.0409**	0.0293	0.0326*
	(0.0485)	(0.0350)	(0.0353)	(0.0182)	(0.0200)	(0.0198)	(0.0177)
Reading	-0.0385	0.0347	0.0307	-0.0711***	0.0367	0.0381**	0.0034
	(0.0392)	(0.0266)	(0.0266)	(0.0194)	(0.0223)	(0.0187)	(0.0201)

Estimates are based on DD specifications including school-specific linear trends, directly comparable to those presented in column (7) of Table B.4. The unit of observation is the school*grade*year. Standard errors in parentheses adjusted for clustering at the school level. * denotes significance at the 0.10 level, ** at the 0.05 level, ** at the 0.01 level.

APPENDIX H

APPENDIX FOR "THE FREQUENCY AND CORRELATES OF TEACHERS GRADE-LEVEL REASSIGNMENTS: EVIDENCE FROM MICHIGAN"

H.1 Within-school Grade-level Reassignment Origins and Destinations

Table H.1: Within-school Grade-level Reassignment Origins and Destinations

In Michigan (REP Administrative Data Universe)						
Grade in year t			Grade in year $t+1$			
	K 1	2	3	4	5	
K	75.20%	3.10%	1.20%	0.80%	0.40%	0.30%
1	1.60%	77.10%	3.60%	1.10%	0.50%	0.30%
2	0.80%	2.90%	76.00%	2.70%	0.90%	0.40%
3	0.40%	0.90%	2.10%	75.50%	2.60%	1.00%
4	0.30%	0.50%	0.80%	2.10%	76.00%	3.00%
5	0.20%	0.40%	0.50%	1.30%	2.70%	75.90%
In U.S. (SASS Nationally Representative Sample)						
Grade in year t			Grade in year $t+1$			
	K 1	2	3	4	5	
K	83.50%	1.60%	0.60%	0.00%	0.00%	0.50%
1	1.70%	67.60%	6.30%	3.80%	0.00%	0.50%
2	0.50%	5.90%	68.70%	1.40%	2.20%	0.00%
3	0.60%	0.00%	5.10%	74.10%	3.00%	2.60%
4	0.00%	0.70%	0.30%	0.70%	73.10%	2.00%
5	0.00%	1.00%	1.40%	0.10%	5.20%	71.30%

Rows do not sum to 100% because the table is restricted to self-contained teachers who remained in a self-contained classroom in the same school in subsequent years.

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