Policy Issues in the Economics of Education: Lessons from Michigan

by

Daniel D. Hubbard

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Economics)
in the University of Michigan
2018

Doctoral Committee:

Professor Brian Jacob, Chair
Professor Charles Brown
Professor Susan Dynarski
Associate Professor Kevin Stange

Daniel D. Hubbard

danieldh@umich.edu

ORCID iD 0000-0001-6831-7096

Dedication

Spending 22 years in school is an uncommon thing. Taking all of that schooling and applying it toward the study of education policy, to pass along the lessons learned along the way and work toward a brighter future for generations of students and teachers to come, is more uncommon yet. I would not have gone down this long and winding road leading to a Ph.D. and beyond without the inspiration of so many teachers, from elementary school through graduate school, who helped to push me to get the most out of myself and showed me how many things education can make possible. This is for them.

Even as I bounced around it looking for the right fit, a lot of people made the Berkley, Michigan school system feel like home. Judy Bauer got me to exercise my creativity when I couldn't sit still. Michelle Karas encouraged me to bring my outside interests into the classroom and introduced me to new ones. Robin Ziebert and Beth Hanna made every day an adventure. Laura Scribner and Rob Snyder-Pitts took fifty rowdy middle-school kids into the woods for a week every year and brought back a community.

The International Academy has been the single greatest blessing in all my years of schooling. Bert Okma's vision for a small school where curious and ambitious students could motivate each other to become global citizens, asking deep questions and bringing out the best in each other, has been a smashing success, and so many people there helped me become the person who I am today. Steve Eschrich introduced me to economics and showed me how it's in everything around us. Scott Wolf made an unspectacular violinist strive to feel the performer's rush, and then taught me to look deeply at the ways that knowledge and information work. Anna Fleury pushed me to pay attention to details in my writing and not to settle for good enough. Rebecca Riggs harnessed my language-learning skills and motivated me to major in Spanish when so many future economists choose math. Robert Uhelski brought history to life and showed how to look for the connections between things that may first seem unrelated.

When I first got to the University of Michigan as an undergraduate, I didn't know what a Ph.D. was, and I thought that all the questions in economics had been answered and the answers were written in the textbooks. Rebecca Thornton introduced me to contemporary research in economics and proved that it is a living social science that can be applied to

make a real difference in people's lives. Jeff Smith helped me start doing a bit of research of my own, and was the first person to tell me to consider a Ph.D. in economics. Frank Stafford saw what I had learned and gave me the opportunity to reinforce it by helping others to get their start in research. Chris Mayer, Catherine Thomas, and Tomasz Piskorski at Columbia Business School gave me an inside look at what it's like to do economics research every day and just how much time and work go into a high-quality academic paper.

The best choice I made in graduate school was to email a professor I'd never met named Sue Dynarski and ask for a meeting. I started working with Sue as a research assistant the summer after my first year and never really left, and it has been a joy to watch the world learn what I already knew: she is a world-class researcher with a passion for public engagement and a fierce advocate for young and prospective scholars. Brian Jacob's door has always been open to have a helpful and friendly conversation about my work and his, cutting against the stereotypes of academics and economists in all the best ways. Charlie Brown has kept me tied to the economics field when I've been drifting off toward the policy side. Kevin Stange has shared the latest methodological rigor and given valuable advice as someone who's been in my shoes not too long ago. It has been a blessing to have this team behind me, and I look forward to keeping in touch with them as colleagues in the field.

The Education Policy Initiative has been a perfect home for me as an applied interdisciplinary scholar passionate about education, and so many people behind the scenes have made it all possible. Mahima Mahadevan was an endless source of knowledge and good cheer, even in the most dire circumstances. Tedi Engler and Jasmina Camo-Biogradlija made sure the research people in Ann Arbor and the data people in Lansing stayed on the same page. Julie Monteiro de Castro has been a steadying constant in the midst of frequent staff turnover. And of course, the benefit of affiliating with a research institute like EPI includes a great network of students and postdocs working on similar topics, who have been valuable mentors, patient sounding boards for new ideas, and great friends. I cannot possibly thank all of them, but I will start with Meredith Billings, Sarah Cannon, Fernando Furquim, Kolby Gadd, Max Gross, Mónica Hernández, Jonathan Hershaff, Julian Hsu, Josh Hyman, Dan Kreisman, Andrew Litten, Elizabeth Mann, Kathy Michelmore, Stephanie Owen, Silvia Robles, Rachel Rosen, Brittany Vasquez, Mark Wiederspan, Carrie Xu, and Xiaoyang Ye.

A support network of people outside of academia is an extremely important part of a successful dissertation, because sometimes you need to talk to someone who has no idea how to spell "heteroskedasticity", much less what it is and how to correct for it. Umair Ali Daimee, Becca Denison, Ajin John, Michael Novinson, and Kenny Tang have been there for me for over half of my life now, and they've helped to keep me grounded and take my mind off of my work when I need to. I am lucky to have them in my life.

Mimi Takami and Megan Riehl at the University of Michigan Health System helped me to solve a problem that I never expected to face in graduate school. I am incredibly grateful that I got to study at a university that has a world-class hospital right on campus. They made sure that I saw the light at the end of the tunnel, both for my health and for my studies.

Jessica Kahan came into my life at a time when I was just starting to gain traction on my research, far enough along to have developed a routine but not far enough along to see the next steps on the path. She pulled me out of that rut and brought joy, excitement, adventure, and the world's friendliest cat. The memories I've shared with her are many of the happiest of my life. Her areas of expertise couldn't be further from my own, but I couldn't imagine having gone through this without her. I can't wait for all the adventures to come.

My mother, Linda Hubbard, has been at my side through every step of my long academic journey. She asks insightful questions and keeps me motivated when I'm struggling; her calm, even-keeled demeanor has been a necessary antidote to some of graduate school's frustrations. Returning to Michigan for graduate school has allowed me to share a home-cooked meal or a walk in the woods with her, and that has recharged me when my energy has been low. I could fill a whole dissertation with thanks and praise for her, but I'll leave with this: retiring to Ann Arbor was a smart move and a tremendous blessing. I'll be back.

Finally, my father, Roger Hubbard, was a fiercely devoted advocate for my education. From the earliest ages when he showed me that the most exciting place on Earth was the local library, to my early adulthood as he beamed while walking across Columbia's campus, relishing the thought of me getting to explore his beloved New York City while working at a world-class university, and all of the long nights in between when we pondered the next step in my educational process, he took no greater joy than in my growth and exploration as a scholar. I wish he could join me at the culmination of our great academic adventure, but I know that he, more than anyone, helped to light the way. We made it, Dad. Thank you for everything.

Acknowledgements

Thanks to the Institute of Education Sciences, U.S. Department of Education for providing support through Grant R305E100008 to the University of Michigan. Thanks to our partners at the Michigan Department of Education (MDE) and Michigan's Center for Educational Performance and Information (CEPI). This research result used data structured and maintained by the Michigan Consortium for Education Research (MCER). MCER data are modified for analysis purposes using rules governed by MCER and are not identical to those data collected and maintained by MDE and CEPI. Results, information and opinions are the author's and are not endorsed by or reflect the views or positions of MDE or CEPI.

Chapter I is inspired by work in progress by Stacey Brockman. Kolby Gadd is a full co-author on Chapter III. Brian Jacob began the project in Chapter III as a co-author and gave valuable guidance to the work, although Kolby and I later continued the project on our own and all writing is ours.

This dissertation has benefitted from comments from seminar participants at the University of Michigan, American Institutes for Research, the Midwest Economic Association, APPAM, and AEFP, and from conversations with a tremendous number of scholars, including John Bound, Stacey Brockman, Charlie Brown, Sarah Cannon, Celeste Carruthers, Anne Clark, Sue Dynarski, Guanlin Gao, Mónica Hernández, Julian Hsu, Brian Jacob, Lance Lochner, Elizabeth Mann, Isaac McFarlin, Katherine Michelmore, Nicolás Morales, Philip Oreopoulos, Alexander Persaud, Silvia Robles, Jeff Smith, Shannon Stackhouse, Kevin Stange, Lesley Turner, Gonzalo Vázquez, and Pieter de Vlieger.

Contents

List of Tables

List of Figures

Abstract

In this dissertation, I apply administrative data from Michigan's public schools to address crucial policy questions in the economics of education. In Chapter I, I shed light on the persistent effects of attending high-quality high schools by creating a value-added model that isolates each high school's effect on students' test scores, then matching the results to students' college transcripts to determine the relationship between high-school value added and first-year college grades. I find that students who attend high schools with one standard deviation higher value added receive first-year grades about 0.09 grade points higher than their otherwise-identical counterparts. These gains in college are not driven solely by math and English, but are evenly distributed across subjects. This result is robust to adjustments for a number of potential biases that arise throughout the process, including selection into high schools and selection into college attendance. Overall, I find evidence against some of the more skeptical interpretations of test-score improvement, such as the claim that schools "teach to the test" or the concern that the content tested on standardized exams is not relevant to future learning.

Human capital theory suggests that when students would graduate into a weak labor market, the opportunity cost of schooling declines, and they should instead invest in themselves and get more education. However, this assumes that they have no borrowing constraints; if students are credit-constrained and their families are hurt by the struggling labor market, then their educational options may actually diminish as they are less able to pay for college. In Chapter II, I determine which of these effects predominates empirically using data on plant closings and mass layoffs from the Worker Adjustment and Retraining Notification ("WARN") Act, examining the impact of exposure to job losses during the senior year of high school on whether and where students attend college. A 1-standard deviation increase in per-capita job losses is associated with a small but statistically-significant 0.2-percentage point increase in the probability of attending college, driven entirely by attendance at community colleges. This result supports the argument that the opportunity cost effect dominates, as any movement out of college as a result of credit constraints and firsthand exposure to job losses is comparatively small.

Having access to an effective and experienced teacher can make a crucial difference in a student's academic achievement. In Chapter III, written with Kolby Gadd, I examine the factors that predict whether teachers will stay in their first jobs or leave for opportunities elsewhere, and then study how students perform after teachers leave, looking both at teacher turnover in general and at teacher departures for particular destinations such as new districts or the private sector. In a multinomial logit framework in which we examine each teacher's employment status in his or her fifth year, we find that the characteristics that predict departures most consistently are the fraction of Black students in the teacher's first school, the fraction of economically-disadvantaged students in the teacher's first school, the teacher's first job being in special education, and the teacher's first school being a charter. Turning to how student achievement changes in the wake of teacher turnover, we find a modest decline in test scores after teacher exits, driven by students in schools that lost teachers to other full-time teaching positions, both within and across districts.

I More Gains Than Score Gains? High School Quality and College Success

I.1 Introduction

Most measures of school quality focus only on how a school's students perform while they attend that school. Accountability measures such as average test scores, school value-added measures, graduation rates, and many other metrics focus on outcomes that occur before (or as) a school's students graduate. While this allows the metrics to focus on the things that the schools being evaluated influence most directly, it also treats success at the given level of education as an end goal rather than a stepping stone. In reality, earning a high score on a standardized test does not, or should not, mean anything on its own. Test scores and other such metrics are valuable as signals of what students have learned and, perhaps more importantly, how much knowledge they have accumulated to support them in further education and in their careers.

Test-score value-added models are the current methodological gold standard, but even these models generally stop at or before graduation. The skills used to perform well on a test may not transfer well into other contexts, and the knowledge accumulated may fade out before students can continue to apply it. Even well-designed exams can have unique types of questions that test students' exam-preparation skills more than their content knowledge. When faced with pressure to have high test scores, either in average scores or in value added, teachers can "teach to the test", drilling students on specific aspects of the exams in lieu of maximizing their content knowledge or providing transferrable skills. Students who are "taught to the test" will score highly on that particular exam, but will not have knowledge of the subject matter that will persist into other contexts.

I measure the persistent value of going to a "good high school", defined here as a high school that raises students' test scores. Particularly, I evaluate the relationship between high schools' contributions to test-score gains and their alumni's achievement in college. In this study, I develop a theoretical model in which schools allocate their resources between test-specific preparation and teaching of content, subject to an endowment budget constraint and accountability measures of varying strictness. Schools with high endowments (generous funding, for instance, or talented students) are unconstrained by the test score requirement and can allocate their resources as they please, while schools with lower endowments may need to allocate resources away from teaching and toward test preparation in a way that they would not if there was no accountability. Some schools may not be able to satisfy high-stakes accountability constraints regardless of their resource allocation, and they go out of business.

I then test the findings using administrative data from public high schools and public colleges in Michigan. I develop value-added scores for each high school in Michigan using test scores in math and reading (the two most highly-emphasized subjects in school accountability), then match high school students to their college transcripts and examine the effects of test-score value added on their grades in their first-year college courses in tested subjects and other subjects. I show that attending a school with high test-score value added predicts higher first-year grades across the board, in both tested and untested subjects, controlling for students' middle-school test scores and an extensive set of covariates. I include a number of adjustments for selection into college and into high school, and the result is robust to all of them. Effects are similar across racial groups, socioeconomic groups, and college settings; the benefits are not limited to more-privileged students or students in four-year colleges.

I.2 Literature Review

This study fits into two principal strands of literature. The first strand deals with the long-term effects of attending a "good" school on outcomes such as college graduation, earnings, or disciplinary incidents. The second focuses on how schools respond to high-stakes testing and other forms of accountability.

The first strand largely takes advantage of excellent integrated state data systems in states such as Texas and North Carolina. Deming et al. (2014) take advantage of a school choice lottery in Charlotte, finding that students who attend their first-choice (and presumably higher-quality) school are more likely to complete college, with effects concentrated among female students. Deming et al. (2016) find that Texas students who attend schools that raise high-stakes test scores in response to school accountability are more likely to attend and graduate from a four-year college, and their earnings at age 25 are higher. Jennings et al. (2015) find that high school quality explains more of the difference in college attainment than in test scores, and that high school quality can reduce racial gaps in student outcomes but exacerbates income gaps. Jackson (working) uses data from North Carolina to show that teachers can have larger and more-persistent effects on behavior, grades, and on-time completion that surpass their impacts on test scores, particularly for English teachers.

Other analyses in this strand make use of charter-school lotteries and other randomized experiments. Dynarski, Hyman, and Schanzenbach (2013) revisit the Tennessee STAR experiment and find that assignment to a small class increased students' probability of attending and completing college and their probability of studying a high-earning field such as science, engineering, or business; the effects were particularly large among Black students, and they were well-predicted by the shorter-term effects on standardized test scores. Angrist et al. (2016) find significant effects of charter attendance on exit exam scores, SAT scores, and AP scores; the effects on college attendance are more modest and mostly involve movement away from community colleges and toward four-year colleges. Dobbie and Fryer (2015) find wide-ranging effects of assignment to the Harlem Children's Zone, ranging from increased test scores to reductions in the probability of teen pregnancy and incarceration. Allensworth et al. (2017) find improvements on academic outcomes from attending higher-performing non-selective schools in Chicago, though the effects do not extend to selective schools.

A number of studies have weighed in on the effects of accountability on teaching practices and student learning. Cohodes (2016) finds reason for optimism in Boston's charter schools, as these schools manage to raise students' test scores without placing disproportionate weight on higher-stakes subjects or common question types. Merseth (2010) views the high-performing Boston charters with more skepticism, noting their students' more modest gains on college entrance exams.

Other schools' test score gains may owe more to behaviors that have less to do with sustainable learning. Jennings and Bearak (2014) find that most of the score gains in several large states come from the most common question types, implying that such questions are particularly emphasized in the test preparation as well as in the testing. Jacob (2005) examines a new accountability policy in the Chicago public school system and finds that schools that raise high-stakes exam scores often do not raise low-stakes exam scores, as the schools focus heavily on test preparation, retention of underperforming students, and careful selection of the set of students to be tested. McNeil and Valenzuela (2000) find an almost single-minded focus on test score gains in Texas schools, crowding out many other valuable school functions. Both Jacob (2005) and Neal and Schanzenbach (2010) note the reallocation of resources toward students who are on the margin between passing and failing high-stakes exams in the Chicago public schools. Ahn (2016) and Muralidharan and Sundararaman (2011) outline valuable theoretical models of behavior under accountability; Ahn (2016) focuses on school-level investments, proposing that schools invest in test preparation when they are in danger of sanction but this detracts from teaching, while Muralidharan and

Sundararaman (2011) focus on teacher merit pay and show that teachers may similarly be tempted to move away from curriculum teaching and toward test preparation when their bonuses depend on test scores.

I.3 Theoretical Framework

I.3.1. Setup

Schools exist for the purpose of helping students learn, and in the absence of any testing, schools would expend as much effort as they saw fit on student learning. However, once low-stakes testing is put into place, if test scores are less than perfectly correlated with student learning, schools may adjust their practices to maximize some function of test scores and student learning. This does not impose any constraints; it merely adds another variable to the schools' objective functions. In practical terms, even if there is no formal accountability system in place, families may still be hesitant to send children to a school with low standardized test scores, giving schools an incentive to consider them in their resource allocation decisions. In turn, this incentive may induce schools to increase their effort.

High-stakes accountability imposes formal penalties for low performance. A simple high-stakes accountability system states that a school's average test score must be above some threshold score θ, or else the school will be closed. High-stakes accountability may also induce a further increase in effort above the level under low-stakes accountability.

Let each school j consume two types of resources: short-term resources S, which only affect standardized test scores T, and long-term resources L, which simultaneously[1] affect both test scores and student learning G. Short-term resources are more effective in producing test scores than are long-term resources. In other words, $T_j = f(S_j, L_j); G_j = g(L_j); \frac{\partial T}{\partial S} > \frac{\partial T}{\partial L} > 0$.

Schools get some utility[2] U_j from student learning and, if there is a testing regime in place, test scores. However, they gain no utility if they are shut down because they do not meet the high-stakes accountability threshold. We can phrase this as $U_j = p(G_j)$ under no accountability, $U_j^* = p(T_j, G_j)$ under low-stakes testing, and $U_j^{**} = \mathbf{1}(T_j \geq \theta)p(T_j, G_j)$ under high-stakes testing. Under any accountability regime, $\frac{\partial U_j}{\partial T_j} > 0, \frac{\partial U_j}{\partial G_j} > 0$.

[1] This is extremely important. While most traditional production equations require resources to be allocated toward producing one good or another, if a school invests in long-term resources, those resources increase the output of student learning and test scores at the same time. This models the true usefulness of those resources more effectively, in addition to making the model much easier to solve.

[2] I am modeling schools as firms here, but I use "utility" in place of "profit" in order to prevent confusion related to for-profit and non-profit schools. The schools in this model do not have a financial motive.

The resources have costs c_S and c_L, respectively. Schools' effort E is an increasing function of the strictness of accountability A; for convenience, I normalize $E(0) = 1$, to phrase all effort levels as relative to the baseline of no accountability. Each school has an endowment Ω_j; schools face the effort-weighted budget constraint $\Omega_j E(A) = c_S S_j + c_L L_j$. To emphasize the shortcut nature of teaching to the test, let $c_S < c_L$. High-stakes accountability is more stringent than low-stakes accountability, which in turn is stricter than no accountability; $A^{**} > A^* > 0$.

I.3.2. An Example With Cobb-Douglas and Linear Functions

Let both the utility function and the production functions be Cobb-Douglas, and let the effort function be linear, as follows. All j subscripts are removed for ease of reading.

$$U = T^\alpha G^\beta \tag{I.1}$$

$$T = KS^\gamma L^\delta; \gamma > \delta \tag{I.2}$$

$$G = BL^\zeta \tag{I.3}$$

$$E = 1 + A \tag{I.4}$$

K, B, and all lower-case Greek letters are non-negative constants. Different schools may have different values of these parameters.

I begin with the case of no accountability, in which $A = 0$ and thus $E = 1$. The school spends its entire endowment on long-term resources:

$$L_0 = \frac{\Omega}{c_L} \tag{I.5}$$

Under low-stakes testing $A^* > 0$, the school's effort increases to $1 + A^*$. Substitute the production functions into the profit function in order to create a utility function over the consumption of the teaching resources. I remove j subscripts for readability.

$$U = (KS^{\gamma}L^{\delta})^{\alpha}(BL^{\zeta})^{\beta} \tag{I.6}$$

Rearrange and collect terms, and define a new constant $D \equiv K^{\alpha}B^{\beta}$.

$$U = DS^{\alpha\gamma}L^{\alpha\delta+\beta\zeta} \tag{I.7}$$

The marginal rate of substitution between S and L is:

$$MRS_{S,L} = \frac{\alpha\gamma L}{(\alpha\delta+\beta\zeta)S} \tag{I.8}$$

Set this equal to the price ratio $\frac{c_S}{c_L}$, substitute in from the budget equation, and get:

$$S^* = \frac{\Omega(1+A^*)}{c_S}\left(\frac{\alpha\gamma}{\alpha\gamma+\alpha\delta+\beta\zeta}\right) \tag{I.9}$$

$$L^* = \frac{\Omega(1+A^*)}{c_L}\left(\frac{\alpha\delta+\beta\zeta}{\alpha\gamma+\alpha\delta+\beta\zeta}\right) \tag{I.10}$$

$L^* > L_0$ if $1 + A^* > \frac{\alpha\gamma+\alpha\delta+\beta\zeta}{\alpha\delta+\beta\zeta}$; in other words, long-term resource consumption (and, by extension, learning) is higher under low-stakes testing than under no testing if the increase in effort is greater than the relative importance of long-term resources in the school's utility function.

Moving to a high-stakes testing regime $A^{**} > A^*$ is unequivocally beneficial in schools that meet the threshold score with their unconstrained optimal resource bundle; the math is the same except A^{**} replaces A^* in the respective formulas. Even if the school would not have met the threshold at its low-stakes optimum, if the additional effort induced by the high-stakes testing puts the school over the threshold, there will be a commensurate increase in long-term resource consumption and therefore student learning.

The problem is not analytically tractable under high-stakes testing if the unconstrained optimum bundle does not meet the constraint; the school will alter its its resource bundle so that the constraint is just satisfied and consume the solution to the following system:

$$KS^{\gamma}L^{\delta} = \theta \tag{I.11}$$

$$\Omega(1 + A^{**}) = c_S S + c_L L \tag{I.12}$$

In lieu of determining the exact levels of S and L that a school will consume under high-stakes accountability in order to meet the threshold, I show that schools will respond to falling short of the threshold score by moving toward short-term resources if $\frac{\alpha\delta+\beta\zeta}{c_L} > \frac{\alpha\delta}{c_S}$. If the costs of the short-term and long-term resources are equal, it is always more profitable to move toward short-term resource use from the unconstrained optimum. Details are in the Mathematical Appendix.

Some schools have such low endowments that they cannot meet the constraint; there does not exist a pair (S, L) in their budget set such that $KS^\gamma L^\delta \geq \theta$. In this case, the school shuts down.

I.3.3. Interpretation

The main implication of this model is that test score gains under high-stakes accountability are more likely to reflect increased content knowledge in wealthier schools or schools with higher-performing students, while schools without these luxuries may make more use of test preparation methods to improve scores. As such, these schools with greater endowments will have a stronger relationship between test score improvements and long-term learning than their poorer counterparts. Schools with high endowments are more likely to be able to choose their utility-maximizing levels of resources without being bound by the constraint. Because the constraint requires only a certain level of test scores, constrained schools will be forced to sacrifice student learning to meet the test score minimum, and will often do this through trading long-term resources for short-term ones. The endowment can be thought of as the school's financial budget, but this is not the only interpretation; a school could also have a high endowment because its teachers are effective or its students are talented. If its teachers and/or its students are especially skilled, a school does not need to devote many resources to test preparation and can focus as much as it wishes on student learning[3].

In the Cobb-Douglas example, schools will consume more short-term resources S if their endowment Ω increases, the cost of short-term resources c_S decreases, the productivity of short-term resources γ increases, or the importance of test scores in the utility function α increases. Schools will consume more long-term resources L if Ω increases, the cost of long-term resources c_L decreases, the productivity of long-term resources in producing test scores δ increases, the productivity of long-term resources in student learning ζ increases, or the importance of student learning in the utility function β increases.

[3]Alternatively, these qualities could be seen as lowering the costs of the resources; better teachers can provide content knowledge more easily, for instance.

I am intentionally agnostic about the proper values of α and β. Some schools may be philosophically opposed to testing and have an α of 0; that is, test scores have no role in their utility function other than through the constraint. Others may place heavy weight on test scores and have very high values of α; one could imagine a for-profit charter school in a poor and densely-populated area, for instance. There are many schools to choose from in the area, but they are generally seen as being low-quality. The charter must attract enough students to make a profit, and the easiest signifier of quality when the competing schools are low-performing is higher test scores.

Note that the threshold score θ does not enter into the expressions for the unconstrained maximizing resource consumption bundle. If the threshold is met by the actions that the school would take anyway, the numeric value of the threshold score does not matter. However, when the constraint binds, θ does enter into the expressions for the maximizing bundle. Schools must adjust their consumption and consume the right amounts of resources to just meet the threshold.

This model assumes a single representative student in a school; it does not account for how resources could be targeted within a school. In reality, of course, schools are composed of wide varieties of students with abilities all over the distribution. If schools have an idea of how their students would perform on a standardized test at the moment, they can allocate their resources among the students in a more targeted fashion. Under an accountability regime in which schools are rated based on the fraction of students exceeding a proficiency threshold, for instance, schools may apply short-term resources to students near the threshold to ensure that they pass the test, while students in the upper part of the distribution may receive long-term resources and students at the very bottom might receive nothing at all. A regime like the one outlined here, in which schools are rated based on average test scores, might leave more room for students in most of the distribution to receive short-term resources, although if one assumes the returns to short-term resources are low for students near the top of the distribution (both because they cannot score much higher and because the last few concepts are the most difficult), the highest-ranking students are still less likely than their lower-achieving peers to receive short-term resources.

The Cobb-Douglas functional form is a convenient illustration, but some of the most basic conclusions hold when the functional form assumption is relaxed. Specifically, as the cost of a resource decreases, schools will demand more of it; as endowments increase, schools will demand more of both resources. As schools place more emphasis on test scores (via increasing $\frac{\partial \Pi_j}{\partial T_j}$), their demand for short-term resources will go up. Effects of some other parameters are more ambiguous.

I.4 Data and Methodology

I.4.1. K-12 Data

This project draws from several different administrative data sources. I begin by using student-year level test scores and demographic data from public middle and high schools in Michigan to estimate school value added, then merge the K-12 data with several sources of college data to create the variables necessary to measure postsecondary outcomes.

My base sample is all students in Michigan public[4] schools, who first sit for the 8$^{\text{th}}$-grade math and reading Michigan Educational Assessment Program ("MEAP") test between the 2005-06 and 2007-08 school years. Students must take the 11$^{\text{th}}$-grade standardized exam (which includes the ACT) to be included in deriving the value-added model; other outcomes do not condition on having any data past eighth grade. Students who take the 8$^{\text{th}}$-grade MI-Access exam for special-education students are dropped from both samples.

I merge in some student characteristics (race, gender, age, limited English proficiency, economic disadvantage[5], special education status) measured each year, and the student's home district and the school that the student attends measured three times per year[6], as well as the student's ZIP code and Census block group. The latter two variables allow me to merge in neighborhood household income from the American Community Survey (along with a missing indicator if such data are unavailable for the given student).

Because students may change schools within an academic year, I briefly reshape the data to the student-collection period level, so that I can determine the fraction of periods between the middle-school exam and the high-school exam that a student attends each school. I use this fraction to assign each student to the school that the student attends for the most collection periods; students who do not attend any school for four collection periods or more are not used to derive the VAM.[7] I keep the values of economic disadvantage, limited English proficiency, and special education enrollment from the student's eighth-grade year as to avoid any manipulation by their high schools. I bring in a few school-level aggregate variables, keeping the most common values for indicator variables and the means for continuous variables. Finally, I reduce the sample to one observation per student, keeping the student's

[4]For the purposes of this paper, "public" schools include both traditional public schools run by local school districts and charter schools.

[5]During the sample period, economic disadvantage is measured by a student's eligibility for subsidized school lunch.

[6]Following the Center for Educational Performance and Information's terminology, I refer to the three measurement dates per year (once each in the fall, spring, and end of year) as "collection periods".

[7]Previous versions of this paper have weighted the contributions of each student+school combination according to the fraction of time spent in that school; the results do not change. The current specification is simpler to explain.

first home district and Census block group while attending their longest-tenured school.

I.4.2. Value-Added Estimation

To calculate schools' value added, I follow the procedure outlined in Chetty, Friedman, and Rockoff (2014; henceforth "CFR"). This process starts by regressing students' 11^{th}-grade test scores Y_{ijnt} (an average of math and reading) on their 8^{th}-grade scores $Y_{i,t-3}$ (math, reading, and the interaction of the two), a variety of student, school, and neighborhood demographics[8] (X_i, $\mathbf{\bar{X}_{j,t-1}}$, and $\mathbf{\bar{Z}_n}$, respectively), cohort dummies τ_t and a high school fixed effect σ_j.

$$Y_{ijnt} = \lambda + \psi_1 Y_{i,t-3} + \mathbf{\Psi_2 X_i} + \mathbf{\Psi_3 \bar{X}_{j,t-1}} + \mathbf{\Psi_4 \bar{Z}_n} + \tau_t + \sigma_j + \epsilon_{ijnt} \tag{I.13}$$

I take a "residualized score" for each student, consisting of the school fixed effect and the error term, and collapse the data to leave one observation per school-by-year combination, keeping an average residualized score ρ_{jt} for school j in year t.

$$\rho_{jt} = \frac{1}{N_{jt}} \sum_{i=1}^{N_{jt}} \sigma_j + \epsilon_{ijnt} \tag{I.14}$$

I then regress the average residualized score on the same school's average residualized scores from each of the preceding and following two years, plus the relevant missing indicators. The predicted value $\hat{\rho}_{jt}$ from this regression is the value added for the given school in the given year.

$$\rho_{jt} = \sum_{y=-2, y\neq 0}^{2} \kappa_y \rho_{j,t+y} + \xi_y Missing_{j,t+y} + \o_{jt} \tag{I.15}$$

The main advantage of the CFR model is that it is robust to noise, measurement error, and cohort-specific shocks, through its Bayesian shrinkage "leave-one-out" framework. Results do not change significantly if I use a Bayesian shrinkage estimate of the school value added without the leave-one-out specification (see Koedel, Mihaly, and Rockoff 2015; Herrmann, Walsh, and Isenberg 2016), a simpler one-step VAM, or a two-step model (as in Ehlert et al. 2014).

[8] See Appendix B for the full list.

Figure I.1 presents the distribution of value added across schools, weighted by the number of students. Most estimates are between -0.3 and 0.3 standard deviations; there are fewer positive estimates than negative estimates. Figure I.2 repeats the exercise but with one observation per school instead of per student; there is more dispersion in these estimates, but the outlier schools tend to be small. A school that is one standard deviation better than average improves students' test scores by 0.234 student-level standard deviations over their 8^{th}-grade baseline scores, corresponding to about 1.1 points out of 36 on the ACT composite.

Table I.1 provides detail about how students are distributed across levels of school value added. Even as the measure focuses on student improvement rather than raw scores, the students in higher-value added schools are more privileged and higher achieving than their counterparts in lower-value added schools. 58% of students in these schools are economically disadvantaged, and 35% are Black; these figures are 19% and 13%, respectively, in the highest-quartile schools. The average 8^{th}-grade exam score in the lowest-quartile schools is 0.341 standard deviations below the statewide mean, while the average in the highest-quartile schools is 0.384 standard deviations above the statewide mean. I also include several intermediate outcome measures separated by school VAM quartile; students in schools with higher value added perform better on their 11^{th}-grade exams and are more likely to graduate from high school and enroll in college.

I.4.3. College Data

Most of the outcome data used in this paper come from a data set called STARR. The STARR data consist of student-course level records for all public colleges in Michigan, starting with students who attended college in 2009. Each student would have a separate observation for each course that the student has taken at a Michigan public college (including both community colleges and four-year colleges), containing information about the student, the course, and the student's grade in the course. Unless stated otherwise, grades are expressed on a 4.0 scale (3.7 for an A-, 3.3 for a B+, etc.) in this study.

I keep only credit-bearing courses from a student's first year in a Michigan public college[9]. I drop courses titled "Departmental Credit" (which tend to be credits for Advanced Placement or International Baccalaureate scores rather than for college coursework), drop observations from students who take the subject in question at multiple institutions in the same year, and restrict math and English courses[10] to be the first course taken in the given subject; if a student takes multiple math courses or multiple English courses at once in the student's

[9]Because STARR only contains data from Michigan public colleges, students technically could have taken courses at other colleges first, but this is unlikely given later restrictions on time since graduation.

[10]I identify math and English courses using the course codes listed in Appendix B.

first semester taking a course in that subject, the course with the lower course number is kept (for instance, "MATH 215" over "MATH 217"). I keep all first-year courses in subjects other than math and English.

The final data preparation step is to merge the data sets together. I merge the value-added measures onto the 8^{th}-11^{th} grade student observations, and then merge the resulting file into the college data. What remains is one observation per student-course combination, containing the student's demographics, high-school value added, and course performance. In order to reduce the impact of events that happen between high school and college, college observations are dropped if a student does not start college "on time", meaning five years after taking the 8^{th}-grade exam.

In order to have at least some information about students who do not attend college at a Michigan public institution, I also merge in enrollment and graduation information from the National Student Clearinghouse. These data are available for colleges attended by over 90% of Michigan public-school students. The focus of the paper is on the course outcomes, but examining the effects of high school quality on other outcomes such as college attendance and completion informs the calculations made to ensure that the course grade results are robust.

Table I.2 outlines the changes in college attendance across the distribution of test-score value added. While every quartile of schools in the distribution sends around 60% or more of its students to college, students who attend schools with higher value added are more likely to attend college, a gain driven mostly by increased probability of attending a public four-year college in Michigan. The fraction of students attending a private college in Michigan and the fraction attending a community college is higher in the middle of the distribution than at the ends but stays in a fairly narrow band. Of particular concern for the identification of this paper, however, is the steady increase in the probability of being in the college grade sample as high school VAM increases. I explore this more in Section I.4.5.

I.4.4. Empirical Specification

To determine the effect of high-school value added on college performance, I build up to the following specification:

$$\text{(I.16)}\quad Grade_{cijknt} = \iota + \phi_1 Y_{i,t-4} + \phi_2 ValueAdded_{j,t-1} + \mathbf{\Phi_3 X_i} + \mathbf{\Phi_4 \bar{X}_{j,t-1}} + \mathbf{\Phi_5 \bar{Z}_n} + \chi_{ckt} + \nu_{ijkct}$$

The course grade earned by student i from neighborhood n, who attended high school j before enrolling at college k and taking course c in semester t, is a function of the student's middle-school test score $Y_{i,t-4i}$; student characteristics $\mathbf{X_i}$; school-level average characteristics $\bar{\mathbf{X}}_{\mathbf{j,t-1}}$; neighborhood characteristics $\bar{\mathbf{Z}}_{\mathbf{n}}$; a course fixed effect χ_{ckt}; and the high-school[11] value added.

Empirically, the course grade is measured on a 4.0 scale, and the student characteristics are the same ones used in the value-added model (except with the 8$^{\text{th}}$-grade score included as an average of math and reading, up to a fourth-order polynomial, as opposed to separating them and including an interaction). I scale the value added in terms of its school-level standard deviation; the coefficient ϕ_2 represents the effect of raising a school's test-score value added from the statewide average to one standard deviation above it. Standard errors are clustered by school; this accounts for serial correlation and is generally more conservative than clustering by school and year. To account for the generated regressor in the value-added term, I present bootstrapped standard errors in the full specifications, following Bastian (working), among others. The fixed effects χ_{ckt} are for each combination of college, year, semester, subject code, and course number (for instance, University of Michigan-Ann Arbor, fall 2011, ENGLISH 125). Only students who take a course in a Michigan public college within five years of the year in which they take their 8$^{\text{th}}$-grade test are included. Observations in the "all subjects" and "other subjects" specifications are weighted by their fraction of the student's relevant credits in the non-bootstrapped specifications.

The specifications for outcomes other than college course grades are similar, but with a few important modifications. There is one observation per student, rather than one observation per student-course combination; there is no course fixed effect, because there is no course being measured; and the sample no longer consists only of students who enroll in college, or even students who take the 11$^{\text{th}}$-grade exam. Instead, anyone who has an 8$^{\text{th}}$-grade exam score that is not from the MI-Access special education exam is included in the sample.

I.4.5. Threats to Identification

Potential biases lurk throughout the empirical analysis process. First, the 8$^{\text{th}}$-grade exam scores may be measured with error, stemming from anything from poorly-filled bubbles to a malfunctioning Scantron machine. If the measurement error is classical, this would introduce attenuation bias into the value-added estimates, and in turn the coefficients in the outcome regressions would be biased upward as they measure the effect of an attenuated regressor. The measurement error is not precisely classical, as scores are necessarily bounded between 0% and 100%, but as 0.02% of students receive the minimum or maximum score on the math

[11] I use this term for convenience to describe the school that a student attends most frequently between the exam taken in grade 8 and the exam taken in grade 11. It could be a junior high school, which has grades 7-9; it could be a school that contains grades K-12. It does not need to only contain grades 9-12.

exam and 0.01% receive the minimum or maximum on the reading exam, the bounds are so rarely reached that I treat the measurement error as classical.

The next threat comes from selection into high schools. One of the most important issues in the value-added literature is that attendance in "better" schools is not random. Even though they cannot observe value added explicitly at the time that students enroll, schools' reputation for quality is presumably at least somewhat positively correlated with value added. Students then sort across districts across two dimensions that reinforce each other. Families sort across home districts based on preferences for education and available resources, among other things; students can then, conditioning on where they live, take advantage of policies that allow them to attend schools in other districts or in specially-designed settings such as charter schools and magnet schools. In both cases, students best equipped to succeed in college will be sorting into higher-quality high schools, biasing my estimates upward.

I present a handful of falsification tests in Table I.4 to quantify the degree of the sorting. If there was no sorting, then high-school value added should not predict 7$^{\text{th}}$-grade test scores, 7$^{\text{th}}$-grade attendance, Census block group poverty rates, or Census block group education levels. However, all of those variables except for poverty rates are indeed predicted by high-school VAM. For instance, students who attend high schools with one standard deviation higher value added measures have 7$^{\text{th}}$-grade scores that are about 0.043 standard deviations higher, even after controlling for 8$^{\text{th}}$-grade scores and the other typical covariates. This is an economically modest but statistically significant bias.

Finally, students in higher-VAM schools are more likely to attend college, most notably at the in-state public institutions that collect the transcript data used in this study, as shown in Table I.3. Table I.3 contains probit marginal effects for various attainment outcomes: taking the 11$^{\text{th}}$-grade state exam, graduating high school, attending college, and being in the sample for the course grade specifications. All of these are predicted very well by the test-score value added of a student's high school. The college outcomes track closely with the results shown in Table I.2, showing that the unconditional results in Table I.2 are not driven solely by covariates that are controlled for in the probits. However, they raise concerns about selection into the college grades sample.

There are two different interpretations of the selection. On one hand, if students are more likely to get into college if they attend a good high school, then perhaps the students who still manage to get there despite attending a low-performing high school must be particularly resilient, which makes them likely to perform better in college. This would argue against finding a positive effect of high-school value added on college performance. However, an alternative interpretation is that students are sorted into high schools by some unobserved quality; this quality makes the students perform better in high school, raising their value added, and in college, raising their grades, but not due to anything that their high schools contributed. This would bias toward finding a positive effect.

I.5 Course Results

I.5.1. Main Specifications

Table I.5 presents results for the full sample, adding more covariates with each column. The specification in column 5 weights the contribution of each observation by its fraction of the student's total credits, but does not bootstrap the standard errors; column 6 includes bootstrapping but weights 1-credit classes equally to 4-credit classes. Regardless of specification, there is a positive and significant relationship between high-school value added and college course grades. The effect of attending a school with one standard deviation higher VAM is about 0.086 grade points, almost one third of the difference between a B and a B+. This result provides evidence against the hypotheses that schools are teaching to the test; there appear to be long-term benefits to attending a school that raises high-stakes test scores. The effect size does fall short of the 0.1-standard deviation threshold that denotes a large impact in the education literature, however; the standard deviation of course grades is about 1.34 grade points in this sample, meaning that the effect size is closer to 0.07 standard deviations.

Table I.6 presents results by subject, using the specification from column 6 of Table I.5; the effect is very similar. Controlling for a full set of demographics and bootstrapping standard errors, the effect of going to a school with one standard deviation higher VAM is about 0.131 grade points in math, positive and highly significant. For English, the effect sizes are slightly smaller at about 0.061 grade points, still highly significant when including full controls and bootstrapped standard errors. In subjects other than math and English, the impact of attending a one-standard deviation more-effective school is about 0.082 grade points. This is evidence against the hypothesis that schools are focusing only on subjects that have high-stakes tests, at the expense of overall skills and learning in fields not subject to test score-based accountability. The courses in this category include anything from psychology to business to welding; none of these subjects are included in accountability measures and many of them are not even taught in high schools.[12]

[12] One reason why there may not be reallocation toward high-stakes subjects in high school is the Michigan Merit Curriculum ("MMC"), which requires that students take biology, either chemistry or physics, three social studies courses, two years of a foreign language, gym, art, and an online course (in addition to its math and English requirements). The first cohort exposed to the MMC entered high school in the fall of 2007; some cohorts in this study preceded the MMC, while others were exposed to it. (Jacob et al. 2017)

I.5.2. Accounting For Biases

I begin by accounting for the attenuation bias in the value-added model. Although students' 7$^{\text{th}}$-grade test scores are likely measured with a similar type of error as their 8$^{\text{th}}$-grade scores, if these measurement errors are not correlated with each other, one can be used as an instrument for the other. In this case, I use 7$^{\text{th}}$-grade test scores to instrument for their 8$^{\text{th}}$-grade counterparts in the first step of the VAM. I then proceed with the model as normal, using the residualized scores from the instrumented first step to constructed the predictions used in the value added, and then using the resulting value-added measures as regressors in the outcome specifications.

To reduce the bias from selection across high schools, I construct a "home-district" sample. This sample consists only of students who attend a non-charter, non-magnet high school in their zoned school district. These students often, though not always if their district offers multiple high schools, attend the "default" high school associated with their home address. This reduces the upward bias from selection into higher-quality high schools, although it does not eliminate it, as families also sort residentially by income, education, and preferences for school quality. Appendix Table I.12 presents the results from Table I.4 for this sample; it remains selected on some observable characteristics, even if the home-district sample deals with selection on some unobservable characteristics such as motivation.

I conduct three different adjustments for selection into college. First, I present results estimated for a smaller sample of students who are very likely to go to college. These students have less margin to have their college-going decisions altered by the quality of their high school. To construct this subsample, I take advantage of ACT's cutoff score for college readiness: a student who meets all of ACT's benchmarks for college preparation would have a composite score of 21 (ACT, Inc.). 31% of students in the sample received an ACT composite of 21 or higher. Because ACT scores are, by construction, influenced by high school quality, I instead include the top 31% of scorers on the 8$^{\text{th}}$-grade standardized test. 86% of these students in the lowest-VAM schools attended college; 92.4% of these students in the highest-quartile schools attended college. Other than in the very lowest quartile, there does not seem to be a significant unconditional relationship between high-school value added and college attendance for these "college-ready" students. A relationship remains when I condition on the usual set of covariates, but it is notably smaller than the equivalent relationship in the full sample, and there is no significant relationship between high-school value added and presence in the grade sample for the college-ready students, as shown in Appendix Table I.11

Second, Oster (2016) derives a method of accounting for selection on unobservables that incorporates the relative changes in treatment effects and R^2 values as covariates are added to the model, building on work by Altonji, Elder, and Taber (working). To construct this, I start by regressing college grades on high-school value added with no other covariates, saving the R^2 value R_0 and the treatment effect ϕ_0. I then run the fully-specified outcome model, again saving the R^2 value R_{full} and the coefficient on value added ϕ_{full}. Oster (2016) imposes that a specification with all possible controls, observable and unobservable, would explain 30% more of the variation than the model with the full set of covariates[13]; therefore, I let $R_{max} \equiv 1.3R_{full}$. The formula for the bias-adjusted coefficient ϕ^* is:

$$\phi^* = \phi_{full} - (\phi_0 - \phi_{full})\Big(\frac{R_{max} - R_{full}}{R_{full} - R_0}\Big) \tag{I.17}$$

The bias-adjusted coefficients that result can provide a bound on the effect size; it is a good sign if the bias-adjusted coefficients are not statistically different from their unadjusted counterparts, while if the bias-adjusted coefficient is actually larger than its unadjusted counterpart, then the bias is skewing the effect toward zero and is of relatively less concern. The fundamental assumption behind the Oster (2016) formulation is that the unobservables bias the result in the same direction that the observables do; if the sample is positively selected on observables, then it must be positively selected on unobservables, and the opposite.

Third, I present results in which I impute grades for students who are not in the grade sample. I geocode all public colleges in the state, and assign all students who did not attend college to their nearest community college, while assigning all students who attended private or out-of-state colleges, or did not take any large enough credit-bearing courses, to their nearest four-year public college. After "placing" each non-attendee in a college, I then assign the non-attendees to courses: each of these students is assigned to a math class, an English class, and one other class in their respective college in the appropriate year. The courses are chosen randomly, with the probabilities of course placement equal to the observed fractions of students enrolled in that course. In other words, if 50% of freshmen enrolled in a math class at Washtenaw Community College in 2011 take college algebra, 25% take calculus, and 25% take statistics, a non-attendee assigned to Washtenaw Community College would have a 50% probability of being placed in college algebra, 25% probability of being placed in calculus, and a 25% probability of being placed in statistics.

I assign grades to these students in two ways. First, as a bounding exercise, I assign 0.0 GPAs to all students in the counterfactual sample. Second, to obtain a more realistic effect,

[13] This parameter is chosen to allow the results of 90% of randomized experiments to be upheld in the bias-adjusted framework.

I use out-of-sample prediction to give grades to these students. In the sample of students who have course grades available, I regress their course grades on the full set of observables, except for the value added of their high schools. I then use these estimates to impute the grades of the students who do not actually attend public colleges in Michigan. For instance, a student with a high 8^{th}-grade test score taking pre-algebra would likely receive a high grade, while a student with a low 8^{th}-grade score taking calculus would likely receive a low grade.

I.5.3. Robustness Results

Table I.7 presents estimated coefficients on high-school value added, separated by subject, for each of the bias adjustments outlined in Section I.5.2. While some of those adjustments shrink the point estimates, almost none of them alter the statistical significance of the results. The estimates are especially robust to the attenuation bias in the VAM and to across-district school choice mechanisms; neither the instrumented VAM nor the home-district sample changes the results meaningfully in any subject. The selection into college makes a slightly larger difference, but all of the corrections for this selection leave a positive and significant effect of at least 0.04 grade points in the all-subjects sample.

Even after all of these corrections, there remains residential sorting across districts. To place an upper bound on the effect of the sorting, I follow the methodology in Altonji and Mansfield (working). This involves estimating the contribution of school characteristics to outcomes, controlling for student-level and aggregated student characteristics. In place of Equation I.13, I run a slightly-altered first-step regression, in which I replace the school fixed effects with school characteristics $\mathbf{W_{j,t-1}}$ that are not student aggregates: per-pupil expenditure (at the school and district level), pupil-teacher ratio, the fraction of students who transfer or drop out, and the average certification exam score of the school's teachers.

$$Y_{ijmnt} = \mu + \pi_1 Y_{im} + \mathbf{\Pi_2 X_i} + \mathbf{\Pi_3 \bar{X}_{j,t-1}} + \mathbf{\Pi_4 \bar{Z}_n} + \mathbf{\Pi_5 W_{j,t-1}} + \upsilon_t + \eta_{ijmnt} \tag{I.18}$$

I then find the variance of the contribution of those school observables $\mathbf{\Pi_5 W_j}$ and compare it to the variance of the VAM estimates from Section I.4.2. The ratio of these variances $\frac{\text{var}(\mathbf{\Pi_5 W_j})}{\text{var}(ValueAdded_j t)}$ represents the relative contribution of school observables to school value added, which places a lower bound on the contribution of the school itself to value added and an upper bound on the contribution of students' sorting across schools and districts to value added. I multiply my regression coefficients by this ratio to determine the minimum effect size that can be attributed definitively to school quality and not to sorting.

These results are shown in Table I.7 as well. I find that this lower bound estimate teeters along the edge of statistical significance, while its economic significance is somewhat limited, as a vast change in school effectiveness is met with a fairly modest change in college performance. This does cast some doubt upon my overall results if taken literally. However, it is an intentionally-conservative lower bound backed by a fairly prohibitive assumption: that all of schools' contribution to value added can be contained by observable variables such as per-pupil expenditure, teachers' certification scores, and student turnover. This leaves no room for schools to benefit students by teachers' creativity and hard work or by creating a positive learning environment, reducing the secret to student learning to a formula. In reality, this assumption is too punitive to be realistic; some of the effect can likely be attributed to students' sorting across districts, but there is room for schools to help students learn in ways that cannot be captured in a spreadsheet.

I.5.4. Results by Subgroup

One interpretation of the model in section I.3 is that schools whose students have weaker academic backgrounds may be forced to focus on test preparation in order to meet high-stakes accountability standards, whereas schools with better-prepared students can focus on content. This would imply a stronger relationship between test-score value added and college performance in the latter schools, as they provide learning that students can build upon while the test preparation emphasized in the former schools has less application in other environments. I test this in Table I.8, running the final specification from Table I.5 separately by quartile of high schools' average 8^{th}-grade exam scores. The results do not support this hypothesis. The largest point estimates are found in the schools in the bottom quartile; students gain more from attending schools that raise scores from very low to somewhat low than from attending schools that raise scores from high to very high.

Brand and Xie (2010) find that students who come from groups less likely to complete college, such as Black students and lower-income students, benefit most from attending college. This result could possibly extend to attending a good high school, but an opposing argument is also certainly plausible: that students from disadvantaged groups are underrepresented in higher-VAM schools (as shown in Table I.1), and thus the schools are not designed to serve them, or they are socially excluded in these schools; either of these may lead to such students receiving fewer benefits from attending a "better" school.

I test this by estimating college grades results separately for Black and white students, and separately for students who were economically disadvantaged and those who were not, presented in Table I.9. Effect sizes are somewhat larger for Black students than for white students; effects are also slightly larger for poor students than for non-poor students, but not significantly so. This result is reassuring, as it allays the fear that marginalized students are being left behind in high-VAM schools while white and wealthier students reap the benefits. High school VAM alone cannot close the state's gaps in college achievement, but it may be a part of the solution.

Finally, the ACT tests academic subjects and is used for admission at four-year colleges; schools that raise ACT scores may not be giving students the tools needed for success at community colleges as effectively, particularly in courses outside of the traditional academic fields. These schools may be focusing on their students who are on the four-year academic track at others' expense. I test this by running the college grades results separately at two-year and four-year institutions, as shown in Table I.10. If anything, I find the opposite. The effect sizes are positive and significant in both two-year and four-year institutions; the returns to attending a high-quality high school are actually marginally bigger in two-year colleges.

I.6 Discussion and Conclusion

I.6.1. Policy Context

The students studied in this paper, if they complete their secondary education on time, are members of the high school classes of 2010, 2011, and 2012. During this period, Michigan public schools were subject to the accountability measures in the No Child Left Behind Act of 2001 ("NCLB"). High schools were judged on whether they met Adequate Yearly Progress in math and reading proficiency rates, as well as graduation rates, both for the student body as a whole and for subgroups of interest such as Black students, Hispanic students, and students eligible for subsidized lunch. The necessary proficiency rates to achieve Adequate Yearly Progress grew more stringent year by year, putting schools under pressure to improve rapidly (Bielawski 2006). Additionally, the required 11^{th}-grade state standardized test incorporated the ACT college entrance exam during this time period. The ACT is specifically designed to be predictive of college performance, which implies a stronger relationship between state standardized test scores and college outcomes than one might expect from another standardized test. Additionally, the designers of the ACT state that a composite score of 21 is the benchmark for college readiness; this standard allows easier comparisons within the group of students who are expected to go to college. Finally, the Michigan Merit Curriculum required students during this sample period to take four years of math and English, three years of social studies, two years of a foreign language, and courses in biology, art, music, and either chemistry or physics (Jacob et al. 2017). This resulted in more challenging high-school coursework, and it prevented high schools from dedicating entire days to math and reading in the way that some elementary and middle schools did to prepare for standardized tests (McNeil and Valenzuela 2000).

From a policy perspective, or from the perspective of someone who cares about educational equality in the United States, the empirical results of this paper are encouraging. The model presents a bleak scenario in which schools with low-achieving incoming students cannot teach lasting lessons to students because they need to focus so completely on exam preparation. I do not find empirical evidence to support this claim; if anything, effect sizes are largest in these schools. Furthermore, effect sizes are notably large for Black students and for economically disadvantaged students; exposing these students to better schools, either by improving integration of disadvantaged students into more-advantaged schools or by making investments in schools with more-disadvantaged populations, can make a meaningful difference in their long-run success.

I.6.2. Further Research

While this study makes a meaningful contribution to the literature about the student-level returns to high-school quality and the usefulness of value-added models, room for further investigation remains. For instance, other measures of high-school quality may predict future success and long-term learning more effectively than test-score value added does; replicating the exercises done here, replacing test-score value added with measures related to graduation rates or other quality metrics, and seeing which best predicts long-term learning, would be a useful extension. Additionally, the set of students who attend in-state public colleges immediately after graduation tend to be fairly stable and high-achieving students; looking at the effects of high-school quality on persistence, completion, and grades at for-profit colleges would also be valuable, although the selection concerns might run in the opposite direction from the ones in this paper.

Future researchers and policymakers alike may be interested in learning more about the characteristics of the schools that see high value added and large effects on college performance, particularly those that do so despite having low-performing students at entry. If there are patterns in the classroom practices most prevalent in these schools, or in how they spend money, assign teachers, and allocate other scarce resources, then other schools may mimic these patterns, hoping to achieve similar results. Conversely, there is also a chance that these schools have unique characteristics that other schools cannot replicate to the same effect.

I.6.3. Summary

I seek to measure the effect of high-school quality on first-year college course grades, in order to determine how much of high schools' value added truly comes in the form of persistent learning and skills. I match school-year level test-score value added measures onto college transcripts, looking separately at the effects on grades in all subjects, tested subjects, and non-tested subjects. Even after numerous adjustments for selection into high schools and colleges, a stable positive effect of high-school quality on college grades remains; students who attended a high school one standard deviation above the school-level average receive first-year grades between 0.04 and 0.1 grade points higher than their otherwise-identical counterparts who graduated from average schools. This implies that much of these high-quality schools' improvements in test scores are driven by durable student learning, a result that should ease some concerns of some skeptics of standardized testing.

I.7 Supplemental Figures

Figure I.1.: Distribution of Value Added, Student Level

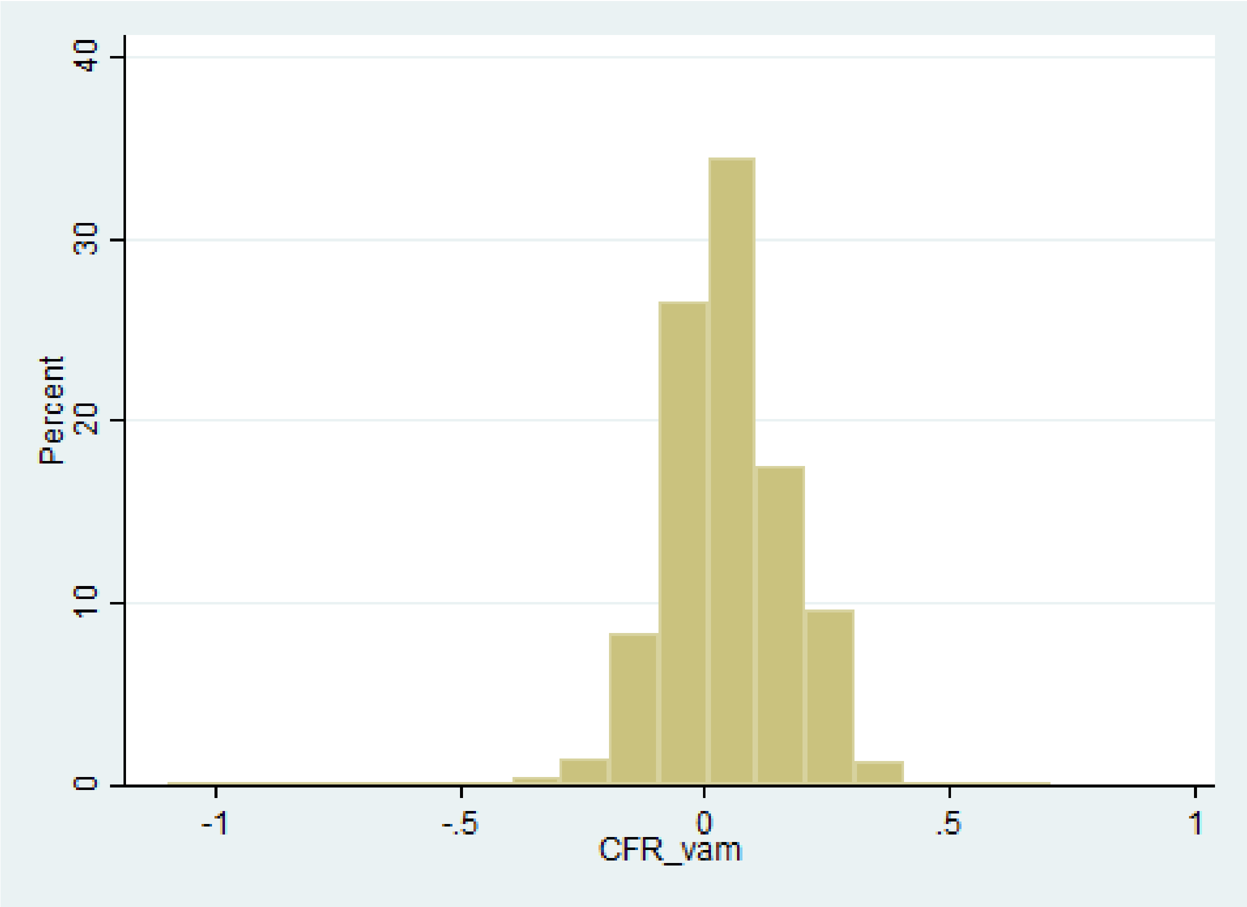
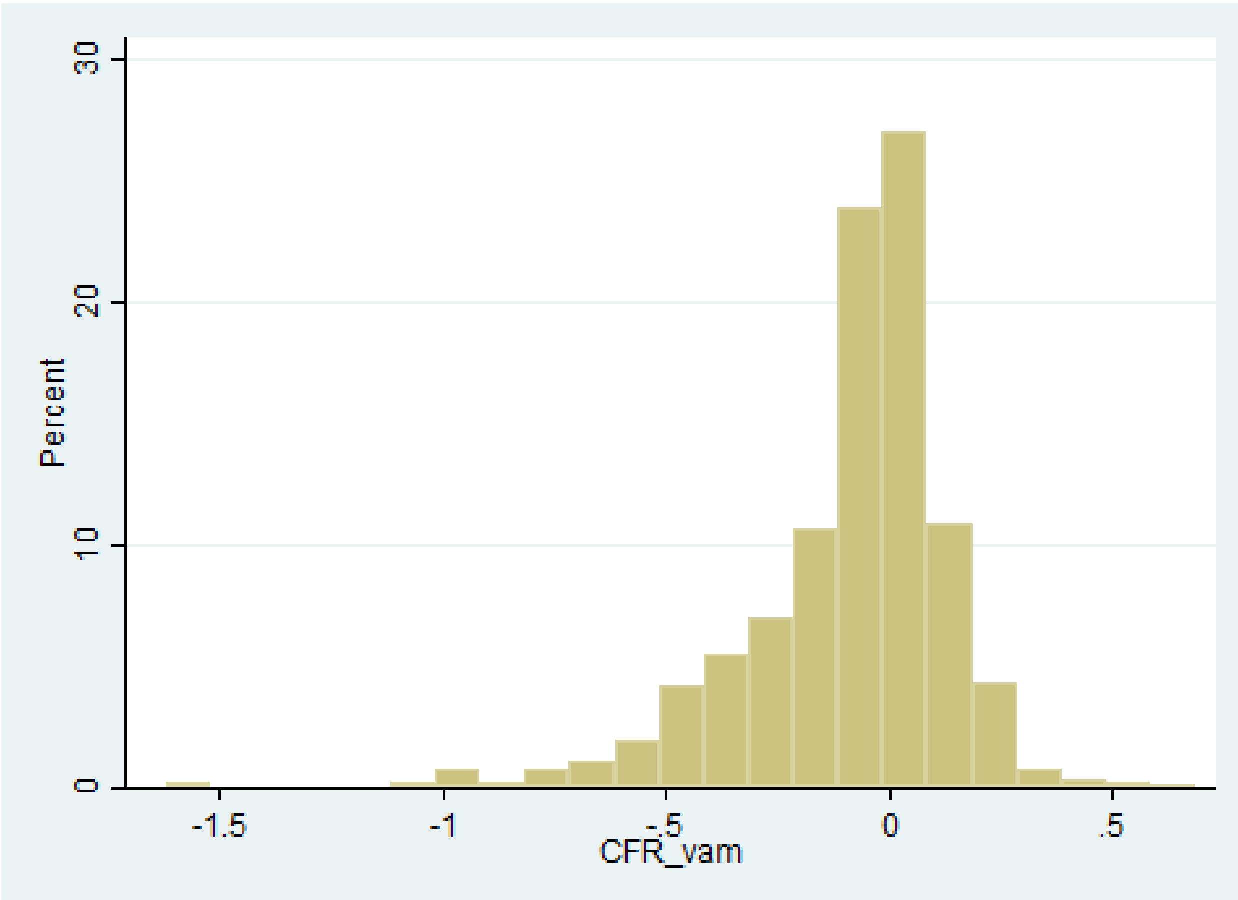


Figure I.2.: Distribution of Value Added, School Level



I.8 Supplemental Tables

Table I.1.: Student Characteristics by VAM Quartile

	Lowest	2^{nd}	3^{rd}	Highest
Fraction Black	0.353	0.154	0.094	0.137
Fraction Hispanic	0.054	0.046	0.039	0.029
Fraction Asian	0.012	0.013	0.019	0.047
Fraction in Special Education	0.145	0.128	0.113	0.102
Fraction Limited English Proficiency	0.044	0.025	0.021	0.021
Fraction Economically Disadvantaged	0.575	0.379	0.277	0.199
Fraction in Charter Schools	0.052	0.022	0.022	0.033
Fraction in Magnet Schools	0.107	0.163	0.089	0.104
Average 8^{th}-Grade Standardized Test Score	-0.335	-0.035	0.158	0.375
Average 11^{th}-Grade Standardized Test Score	-0.348	-0.032	0.16	0.362
Fraction Graduating High School	0.69	0.801	0.847	0.869
Fraction Entering College	0.613	0.705	0.769	0.837
Number of Observations	112,147	93,912	84,504	76,103

One observation per student. VAM is estimated following the method in Chetty, Friedman, and Rockoff (2014).

Table I.2.: College Placement by VAM Quartile

	Lowest	2nd	3rd	Highest
No College	0.387	0.295	0.231	0.163
In-State Community College	0.356	0.373	0.351	0.304
In-State Public Four-Year	0.118	0.188	0.26	0.349
In-State Private	0.067	0.074	0.075	0.066
Out of State	0.071	0.07	0.082	0.118
Any College	0.613	0.705	0.769	0.837
In Grade Sample	0.4	0.505	0.564	0.632
Takes Math, If Ever in Michigan Public College	0.403	0.445	0.473	0.486
Takes English, If Ever in Michigan Public College	0.493	0.513	0.501	0.477
Number of Observations	112,147	93,912	84,504	76,103

One observation per student. VAM is estimated following the method in Chetty, Friedman, and Rockoff (2014).

Table I.3.: College Attendance and Graduation - Probits

	Takes ACT	Graduates HS	Any College	In Grade Sample
Scaled High School VAM	0.015***	0.025***	0.036***	0.023***
	(0.005)	(0.005)	(0.004)	(0.006)
8^{th}-Grade Test Score	0.078***	0.1***	0.161***	0.149***
	(0.002)	(0.002)	(0.002)	(0.003)
Female	0.021***	0.046***	0.081***	0.065***
	(0.001)	(0.002)	(0.002)	(0.002)
Black	-0.002	0.009**	0.093***	0.05***
	(0.004)	(0.004)	(0.005)	(0.006)
Hispanic	-0.038***	-0.037***	-0.031***	-0.041***
	(0.005)	(0.004)	(0.005)	(0.007)
Asian	-0.018**	-0.013	0.032***	0.051***
	(0.008)	(0.01)	(0.013)	(0.011)
Special Education	-0.02***	-0.017***	-0.067***	-0.077***
	(0.003)	(0.002)	(0.002)	(0.003)
Limited English Proficiency	0.008	0.025***	0.063***	0.043***
	(0.007)	(0.007)	(0.01)	(0.014)
Economically Disadvantaged	-0.076***	-0.099***	-0.092***	-0.11***
	(0.002)	(0.002)	(0.002)	(0.002)
Block-Level and School-Level Variables?	Yes	Yes	Yes	Yes
Number of Observations	366,663	366,663	366,665	366,663

One observation per student. Average marginal effects shown. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Bootstrapped standard errors in parentheses, clustered by high school.

Table I.4.: Falsification Tests

	7^{th}-Grade Score	7^{th}-Grade Attendance	Block-Grp. Pct. In Pov.	Block-Grp. Pct. With BA
Scaled High School VAM	0.029***	0.002*	-0.017	1.61***
	(0.006)	(0.001)	(0.303)	(0.41)
Student-Level Variables?	Yes	Yes	Yes	Yes
Block-Level Variables?	Yes	Yes	No	No
School-Level Variables?	Yes	Yes	Yes	Yes
Number of Observations	352,629	350,058	366,666	366,666

One observation per student. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Standard errors in parentheses, clustered by high school.

Table I.5.: All Grades, Full Sample

	(1)	(2)	(3)	(4)	(5)	(6)
Scaled High School VAM	0.253***	0.198**	0.161***	0.11***	0.095***	0.086***
	(0.027)	(0.021)	(0.018)	(0.012)	(0.015)	(0.014)
8^{th}-Grade Test Score			0.328***	0.274***	0.332***	0.315***
			(0.007)	(0.006)	(0.007)	(0.007)
Female				0.257***	0.258***	0.244***
				(0.006)	(0.006)	(0.005)
Black				-0.427***	-0.309***	-0.294***
				(0.014)	(0.013)	(0.013)
Hispanic				-0.147***	-0.115***	-0.115***
				(0.018)	(0.017)	(0.018)
Asian				0.026	0.041***	0.028
				(0.017)	(0.016)	(0.017)
Special Education				-0.112***	-0.092***	-0.092***
				(0.013)	(0.013)	(0.011)
Limited English Proficiency				0.181***	0.204***	0.185***
				(0.02)	(0.02)	(0.019)
Economically Disadvantaged				-0.17***	-0.152***	-0.154***
				(0.009)	(0.009)	(0.007)
Course ID Fixed Effects?	No	Yes	Yes	Yes	Yes	Yes
Block-Level and School-Level Variables?	No	No	No	No	Yes	Yes
Bootstrapped or Weighted?	Weighted	Weighted	Weighted	Weighted	Weighted	Bootstrapped
Number of Observations	594,239	594,239	594,239	594,239	594,239	594,239

One observation per student-course combination. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Outcome variable is the grade in the student's first-year courses in college, on a 4.0 scale. Math and English courses must also be the lowest-numbered such course in the first semester in which a math or English class is taken. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Standard errors in parentheses, clustered by high school.

Table I.6.: Grades by Subject, Full Sample

	Math	English	Other	Other
Scaled High School VAM	0.131***	0.061***	0.095***	0.082***
	(0.017)	(0.019)	(0.015)	(0.016)
8^{th}-Grade Test Score	0.287***	0.214***	0.366***	0.342***
	(0.013)	(0.01)	(0.008)	(0.007)
Female	0.287***	0.345***	0.223***	0.21***
	(0.009)	(0.009)	(0.007)	(0.006)
Black	-0.312***	-0.299***	-0.308***	-0.289***
	(0.024)	(0.02)	(0.015)	(0.014)
Hispanic	-0.132***	-0.148***	-0.108***	-0.103***
	(0.032)	(0.027)	(0.02)	(0.021)
Asian	0.034	0.043	0.04***	0.025*
	(0.032)	(0.027)	(0.015)	(0.015)
Special Education	-0.101***	-0.101***	-0.081***	-0.086***
	(0.021)	(0.02)	(0.013)	(0.012)
Limited English Proficiency	0.246***	0.164***	0.191***	0.176***
	(0.037)	(0.029)	(0.021)	(0.025)
Economically Disadvantaged	-0.094***	-0.159***	-0.16***	-0.166***
	(0.014)	(0.011)	(0.01)	(0.009)
Course ID Fixed Effects?	Yes	Yes	Yes	Yes
Block-Level and School-Level Variables?	Yes	Yes	Yes	Yes
Bootstrapped or Weighted?	Bootstrapped	Bootstrapped	Weighted	Bootstrapped
Number of Observations	79,728	86,566	427,945	427,945

One observation per student-course combination. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Outcome variable is the grade in the student's first-year courses in college, on a 4.0 scale. Math and English courses must also be the lowest-numbered such course in the first semester in which a math or English class is taken. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Standard errors in parentheses, clustered by high school.

Table I.7.: Estimates Accounting for Bias

	All	Math	English	Other
Instrumented VAM	0.068***	0.09***	0.066***	0.064***
	(0.011)	(0.016)	(0.015)	(0.012)
	[594,239]	[79,728]	[86,566]	[427,945]
Home-District Sample	0.09***	0.139***	0.069***	0.085***
	(0.018)	(0.021)	(0.021)	(0.018)
	[482,889]	[64,953]	[69,758]	[348,178]
College-Ready Sample	0.04**	0.086***	0.042*	0.03**
	(0.016)	(0.022)	(0.025)	(0.014)
	[277,361]	[36,024]	[32,812]	[208,515]
Oster (2016) Bias-Adjusted Treatment Effect	0.049***	0.092***	0.011	0.047***
	(0.013)	(0.017)	(0.019)	(0.016)
	[594,239]	[79,728]	[86,566]	[427,945]
Counterfactual Sample, 0.0 GPAs	0.087***	0.062***	0.07***	0.115***
	(0.015)	(0.015)	(0.02)	(0.016)
	[1,136,352]	[243,585]	[254,451]	[638,316]
Counterfactual Sample, Imputed Grades	0.043***	0.025***	0.028***	0.062***
	(0.007)	(0.005)	(0.005)	(0.012)
	[1,136,352]	[243,585]	[254,451]	[638,316]
Altonji and Mansfield (working) Lower Bound	0.023*	0.036**	0.017	0.022
	(0.014)	(0.017)	(0.019)	(0.016)
	[594,239]	[79,728]	[86,566]	[427,945]
Course ID Fixed Effects?	Yes	Yes	Yes	Yes
Block-Level and School-Level Variables?	Yes	Yes	Yes	Yes

One observation per student-course combination. Estimates shown are the coefficients on the high-school value added term in the respective regression specification. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Outcome variable is the grade in the student's first-year courses in college, on a 4.0 scale. Math and English courses must also be the lowest-numbered such course in the first semester in which a math or English class is taken. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Bootstrapped standard errors in parentheses, clustered by high school. Sample sizes in brackets.

Table I.8.: Estimates by School 8th-Grade Exam Quartile

	All	Math	English	Other
Lowest Quartile	0.115***	0.159***	0.129***	0.101***
	(0.024)	(0.031)	(0.031)	(0.021)
	[138,781]	[18,913]	[22,927]	[96,941]
2nd Quartile	0.041*	0.091***	0.014	0.037
	(0.024)	(0.035)	(0.041)	(0.026)
	[148,373]	[19,994]	[22,862]	[105,517]
3rd Quartile	0.062**	0.127***	0.001	0.062**
	(0.022)	(0.038)	(0.032)	(0.029)
	[153,237]	[20,632]	[21,788]	[110,817]
Highest Quartile	0.055**	0.082**	0.008	0.058**
	(0.026)	(0.036)	(0.039)	(0.028)
	[153,848]	[20,189]	[18,989]	[114,670]
Course ID Fixed Effects?	Yes	Yes	Yes	Yes
Block-Level and School-Level Variables?	Yes	Yes	Yes	Yes

One observation per student-course combination. Estimates shown are the coefficients on the high-school value added term in the respective regression specification. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Outcome variable is the grade in the student's first-year courses in college, on a 4.0 scale. Math and English courses must also be the lowest-numbered such course in the first semester in which a math or English class is taken. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Bootstrapped standard errors in parentheses, clustered by high school. Sample sizes in brackets.

Table I.9.: Estimates by Race and Income

	All	Math	English	Other
Black	0.124***	0.138***	0.108***	0.124***
	(0.023)	(0.037)	(0.024)	(0.027)
	[80,442]	[11,176]	[13,072]	[56,194]
White	0.058***	0.114***	0.031*	0.053***
	(0.015)	(0.022)	(0.018)	(0.013)
	[479,533]	[63,867]	[68,852]	[346,814]
Economically Disadvantaged	0.105***	0.119***	0.119***	0.098***
	(0.016)	(0.028)	(0.024)	(0.018)
	[136,079]	[18,753]	[22,269]	[95,057]
Not Economically Disadvantaged	0.075***	0.13***	0.037**	0.072***
	(0.013)	(0.024)	(0.015)	(0.015)
	[458,160]	[60,975]	[64,297]	[332,888]
Course ID Fixed Effects?	Yes	Yes	Yes	Yes
Block-Level and School-Level Variables?	Yes	Yes	Yes	Yes

One observation per student-course combination. Estimates shown are the coefficients on the high-school value added term in the respective regression specification. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Outcome variable is the grade in the student's first-year courses in college, on a 4.0 scale. Math and English courses must also be the lowest-numbered such course in the first semester in which a math or English class is taken. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Bootstrapped standard errors in parentheses, clustered by high school. Sample sizes in brackets.

Table I.10.: Estimates by College Type

	All	Math	English	Other
Four-Year Colleges	0.076***	0.109***	0.047**	0.073***
	(0.015)	(0.027)	(0.024)	(0.019)
	[332,708]	[41,054]	[36,964]	[254,690]
Two-Year Colleges	0.092***	0.137***	0.07***	0.087***
	(0.017)	(0.022)	(0.021)	(0.019)
	[254,343]	[37,630]	[48,510]	[168,203]
Course ID Fixed Effects?	Yes	Yes	Yes	Yes
Block-Level and School-Level Variables?	Yes	Yes	Yes	Yes

One observation per student-course combination. Estimates shown are the coefficients on the high-school value added term in the respective regression specification. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Outcome variable is the grade in the student's first-year courses in college, on a 4.0 scale. Math and English courses must also be the lowest-numbered such course in the first semester in which a math or English class is taken. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Bootstrapped standard errors in parentheses, clustered by high school. Sample sizes in brackets.

Bibliography

[1] ACT Inc. (2010). *What Are ACT's College Readiness Benchmarks?*. `http://files.eric.ed.gov/fulltext/ED510475.pdf`.

[2] Ahn, T. (2016). A Theory of Dynamic Investment in Education in Response to Accountability Pressure. *Economics Letters*, 75-78.

[3] Allensworth, E.M., Moore, P.T., Sartain, L. & de la Torre, M. (2017). The Educational Benefits of Attending Higher Performing Schools: Evidence From Chicago High Schools. *Educational Evaluation and Policy Analysis* 1-23.

[4] Altonji, J.G., Elder, T.E., & Taber, C.R. *An Evaluation of Instrumental Variable Strategies for Estimating the Effects of Catholic Schools*. Working paper.

[5] Altonji, J.G. & Mansfield, R.K. *Estimating Group Effects Using Averages of Observables to Control for Sorting on Unobservables: School and Neighborhood Effects*. Working paper.

[6] Angrist, J.D., Cohodes, S.R., Dynarski, S.M., Pathak, P.A., & Walters, C.R. (2016). Stand and Deliver: Effects of Boston's Charter High Schools on College Preparation, Entry, and Choice. *Journal of Labor Economics* 275-318.

[7] Angrist, J.D., Hull, P.D., Pathak, P.A., & Walters, C.R. *Leveraging Lotteries for School Value-Added: Testing and Estimation*. Working paper.

[8] Bastian, J. *The Rise of Working Mothers and the 1975 Earned Income Tax Credit*. Working paper.

[9] Bielawski, P. (2006). Michigan School Accountability: AYP and Education YES! *Michigan Department of Education, Office of Educational Assessment and Accountability*.

[10] Brand, J.E. & Xie, Y. (2010). Who Benefits Most from College? Evidence for Negative Selection in Heterogeneous Economic Returns to Higher Education. *American Sociological Review*, 273-302.

[11] Chetty, R., Friedman, J.N., & Rockoff, J.E. (2014). Measuring the Impacts of Teachers I: Evaluating Bias in Teacher Value-Added Estimates. *American Economic Review*, 2593-2632.

[12] Cohodes, S.R. (2016). Teaching to the Student: Charter School Effectiveness in Spite of Perverse Incentives. *Education Finance and Policy*, 1-42.

[13] Deming, D.J. (2014). Using School Choice Lotteries to Test Measures of School Effectiveness. *American Economic Review: Papers & Proceedings*, 406-411.

[14] Deming, D.J., Hastings, J.S., Kane, T.J., & Staiger, D.O. (2014). School Choice, School Quality, and Postsecondary Attainment. *American Economic Review*, 991-1013.

[15] Deming, D.J., Cohodes, S., Jennings, J., & Jencks, C. (2016). School Accountability, Postsecondary Attainment, and Earnings. *Review of Economics and Statistics*, 848-862.

[16] Dobbie, W. & Fryer Jr., R.G. (2015). The Medium-Term Impacts of High-Achieving Charter Schools. *Journal of Political Economy* 985-1037.

[17] Dynarski, S., Hyman, J., & Schanzenbach, D.W. (2013). Experimental Evidence on the Effect of Childhood Investments on Postsecondary Attainment and Degree Completion. *Journal of Policy Analysis and Management*, 692-717.

[18] Ehlert, M., Koedel, C., Parsons, E., & Podgursky, M.J. (2014). The Sensitivity of Value-Added Estimates to Specification Adjustments: Evidence From School- and Teacher-Level Models in Missouri. *Statistics and Public Policy*, 19-27.

[19] Hanushek, E.A., & Rivkin, S.G. (2010). Generalizations about Using Value-Added Measures of Teacher Quality. *American Economic Review: Papers & Proceedings*, 267-271.

[20] Herrmann, M., Walsh, E., & Isenberg, D. (2016). Shrinkage of Value-Added Estimates and Characteristics of Students with Hard-to-Predict Achievement Levels. *Statistics and Public Policy*, 1-10.

[21] Jackson, C.K. *Non-Cognitive Ability, Test Scores, and Teacher Quality: Evidence from 9^{th} Grade Teachers in North Carolina*. Working paper.

[22] Jacob, B.A. (2005). Accountability, Incentives and Behavior: the Impact of High-Stakes Testing in the Chicago Public Schools. *Journal of Public Economics*, 761-796.

[23] Jacob, B., Dynarski, S., Frank, K., & Schneider, B. (2017). Are Expectations Alone Enough? Estimating the Effect of a Mandatory College-Prep Curriculum in Michigan. *Educational Evaluation and Policy Analysis*, 333-360.

[24] Jennings, J.L. & Bearak, J.M. (2014). "Teaching to the Test" in the NCLB Era: How Test Predictability Affects Our Understanding of Student Performance. *Educational Researcher*, 381-389.

[25] Jennings, J.L., Deming, D., Jencks, C., Lopuch, M., & Schueler, B.E. (2015). Do Differences in School Quality Matter More Than We Thought? New Evidence on Educational Opportunity in the Twenty-first Century. *Sociology of Education*, 56-82.

[26] Master, B., Loeb, S., & Wyckoff, J. (2017). More Than Content: The Persistent Cross-Subject Effects of English Language Arts Teachers Instruction. *Educational Evaluation and Policy Analysis*, 1-19.

[27] McNeil, L. & Valenzuela, A. (2000). The Harmful Impact of the TAAS System of Testing in Texas: Beneath the Accountability Rhetoric. *Educational Resources Information Center.*

[28] Merseth, K.K. (2010). High-Performing Charter Schools: Serving Two Masters? In R.J. Lake, *A Balanced Look at American Charter Schools in 2009* (pp. 27-38). Bothell, WA: National Charter School Research Project.

[29] Muralidharan, K. & Sundararaman, V. (2011). Teacher Performance Pay: Experimental Evidence from India. *Journal of Political Economy*, 39-77.

[30] Neal, D. & Schanzenbach, D.W. (2010). Left Behind by Design: Proficiency Counts and Test-Based Accountability. *Review of Economics and Statistics*, 263-283.

[31] Oster, E. (2016). Unobservable Selection and Coefficient Stability: Theory and Evidence. *Journal of Business and Economic Statistics*, 1-18.

A Mathematical Appendix

As stated above, when the high-stakes accountability constraint binds, the result must satisfy the accountability constraint $KS^\gamma L^\delta = \theta$ and the budget constraint $\Omega = c_S S + c_L L$. Solve the accountability constraint for S:

(I.19)
$$S^\gamma = \frac{\theta}{AL^\delta}; S = \frac{\theta^{\frac{1}{\gamma}}}{K^{\frac{1}{\gamma}} L^{\frac{\delta}{\gamma}}}$$

Substitute this into the budget constraint:

(I.20)
$$\Omega(1 + A^{**}) = c_S \frac{\theta^{\frac{1}{\gamma}}}{K^{\frac{1}{\gamma}} L^{\frac{\delta}{\gamma}}} + c_L L$$

This cannot be solved analytically unless $\delta = \gamma$. However, if $\frac{\partial T}{\partial S} > \frac{\partial T}{\partial L}$ at the unconstrained optimum under high-stakes accountability, then the school will reallocate toward short-term resources (i.e. focus more on test preparation) in order to move toward the threshold. Since $T = KS^\gamma L^\delta$:

(I.21)
$$\frac{\partial T}{\partial S} = \gamma K S^{\gamma-1} L^\delta; \frac{\partial T}{\partial L} = \delta K S^\gamma L^{\delta-1}$$

Evaluate the derivatives at the unconstrained maximizing values of S and L:

(I.22)
$$\frac{\partial T}{\partial S} = \gamma K \left(\frac{\Omega(1 + A^{**})}{c_S}\left(\frac{\alpha\gamma}{\alpha\gamma + \alpha\delta + \beta\zeta}\right)\right)^{\gamma-1} \left(\frac{\Omega(1 + A^{**})}{c_L}\left(\frac{\alpha\delta + \beta\zeta}{\alpha\gamma + \alpha\delta + \beta\zeta}\right)\right)^{\delta}$$

(I.23)
$$\frac{\partial T}{\partial L} = \delta K \left(\frac{\Omega(1 + A^{**})}{c_S}\left(\frac{\alpha\gamma}{\alpha\gamma + \alpha\delta + \beta\zeta}\right)\right)^{\gamma} \left(\frac{\Omega(1 + A^{**})}{c_L}\left(\frac{\alpha\delta + \beta\zeta}{\alpha\gamma + \alpha\delta + \beta\zeta}\right)\right)^{\delta-1}$$

Start by dividing both equations by $\left(\frac{\Omega(1+A^{**})}{c_S}\left(\frac{\alpha\gamma}{\alpha\gamma+\alpha\delta+\beta\zeta}\right)\right)^{\gamma-1}\left(\frac{\Omega(1+A^{**})}{c_L}\left(\frac{\alpha\delta+\beta\zeta}{\alpha\gamma+\alpha\delta+\beta\zeta}\right)\right)^{\delta-1}$; then $\frac{\partial T}{\partial S} > \frac{\partial T}{\partial L}$ if:

(I.24)
$$\gamma \left(\frac{\Omega(1 + A^{**})}{c_L}\left(\frac{\alpha\delta + \beta\zeta}{\alpha\gamma + \alpha\delta + \beta\zeta}\right)\right) > \delta \left(\frac{\Omega(1 + A^{**})}{c_S}\left(\frac{\alpha\gamma}{\alpha\gamma + \alpha\delta + \beta\zeta}\right)\right)$$

Then divide through by $\frac{\gamma\Omega(1+A^{**})}{\alpha\gamma+\alpha\delta+\beta\zeta}$, and obtain that $\frac{\partial T}{\partial S} > \frac{\partial T}{\partial L}$ if:

$$\frac{\alpha\delta+\beta\zeta}{c_L} > \frac{\alpha\delta}{c_S} \tag{I.25}$$

If the costs are equal, the school will always move toward short-term resources if constrained, because $\beta\zeta > 0$.

B Data Appendix

B.1. Covariates In Fully-Specified Regressions

The first stage of the value-added model contains the following covariates.

- Individual test scores: standardized 8^{th}-grade math score, standardized 8^{th}-grade reading score, interaction of standardized 8^{th}-grade math and reading scores.
- Individual demographics: Black, Hispanic, Asian, female, economically disadvantaged, limited English proficiency, special education.
- Census block group averages: household income, has a high school diploma, has a bachelor's degree, white, Black, Asian, Hispanic, married, employed, home owner, below poverty line.
- School averages: Black, economically disadvantaged, 8^{th}-grade test score.
- School-level variables: enrollment.
- Missing indicators for all of the above.
- Cohort fixed effects.
- School fixed effects (variable of interest).

The second stage of the value-added model contains the following covariates.

- Average residualized scores: one year forward, two years forward, one year backward, two years backward.
- Missing indicators for all of the above.

The Altonji and Mansfield (working) decomposition regression contains the following covariates.

- Individual test scores: standardized 8^{th}-grade math score, standardized 8^{th}-grade reading score, interaction of standardized 8^{th}-grade math and reading scores.
- Individual demographics: Black, Hispanic, Asian, female, economically disadvantaged, limited English proficiency, special education.
- Census block group averages: household income, has a high school diploma, has a bachelor's degree, white, Black, Asian, Hispanic, married, employed, home owner, below poverty line.
- School averages: Black, economically disadvantaged, 8^{th}-grade test score.
- School-level variables: enrollment, per-pupil instructional expenditure, district per-pupil instructional expenditure, pupil/teacher ratio, fraction of students leaving the school without graduating, teacher certification exam score.
- Missing indicators for all of the above.
- Cohort fixed effects.

The outcome regressions contain the following covariates.

- Individual test scores: standardized 8^{th}-grade scores in a fourth-order polynomial.
- Individual demographics: Black, Hispanic, Asian, female, economically disadvantaged, limited English proficiency, special education.
- Census block group averages: household income, has a high school diploma, has a bachelor's degree, white, Black, Asian, Hispanic, married, employed, home owner, below poverty line.
- School averages: Black, economically disadvantaged, 8^{th}-grade test score, attendance.
- School-level variables: enrollment, magnet, charter.
- Missing indicators for all of the above.
- High school value added, expressed in terms of its school-level standard deviation (variable of interest).

B.2. Subject Abbreviations

The following abbreviations correspond to English courses at the listed institutions:

- "ENG": Alpena Community College, Central Michigan University, Delta College, Gogebic Community College, Grand Valley State University, Henry Ford Community College, Jackson Community College, Kalamazoo Valley Community College, Kirtland Community College, Marygrove College, Mid Michigan Community College, Muskegon Community College, North Central Michigan College, Northwestern Michigan College, Oakland Community College, Schoolcraft College, St. Clair County Community College, University of Michigan-Flint, Washtenaw Community College, Wayne County Community College, Wayne County Community College District, Wayne State University, West Shore Community College
- "ENGL": Bay de Noc Community College, Eastern Michigan University, Ferris State University, Kellogg Community College, Lake Michigan College, Lake Superior State University, Macomb Community College, Monroe County Community College, Montcalm Community College, Mott Community College, Saginaw Valley State University, Southwestern Michigan College, The Robert B. Miller College, Western Michigan University
- "DEN": College for Creative Studies
- "COM": Glen Oaks Community College
- "EN": Grand Rapids Community College, Northern Michigan University
- "WRIT": Lansing Community College
- "WRA": Michigan State University
- "UN": Michigan Technological University
- "WRT": Oakland University
- "ENGLISH": University of Michigan-Ann Arbor
- "COMP": University of Michigan-Dearborn

The following abbreviations correspond to math courses at the listed institutions:

- “MTH”: Alpena Community College, Central Michigan University, Delta College, Gogebic Community College, Grand Valley State University, Jackson Community College, Kirtland Community College, Marygrove College, Michigan State University, North Central Michigan College, Northwestern Michigan College, Oakland University, St. Clair County Community College, University of Michigan-Flint, Washtenaw Community College, West Shore Community College

- “MATH”: Bay de Noc Community College, Eastern Michigan University, Ferris State University, Henry Ford Community College, Kalamazoo Valley Community College, Kellogg Community College, Lake Michigan College, Lake Superior State University, Lansing Community College, Macomb Community College, Monroe County Community College, Montcalm Community College, Mott Community College, Muskegon Community College, North Central Michigan College, Saginaw Valley State University, Schoolcraft College, Southwestern Michigan College, The Robert B. Miller College, University of Michigan-Ann Arbor, University of Michigan-Dearborn, Western Michigan University

- “NSM”: Glen Oaks Community College

- “MA”: Grand Rapids Community College, Michigan Technological University, Northern Michigan University

- “MAT”: Mid Michigan Community College, Oakland Community College, Wayne County Community College, Wayne County Community College District, Wayne State University

- “MMTH”: West Shore Community College

B.3. Appendix Tables

Table I.11.: College Attendance and Graduation - Probits, College-Ready Sample

	Takes ACT	Graduates HS	Any College	In Grade Sample
Scaled High School VAM	0.015***	0.024***	0.028***	0.005
	(0.004)	(0.004)	(0.004)	(0.009)
8th-Grade Test Score	0.052**	0.056***	0.151***	0.258***
	(0.021)	(0.022)	(0.041)	(0.058)
Female	0.006***	0.015***	0.045***	0.032***
	(0.001)	(0.002)	(0.002)	(0.003)
Black	-0.016***	-0.012**	0.022***	-0.03***
	(0.003)	(0.006)	(0.007)	(0.011)
Hispanic	-0.021***	-0.023***	-0.012	-0.025***
	(0.004)	(0.005)	(0.008)	(0.009)
Asian	-0.021***	-0.022***	-0.002	0.036***
	(0.003)	(0.004)	(0.009)	(0.011)
Special Education	-0.032***	-0.044***	-0.052***	-0.073***
	(0.004)	(0.005)	(0.006)	(0.009)
Limited English Proficiency	-0.037***	-0.037***	-0.037***	-0.06***
	(0.006)	(0.01)	(0.009)	(0.015)
Economically Disadvantaged	-0.037***	-0.052***	-0.064***	-0.091***
	(0.002)	(0.003)	(0.003)	(0.004)
Block-Level and School-Level Variables?	Yes	Yes	Yes	Yes
Number of Observations	115,116	115,116	115,116	115,116

One observation per student. Marginal effects at means shown. There are as many "college-ready" students as have a maximum ACT composite of 21 or higher; this same number of students is then chosen based on their 8th-grade test score. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Bootstrapped standard errors in parentheses, clustered by high school.

Table I.12.: Falsification Tests, Home-District Sample

	7^{th}-Grade Score	7^{th}-Grade Attendance	Block-Grp. Pct. In Pov.	Block-Grp. Pct. With BA
Scaled High School VAM	0.054***	0.003**	-0.145	1.92***
	(0.007)	(0.001)	(0.365)	(0.583)
Student-Level Variables?	Yes	Yes	Yes	Yes
Block-Level Variables?	Yes	Yes	No	No
School-Level Variables?	Yes	Yes	Yes	Yes
Number of Observations	275,601	276,897	287,348	287,348

One observation per student. Only students who attend non-charter, non-magnet high schools in the district in which they reside are included. Value added is estimated following the method in Chetty, Friedman, and Rockoff (2014), normalized in terms of its school-level standard deviation. Missing values of covariates are recoded to 0; missing indicators are included but not shown. Standard errors in parentheses, clustered by high school.

II The Impact of Local Labor Market Shocks on College Choice: Evidence from Plant Closings in Michigan

II.1 Introduction

Students who face a weak local labor market when they graduate from high school are left with a daunting decision to make. If they choose to acquire additional education, they gain credentials that will help them get higher-paying work when they finish and they do not forgo lucrative immediate employment options, but they incur heavy costs at a time when their families' financial situations are at their most precarious. If they instead choose to enter the labor market immediately, they avoid burdening themselves and their families with high costs and debt when unemployment is prevalent, but they may have trouble finding work in the weak labor market and they will have fewer opportunities for higher-paying work without a college degree.

Economic justification exists for both choices. Human capital theory dating back to Becker (1975) and Rosen (1977) suggests that when the local labor market is in a recession, students should respond by acquiring more education. If there are no credit constraints, then a substantial negative shock to local employment opportunities should actually induce an increase in college attendance; students will be less able to acquire a job with only a high school diploma, making additional credentials more valuable. Furthermore, the opportunity cost of attending college will be much lower when the local job market is in bad shape, as high school graduates' expected wages if they enter the job market (including zero as a possible value if they are unable to find work) will be low. Empirical studies such as Black, McKinnish, and Sanders (2005) and Cascio and Narayan (2015) validate this theory in the case of public high school education, which has no cost to the student except the opportunity cost of the time spent in school. However, many studies, most famously including Lochner and Monge-Naranjo (2011), have indicated that students are heavily influenced by their current resources when deciding whether or not to enter college, indicating that students are sufficiently credit-constrained that they cannot always fully borrow the cost of college. Therefore, the shock to family resources resulting from a local economic downturn (particularly if a parent becomes unemployed) may well diminish or even outweigh the positive response to the reduced opportunity cost.[1]

In this study, I evaluate the effects of local job losses on several measures of students' postsecondary education choice. To do this, I take advantage of the reporting requirements of the WARN Act of 1988, which requires employers to post public notices and send written notifications to city and state officials if they lay off more than 50 workers or close a facility with over 50 employees. Controlling for students' academic backgrounds and demographics, I determine whether students alter their choices of whether to attend college (and if so, what type of college to attend) as they see larger percentages of people in their local areas lose their jobs in these plant closings[2] and mass layoffs. In order to create some context for the college findings, I also examine the effects of these events on 8$^{\text{th}}$-grade test scores, following a similar approach to Ananat, Gassman-Pines, and Gibson-Davis (2009); this separates the effects of psychological and school resource shocks from the changes in relative costs that are assumed to drive much of the college results.

The results suggest a significant role for credit constraints in driving students' postsecondary education decisions. In OLS specifications, a 1-standard deviation increase in the fraction of the local population losing jobs in a year is associated with a 0.2-percentage-point (0.3%) increase in the probability of attending college. This effect is composed entirely of a 0.3-percentage-point (1.2%) increase in the probability of attending a two-year college, implying that many of the students who move into attending two-year colleges would have otherwise skipped college; a few students who otherwise would attended a four-year college do attend community college instead. These results imply that one additional student will enter college for each 6.51 jobs lost. Using per capita WARN Act job losses as an instrument for the county unemployment rate leads to a substantively similar result: a 1-percentage point increase in the local unemployment rate is associated with a 3.2-percentage point (4.5%) increase in the probability of attending college, again driven by a large (3.8 percentage points, 15.3%) increase in the probability of attending community college.

In any specification, the overall number of students in college increases slightly in response to an adverse labor market shock, even as there is a slight (usually not statistically significant) decrease in the probability of attending a four-year college. As some students are

[1] Additionally, students who attend college under shocks to family resources may be more likely to work increased hours while enrolled, which may diminish academic performance (Stinebrickner and Stinebrickner 2003).

[2] Not all events categorized as "plant closings" involve factories. A supermarket, a restaurant, a bank branch, or a car dealership could close; as long as an entire physical location is shut down, it is counted as a plant closing. The term is commonly used to avoid confusion with firm shutdowns (a term referring to the closure of an entire company, rather than a single location; a firm shutdown implies one or more plant closings but the reverse is not true) and I use it throughout this paper.

responding to labor market shocks by acquiring less education, which is opposite to their best interest in the labor market, credit constraints are likely to be a factor, but not enough to outweigh the opportunity cost aspect of the decision. I also examine the results for different subgroups, dividing students according to factors such as race, gender, eligibility for subsidized lunch, and predicted probability of attending a four-year college. The results are broadly consistent, particularly the effect on two-year attendance, but they are most pronounced among white students, male students, students in the middle (particularly the upper middle) of the distribution of predicted attendance, and students not eligible for free or reduced-price lunch.

II.2 Literature Review

The theoretical literature that provides context for this study centers around Lochner and Monge-Naranjo (2011). The authors model the college attendance decision, imposing a framework of credit constraints and determining how theoretical students will act. The constraints are similar to the designs of federal and private student loan programs; students must spend all of their federal loan money on education, subject to a fixed dollar maximum, and private lenders will not provide loans greater than the amount that they can expect to be able to garnish from the student's wages after graduation in the event of a default. These constraints are incorporated into a life cycle model and calibrated using observed parameter values. As expected, an increase in the cost of education along with an increase in the return to education, both of which have occurred in the United States in recent years, is associated with a much higher number of students being constrained out of being able to afford education. As stated in the previous section, this result is essentially a prerequisite for students to respond to a negative labor market shock by reducing their educational investment rather than increasing it; otherwise, the opportunity cost effects from Becker (1975) and Rosen (1977) predominate. Manoli and Turner (working) find evidence that these credit constraints are relaxed by tax refunds.

This study falls at the intersection of two empirical literatures. The first focuses on the effect of shocks to family resources on college attendance and other educational outcomes, providing context for students' reaction to a job loss or an income loss in their families. Hilger (2016) matches fathers to their children in U.S. tax data and studies the impact of any job loss (not just those that result from plant closings or firm shutdowns) on students' post-secondary education choices. The effects are negative, particularly among middle-class students who are not poor enough to receive full need-based aid but not wealthy enough to pay for their education out of pocket; however, the estimated effects are fairly small, regardless of the outcome of interest; students whose fathers are laid off are 0.43 percentage points less likely to attend college, and attend colleges whose alumni earn $84 less per year. Coelli (2011) uses Canadian data to match children to their families' main income earners and looks at the effects of those earners' job loss (either through mass layoff, firm shutdown, or plant closing) on the child's post-secondary choices. There are significant negative effects on college attendance, as a main income earner's job loss is associated with a 10-percentage point decrease in the probability of attending a postsecondary institution; there is no change in the probability of attending a community college, while almost all of the effect is on the probability of attending a university. However, there are no controls for students' academic ability, so any counterfactual predictions are based solely on demographics such as race, parents' education, and family income. Oreopoulos, Page, and Stevens (2008) match fathers to sons, looking at the effects of a father's job loss (due to plant closing or firm shutdown) while his son is in adolescence on the son's long-run outcomes. Sons do indeed lose about 9% of long-run income when their fathers lose their jobs at this stage, and this effect is even stronger among children in the least-wealthy families. Kalil and Wightman (2011) look in the Panel Study of Income Dynamics at the relationship between parental job losses and postsecondary enrollment, finding that Black students are three times as vulnerable to being induced to skip college than their white counterparts, even among middle-class students.

Lovenheim and Reynolds (2013) look at positive income shocks instead, focusing on house price appreciation. They use data from the 1997 National Longitudinal Survey of Youth (NLSY97), allowing them to capture the rapid increase in home prices that occurred in the late 1990s and early 2000s. Their key finding is that a $10,000 increase in home prices in the four years prior to high school graduation is associated with a 0.2% increase in a student's likelihood of attending his or her state flagship public university (such as the University of Michigan, University of Virginia, or University of Florida) as opposed to a non-flagship public university (such as Eastern Michigan University, Virginia Commonwealth University, or University of Central Florida) and a 0.6% decrease in the likelihood of attending a community college. There is no effect on the probability of attending a private university. Most of the variation is driven by students in the lowest income categories. Charles, Hurst, and Notowidigdo (working), however, find opposite results. An increase in housing demand in a metropolitan area, instrumented by a structural break in housing prices between 2000 and 2006, is associated with a decrease in college attendance in that metropolitan area as measured both by Census data and by the Integrated Postsecondary Education Data System (IPEDS).

The second focuses on the effect of changes in local labor market opportunities on educational investments and other related outcomes. Ananat et al. (2009) use county-level data on job losses in North Carolina between 1997 and 2007, focusing on the impact of job losses on 8$^{\text{th}}$ graders' test scores. Their identification strategy includes a weighted index of local job losses (again, measured through plant closings and mass layoffs) from the past several quarters, using an exponential decay function to determine the weights. After regressing 8$^{\text{th}}$ grade exam scores on this weighted index of job losses, student demographics, time trends, and county fixed effects, Ananat et al. find a small and insignificant negative effect of job losses for the full student population, but the effect grows to a statistically significant drop of 2.4 percentage points per percentage point of job loss when the sample is limited to students from low socioeconomic status families. The authors conclude that despite the county-wide measure of job losses, the effects do indeed result from family income shocks, rather than declines in school funding (which is largely determined by the state and responds more slowly); even though parents' job losses are not identified directly, they explain most of the phenomena.

Studies such as Black, McKinnish, and Sanders (2005) and Cascio and Narayan (2015) examine the effects of changes in employment in extractive industries on educational outcomes; the former paper focuses on the coal boom and bust, while the latter focuses on hydraulic fracturing for natural gas. Both find that as employment in extractive industries increases, male students are less likely to go to college, while female students' college decisions do not change; the opportunity cost of college increases as more high-paying jobs are available for low-skilled men in the coal mines and fracking rigs. Ost, Pan, and Webber (working) look at how mass layoffs affect working college students' time allocation and educational persistence; they find that few effects on enrollment but an increase in credits attempted due to a decline in the opportunity cost of coursework. Ge (2015) finds spillovers from mass layoffs associated with the reform of Chinese state-owned enterprises near the turn of the 21$^{\text{st}}$ century, as students in cities where more people lost their jobs in these events are even less likely to continue their education than those whose fathers worked in businesses with smaller layoffs.

What I aim to do in this study is to adapt the local-shock framework used by Ananat et al. (2009) to study similar outcomes to those examined by Hilger (2016). The results from Ananat et al. provide reassurance that there are legitimate effects of plant closings and mass layoffs on educational outcomes, and that the mechanism of these effects is through family income shocks rather than through schools' resources. Foote, Grosz, and Stevens (2015) support the use of WARN data as a proxy for unemployment data, for reasons that include the endogeneity of local unemployment with respect to labor supply and migration, the discrete nature of the shocks in a mass layoff, and the possible measurement error in unemployment rates collected for small geographic areas. With these results in mind, I can confidently use job losses within a commuting radius to proxy and/or instrument for directly-measured job losses, and take advantage of some of the advantages of Michigan's student administrative data to more completely capture students' postsecondary choice sets, with and without large local job losses. I describe these data more completely in Section III.3.

II.3 Data

II.3.1. Student Data

The state of Michigan collects detailed information on all its public school students, from elementary school through high school as well as any postsecondary school that they may attend. Each observation used in this analysis is a record from the Michigan Student Data System (MSDS), which observes students in Michigan public elementary and secondary schools three times per year from the 2002-03 academic year onward, collecting information ranging from attendance to race to disciplinary incidents. I obtain the latitude and longitude associated with each student's home ZIP code in the student's last year in the data. If the student's home ZIP code is unavailable, I use the ZIP code of the student's high school. The other variables that I gain from the MSDS are demographics such as race, gender, special education, and limited English proficiency.

For students who attend college, I use a common unique identifier to merge in variables from the National Student Clearinghouse (NSC), a nationwide database of attendance at colleges of all sorts, including community colleges, private colleges, for-profit institutions, and public universities. The vast majority of these institutions[3] nationwide report to the NSC, and the state of Michigan collects data from the NSC on all college students who ever attended public school in Michigan. The variables of interest from the NSC database are the college that a student attends and the year that he or she begins to attend. 76% of MSDS observations at this point merge into the NSC data; the remaining students do not attend any college during the sample period or attend a college that does not report to NSC. College attendance outcomes are measured as of three years after expected graduation, thus making the last year of the high school sample 2011-12.

The last state data set that I use is a database of students' scores on 8^{th} and 11^{th}-grade state standardized exams. Michigan's high school standardized testing regime changed significantly during the sample period, as the state replaced the high school MEAP (Michigan

[3] Michigan's NSC records account for colleges attended by over 90% of college students; most of the colleges still missing are small, for-profit, less-than-two-year institutions.

Educational Assessment Program) with a new assessment, known as the Michigan Merit Exam (MME), that incorporates the ACT college entrance exam. To make results easier to interpret, I normalize the scores within each grade, subject, and year such that they have a mean of zero and a standard deviation of 1. Thus, it is straightforward to compare students who scored the same number of standard deviations above or below the mean against each other, regardless of subject area, year, or test type.

I match the state data with a database of ACT scores and self-reported high-school grade-point averages provided by ACT, Inc. Even before the MME came into effect, making the ACT mandatory, it was the most popular college entrance exam in Michigan. This data set contains observations for all students who took the test in Michigan between 2003 and 2011. It contains the same unique identifier variable as the state data; students in the state data who do not merge into this data set are assumed not to have taken the ACT.

II.3.2. Unemployment and Plant Closings

The simplest measure of a local labor market shock is the unemployment rate. I use county-level unemployment rates from the Bureau of Labor Statistics, averaging them out over a school year, and matching the average to a student's home county and year of expected graduation. For instance, if a high school student in Grand Traverse County takes the 11^{th}-grade standardized test in 2006-07, that student is expected to graduate in the spring of 2008, and is assigned the average of all monthly unemployment rates in Grand Traverse County between September 2007 and August 2008, the period that would most likely be that student's senior year and the period in which the student would most likely decide whether and where to attend college. For middle-school students, I use the average of the 12 months before the 8^{th}-grade exam; if a middle school student in Kent County takes the 8^{th}-grade standardized test on February 11^{th}, 2007, that student is assigned the average of all monthly unemployment rates in Kent County between February 2006 and January 2007. However, there are well-documented concerns about measurement error and endogeneity in county unemployment rates (see Foote et al. 2015), and many workers in Michigan commute across county lines. To address some of these concerns, I instrument and proxy for county unemployment rates with data on plant closings and mass layoffs from the WARN Act.

The federal Worker Adjustment and Retraining Notification Act ("WARN Act") of 1988 requires all employers with 100 or more employees to provide 60 days' advance notice before laying off at least 50 workers at once or closing a facility (such as a store, a factory, or an office) with at least 50 employees. Companies must not only inform their employees of plant closings and mass layoffs, but they also must provide the same notice to the mayor of the city where the relevant facility is located and to the relevant state agency responsible for dislocated workers. In Michigan, these notices are reported to the Michigan Workforce Development Agency (WDA); the web sites of the WDA and of Michigan Labor Market Information (MILMI), a state repository of economic statistics, provide data from the notices to the public in an electronic format. All notices dating back to 2000 are available through these channels, with identifying information including the company name, the type of action (mass layoff or facility closing), the number of workers displaced, the date of the action, and the city in which the affected facility is located. Additional searching allowed me to find the addresses associated with a large majority of the sites where a layoff or closing took place, usually by locating a scanned version of the paper notice[4]. Using Google Maps' geocoding API through the `geocode3` package for Stata, I obtained precise coordinates associated with these addresses, which I use to determine the distance from each layoff site to each high school. 92% of WARN Act events between 2002 and 2011 were geocoded successfully.

Figure II.1 graphs the distribution of jobs lost per event. The left-most bar represents the number of plant closings or mass layoffs in which the total number of jobs lost was between 50 and 99; each additional bar has a similar interval of 50. Most of the events displace fewer than 200 workers, but some of them displace over 1,000 workers. Five of the ten largest plant closings in Michigan during the sample period involved the decommissioning of General Motors factories; the discontinuation of the Oldsmobile brand resulted in three factories in Lansing (Michigan's capital and Oldsmobile's longtime home) being closed down and laying off more than 900 workers each, while plant closings in Pontiac (an industrial suburb north of Detroit) and Grand Rapids (a mid-sized city on the west side of the state) displaced 1,230 and 1,500 workers, respectively.

Figure II.2 collapses the job losses listed in Figure II.1 to the year level. Not surprisingly, the level of job losses was high throughout the 2000s as auto manufacturers and suppliers condensed operations and moved production overseas, and it hit its peak in 2007-2009, as General Motors and Chrysler declared bankruptcy and Ford teetered on the edge. Soon thereafter, the frenzy of plant closures calmed, and fewer than 5,000 workers per year were laid off as a result of WARN Act eligible plant closings and mass layoffs in Michigan in 2010-2013.

Figure II.3 overlays job loss events on a map of the Lower Peninsula of Michigan[5], with

[4]The largest single event in the WARN Act data is the closing of the Farmer Jack supermarket chain in 2007, which is coded as a "statewide" event rather than as a series of local store closings. I use the paper notice to obtain the addresses of all the Farmer Jack locations in the state, remove any that were immediately sold to other supermarket chains such as Kroger, and assign equal fractions of the total statewide job losses to each of the remaining store locations. Results are robust to removing the Farmer Jack observations.

[5]There were some job loss events in the Upper Peninsula, but this area is left off the map for space

each dot representing a plant closing or mass layoff; darker markers correspond to more jobs lost. To a large extent, Figure II.3 mirrors the overall population distribution of the state. Much of the job loss activity is in southeast Michigan in the Detroit metropolitan area, but considering that over four million of the state's nearly 10 million residents live in the Detroit Metropolitan Statistical Area (MSA), this is hardly disproportionate. Large plant closings and mass layoffs certainly occurred in the state's secondary population centers; cities such as Lansing, Grand Rapids, Flint, Battle Creek, and Kalamazoo all saw major WARN events during the sample period. A few coastal communities along Lake Michigan and in the tourist-friendly northern region of the state saw some job losses as well; fewer jobs were lost in these regions, but these areas are populated quite sparsely. Overall, none of the evidence in Figure II.3 indicates that job losses are concentrated geographically in a fashion that is not proportional to population density.

II.3.3. College Analysis Sample

For the college analysis, I keep all students who take the 11$^{\text{th}}$-grade Michigan standardized test between 2001-02 and 2010-11, regardless of whether they graduate. I drop any students who are missing common demographic information (race, gender, subsidized lunch eligibility, limited English proficiency, or special education enrollment). This leaves me with approximately 950,000 students.

I now have a detailed picture of the academic trajectory and personal characteristics of each of the approximately 950,000 students in the data; what remains is to match them to the job losses in their surrounding neighborhoods. A student expected to graduate in the 2005-06 school year will be assigned the job losses from September 2005 through August 2006. I calculate the number of jobs lost within radii of 30 miles[6] of each ZIP code in each academic year, then divide it by the number of people living in tracts within that radius according to the 2000 Census.[7] Figure II.4 shows the plant closings that occurred in September 2005 through August 2006 within a 30-mile radius of an address in southern Oakland County.

When I use WARN as the independent variable of interest in an OLS regression, I standardize these per capita measures at the student level, so that the average student has a value of 0 and a value of 1 represents one standard deviation above the student-level average.

reasons. A map including the Upper Peninsula is available upon request. The Upper Peninsula contained approximately 3% of the population of Michigan as of the 2010 Census.

[6] According to the American Community Survey, the average commuting time in Michigan in 2009-2013 was 24 minutes. At a 70-mph highway speed (the speed limit on many freeways in Michigan), this corresponds to a 28-mile commute. For robustness, I also use radii of 10 and 50 miles; results, available upon request, are largely similar.

[7] Later population sizes would presumably be affected by changes in the labor market, so I use a population size that predates the sample period to avoid endogeneity.

This allows the empirical estimates to be interpreted as the effects of a 1-standard deviation increase in per capita job losses. A kernel density plot of the distribution of this variable is shown in Figure II.5. When WARN is used to instrument for a measure of unemployment, I leave it in per capita terms.

II.3.4. The Big Picture

Table II.1 presents a summary of high school student characteristics separated by their postsecondary education choices.[8] Most of the results are not surprising. Students who ever attend four-year colleges have higher scores on state standardized tests, higher ACT scores, and higher grade-point averages. These students are also less likely to be Black or Hispanic, less likely to be eligible for subsidized lunch, and less likely to be enrolled in special education. Two-year attendees and students who do not go to college are similar on some measures, but two-year attendees perform somewhat better on the state exam (if not on the ACT) and are less likely to be in special education or eligible for subsidized lunch.

Table II.2 divides high school students into quartiles by the per capita job losses in their surrounding areas in their expected graduation year. One might expect that areas where very few job losses occur have substantially different populations from areas that are harder-hit. The actual pattern is not as clear. Most notably, the average state standardized test scores in the hardest-hit and least hard-hit areas are separated by about 0.13 standard deviations. Students are somewhat different across quartiles in terms of characteristics such as race and subsidized lunch eligibility, but not in consistent monotonic patterns; furthermore, some regions of the state suffered more job losses in particular years. For instance, more than 4,000 jobs were lost due to plant closings and mass layoffs in Lansing in 2005, while fewer than 200 were lost in Lansing in 2008; meanwhile, more than 2,000 jobs were lost in similar events in Grand Rapids in 2008, while about 1,000 were lost in Grand Rapids in 2005. The least hard-hit quartile does look somewhat different from the rest of the state in several ways: it is much more concentrated in rural areas and small towns, which explains the disparities in race and income between this quartile and the other three quartiles, as Michigan's rural population tends to be whiter and poorer than the state as a whole. These areas were economically hard-hit, but since they have fewer businesses employing enough people to be required to file WARN Act notices, the job losses in these areas are less likely to enter into the data here. [9]

[8] All summary tables are calculated for the high school graduating class of 2006, for whom the ACT was not yet mandatory. Patterns of results, except for ACT results, are broadly similar for other cohorts.

[9] Because of this, I estimate my empirical models both including and excluding the least population-dense areas of the state; results are very similar.

II.4 Empirical Specifications and Results

II.4.1. Specifications

In order to gain a fuller understanding of the effects of local labor market shocks on student outcomes without worrying about issues related to costs, I begin by examining the impact of these shocks on 8$^{\text{th}}$ graders' standardized test scores. These students are affected by reduced family resources and increased uncertainty at home as well as possible reductions in school resources, just like high school students, but there is no direct monetary cost to earning a higher exam score, and the opportunity cost of studying does not generally change because the minimum age for almost all employment in Michigan is 14.[10] The general equation that I use is as follows:

$$TestScore_{8ijsct} = \psi + \delta J_{ct} + \chi \mathbf{X_i} + \kappa_{ct} + \mu_s + \tau_t + \nu_{ict} \tag{II.1}$$

The 8$^{\text{th}}$-grade standardized test score (rescaled to have mean zero and standard deviation 1) of student i in subject j, who attends school s and lives in location c in year t, depends on some measure of local job losses J_{ct} and a set of student characteristics $\boldsymbol{X_i}$, which includes indicators for whether the student is female, Black, Latino, Asian-American, eligible for subsidized school lunch, in a program for students with limited English proficiency, and in special education. κ_{ct} is a county-level linear time trend, μ_s is a school fixed effect, and τ_t is a graduation year fixed effect. The coefficient of interest here is δ, the estimated effect of an increase in job losses on the student's standardized test score.

The focus of this paper is college attendance, though, and the richness of the Michigan student data allows for a number of potential specifications that can shed light on the relationship between local job losses and students' college decisions. Perhaps the most drastic reaction to local economic shocks involves credit-constrained students choosing to forgo attending college entirely and enter the labor force; other students might avoid the poor labor market and attend college instead of seeking work immediately. I model this as follows:

$$AnyColl_{isct} = \alpha + \beta J_{ct} + \gamma TestScore_{11i} + \theta \mathbf{X_i} + \eta_{ct} + \omega_s + \phi_t + \epsilon_{ict} \tag{II.2}$$

The probability of student i, who attends school s and lives in location c, attending

[10] The minimum age to work as a youth sports referee or golf caddy is 11; the minimum age to participate in some farming occupations or set traps for some shooting events is 13 (Youth Employment Standards Act).

any college after graduating in year t depends on some measure of local job losses J_{ct}, the student's score on the state high school standardized test, and student characteristics $\boldsymbol{X_i}$. η_{ct} is a county-level linear time trend, ω_s is a school fixed effect, and ϕ_t is a graduation year fixed effect. The coefficient of interest in these specifications is β, the estimated effect of an increase in job losses on the probability of attending any college in the NSC data.

This framework can also be used to estimate effects more narrowly on attendance at four-year colleges or two-year colleges, by changing the outcome variable to take the value 1 if a student attends a four-year college and 0 otherwise (or a similar setup substituting two-year college attendance for four-year college attendance). In an unconstrained world, we would expect to see students responding to labor market shocks by demanding more education, resulting in higher attendance at four-year colleges; the effects on two-year college attendance would be ambiguous and depend on additional factors. The opposite would be true in a world where credit constraints are important; students would be pushed out of four-year college attendance by labor market shocks, and some of them may choose to attend community colleges, while some students who might have attended community college could choose not to pursue any postsecondary education at all. Examining attendance at two-year and four-year colleges separately is a useful way to pit the competing theories against each other and see which is more accurately reflected in the data.

Additionally, the effects of local unemployment on college attendance may vary across different groups of students. Poor students may be particularly vulnerable to unemployment shocks because their families have little to no money saved, even as they may also have the most to gain from a college education. Similarly, there are massive wealth gaps between the average white household and the average Black household, which could result in different responses from Black students and white students; in 2009, the average white household had a net worth of \$113,149, while the average Black household had a net worth of \$5,677 (Kochhar, Fry, and Taylor 2011). On the other hand, female students and male students may also react differently to changes in the local job market, as many of the most common jobs in Michigan that do not require a college education are dominated by male workers (such as work in automotive factories), and those jobs are also the most likely to be displaced during the state's economic struggles. As such, I include results estimated separately by race, gender, and subsidized lunch eligibility.

II.4.2. Comparing Measures

There are multiple ways to measure the strength of a student's local labor market. The most straightforward measure is probably the county unemployment rate, and I begin by presenting results from OLS linear probability regressions[11] of college outcomes on county unemployment rates (along with other observables). However, as outlined by Foote et al. (2015) and discussed in previous sections, there are reasons to be cautious about using unemployment rates as the sole measure of labor market strength, and the WARN Act data can help to alleviate some of these concerns. Worker separations may be endogenous; workers who become unemployed for reasons other than plant closings and mass layoffs may have been fired for poor performance, or they may have quit their jobs and found no employers willing to hire them. Either of these reasons may be correlated with their children's success at navigating the college application and financial aid system. Additionally, unemployment rates at the county level are measured with error, as they frequently extrapolate from surveys with small sample sizes, while WARN reporting is required by law and thus the WARN data should capture the full population of major job loss events.

Still, it is worth checking to see that these capture the same thing. A first-stage regression of the average county unemployment rate on the per capita WARN Act job loss measure, along with a set of year and county fixed effects, produces a positive and highly statistically significant relationship, with a coefficient of 0.513 and an F-statistic of 25.77. The coefficient means that 1.95% of the local population must lose their jobs in a given year in WARN-eligible events to induce a 1-percentage point increase in the unemployment rate. The median student is exposed to about 0.1% of the local population losing jobs in a year (with a standard deviation of 0.13%), so reporting these results involves reporting the impact of an event that is extraordinarily large. Put differently, the median ZIP code in the data has 470,793 people living within 30 miles of it; 9,180 of these people must lose their jobs in mass layoffs and plant closings to trigger a 1-point increase in the unemployment rate. This is, again, an extremely rare event, and the IV results should be interpreted with this in mind.

Depending on one's preferred interpretation, the WARN variable can be used either as a proxy or an instrument for the local unemployment rate. I report results both with the OLS framework (using WARN job losses as a proxy for unemployment) and the instrumental variables framework (using WARN job losses to instrument for unemployment). In cases where the WARN variable is used as a proxy, I put it into standard deviation terms for ease of interpretation; most WARN shocks involve less than 1% of the local population losing a job in a given year, so leaving it in percentage terms would be illustrating the impact of an event that is so large as to be exceedingly rare. In cases where the WARN variable is used as an instrument, I leave it in per capita terms, because unemployment rates are conventionally

[11]Preliminary results using probits instead of linear probability models had vastly similar results to the OLS models. As a result, OLS results are reported throughout the paper due to computing limitations stemming from the size of the data set and the number of fixed effects.

listed in percentages.

II.4.3. Results

Methodologically, the most similar paper to this one may be Ananat et al. (2009), which studies the effect of plant closings and mass layoffs on 8$^{\text{th}}$ grade exam scores. They find a small negative effect on exam scores in the full sample, which becomes larger when limited to a sample of students of low socio-economic status. As shown in Table II.3 and Table II.4, local job losses are associated with some test score decreases in the Michigan context as well. A 1-standard deviation increase in per capita job losses in the preceding 12 months is associated with a statistically significant 0.005-standard deviation decrease in 8$^{\text{th}}$ grade reading scores; effects on math scores are minimal. When the sample is limited to students eligible for free or reduced-price lunch, the effect on reading scores remains constant. Seeing that the results of a similar specification are in line with a well-known previous study leads to a more confident interpretation of the headline results in this addition to the literature.

In the college attendance results, I present each outcome in a separate table, with results using the county unemployment rate, the standardized WARN variable, and an instrumental variables framework with the latter instrumenting for the former, in separate columns from left to right. Standard errors are clustered by ZIP code throughout the empirical results. Table II.5 presents results for whether a student attends college at all. The variables driving overall college attendance most are the ones that one would expect; students with high standardized test scores are much more likely to go to college than their peers, while students in special education and students eligible for subsidized school lunch are less likely to attend college. The phenomenon of Black students attending college more than their white peers (conditional on observable characteristics) while Hispanic students attend less than their white peers is fascinating, but a proper investigation and explanation of it is outside the scope of this paper. The unemployment rate seems to have no significant impact on the probability of a student attending college, but in both OLS and instrumental variables frameworks, WARN Act job losses do have a positive association with college attendance. In the OLS WARN Act results, a 1-standard deviation increase in per capita job losses is associated with a 0.2-percentage point (0.3%) increase in the probability of attending college, which is significant at the 5% level. This translates to approximately 6.51 jobs lost per student induced to enroll in college[12]. The IV results are similar in sign and significance

[12] There were 9,938,444 people living in Michigan as of the 2000 Census. One standard deviation of job losses is 0.127% of the population, or 12,661 people. Collegegoing increased by 0.2 percentage points in a sample of 944,748, or 1,946 new college students. $12,661 \div 1,946 = 6.51$. Of course, this abstracts from the involvement of population density and which students are exposed to which job losses.

but have a larger magnitude; a 1-percentage point increase in the county unemployment rate is associated with a statistically significant 3.2-percentage point (4.5%) increase in the probability of attending college. This result by itself supports the Becker/Rosen opportunity cost story. High school graduates exposed to more severe labor market shocks are more likely to attend college than those exposed to smaller shocks or no shocks at all. Both effects might be at play, but with this indicator variable for any college attendance as the outcome, the reduced opportunity cost outweighs credit constraints in the aggregate.

However, not all colleges are the same, and there may be significant substitutions within the set of colleges that would support Lochner and Monge-Naranjo's credit constraint hypothesis. Credit-constrained students may move from pricier schools to cheaper colleges, perhaps including community colleges, while students focused on the opportunity cost of college may choose to continue their education beyond a two-year degree. In order to understand which of these explanations might characterize the results here, it is necessary to look at additional outcomes. Table II.6 presents similarly-organized results for four-year colleges. The effects are never statistically significant in any specification, even in the IV framework that otherwise produces large coefficients. Table II.7 finishes the exercise with two-year colleges, which appear to drive the results seen in Table II.5. The local unemployment rate remains statistically insignificant, but in the OLS WARN framework, a 1-standard deviation increase in per capita job losses is associated with a statistically significant 0.3-percentage point (1.2%) increase in the probability of attending a two-year college. A back-of-the-envelope calculation similar to the one above suggests that 5.44 jobs are lost per student induced to attend community college, according to this result. The instrumental variables result is large enough to stretch credibility: a 1-percentage point increase in the county unemployment rate is associated with a 3.8-percentage point (15.3%) increase in the probability of attending community college. These results imply that most of the new students who enter two-year colleges in the aftermath of a local unemployment shock are students who otherwise would not have attended college at all; there is little to no movement out of four-year schools into two-year schools. Due to the difficulty of interpreting the IV effect sizes, I will focus on the OLS proxy specification in future sections, and I will discuss potential biases in Section II.5.1.

II.4.4. Heterogeneity

Table II.8 presents the OLS results for various subgroups. Students who are eligible for subsidized lunch would be the least likely to be able to pay the sticker price for college on their own, due to their parents' low incomes; if charged the full price, they would need to borrow, and these students are also the most likely to be credit-constrained. However, these students are also frequently eligible for need-based grants and scholarships that could reduce their costs significantly. The results are broadly similar to those in the full sample. A 1-standard deviation increase in per capita job losses is associated with a 0.2-percentage point (0.3%) increase in the probability that a subsidized lunch eligible student attends college; this is the same magnitude as in the overall sample, although it is not statistically significant in the subsample due to the smaller sample size. Unlike in the full sample, this is largely driven by four-year attendance; a 1-standard deviation increase in per capita job losses is associated with a 0.2-percentage point (0.6%) increase in the probability of attending a four-year college, but a 0.02-percentage point (0.1%) increase in the probability of attending a two-year college. (Neither is statistically different from zero.) On the other hand, students who are not eligible for subsidized lunch have slightly different responses to local unemployment shocks. There is still a small (0.2 percentage points, or 0.3%, significant at the 10% level) increase in overall college attendance and a somewhat larger (0.5 percentage points, or 2.1%, significant at the 1% level) increase in community college attendance in response to a 1-standard deviation increase in per capita job losses, but there is a 0.3-percentage point (0.6%) decrease in the probability of attending a four-year college. Perhaps students eligible for subsidized lunch have a dimmer view of their labor market prospects without a college education, thus making their perceived opportunity cost of attending a four-year college lower.

The set of jobs commonly filled by young women with a high school diploma is somewhat different from the set of jobs commonly held by their male counterparts, and this may result in different reactions to labor market shocks by men and women. The overall effect on college attendance among women is positive but not statistically significant; the effect on attendance at four-year colleges is essentially zero. There is a 0.2-percentage point (0.8%) increase in the probability that a female graduate attends a two-year college associated with a 1-standard deviation increase in per capita job losses, which is slightly smaller than the effect in the full population, although it is significant at the 10% level. In contrast, all results are stronger for male graduates, including a 0.3-percentage point (0.4%) increase in the probability of attending any college and a 0.5-percentage point (2%) increase in the probability of attending community college. Given that many of the jobs that disappeared in these plant closings and mass layoffs were held by men with high school diplomas, it is not surprising that male students may be more sensitive to events in this particular sector of the labor market. The stronger negative coefficient in the specification examining four-year college attendance is more surprising, but the effect is still not significant at any conventional statistical level, so it is probably worth less concern. In other results (not shown, available by request), I find that none of the coefficients are significant for Black students while the results for white students mimic those of the overall sample, but this may be due more to differences in sample size than any differences between students or between their labor markets.

One may also expect that students are affected differently depending on their academic qualifications and other variables that may determine how likely they would be to attend college absent any economic shocks. Students with exceptional standardized test scores would likely be offered scholarships if their families could not pay the bill, and they may be highly motivated students who do not need the additional external incentive that comes from a shock to the opportunity cost of college. Meanwhile, students who struggled with their coursework and standardized tests may be less internally motivated and have areas of career interest that do not involve college education, and a shock to the opportunity cost of college may not be enough to induce them to attend. Academic preparation, of course, is not the only predictor of college attendance; students from wealthier families, for instance, are more likely to attend college and also are more likely to be able to weather a shock to family resources. Any effects are likely to be concentrated among students at the margin between attendance and non-attendance.

To measure this, I run a preliminary OLS regression of four-year college attendance on all the right-side variables except for job losses. From this, I obtain each student's predicted probability of attending a four-year college, which I split into quartiles. I then run Equation II.2 and its equivalents for two-year and four-year attendance separately for each quartile of predicted probability of attendance, using the standardized WARN variable as the measure of job losses. Table II.9 contains the results. As expected, the effects of job losses are concentrated among students in the middle of the distribution, who are closest to the margin of college attendance. In the middle two quartiles, a 1-standard deviation increase in per capita WARN Act job losses is associated with a 0.4-percentage point increase in the probability of college attendance, stemming from a 0.4 to 0.5-percentage point increase in the probability of attending a two-year college. Effects are close to zero and not statistically significant in the highest and lowest quartiles.

These results combine to show us who the marginal students who are induced into changing their college choices are, and these students are often the same ones whose labor-market opportunities changed most drastically as a result of job losses in their communities. A large percentage of the jobs lost in Michigan during this sample period, particularly in WARN Act eligible events, were manufacturing jobs at automotive companies or their suppliers. These jobs were often held by men without college degrees. The logical alternative to these blue-collar, male-dominated jobs may be a trade such as heating and cooling repair, welding, or construction management, which may require study at a community college. Accordingly, there are sizable increases in community-college attendance among boys exposed to more job losses. These students are rationally readjusting their human capital development in response to changes in their labor-market options.

II.4.5. Potential Mechanisms and Other Outcomes

Earlier sections of this paper address the most direct ways that local job losses might influence students' postsecondary education choices: shocks to family income and shocks to the individual-specific return to education. Measuring these requires well more information than we actually have about individual students; we can make inferences about which effect dominates but cannot isolate each effect. However, there are intermediate outcomes that could be affected by the plant closings, and these outcomes could also serve as mechanisms for the effect of job losses on college choice. Students cannot go on to higher education without finishing high school; they cannot go to four-year colleges without taking a college placement exam, which is usually the ACT in Michigan. Students' takeup of these intermediate steps could determine whether and how they advance to higher education. Additionally, students could be affected by changes in school funding; if they struggle during their senior year because of overfilled and underequipped classrooms or if their guidance counselors are forced to take on additional duties, then this may make the college application process even more daunting.

Table II.10 presents the results. A 1-standard deviation increase in per capita job losses is associated with a 0.3-percentage point increase in the probability that a student graduates, conditional on observables. If the local job market is deteriorating, this makes jobs especially few and far between for high school dropouts as more-credentialed candidates take the jobs that they once held. This gives students an incentive to finish high school, and this effect is indeed visible. Also, a 1-standard deviation increase in per capita job losses in 11$^{\text{th}}$ grade is associated with a 0.5-percentage point decrease in the probability that a student takes the ACT. This is almost certainly an underestimate of the true effect, considering that the ACT was mandatory for much of the later part of the sample period. Before the move to the mandatory ACT, taking one of the college placement exams was costly in terms of both money and time, and so if a student felt fairly sure that he or she would not be applying to four-year colleges, he or she would be more likely to skip the ACT. The move toward two-year attendance (and, in some subsamples, away from four-year attendance) is consistent with the decline in ACT taking, although we cannot observe whether the students knew ahead of time that they would not apply to four-year colleges and thus rationally chose not to take the ACT or whether they were unable to apply to four-year colleges, even if they wanted to, because they lacked an ACT score. However, the specifications focus on job losses in

12^{th} grade, which generally occur after the student has taken the ACT (or when it is too late for him/her to take it), and there is no relationship between 12^{th} grade job losses and ACT-taking when conditioning for observables. Similarly, there is a significant decline in per-pupil expenditure after a major job loss shock during the previous school year, but there is no simultaneous relationship.

Other results (available upon request) indicate that there does not seem to be anything happening differentially by tuition prices at local community colleges. Marginal students may be more able to opt into going to college when it is less expensive, and students who otherwise might have gone to four-year institutions can save more money by attending a community college instead if tuition at the community college is lower; both of these phenomena imply a stronger relationship between local job losses and college (particularly community college) attendance in areas where community college is cheaper. In reality, I find no evidence of such behavior. Splitting the data set by quartile of local community college tuition and estimating the equations shown in Tables II.5-II.7 produces no patterns of differential responses by tuition price. Perhaps marginal students view community college as inexpensive enough that they are not especially price-sensitive.

II.5 Discussion and Conclusion

II.5.1. Validity and Robustness

There are certainly some pieces of unobserved information that would provide additional clarity if I were able to incorporate them into the analysis. For instance, the National Student Clearinghouse data do not contain information on the financial aid that a student receives. While information about financial aid is certainly not always transmitted effectively to college applicants (see, for instance, Dynarski and Scott-Clayton 2007), it is not a stretch to say that some students will at least consider it as they make their college decisions. Generous need-based aid may explain why there is no decrease in four-year attendance among students eligible for subsidized lunch; even though these students may be the most credit constrained, they may be eligible for enough financial aid that tuition at four-year colleges is not much more expensive than tuition at two-year colleges.

There is also an unobserved heterogeneity among the students who are affected by local plant closings. Some of them have parents with stable incomes that are undisrupted by local economic fluctuations; these students see the closed factories out their car windows as they drive by, but there are no immediate effects on them or their families. For these students, the only result of the local economic shock is a change in their expected earnings if they enter the labor market. Their resources on hand to pay for college do not change. These students would react very differently from students whose families lost income, up to and including lost jobs, due to the local shock. The latter group of students now has fewer resources on hand to pay for college, and we expect credit constraints to be more likely to bind for them. I cannot distinguish between these types of students, and so any effects that I estimate are averaging across two very different groups. Perhaps the best available measure of a shock to a student's family income is whether he or she is newly eligible for free or reduced-price lunch; Table II.8 shows that these students are particularly likely to be induced into college attendance; a 1-standard deviation increase in per capita job losses is associated with a 0.9-percentage point (1.5%) increase in the probability of attending college, and a 0.8-percentage point (3%) increase in the probability of attending a two-year college. Credit constraints appear to bind surprisingly little for this population.

Another threat to validity is the nature of the WARN data. Firms that do not employ at least 100 workers are not required to file WARN notices; layoffs or closings that result in fewer than 50 job losses are also not required to be reported. Obviously, densely-populated areas will have higher concentrations of large firms, while more rural areas will have more workers working in firms that are too small to be eligible. To provisionally test the degree to which this matters, I use the Longitudinal Employer-Household Dynamics database to find the percentage of workers working in firms with fewer than 50 employees in each county, then run the main specifications again after removing counties with fewer than 30% of private-sector workers working in firms of sufficient size. This ends up removing five counties, whose observations make up about 0.5% of the data; coefficients on the job loss variable increase in size by about 3% after performing this sample restriction. The inclusion of these small counties biases the full-sample results toward zero.

Furthermore, there are a number of other elements of the college decision process that I do not observe in the data. If I observe a student from a hard-hit community who has extraordinarily high test scores and attends a community college, I implicitly assume that the student is attending community college instead of a four-year school as a response to the job losses. However, the mechanism through which this takes place is not always clear. The student could have been accepted at, say, Michigan State and chosen not to attend; she could have chosen not to even apply to Michigan State for fear that it was too expensive; she may have even been rejected from Michigan State despite her strong credentials. I do not observe the set of colleges to which a student is accepted; I have some information on the set of schools to which the student sends ACT scores, which can serve as a proxy for the set of schools to which the student applies, but the process in which the student sits down with her family at the dinner table and decides which schools she should apply to and which are out of reach is unobservable to researchers.

Additionally, not every student who starts his or her postsecondary career at a community college intends to enter the labor market with an associate's degree. Some community college students intend to transfer to four-year schools and earn a bachelor's degree; others do not intend to graduate at all, but only seek to take a few classes to build their human capital and train themselves for jobs in a particular field or industry. Spending the first two years at a less-expensive community college before earning a degree from a four-year school is a rational response to credit constraints that still enables students to get four years of higher education; if a student gets an associate's degree within two years and transfers to a four-year college immediately afterward, we treat that student as a four-year college student and not as a two-year college student. However, community college students' intentions are unobserved in this data set. Therefore, it is most straightforward to treat community college students as pursuing an associate's degree unless they are seen transferring, even though the reality is significantly more complex.[13]

While Michigan's economic experience in the 21st century has been more challenging than most other states', providing more variation in the data for the purposes of this study, there is little that is unique to Michigan that would make this study not replicable in other locations in the United States. The WARN Act requires all states to report large unemployment shocks due to plant closings or mass layoffs, so the job loss data should be available throughout the country. The state's student data system is generally quite good, complete with a unique ID that allows linking of records across state databases, but other states are catching up and placing more emphasis on tracking student data.

II.5.2. Future Research

While this paper as it currently exists may make a valuable contribution to the literature on the determinants of college attainment and college choice, there is certainly room for expansion upon its findings. For instance, since the data on job losses are calculated at the local level, I am careful to phrase the impact of these plant closings as stemming from local economic shocks, rather than from a direct unemployment shock to the student's family. Again, this is still valuable; students' responses to local economic shocks are worth observing. However, it cannot accurately estimate the effects of plant closings on their most immediate victims: the households that had a family member become unemployed. If a data source

[13] Additional results, available by request, show that there is no effect of local job losses on community college students' transferring to four-year colleges in the full sample. There is a positive and significant effect when the sample is limited to students eligible for free or reduced-price lunch.

allows for the merging of unemployment insurance filings onto student records (using address and perhaps last name as match variables), this would enable a much more precise estimation of the impact of family members' job losses on college outcomes, similar to what Coelli (2011) and Oreopoulos et al. (2008) accomplish with Canadian data or the work by Hilger (2015) using U.S. tax data. There would be flaws in this type of study as well, of course; not everyone who loses a job files for unemployment benefits, and it is certainly possible that there are systematic differences between filers and non-filers that could result in a biased estimate of the effects of unemployment. However, even considering these biases, a match using unemployment benefits data would allow a more definitive estimate of how students' college decisions respond to job losses in their own families.

II.5.3. Conclusion

As Michigan's automotive industry accelerated its decline in the 2000s, a number of factories throughout the state shut their doors for good, putting thousands of workers out of their jobs. The contagion from the industry's downturn spread through the state's economy, shuttering grocery stores, hospitals, shopping malls, and small businesses. In a perfect and frictionless world, high school graduates would likely respond to this by demanding more education; the opportunity cost of a year in college is low when one's job prospects are poor, and education will help a student get a better job if the job market remains in a downturn after he or she graduates. In reality, things may be more complex; if families are credit-constrained, they may be unable to help pay for their children's education after an income loss. This is particularly pronounced among households headed by high school graduates, underrepresented minorities, and lower-income families; their children may benefit greatly from higher education, but they are significantly credit constrained and have less experience navigating the financial aid system.

In my study of Michigan students' educational response to local mass unemployment events, I find evidence that credit constraints matter for the overall population. Students exposed to economic shocks are more likely to attend two-year colleges and less likely to attend four-year colleges. If credit constraints did not bind, there would be movement *toward* attending four-year colleges in response to local economic shocks; this may or may not come at the expense of two-year colleges. Interestingly, the result largely goes away when the population of study is a less-privileged subgroup of Michigan high school graduates; effects are less significant for Black students or for students eligible for subsidized school lunch. These students may be eligible for more types of financial aid, whereas the "next-poorest" students may come from families making too much money to receive much grant aid (or subsidized lunch) but not enough to pay for college without borrowing, particularly in the event of an economic shock. These students are the ones hurt most by credit constraints, as they are prevented from achieving the optimal level of education in the face of a downturn in the local labor market.[14]

If this trend persists, Michigan may see widening income inequality in years to come, as less-wealthy students are priced out of accessing the education that could help them improve their situation, despite having the academic credentials required for admission. Meanwhile, seats at the best universities will remain largely reserved for students from more-privileged backgrounds who can afford the tuition and who are not credit constrained. These temporary unemployment shocks may end up having damaging consequences for poorer students' careers and for the quality of Michigan's labor force in the years to come.

[14]From a glance at some of the more detailed paper WARN notices, the majority of jobs lost in WARN Act-eligible events in the sample period were in occupations that did not require a four-year degree. Students are not re-calculating their unconstrained optimum and, in response to local labor market contractions, choosing less education as their optimal decision.

II.6 Supplemental Figures

Figure II.1.: Distribution of Plant Closing and Mass Layoff Sizes

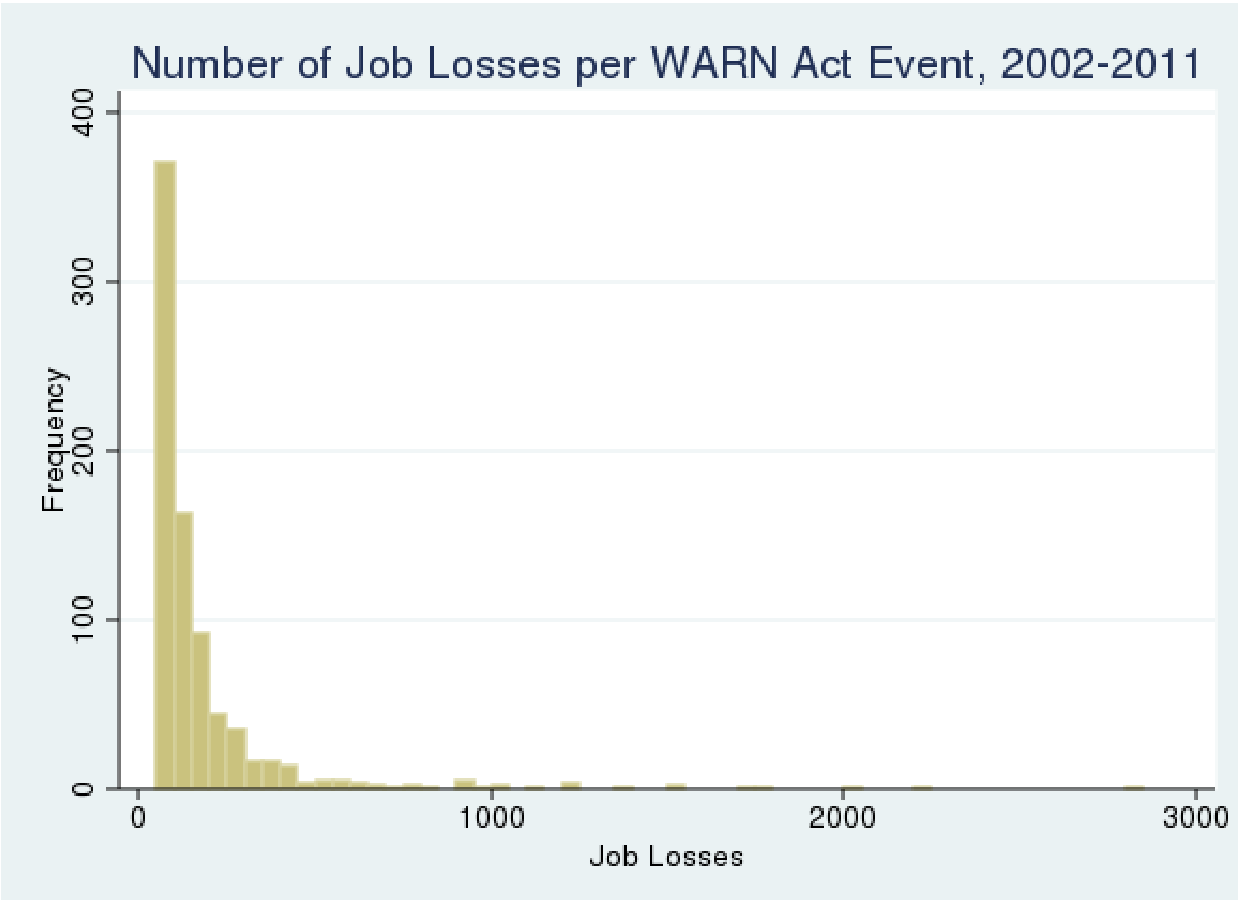


Figure II.2.: Annual Total Job Losses Due to Plant Closings and Mass Layoffs

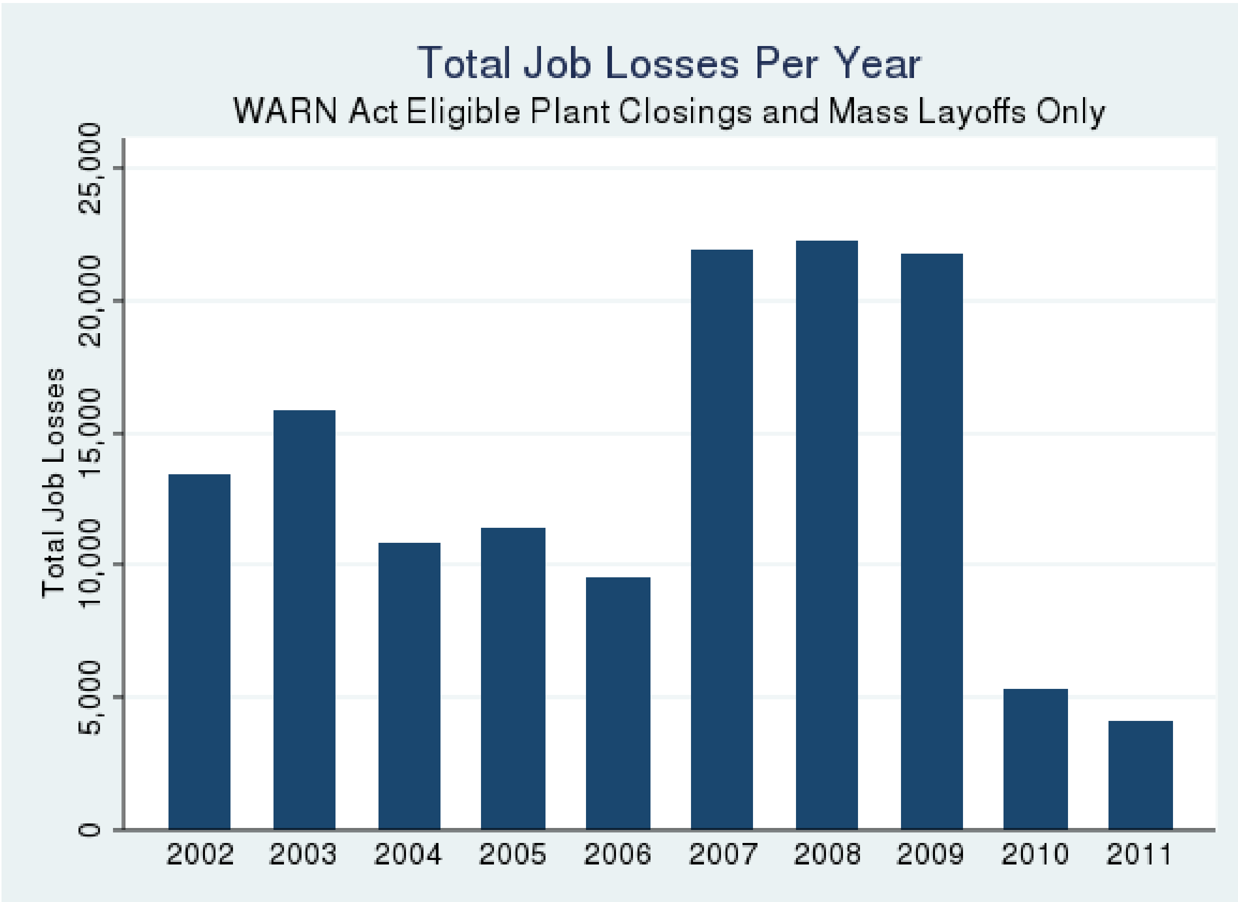
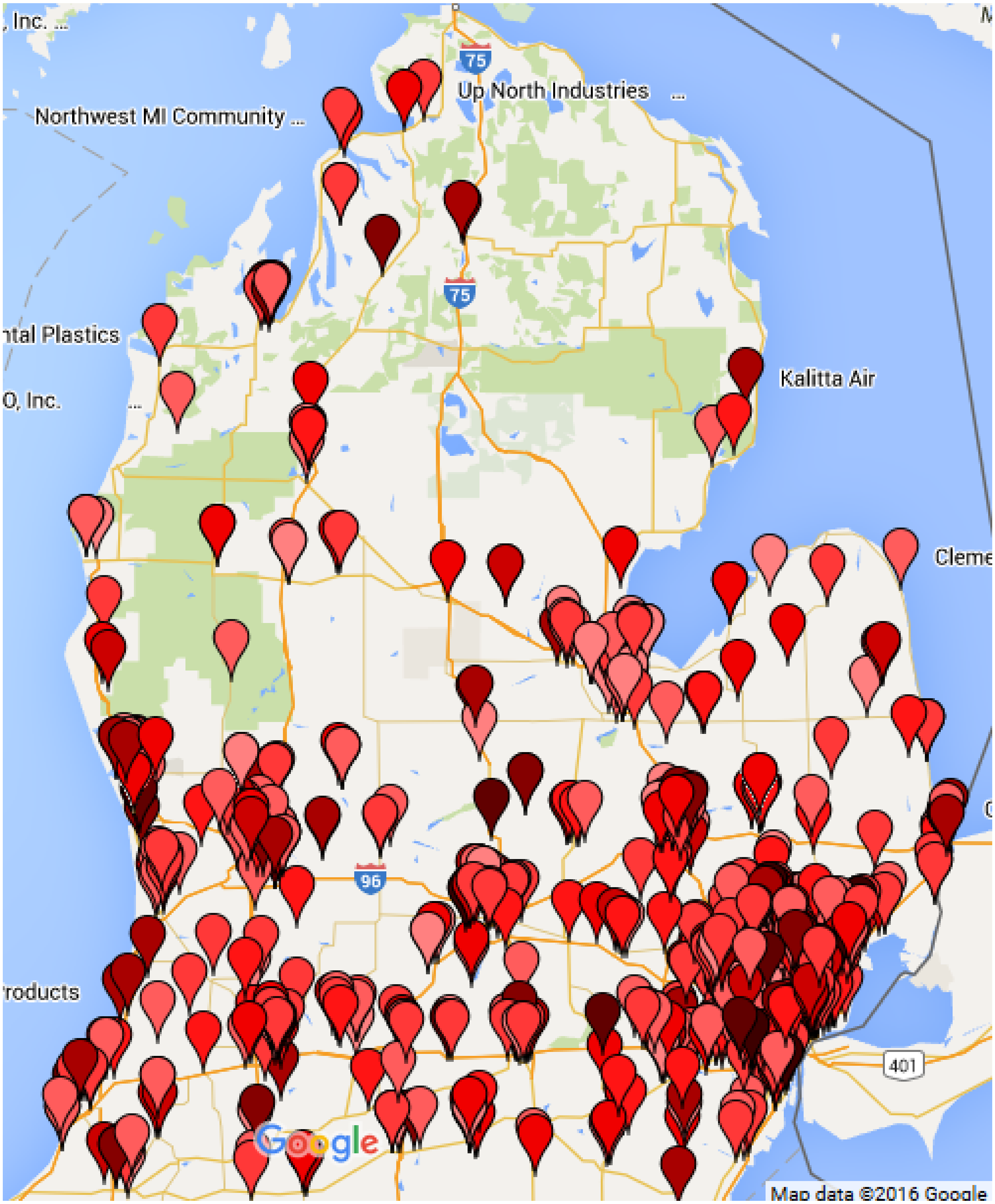
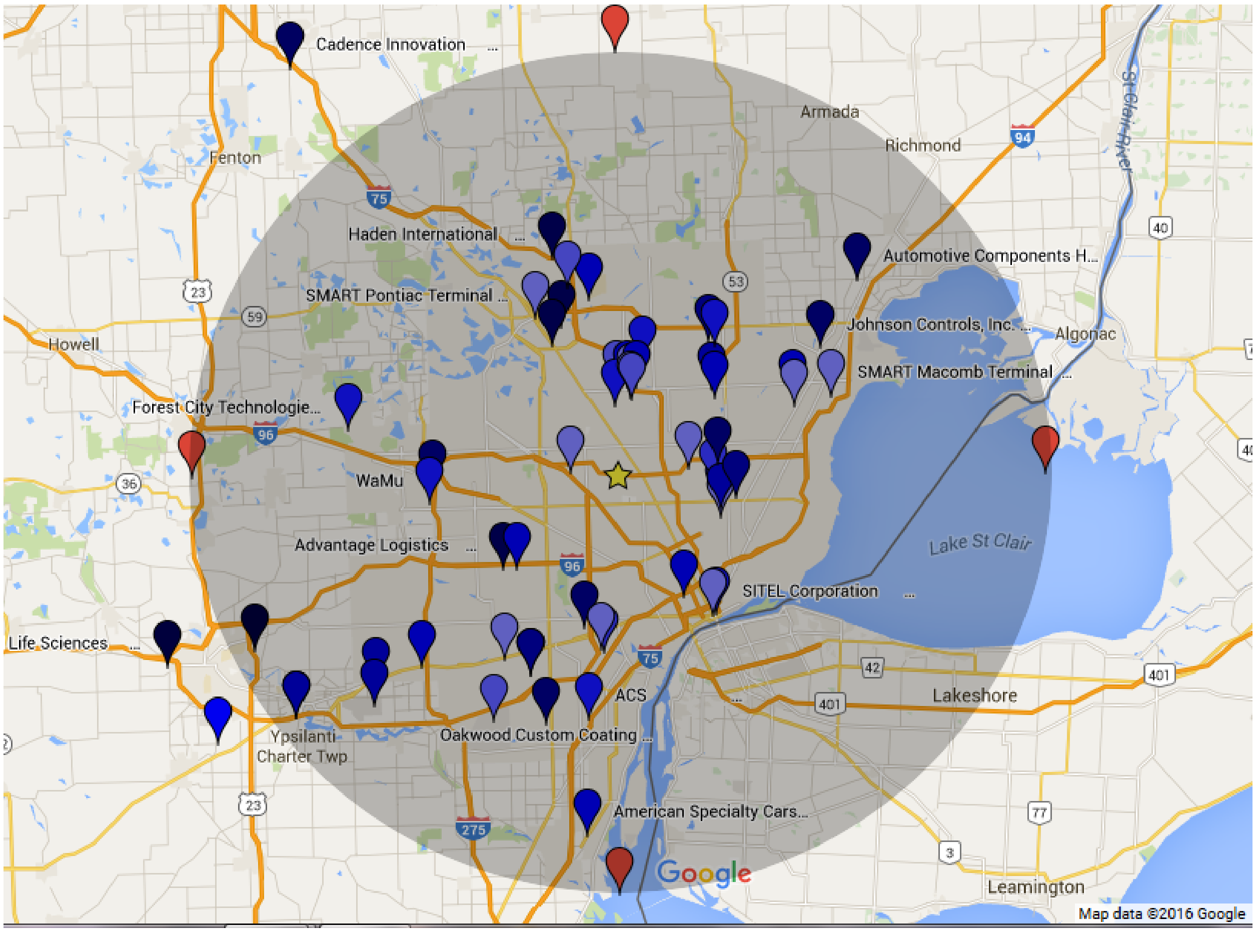


Figure II.3.: Geographic Distribution of Plant Closings and Mass Layoffs



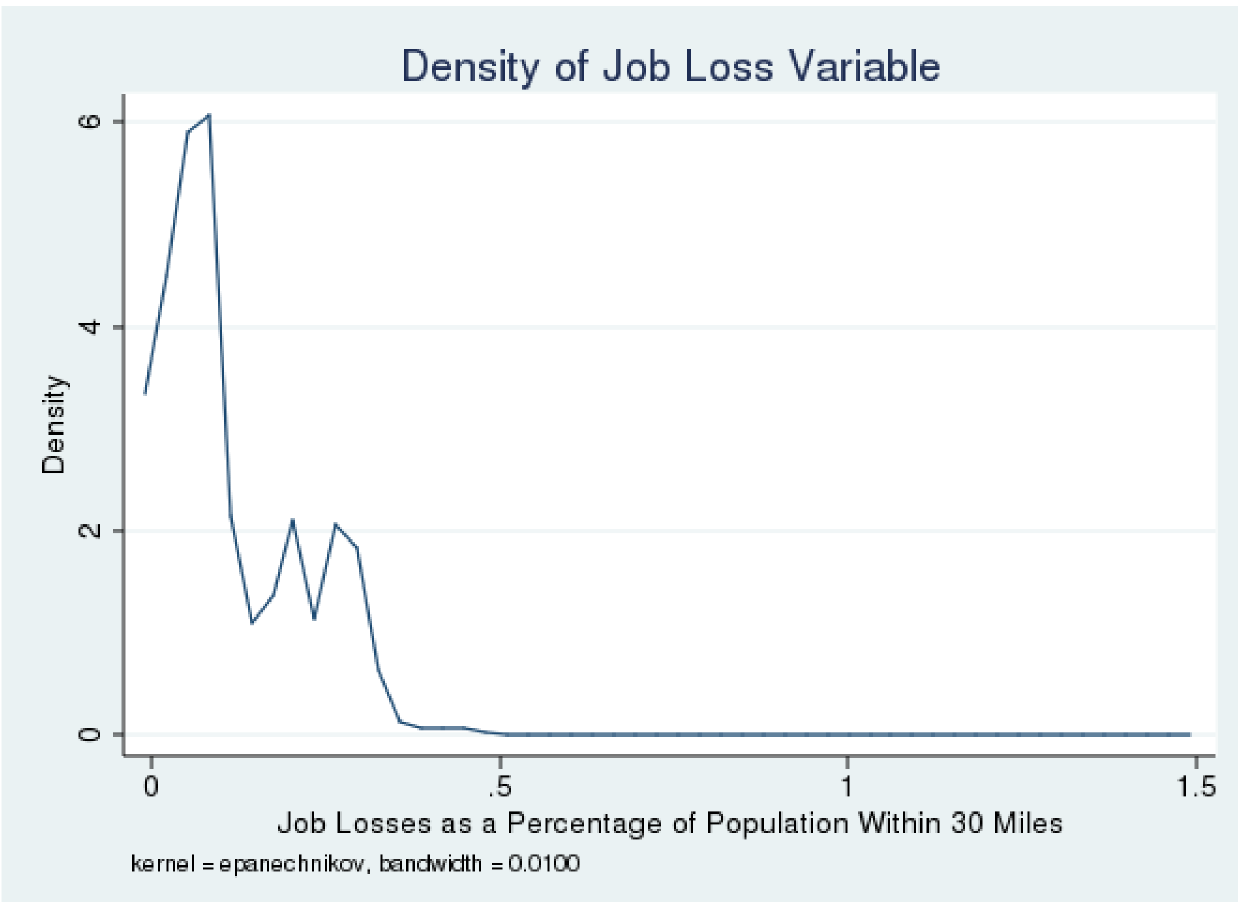
Each red marker is a plant closing or mass layoff. Darker red markers are associated with more jobs lost.

Figure II.4.: An Empirical Example: Plant Closings in Metropolitan Detroit in 2005-2006



Each blue marker is a plant closing or mass layoff that occurred between September 1, 2005, and August 31, 2006. Darker blue markers are associated with more jobs lost. Blue markers inside the circle are counted toward the "per capita job losses" measure of a student who lives at the star and graduates in spring of 2006. Red markers mark the edges of the circle; they do not correspond to plant closings or mass layoffs.

Figure II.5.: Distribution of Job Loss Variable



II.7 Supplemental Tables

Table II.1.: Summary Statistics by Postsecondary Choice

	No College	2-Year Only	Ever In 4-Year
Standardized State Test Score	-0.585	-0.25	0.461
	(1.02)	(0.877)	(0.804)
Percentage Taking ACT	26.4	47.5	87.2
ACT Composite Score	18.6	18.5	22.2
	(4.87)	(3.9)	(4.51)
High-School GPA	2.89	2.9	3.39
	(0.74)	(0.616)	(0.53)
Percentage Female	43.6	48.5	54.6
Percentage Black	17.9	18.2	13.9
Percentage Hispanic	5.15	3.22	1.9
Percentage Asian	1.66	1.35	2.83
Percentage Subsidized Lunch Eligible	37.5	27.7	16.7
Percentage Special Education	19.1	11.9	3.86
Percentage Limited English Proficiency	3.15	2.08	1.13
Percentage in Urban Schools	18.3	18.5	15.4
Percentage in Town/Rural Schools	40.4	35	32.9
Number of Students	30,215	25,543	52,009

Standard deviations in parentheses. Summary tables use only 2006 graduates.

Table II.2.: Summary Statistics by Population-Adjusted Job Loss Quartile

	Lowest	Second	Third	Highest
Standardized State Test Score	0.018	0.08	0.014	-0.114
	(0.955)	(0.998)	(1.01)	(1.03)
Percentage Taking ACT	57.7	61.3	65	58.7
ACT Composite Score	21	21.5	21.3	20.5
	(4.47)	(4.55)	(4.95)	(4.99)
High-School GPA	3.25	3.27	3.26	3.2
	(0.614)	(0.629)	(0.597)	(0.623)
Percentage Female	49.3	49.5	50.4	51.1
Percentage Black	8.11	8.23	22	26.1
Percentage Hispanic	2.82	4.5	2.36	2.85
Percentage Asian	1.11	1.83	2.84	2.83
Percentage Subsidized Lunch Eligible	27.5	21.9	20.2	31.1
Percentage Special Education	11	10.4	8.73	10.1
Percentage Limited English Proficiency	0.704	1.7	2.16	3.16
Percentage in Urban Schools	12.7	13.5	15.7	26.2
Percentage in Town/Rural Schools	66.2	49.5	12.7	13.3
Percentage Attending Any College	70	70.3	75.6	71.9
Percentage Attending 4-Year College	45.3	47.2	53.3	47.1
Number of Students	26,999	26,911	27,340	26,405

Standard deviations in parentheses. Job losses are taken within a 30-mile radius of the student's high school, divided by the population living in census tracts within that radius. Summary statistics use only 2006 graduates.

Table II.3.: 8th Grade Exam Scores, Full Sample

	Math	Reading
Standardized WARN, Last 12 Mos.	0.001	-0.005***
	(0.003)	(0.002)
7th Grade State Exam Score	0.628***	0.614***
	(0.006)	(0.002)
Female	-0.088***	0.1***
	(0.002)	(0.002)
Black	-0.267***	-0.114***
	(0.008)	(0.005)
Hispanic	-0.107***	-0.03***
	(0.009)	(0.007)
Asian	0.309***	0.124***
	(0.023)	(0.009)
Subsidized Lunch Eligible	-0.173***	-0.107***
	(0.004)	(0.003)
Limited English Proficiency	-0.234***	-0.249***
	(0.019)	(0.014)
Special Education	-0.486***	-0.354***
	(0.007)	(0.004)
Year Dummies?	Yes	Yes
School FE & County Trends?	Yes	Yes
Student Demographics?	Yes	Yes
R^2	0.483	0.546
Number of Observations	1,022,588	695,039

Standard errors in parentheses, clustered by school. Exam year dummies and school-specific time trends included. "Standardized WARN" variable is the per-capita number of jobs lost in the preceding year within a 30-mile radius of the student's home ZIP code in 8th grade, adjusted so that the mean is zero and the standard deviation is one. The reading exam was not offered in all years of the sample. * = significant at 0.10 level, ** = 0.05, *** = 0.01.

Table II.4.: 8^{th} Grade Exam Scores, Students Eligible for Subsidized Lunch

	Math	Reading
Standardized WARN, Last 12 Mos.	0.002	-0.005**
	(0.003)	(0.002)
7^{th} Grade State Exam Score	0.585***	0.599***
	(0.006)	(0.003)
Female	-0.071***	0.098***
	(0.002)	(0.002)
Black	-0.205***	-0.102***
	(0.006)	(0.006)
Hispanic	-0.063***	-0.012
	(0.009)	(0.008)
Asian	0.218***	0.129***
	(0.016)	(0.014)
Limited English Proficiency	-0.186***	-0.233***
	(0.02)	(0.018)
Special Education	-0.383***	-0.344***
	(0.006)	(0.005)
Year Dummies?	Yes	Yes
School FE & County Trends?	Yes	Yes
Student Demographics?	Yes	Yes
R^2	0.418	0.504
Number of Observations	386,093	284,695

Standard errors in parentheses, clustered by school. Exam year dummies and school-specific time trends included. "Standardized WARN" variable is the per-capita number of jobs lost in the preceding year within a 30-mile radius of the student's home ZIP code in 8^{th} grade, adjusted so that the mean is zero and the standard deviation is one. The reading exam was not offered in all years of the sample. * = significant at 0.10 level, ** = 0.05, *** = 0.01.

Table II.5.: Effects of Labor Market Shocks on Any College Attendance: Full Sample

	Unemployment Rate	Standardized WARN	IV - WARN for Unemployment
Labor Market Shock, Grade 12	0.003	0.002**	0.032**
	(0.002)	(0.001)	(0.014)
High School State Exam Score	0.138***	0.14***	0.137***
	(0.002)	(0.002)	(0.001)
Female	0.054***	0.054***	0.053***
	(0.002)	(0.002)	(0.001)
Black	0.104***	0.101***	0.103***
	(0.005)	(0.005)	(0.003)
Hispanic	-0.045***	-0.049***	-0.045***
	(0.005)	(0.005)	(0.003)
Asian	0.007	0.008	0.007*
	(0.008)	(0.008)	(0.004)
Subsidized Lunch Eligible	-0.074***	-0.076***	-0.074***
	(0.003)	(0.003)	(0.001)
Limited English Proficiency	0.014	0.009	0.013***
	(0.009)	(0.009)	(0.005)
Special Education	-0.084***	-0.083***	-0.085***
	(0.002)	(0.002)	(0.002)
First Stage			0.513***
			(10.1)
First-Stage F Statistic			25.77
R^2	0.189	0.191	0.188
Number of Observations	945,759	944,748	944,748
Outcome Mean	0.713	0.713	0.713

"Labor market shock" corresponds to the variable at the top of each respective column. Year dummies, school fixed effects, and county time trends included. Unemployment rates are measured at the county level. "Standardized WARN" variable is the per-capita number of jobs lost within a 30-mile radius of the school that the student attends in 11th grade, adjusted so that the mean is zero and the standard deviation is one. Per capita WARN job losses used as an instrument for the county unemployment rate during the 12-month period starting in September of the year after a student starts 11th grade for the first time. Standard errors in parentheses, clustered by ZIP code. * = significant at 0.10 level, ** = 0.05, *** = 0.01.

Table II.6.: Effects of Labor Market Shocks on Four-Year College Attendance: Full Sample

	Unemployment Rate	Standardized WARN	IV - WARN for Unemployment
Labor Market Shock, Grade 12	0.001	-0.001	-0.006
	(0.002)	(0.001)	(0.015)
High School State Exam Score	0.202***	0.203***	0.202***
	(0.002)	(0.002)	(0.001)
Female	0.057***	0.053***	0.057***
	(0.001)	(0.001)	(0.001)
Black	0.098***	0.095***	0.098***
	(0.005)	(0.005)	(0.003)
Hispanic	-0.037***	-0.035***	-0.038***
	(0.004)	(0.004)	(0.003)
Asian	0.047***	0.051***	0.047***
	(0.007)	(0.007)	(0.004)
Subsidized Lunch Eligible	-0.076***	-0.074***	-0.076***
	(0.002)	(0.002)	(0.001)
Limited English Proficiency	0.016**	0.013**	0.016***
	(0.007)	(0.007)	(0.004)
Special Education	-0.044***	-0.037***	-0.044***
	(0.003)	(0.003)	(0.002)
First Stage			0.513***
			(10.1)
First-Stage F Statistic			25.77
R^2	0.249	0.25	0.249
Number of Observations	945,759	944,748	944,748
Outcome Mean	0.464	0.464	0.464

"Labor market shock" corresponds to the variable at the top of each respective column. Year dummies, school fixed effects, and county time trends included. Unemployment rates are measured at the county level. "Standardized WARN" variable is the per-capita number of jobs lost within a 30-mile radius of the school that the student attends in 11th grade, adjusted so that the mean is zero and the standard deviation is one. Per capita WARN job losses used as an instrument for the county unemployment rate during the 12-month period starting in September of the year after a student starts 11th grade for the first time. Standard errors in parentheses, clustered by ZIP code. * = significant at 0.10 level, ** = 0.05, *** = 0.01.

Table II.7.: Effects of Labor Market Shocks on Two-Year College Attendance: Full Sample

	Unemployment Rate	Standardized WARN	IV - WARN for Unemployment
Labor Market Shock, Grade 12	0.001	0.003***	0.038***
	(0.001)	(0.001)	(0.014)
High School State Exam Score	-0.064***	-0.063***	-0.064***
	(0.002)	(0.002)	(0.001)
Female	-0.003**	0.001	-0.003***
	(0.002)	(0.002)	(0.001)
Black	0.006*	0.007*	0.005**
	(0.003)	(0.004)	(0.002)
Hispanic	-0.008**	-0.014***	-0.007**
	(0.004)	(0.004)	(0.003)
Asian	-0.04***	-0.042***	-0.04***
	(0.004)	(0.004)	(0.003)
Subsidized Lunch Eligible	0.002	-0.002	0.002*
	(0.002)	(0.002)	(0.001)
Limited English Proficiency	-0.002	-0.004	-0.002
	(0.009)	(0.009)	(0.005)
Special Education	-0.04***	-0.046***	-0.041***
	(0.003)	(0.003)	(0.002)
First Stage			0.513***
			(10.1)
First-Stage F Statistic			25.77
R^2	0.053	0.051	0.051
Number of Observations	945,759	944,748	944,748
Outcome Mean	0.249	0.249	0.249

"Labor market shock" corresponds to the variable at the top of each respective column. Year dummies, school fixed effects, and county time trends included. Unemployment rates are measured at the county level. "Standardized WARN" variable is the per-capita number of jobs lost within a 30-mile radius of the school that the student attends in 11th grade, adjusted so that the mean is zero and the standard deviation is one. Per capita WARN job losses used as an instrument for the county unemployment rate during the 12-month period starting in September of the year after a student starts 11th grade for the first time. Standard errors in parentheses, clustered by ZIP code. * = significant at 0.10 level, ** = 0.05, *** = 0.01.

Table II.8.: OLS Linear Probability Model Results, WARN: Subgroups

	Any College Attendance	4-Year Attendance	2-Year Attendance
Students Eligible for Subsidized Lunch	0.002	0.002	0.0002
	(0.002)	(0.002)	(0.002)
	[0.596]	[0.319]	[0.276]
Students Not Eligible	0.002*	-0.003**	0.005***
	(0.001)	(0.001)	(0.001)
	[0.764]	[0.526]	[0.238]
Students Newly Eligible	0.009**	0.001	0.008**
	(0.003)	(0.004)	(0.003)
	[0.616]	[0.347]	[0.269]
Female Students	0.002	-0.001	0.002*
	(0.001)	(0.002)	(0.001)
	[0.753]	[0.506]	[0.247]
Male Students	0.003**	-0.002	0.005***
	(0.001)	(0.001)	(0.001)
	[0.673]	[0.422]	[0.252]
Year Dummies?	Yes	Yes	Yes
School FE & County Trends?	Yes	Yes	Yes
Student Demographics?	Yes	Yes	Yes

Coefficients on "Standardized WARN" variable shown. "Standardized WARN" variable is the per-capita number of jobs lost within a 30-mile radius of the school that the student attends in 11th grade, adjusted so that the mean is zero and the standard deviation is one. Standard errors in parentheses, clustered by ZIP code; subgroup means in brackets. * = significant at 0.10 level, ** = 0.05, *** = 0.01.

Table II.9.: Effects by Predicted Probability of Four-Year Attendance

	Any College Attendance	4-Year Attendance	2-Year Attendance
Lowest Quartile	0.003	0.001	0.003
	(0.002)	(0.002)	(0.002)
	[0.467]	[0.166]	[0.301]
Second Quartile	0.004*	-0.0005	0.004**
	(0.002)	(0.002)	(0.002)
	[0.696]	[0.372]	[0.324]
Third Quartile	0.004***	-0.001	0.005***
	(0.001)	(0.002)	(0.002)
	[0.83]	[0.593]	[0.237]
Highest Quartile	-0.001	-0.001	0.001
	(0.001)	(0.001)	(0.001)
	[0.909]	[0.805]	[0.104]
Year Dummies?	Yes	Yes	Yes
School FE & County Trends?	Yes	Yes	Yes
Student Demographics?	Yes	Yes	Yes

Coefficients on "Standardized WARN" variable shown. "Standardized WARN" variable is the per-capita number of jobs lost within a 30-mile radius of the school that the student attends in 11th grade, adjusted so that the mean is zero and the standard deviation is one. Standard errors in parentheses, clustered by ZIP code; subgroup means in brackets. * = significant at 0.10 level, ** = 0.05, *** = 0.01. Students are assigned to a "predicted" group based a regression of an indicator for attending a 4-year college on all covariates except for job losses; I take predicted values from this and break them into quartiles.

Table II.10.: Possible Mechanisms for College Effects

	Graduation	ACT Taking	Gr. 12 Per Pupil Expenditure
Prior-Year Standardized WARN	0.003*	-0.005**	-0.005***
	(0.002)	(0.002)	(0.002)
High School State Exam Score	0.057***	0.103***	-0.004***
	(0.001)	(0.001)	(0.001)
Female	0.025***	0.027***	0
	(0.001)	(0.001)	(0.001)
Black	0.005*	0.05***	0.021***
	(0.002)	(0.003)	(0.004)
Hispanic	-0.021***	-0.007**	0.007***
	(0.003)	(0.003)	(0.002)
Asian	-0.02***	0.014***	0.006**
	(0.003)	(0.005)	(0.003)
Subsidized Lunch Eligible	-0.039***	-0.032***	0.003**
	(0.002)	(0.001)	(0.001)
Limited English Proficiency	0.005	-0.019***	0.004
	(0.004)	(0.005)	(0.003)
Special Education	0.004	-0.054***	0
	(0.003)	(0.003)	(0.004)
Year Dummies?	Yes	Yes	Yes
School FE & County Trends?	Yes	Yes	Yes
R^2	0.139	0.512	0.913
Number of Observations	944,748	944,748	942,788

Standard errors in parentheses, clustered by ZIP code. School and graduation year fixed effects and county-specific time trends included. "Standardized WARN" variable is the per-capita number of jobs lost in the preceding year within a 30-mile radius of the student's home ZIP code, adjusted so that the mean is zero and the standard deviation is one; for ACT taking and per-pupil expenditure, this is measured in 11^{th} grade. * = significant at 0.10 level, ** = 0.05, *** = 0.01.

Bibliography

[1] Ananat, E., Gassman-Pines, A., & Gibson-Davis, C. (2009). *The Effect of Plant Closings on Children's Educational Achievement.* Brookings Institute.

[2] Becker, G. (1975). *Human Capital: A Theoretical and Empirical Analysis, with Special Reference to Education*, 2nd ed. University of Chicago Press, Chicago.

[3] Berhard, S. (2013). *GEOCODE3: Stata Module to Retrieve Coordinates or Addresses from Google Geocoding API, Version 3.* Statistical Software Components S457650, Boston College Department of Economics. `https://ideas.repec.org/c/boc/bocode/s457650.html`. Revised July 25, 2013.

[4] Black, D., McKinnish, T., & Sanders, S. (2005). Tight Labor Markets and the Demand for Education: Evidence from the Coal Boom and Bust. *Industrial and Labor Relations Review*, 3-16.

[5] Cascio, E., & Narayan, A. (2015). *Who Needs a Fracking Education? The Educational Response to Low-Skill Biased Technological Change.* Working paper.

[6] Charles, K., Hurst, E., & Notowidgdo, M. (2015). *Housing Booms and Busts, Labor Market Opportunities, and College Attendance.* Working paper.

[7] Coelli, M. (2011). Parental Job Loss and the Education Enrollment of Youth. *Labour Economics*, 25-35.

[8] Dynarski, S., & Scott-Clayton, J. (2007). *College Grants on a Postcard: A Proposal for Simple and Predictable Federal Student Aid.* The Hamilton Project.

[9] Foote, A., Grosz, M., & Stevens, A.H. (2015). *Locate Your Nearest Exit: Mass Layoffs and Local Labor Market Response.* Working paper.

[10] Ge, R. (2015) *Economic Restructuring and Children's Educational Attainment: Lessons from China's State-owned Enterprises Reform in the Mid-1990s.* Working paper.

[11] Hilger, N. (2016). Parental Credit Constraints and College Attendance: Evidence from Timing of Fathers' Layoffs. *American Economic Journal: Applied*, 247-283.

[12] Hirsch, B.T., & Macpherson, D.A. (2002). *Union Membership, Coverage, Density and Employment by State, 2000.* Retrieved June 11, 2014, from Georgia State University Union Stats: `http://unionstats.gsu.edu/State%20U_2000.htm`.

[13] Hyman, J. *ACT For All: The Effect of Mandatory College Entrance Exams on Postsecondary Attainment and Choice*. Working paper.

[14] Kalil, A. & Wightman, P. Parental Job Loss and Children's Educational Attainment in Black and White Middle-Class Families. *Social Science Quarterly*, 57-78.

[15] Kochhar, R., Fry, R., & Taylor, P. (2011). *Wealth Gaps Rise to Record Highs Between Whites, Blacks, and Hispanics*. Pew Research Center Social & Demographic Trends.

[16] Lochner, L., & Monge-Naranjo, A. (2011). The Nature of Credit Constraints and Human Capital. *American Economic Review*, 2487-2529.

[17] Lovenheim, M., & Reynolds, C. (2013). The Effect of Housing Wealth on College Choice: Evidence from the Housing Boom. *Journal of Human Resources*, 1-35.

[18] Manoli, D., & Turner, N. (2014). *Cash-On-Hand and College Enrollment: Evidence from Population Tax Data and Policy Nonlinearities*. Working paper.

[19] Marcus, J. (2013, December 19). Canada Aims For US Student Growth. *Times Higher Education*.

[20] Oreopoulos, P., Page, M., & Stevens, A.H. (2008). The Intergenerational Effects of Worker Displacement. *Journal of Labor Economics*, 455-483.

[21] Oreopoulos, P., & Petronijevic, U. *Making College Worth It: A Review of Research on the Returns to Higher Education.* Working paper.

[22] Ost, B., Pan, W., & Webber, D. *The Impact of Mass Layoffs on the Educational Investments of Working College Students.* Working paper.

[23] Picard, R. (2010). *GEODIST: Stata Module to Compute Geodetic Distances*. Statisitcal Software Components S457147, Boston College Department of Economics. `https://ideas.repec.org/c/boc/bocode/s457147.html`. Revised Feb. 22, 2012.

[24] Rosen, S. (1977). Human Capital: A Survey of Empirical Research. *Research in Labor Economics* 1, 3-39.

[25] Stinebrickner, R., & Stinebrickner, T. (2003). Working During School and Academic Performance. *Journal of Labor Economics*, 473-491.

[26] Youth Employment Standards Act, Michigan Compiled Laws §§409-101-124 (1978).

III Teacher Turnover in Michigan Public Schools: A Descriptive Analysis

III.1 Introduction

Regardless of the quality of the other inputs in a student's education, teacher quality remains one of the most crucial components in determining how much a student learns in school. If teacher quality were distributed randomly throughout the education system, differences in teacher effectiveness would be more likely to even out over the course of students' careers, making it less of a concern in policymakers' efforts to ensure high-quality education for all. In reality, though, teacher quality is hardly distributed evenly across schools. Low-income students, Black students, Hispanic students, students in urban schools, and students with poor academic preparation all have their socioeconomic disadvantages compounded by having lower-quality teachers than their white and wealthier peers have (Goldhaber, Lavery, and Theobald 2015; Clotfelter, Ladd, and Vigdor 2007; Lankford, Loeb, and Wyckoff 2002). This stands to widen the achievement gaps by race and family income, in spite of policymakers' best efforts toward reducing them.

We argue that teacher turnover is at the heart of this story. If teachers have limited ability to choose their first jobs, they may not stay at their first teaching positions but continue to search for a job that they prefer until they find a better match. Sometimes this better match is a job outside the public school system altogether. Given that novice teachers perform notably worse than teachers with several years of experience (Murnane and Steele 2007, Clotfelter et al. 2007), if certain schools are systematically losing experienced teachers, students at these schools will see their academic performance drop accordingly. Furthermore, if teachers are consistently leaving schools with more disadvantaged students, this would widen the achievement gap even further.

A thorough study of teacher turnover would also be instructive in explaining the behavior of teacher labor markets; hiring may be drastically different at various points in the business cycle. On the labor supply side, during economic booms, there are more jobs available in the private sector, and thus teachers' outside options will generally be higher; candidates who still choose to teach will be either very committed to teaching or "stuck" teaching because their outside options are particularly bad. During recessions, the outside options will be fewer and less lucrative, and more workers will pursue teaching positions. Even if college students must commit to studying education before observing what economic conditions will be upon graduation, graduates may pursue jobs in the private sector or in other localities in response to economic fluctuations. On the demand side, during recessions, there will more likely be more candidates per job as fewer districts will be hiring, and therefore hiring schools can be more selective and choose only the best prospective teachers (if they can identify quality or characteristics associated with it); during booms, schools may not be able to be so picky and thus might have to hire whatever candidates they can get. If teachers hired during recessions are more likely to stay in their initial jobs even after the recessions end, this indicates that schools successfully identify which teachers are the best fits for the open positions; if teachers hired during booms are more likely to stay, then they are sensitive to the diminishing value of their outside options as economic conditions worsen.

We use a database of teachers in public and charter K-12 schools in Michigan who start teaching between 2003-04 and 2011-12 to answer four main questions about the patterns related to teacher turnover. First: what factors predict the probability that teachers leave their first jobs within five years of being hired? Second: what factors predict teachers' choice of a second job, including jobs outside of the Michigan public education system, new teaching positions in the same school district, and moving across school districts? Third: how does teacher mobility affect the distribution of teachers across schools, particularly across socioeconomic levels? Fourth: how do teacher labor markets respond to events in the business cycle?

We then turn to administrative data from the same schools to look at what happens in the wake of teacher turnover. All of these concerns about which schools lose teachers would be overblown if it turns out that these schools are able to replace departed teachers with instructors of similar quality, resulting in no drop-off in student performance. Matching annual teacher mobility data with students' test scores and demographics, we answer three additional questions about the aftermath of teacher mobility. One: how does student performance change after teachers leave? Two: how does this effect differ based on where teachers go after they leave? Finally, three: do the responses differ based on students' backgrounds, paying particular attention to the repercussions of Black students losing Black teachers?

While teacher turnover is a well-worn subject, and we owe much methodologically and organizationally to studies such as Boyd et al. (2005), Hanushek and Rivkin (2007), and Hanushek, Rivkin, and Schiman (2016), we believe we have valuable data to add. Michigan in the late 2000s is a unique setting compared with the literature that largely focuses on North Carolina, New York, and Texas a few years earlier. With nine cohorts of teachers, centered around the late-2000s recession, we observe a full business cycle and how teacher hiring and mobility may change for teachers hired before, during, and after the economic downturn (which was especially damaging in Michigan). Furthermore, Michigan has a much more expansive and *laissez-faire* charter school sector than most states, in which teachers can be dismissed more easily and have less-generous salaries and benefits. Methodologically, most of the work to date on the consequences of teacher turnover assumes a linear response, which may not be sufficient; schools may be able to handle going from losing 0% of their teachers to losing 5%, whereas going from losing 15% to losing 20% may be quite damaging as fewer and fewer people have institutional knowledge and more new teachers must be trained. We use a fractional polynomial framework in addition to an OLS framework to capture the full shape of the aftermath of teacher turnover.

Many of our findings will come as no surprise to readers who are well-acquainted with the existing literature on teacher turnover. As in studies such as Goldhaber et al. (2011), Hanushek and Rivkin (2007), and Loeb, Darling-Hammond, and Luczak (2005), teachers are more likely to leave schools with more students who are disadvantaged in some way or who are seen as "difficult" to teach, including students eligible for subsidized lunch, Black students and lower-performing students. Teachers in charter schools are more likely to leave the public school system. Teachers in schools with higher per-pupil expenditures are less likely to move across districts. Michigan-educated teachers are less likely to leave the public school system, in line with the Boyd, Lankford, Loeb, and Wyckoff (2005) "draw of home" hypothesis.

In some ways, the things that we find not to matter are more interesting than the things that we do find significant. There are no significant mobility differences by gender, not even into part-time work[1], even as more flexible work schedules are growing increasingly popular among women with children. Teachers in math or science, fields where outside options are much more lucrative, are only marginally more likely to leave the public schools than teachers in less-lucrative fields such as English or history. Cohort effects are minimal until a large short-term program enters the state in 2010, implying that business-cycle fluctuations have less influence than expected on teachers' mobility decisions (compare to Oreopoulos, von Wachter, and Heisz 2012). Similarly, local unemployment rates have almost no effect. The economic forces that affect teachers' labor mobility appear to be more unique to teaching.

Turning to the consequences, we find that teacher mobility does predict declines in student performance, but that those declines are economically small. Students in schools that lose

[1] Much of the literature does not include part-time or administrative work as an outcome. We argue that it is relevant and important, as sociological work dating back to Lortie (1977) outlines desirable career paths in education that do not involve full-time teaching for extended periods of time.

teachers to other schools, particularly but not exclusively to schools in other districts, see the most significant fall in test scores afterward; the consequences of losing teachers to the private sector[2], for instance, are modest. Finally, Black students' math achievement declines modestly in schools with more turnover among Black teachers, but the results for lagged turnover are similar enough to the results for upcoming turnover that a causal interpretation of these estimates is particularly tenuous.

III.2 Literature Review

A number of studies have attempted to determine the factors that predict teacher turnover. Perhaps the best-known study in this literature is by Boyd et al. (2005), who focus on how close a teacher's job is to his or her home town. Using data from the state of New York between 1998-99 and 2002-03, they find that new teachers are twice as likely to take jobs within five miles of home as to take jobs between five and 20 miles of home, and that teachers from the suburbs prefer to stay in their home metropolitan area while teachers from rural areas tend to stay in rural areas. However, this study necessarily excludes teachers who did not grow up in New York. The prospect of living in or near New York City may be a draw to many teachers from out of state, and the New York metropolitan area itself spans state boundaries.

Numerous studies have looked at the influences of student composition on teachers' mobility decisions. Most of these concentrate on how teachers react to teaching more poor students, low-performing students, and students of color, finding that teachers are more likely to leave schools and districts with more poor students and white teachers in particular are more likely to leave schools with more Black students. Using a multinomial logit framework, Goldhaber et al. (2011) find that teachers in North Carolina are more likely to move within their districts if they teach in low-performing schools, while they are more likely to transfer to other districts if they teach more Black students or more poor students. Hanushek and Rivkin (2007) find similar patterns in Texas, showing that the average teacher has 2% fewer Black students, 4.4% fewer Hispanic students, and 6% fewer students eligible for subsidized lunch in his or her second school as in his or her first school. Loeb, Darling-Hammond, and Luczak (2005) use a teacher survey to decouple student characteristics from administrative characteristics, finding that some of the effect of teaching more Black or poor students actually can be attributed to poor facilities, old textbooks, and large class sizes,

[2]Our term for leaving the Michigan public school system. These teachers could also have left to teach in a public school in another state or in a private school.

which are disproportionally found in schools with more disadvantaged students; however, even controlling for these working conditions, teachers are still significantly more likely to leave schools with more Black or more poor students.

An understanding of the distributional effects of teacher placements and turnover is important. Lankford et al. (2002) find that teachers who leave their first jobs in New York are more qualified than those who stay, based on college selectivity, certifications, and advanced degrees. Goldhaber et al. (2011) look at teaching effectiveness, as measured by value added, instead of pre-service qualifications, and find the opposite; teachers who stay in their initial placements have higher value added than those who leave. Boyd et al. (2011) find both of these results simultaneously in the New York City system. Schools have some ability to identify good teachers, based on both pre-service qualifications and teaching effectiveness; they also prefer teachers who live nearby, as Boyd et al. (2013) show. Even so, urban schools are at a disadvantage due to their late hiring periods, when their top targets have often already taken jobs (Levin and Quinn 2003).

Policymakers have attempted to address teacher turnover in a few ways, some of which have been evaluated rigorously. Hansen, Backes, and Brady (2016) look at Teach For America's concentrating its Miami-area teaching corps in a few schools after years of spreading them more widely through the district; using a difference-in-difference framework, they find that the clustering made corps teachers more likely to stay in their first school after their first year, although it had no effect on mobility after their two-year commitment. Increased salaries, of course, can help keep teachers in their schools; Miller (2012) finds that salaries lag particularly in rural schools, and these schools suffer most from turnover, while Springer, Swain, and Rodriguez (2015) see some positive effects from retention bonuses paid to effective teachers in tested subjects in low-performing schools. Accountability policy seeks to weed out ineffective teachers while retaining effective ones; Sun, Saultz, and Ye (2017) are able to separate teachers who leave voluntarily from teachers who are removed by their schools, finding that No Child Left Behind induced a slight increase in involuntary transfers from economically disadvantaged schools.

Some studies have also examined the repercussions of teacher turnover, finding mixed results. Hanushek and Rivkin (2007) find that teachers who move across districts are similarly effective to teachers who stay in their first jobs, whereas teachers who move within a district are less effective than their colleagues who stay. Hanushek, Rivkin, and Schiman (2016) account carefully for endogenous and nonrandom sorting of teachers and students, finding negative effects of teacher turnover (focused in low-achieving schools) despite worse teachers leaving the profession; the result stems from how experienced teachers are reallocated in the wake of turnover. Ronfeldt, Loeb, and Wyckoff (2013) use a fixed-effects strategy to look at within-school-year variation by grade and within-school-grade variation by year, also finding negative effects of turnover concentrated in schools with more low-achieving and Black students. Adnot et al. (2016) find positive effects from turnover induced by accountability methods; students perform better in schools where teachers were forced out, whereas the effects of non-penalized teaching on student performance are negative but insignificant.

III.3 Data

III.3.1. Predictors

To determine what factors predict teacher mobility, we use administrative data from 2003 to 2017 for all Michigan public schools. Specifically, we use personnel data merged with school-level and student-level data for the school in which each employee spent the most time working in each year. This data set makes it possible to analyze the characteristics of schools and teachers associated with mobility in Michigan public schools.

Our analytic sample includes all teachers hired into Michigan public schools between the fall of 2003 and fall of 2011. We restrict this sample to those who were initially hired as full-time teachers, defined as those with at least 1.0 FTE[3] in teaching assignments for the school year. In other words, any employees who began working in Michigan public schools as part-time teachers or in any assignment other than teaching are not included in the analysis. We further restrict our sample to teachers who accepted their first job at a school that remained open for five years thereafter[4]. By doing so, our analysis of teacher mobility is restricted to mobility by choice — either on the part of the teacher or the school — rather than by force.

The main data set is a panel of the first five years after the date of hire for each teacher in the analytic sample. To carry out our analysis, we create a balanced data set at the teacher-year level. We assign teachers who worked in multiple schools during a single year to the school where they worked most often. In cases where teachers split their time exactly evenly between two schools, we break ties randomly. For teachers who did not work in the Michigan public school system in a particular year, we create an indicator to note their absence. For analyses of factors associated with where teachers accept their second full-time teaching job, we limit our sample to teachers who accepted another job within five years of their initial date of hire.

[3] "FTE" stands for "full time equivalent"; a teacher with an FTE of 1 teaches the same amount as a full-time teacher, while a teacher with an FTE of 0.5 teaches half as many hours as a full-time teacher.

[4] This removes 1.9% of teachers from the sample.

If the lowest grade that a teacher teaches during a given year is ninth grade or higher, we define that teacher as a secondary school teacher; all other teachers are defined as elementary school teachers. We define hiring cohorts based on the spring of the first academic year that the teacher works in Michigan public schools; teachers first found in the data in the 2006-2007 school year are assigned to the 2007 cohort.

To analyze teacher mobility outcomes, we create indicators to specify teachers' employment status in Michigan public schools in each year for five years after their initial date of hire. The primary outcome of interest is whether or not teachers were in the same job at the same school where they were originally hired. This outcome is then broken down by type of move: teachers can move within their first districts, move to full-time teaching jobs in different districts, leave the Michigan public school system, or move to part-time or administrative jobs in the public schools. We also break down moves by how economically disadvantaged the student body of the teacher's second school is, using the fraction of students eligible for free or reduced-price lunch (FRPL) and whether the school is eligible for Title I funding.

III.3.2. Aftermath

To determine what effects teacher turnover might have on student achievement, we continue to use administrative data from the Michigan Department of Education and its partners, creating a student-year level panel matched to school-year level aggregate measures of teacher mobility. We begin with all teachers employed in Michigan public schools from 2003-04 through 2015-16, determining each teacher's "main school" the same way that we do in Section III.3.1 and measuring the same mobility outcomes: leaving one's school at all, moving to a school in another district, moving to a school in the same district, taking a part-time or non-teaching job in the Michigan public schools, and leaving the school system entirely. We mark teachers who leave the system and do not return as retiring if they are above 55 years old at the time that they leave. Teachers who leave the system temporarily and return to the same school are assumed to be on parental leave and are not coded as changing jobs.

One key difference between this sample and the sample used in the "predictors" sections of the paper is that we do not limit the set of teachers to include only those in their first five years of employment. While we as researchers are particularly interested in the initial placements of teachers and how those initial placements can influence teachers' choices of whether and where to take a new job, there is no immediate reason to believe that students' success is only influenced by the mobility of early-career teachers.

Once we have the mobility decision of each teacher in the system, we collapse the data to the school-year level to determine the fraction of teachers who leave each school in each year, including the fraction of teachers who make each individual mobility decision (e.g. how many teachers at Central High School left for the private sector, how many teachers at Lincoln Elementary School moved to a different school district). We match this school-year level data to student-year level administrative records containing individual students' demographic information, test scores, and the school in which they spent the most time in the given year. This leaves us with a student-year panel with school-year level turnover for each school that a student attends. The panel structure of this data set allows us to include students' prior test scores as predictors of their current ones. We also merge this panel onto the school-level data that we use in the "predictors" section, allowing us to control for environmental factors that could influence both teacher departures and student outcomes.

III.3.3. Descriptive Statistics

Table III.1 shows some characteristics of all teachers (regardless of date of hire) in Michigan public schools in the 2015-16 school year by FRPL eligibility quartile. A few characteristics of this distribution are especially important in motivating our work on teacher turnover. First, teacher race is unevenly distributed across Michigan public schools. In particular, Black teachers predominantly teach in schools with high FRPL. 17.1% of teachers in the poorest schools are Black, compared with only 1.2% of teachers in the wealthiest schools. Second, beginning teachers were also disproportionately concentrated among teachers who teach in the poorest schools. Among the quarter of teachers who work in the poorest schools, 16.1% have 3 or fewer years of experience. Among the quarter of teachers who work in the least poor schools, however, only 7.4% have 1-3 years of experience. Given such inequalities, we explore the extent to which the initial placement of teachers and their early-career mobility might shape the distribution of teachers in Michigan public schools.

Table III.2 looks at the initial placement of teachers in our analysis sample (in other words, teachers hired between fall 2003 and fall 2011), again separating them by quartile of FRPL eligibility. The patterns shown in Table III.1 are also present in Table III.2, despite the different time frames in the respective samples. Specifically, disparities in terms of race, degree attainment, and selectivity of undergraduate institution between teachers who work in schools with high FRPL compared to teachers who work in schools with lower FRPL appear in the initial placement of teachers in Michigan public schools. Given how closely disparities in Table III.2 match those in Table III.1, the effect of teacher turnover may look minimal; however, it is notoriously difficult to predict teacher quality based on observable characteristics (see, for example, Clotfelter, Ladd, and Vigdor 2007), so this churn of teachers with similar qualifications may mask differences in unobservable measures of motivation and skill. Furthermore, the fact that low-qualification teachers are replaced by other low-qualification teachers can further disadvantage schools that are already disadvantaged by the distribution of teachers' initial placements.

Table III.3 ties together the lessons found in Tables III.1 and III.2, presenting characteristics of teachers and their assignments in the 2015-16 school year, separated by how long ago they were hired. While there are minimal differences in the fraction of teachers who are highly qualified across experience levels, and the fraction with a master's degree largely differs as a function of age and time (as many teachers earn their master's degrees after they begin teaching), there are noticeable differences in the settings in which novice and experienced teachers work. The average teacher with one year of experience works in a school with 31.8% Black students and 60.7% poor students; the average teacher with ten years of experience works in a school with 19.3% Black students and 47.8% poor students. Disadvantaged students are disproportionally exposed to teachers with less experience on the job.

Table III.4 contains descriptive statistics related to teacher mobility events during the first five years of teachers' careers. After the first year of teaching, 77.5% of teachers continued working in their initial jobs, and 10.9% of teachers left Michigan public schools. The remaining teachers continued to work in Michigan public schools in some capacity. For these teachers, a full-time teaching job within their original district was the most common mobility event. After five years from the date of hire, 42.6% of teachers remained in their initial job, and 23.6% had left Michigan public schools. 26.1% of teachers had accepted a full-time teaching job in a different school by five years since their date of hire, and the majority of these teachers had moved to a job outside their original school district.

Given that our sample of teachers spans significant change in Michigan's economic conditions, variation in teacher mobility events may occur among cohorts. Table III.5 contains descriptive statistics of teacher mobility events by cohort. These statistics suggest that, unconditional on any other relevant factors, teacher mobility events were roughly constant among cohorts until the 2010-2012 period, when far fewer teachers continued to teach full-time in their first schools for five years. One contributing factor to these changes is the introduction of Teach for America (TFA) to the Detroit area, which began with the 2010-11 school year (Teach for America n.d.). Teach for America has sent over 800 teachers to Detroit since 2010, most of whom were on two-year commitments to their schools. In the cohorts that include TFA teachers, more teachers leave for other school districts and the private sector than in the pre-2010 cohorts.

One concern about teacher mobility is that these events can disproportionately affect disadvantaged students. Descriptive statistics in Table III.6 indicate that evidence may support this concern. As seen in Tables III.4 and III.5, roughly 45% of teacher continued working in their initial job five years after their date of hire, but not all schools retain teachers at the same rate. Schools with the fewest economically disadvantaged students had 53.4% of the teachers they hired continue working in the same school and same job five years after the original date of hire. Schools with the most economically disadvantaged students, on the other hand, had 27.3% of the teachers they hired continue working in the same school and same job five years later. Each of the other teacher mobility events also provide evidence of legitimate concern that economically disadvantaged students have borne the largest burden of teacher mobility.

III.4 Predictors Methodology

There are numerous factors that may contribute to teachers' mobility decisions. Some of these are impossible to observe directly with school administrative data (for instance, a teacher's spouse may find a job in another state, causing the teacher to move to that state and thus leave the Michigan public school system), but many factors such as teacher age and school size are observable. Furthermore, if teachers generally seek to leave schools with difficult working conditions and struggling students, we can observe this through variables such as the school's per-pupil instructional expenditure or the school's relative performance on state standardized tests. Combining all these factors, we can get a fuller picture of what drives teachers to leave their first job, and even what drives them to make particular types of moves. The teachers who leave their first job to take a new job in a school in a poor district may be very different from the teachers who leave the teaching profession after their first job.

We can think of a teacher's mobility decision, Y_{ist}, as a function of six types of factors:

$$Y_{ist} = f(D_{st}, P_s, Z_{st}, G_{st}, X_i, C_i) \tag{III.1}$$

Variables contributing to the mobility decision of teacher i, who accepted a full time teaching position at school s, after t years since the initial date of hire, are grouped into five broad categories. The first set of factors, D_{st}, measures the demographics of the student body of the initial school. Some teachers prefer not to teach in schools with more poor students or students of color, whether this is due to the teachers' own prejudices or the inequitable distribution of resources and working conditions across schools (Hanushek & Rivkin 2007).

Additionally, teachers may find students with unique needs, such as English language learners and special education students, to be more challenging to teach and may seek a less difficult environment. Our demographic variables include the fractions of students who are Black, Hispanic, enrolled in special education, English language learners, and economically disadvantaged[5].

The second set of factors, P_{st}, is the academic performance of students at school s, which we measure as of the time that the teacher is hired. We break the school's average standardized score in all state tests in the given year into quintiles, using the first (lowest) quintile as the reference group. Teachers may be frustrated by teaching low-performing students and may seek to leave low-performing schools. Additionally, low-performing schools frequently have administrative turmoil and other concerns that may make a teacher want to leave.

Third, Z_{st} represents characteristics of the teacher's first school, including whether the school is a charter, whether it is a high school, its per-pupil expenditure, and its change in enrollment during the five years after the teacher begins working there. Teachers in charter schools have fewer protections and lower pay than their counterparts in traditional public schools. Teachers in high schools may have more lucrative outside options, because there is more emphasis on subject mastery in high school teaching than in earlier grades, and this is more transferrable to the private sector. Schools with higher expenditures may pay teachers better or provide superior working conditions. Schools in which enrollment is declining may lay off or reassign teachers in response to the smaller student body.

G_{st} represents the characteristics of the area in which the school is located, including whether it is a city or a rural area, local unemployment rates, and the average per-pupil expenditures of schools in the surrounding county. Teachers in urban or suburban areas, in which other schools with job openings are more numerous, may feel more comfortable leaving their jobs than teachers in rural areas, who might need to move in order to take a new job. If unemployment is high, teachers may not want to leave teaching and face the open labor market. High expenditures in surrounding schools are a sign of a strong labor market and good working conditions in other nearby schools, both of which may tempt a teacher to leave his or her job.

The fifth set of factors, X_i, is the teacher's personal characteristics. We include demographics, such as age, gender, and race; qualifications, such as whether the teacher has a masters degree, a degree from a selective college, or highly qualified status under No Child Left Behind; indicators for whether the teacher teaches math, science or special education;

[5] "Economic disadvantage" is measured by eligibility for free or reduced-price school lunch. CEPI replaced this variable with a broader variable that includes direct certification for state aid beginning with the 2013-14 school year, which is after the end of our sample period.

and an indicator for whether the teacher attended college in Michigan. Younger teachers may feel less established in their communities and more comfortable leaving. Female teachers may be more likely to move to part-time teaching for family purposes. Teachers in certain fields that are in high demand, such as special education or high-school math, may have more opportunities to leave than teachers in other fields; math or science teachers are also likely to have more lucrative options in the private sector. Teachers who attended college in Michigan may feel more loyalty to their communities, while teachers who attended selective universities or have master's degrees are likely to have more job opportunities, both in and out of teaching.

Finally, teachers who entered the profession in each cohort C_i face different labor markets and opportunities. Our sample includes teachers who began teaching in 2003-04 (our reference group), when the labor market in Michigan was robust; the sample also includes teachers who started in 2008-09 — at the depth of the U.S. recession and the automotive crisis which crippled Michigan's economy. As the economy and labor market struggled, teachers had fewer opportunities to find employment both in and out of the profession. At the same time, struggling districts may have laid off teachers in the leanest years.

We attempt to control for as many factors as possible, but some variables remain unobserved. For instance, teachers may relocate in order to accommodate a spouse's new job[6], but we do not observe marital status in the data; as older people are more likely to be married, this may bias the coefficient on age downward in the main specification. Similarly, we do not observe distance to home directly; therefore, white teachers who are moving away from urban schools with more Black students may not be reacting specifically to the racial composition of the school or the urban environment, but instead may be looking to move closer to where they were raised (Michigan is the third-most segregated state in the country during the sample period; see Frey and SSDAN, n.d.), and this would bias the coefficients on the race and urbanicity variables downward in the main specification for these teachers.

We also do not observe whether teacher turnover is driven by the supply side or the demand side; that is, whether teachers are leaving of their own accord or being pressured to leave by the school. Some factors may have similar effects on both the supply side and the demand side; for instance, schools with more poor students are frequently in financial challenges and are looking to cut or reallocate teachers, but teachers may also leave these schools voluntarily due to the poor working conditions. However, on other factors, such as teacher qualifications, the supply and demand sides pull in opposite directions. Schools will want to keep their most qualified teachers if at all possible, but the most qualified teachers

[6]Stinebrickner (2001) observes marital status and incorporates it into the model, which is rare in this literature.

are also the ones who can get new jobs most easily, so they have more opportunities to leave even when their employers want them to stay. This will bring the net effect of these factors closer to zero.

We begin with a multinomial logit specification, in which our outcome is the teacher's employment status in the fifth year of the data. The base case is that the teacher remains in his or her first job full-time in the fifth year; the alternative outcomes are that he or she moves to a different full-time teaching job in the same district, moves to a different full-time teaching job in the same district, transitions to a part-time or non-teaching position (regardless of school or district), and leaves the Michigan public school system entirely. Teachers whose first schools closed less than five years after the teacher was hired are removed from the sample; we aim to study teacher turnover, not school closing, and the repercussions of each are somewhat different. All school and student characteristics are measured in the teacher's first school at the time that he or she was hired, unless explicitly labeled otherwise. We also include missing indicators for any variables that are ever missing in our data set. Standard errors are clustered at the school level.

The interpretations of most of the covariates are fairly straightforward. The most interesting coefficients shed light on the labor markets in which these teachers are participating. For instance, teachers with advanced degrees or bachelor's degrees from selective colleges are likely to be highly sought-after candidates for other jobs, both in and outside of the Michigan public school system. If these teachers are more likely to stay in their first job despite these prevalent outside options, it demonstrates that these teachers are either particularly dedicated to teaching or they can choose a first job that is a particularly good fit for them. The story behind the coefficients on the recession-era cohort dummies is similar. Michigan's economy was in a deep slump in 2007-2009. Because private-sector jobs were few and far between, any available jobs were likely to draw more applicants than usual, and not all applicants for teaching jobs were necessarily highly committed to teaching. A positive coefficient on the recession cohort dummies would indicate that many people leapt at the first job available with little intent to stay at it, or even to stay in the teaching sector; they moved on to other jobs as soon as the economy improved and these jobs became available. Conversely, a negative coefficient on these dummies would indicate that schools could screen candidates effectively, choosing the best fits from the large applicant pool and getting teachers who will continue teaching at their schools for years.

This specification helps us to answer several questions about the data. For instance, we can gain further insights about the role of teachers' outside options. Teachers with a background in science or math are likely to have lucrative outside options in engineering or health; if these teachers are more likely to leave the public school system, these outside options are important. Outside options also respond to local unemployment rates; as the local unemployment rate rises, the expected value of the outside option decreases as fewer jobs are available. If teachers are more likely to stay put in response to increases in unemployment rates, then this also demonstrates the importance of the outside option.

We also estimate another multinomial logit specification in which we separate moving to economically disadvantaged schools from moving to non-disadvantaged schools[7]. There is an extensive literature about the disparity in teacher quality between poorer and wealthier schools (Goldhaber et al. 2015, Clotfelter et al. 2006), so by treating movement to an economically disadvantaged school[8] as a separate outcome from movement to a wealthier school, we can determine which types of teachers are willing to leave their first job and take a job in a poor school. Under the practice referred to by Bridges (1991) as "the dance of the lemons", low-performing or underqualified teachers move from one poor school to another, often within the same district, because they cannot easily be fired. If this pattern holds in Michigan, then teachers who do not have master's degrees or are not highly qualified under the standards of No Child Left Behind will be more likely to move to poor schools or leave teaching than to move to non-poor schools or stay put. Similarly, teachers coming from schools with many low-income or low-performing students will be more likely to move to poor schools or leave teaching. On the other hand, teachers may also move to seek better working conditions and students who they deem "easier" to teach, and this would induce teachers in economically disadvantaged schools to try to leave for wealthier districts.

III.5 Predictors Results

Table III.7 presents results from the primary multinomial logit specification. All results are relative risk ratios, in which the base case is that the teacher remains in his or her first job five years after beginning to teach in the Michigan public school system. Standard errors are clustered by the teachers first school.

Several measures of student disadvantage are highly correlated with teacher turnover. Teachers with more Black students are more likely to take full-time jobs in other schools, regardless of district, and more likely to leave the public school system. Teachers with more economically disadvantaged students are more likely to leave their district, either for

[7]Leaving the public school system remains in the model as an third option for the outcome variable.

[8]We define a school as "economically disadvantaged" if at least 30% of its students are eligible for free or reduced-price lunch in the first year that we observe that school in the data. This allows for a definition of economic disadvantage that is not time-dependent, which is important given the business cycle fluctuations during the sample period. The 30% threshold follows the definition used by the federal government to determine Title I eligibility.

a teaching job in another district or for a job outside the public school system. A 10-percentage point increase in the fraction of subsidized lunch eligible students is associated with a 13.7% increase in the probability of moving to another district and a 3.5% increase in the probability of leaving the public school system; the pattern is somewhat reversed for an increase in the fraction of students who are Black, as a 10-point increase is associated with an 11.3% increase in the probability of leaving public schools and an 8.9% increase in the probability of moving to another district. Teachers with students who scored better on standardized tests are less likely to leave for other districts or the private sector. As Hanushek and Rivkin (2007), along with others, find, the students who would benefit most from an effective and experienced teacher, but who are deemed most difficult to teach, are most likely to lose their teachers due to turnover.

Charter schools seem to exhibit particularly high rates of teacher turnover; charter teachers are 61% more likely to move to new districts than non-charter teachers and 87% more likely to leave for the private sector. High school teachers also are highly likely to leave, although their differences from elementary-school teachers are not quite as stark as those between charter and traditional public-school teachers: 28% increases in both the probability of moving to a new district and the probability of leaving for the private sector. With the exception of within-district moves (which are frequently impossible because charters are usually one-school "districts" and many public school districts have only one high school), teachers in these settings are significantly more likely to leave for any other position. Teachers in urban schools are more likely to take jobs in other schools within the same district (perhaps because of the large district sizes), while teachers in rural schools are less likely to move across districts. Increases in enrollment are associated with an increased likelihood of moving to another district or moving to a part-time or non-instructional job. Local economic variables, such as school expenditures and unemployment, are less predictive than one might expect.

Most teacher characteristics are predictive of at least some types of movement, although only teaching special education predicts turnover of all types, regardless of destination. Race, gender, and age are less predictive than one might expect; interestingly, there are no significant differences by gender in the probability of moving to a part-time or non-instructional position (or anywhere else). Another surprising result is that teachers in math and science are only marginally more likely to leave for a position in the private sector (or any position outside the Michigan public school system) than teachers of English, social studies, or elementary education. Teachers with master's degrees or degrees from selective colleges are more likely to take part-time or non-instructional jobs (perhaps in administration) or leave the public school system, while teachers who are highly qualified according to No Child Left Behind or attended college in Michigan are the opposite.

Differences by cohort[9] are unclear. The probability of changing districts appears to have increased with time, although this does not follow any business-cycle pattern. For the most part, with the exception of the last two cohorts (which go in very different directions), there are no cohort-level differences on the probability of leaving the teaching profession, undermining the hypotheses about who attempts to enter teaching (and who is hired) at different points in the business cycle. Teachers who entered in 2010-11 are 32% more likely to leave for a new district, 38% more likely to move to a part-time or administrative position, and 26% more likely to move to the private sector than those who entered in 2003-04; on the other hand, teachers who entered in 2011-12 are also (86%) more likely to move to a new district, but they are 35% less likely to move to a part-time or administrative position and 66% less likely to move to the private sector than their counterparts in the 2003-04 cohort.

The interpretation of these results is unclear, but fascinating. Michigan was not in a drastically different part of the business cycle in 2010-11 than in 2011-12. The entrance of Teach for America into Michigan in 2010-11 explains some of the volatility, as its teachers were hired on two-year contracts with the intention of moving to other positions (often outside of teaching) after their two-year commitments; however, preliminary results (available upon request), imply that the differences in turnover are not driven solely by the Detroit area, where TFA was focused. Other potential explanations for the cohort effects include changes in teacher training over time, changes in norms about job switching, changes in the appeal of education as a career, differences in union strength, and new policies regarding teacher or school accountability. Testing for these effects is very difficult and any explanation would be very speculative.

Table III.8 divides new jobs in the Michigan public schools by economic disadvantage (marked by the new school being eligible for Title I funding) rather than by district. The base case is staying in one's first job; leaving for the private sector is also a possible outcome. Not surprisingly, teachers in schools with more economically disadvantaged students are much more likely to take new jobs in poor schools. Teachers with more Black students are more likely to leave for any new job, regardless of how well-off the new school is. Teachers with higher-achieving students are much less likely to take new jobs in disadvantaged schools.

Teachers in rural areas are less likely to take jobs in non-disadvantaged schools, while teachers in urban areas are more likely to leave the public school system. Teachers in charter schools are less likely to take new teaching jobs regardless of student socioeconomic status but more likely to leave the school system; relationships for high school teachers are somewhat similar but there are much smaller differences in their probability of taking a job in a disadvantaged school. With the exception of enrollment change, which induces

[9] All cohort results are compared against teachers who entered during the 2003-04 academic year.

movement across schools regardless of student disadvantage, any relationships with local economic variables are minimal. This is largely true for the teacher characteristics and the cohort effects as well; fairly few of them have consistent differential effects across student disadvantage. The sharp exception is for Black teachers: they are far less likely (about 60%) to take new jobs in non-poor schools and more likely (about 37%) to take them in poor schools. This reinforces the initial placement results from Table III.2, and deserves much more examination in a separate study.

Tables III.9 and III.10 test for robustness, particularly regarding effects operating through changes in the composition of the teaching force. Table III.9 estimates the same specification as Table III.7 but without the teacher characteristics; Table III.10 separates the 2007-08 and 2008-09 cohorts from the others to see if schools had vastly different hiring practices during the recession compared to before it. Labor economics papers such as Oreopoulos, von Wachter and Heisz (2012) and Altonji, Kahn, and Speer (2016) find long-term negative effects on wages for employees who graduated during recessions; Oreopoulos et al. (2012) find that more advantaged graduates can recover more quickly by switching firms (or switching school districts, in our case). Different types of people may enter teaching as economic conditions change as well. The results in these tables are broadly similar to those in Table III.7, suggesting that the results are not driven by compositional changes.

Appendix Table III.14 separates the factors associated with mobility outcomes for teachers with different levels of pre-service qualifications, to see if teachers with more items on their résumés have different options in the labor market. We include three observable factors that may make a teacher appear more qualified: a master's degree, a bachelor's degree from a selective institution, and "highly qualified" status under NCLB, and we estimate results separately for teachers who satisfy zero, one, and two or more of these criteria[10]. Two results stand out. One is that Black teachers who fit none of the criteria are much less likely than white teachers who fit none of the criteria to get teaching jobs in new districts and much more likely to move to part-time positions. This is not true for teachers who fit one or more of the criteria, suggesting that districts have less patience for Black teachers with weak résumés than their white counterparts and limit their opportunities for new jobs disproportionately to part-time and non-instructional positions. The other fascinating result is that teachers with two or more pre-service qualifications (who must either have a master's degree or a degree from a prestigious college) are almost unaffected by the fraction of economically disadvantaged students in their schools, with only a marginally-significant positive relationship between the fraction of poor students and the probability of moving to a new district. The relationships between the socioeconomic status of the student

[10]If a teacher satisfies one of them, it is almost always the NCLB status.

body and teacher turnover are much stronger for almost all outcomes for teachers with fewer pre-service qualifications. This result suggests that these highly-educated teachers are less intimidated by teaching disadvantaged students and chose to do so intentionally, rather than as a stopping point before getting a "better" job with wealthier students. However, this is tempered by the disparities in initial placements, as more-qualified teachers are often placed into wealthier schools, so perhaps we are showing more that teachers with stronger pre-service qualifications are more effectively able to self-select and teach the students that they want.

To ensure that there is nothing unique about five-year mobility, we estimate the same specification shown in Table III.7 separately for mobility in the second, third, and fourth year after hire, with results shown in Appendix Tables III.15-III.17. Results are largely similar regardless of the year studied. 76% of significant coefficients in Table III.7 are significant in the same direction in the fourth-year specification; 57% are significant in the third-year specification, and 48% are significant in the second-year specification. Important predictors such as the fraction of students eligible for subsidized lunch, the fraction of Black students in the school, and whether a teacher attended college in Michigan are consistent across time horizons.

III.6 Aftermath Methodology

The main outcomes that we study are students' standardized test scores in math and reading. The simplest specification measures the relationship between the fraction of teachers who left the student's school and the student's test score in the given subject:

$$TestScore_{ist} = f(FracLeaving_{st}) \tag{III.2}$$

Of course, many characteristics of a school (both observed and unobserved) are correlated with teacher turnover; that is one of the major points of this study. Many of these characteristics are also correlated with student performance. Schools with poor working conditions such as outdated textbooks, crumbling facilities, and overworked support staff will hold back student learning just as they push teachers to leave. We deal with these biases in two ways. First, we control for the school-level and geographic characteristics that predict teacher turnover (D, P, Z, and G, in the language of Section III.4). What remains is the effect of losing teachers, conditional on many of the factors that may lead them to leave.

Second, because some factors that affect both teacher turnover and student performance are unobserved or unquantifiable, we use the turnover that occurs *after* the year of observation to perform a falsification test. These teachers have not yet left the school, but they and their students have been exposed to the working conditions that may induce turnover, and that will be observed in students' scores. If the relationship between the turnover from the previous summer ("lagged" turnover) and student performance is stronger than the one with the next summer's turnover ("future" turnover), then there is likely to be a true effect of turnover on student achievement that is not driven solely by other variables.

How students may react to teachers leaving, conditional on the working conditions and other factors that encourage them to leave, depends on the type of human capital that makes teachers effective. If teaching effectiveness is driven largely by firm-specific human capital, in which teachers need skills specific to their schools to be successful, then teacher turnover may be very damaging, as teachers who have accumulated this human capital are replaced by teachers with none. Firm-specific human capital in the teaching case may refer to understanding of the school's mission and curriculum, good relationships with administration and parents, and understanding of the lives and needs of the unique group of students at a particular school. If instead the human capital needed for effective teaching is industry-specific and acquired on the job, then the effect of losing teachers depends on who replaces them; if a teacher with three years of experience leaves and is replaced by one with twelve years of experience (spent at other schools), then this could actually improve student outcomes as the new teacher will have learned more of the skills necessary to teach effectively. Industry-specific human capital in this case may include things such as experience making lesson plans, dealing with uncooperative students, and balancing lectures with activities. Finally, if teaching effectiveness is driven by an innate intelligence and ability that is not unique to teaching and not acquired with experience, then the effect of teacher turnover on student achievement would depend on which teachers leave, how well schools can identify this ability, and whether the composition of the teaching force changes. Replacing experienced teachers with novices would not inherently cause a problem in this scenario; some schools may lose their best teachers to other schools, while others may be particularly adept at identifying and hiring the most promising candidates for openings.

There is no reason to believe that students' response to teacher turnover is linear in the fraction of teachers who leave. Students may be very resilient to modest levels of churn but begin to suffer greatly once the turnover reaches a certain level. As such, we estimate the relationship between teacher turnover and student achievement both parametrically (using OLS regression with school fixed effects) and semi-parametrically (expressing turnover as a flexible fractional polynomial of dimension 2). We express the semi-parametric results graphically, showing the effect of teacher turnover on student achievement at each point in the distribution of turnover. In addition to the school fixed effects, our preferred specifications also control for students' prior test scores in the given subject and for a fairly standard set of student demographics (race, gender, special education enrollment, economic disadvantage, and limited English proficiency).

Not all teacher exits mean the same thing. We are also interested in the relationships between particular types of teacher turnover and student success. For instance, if a school is particularly good at identifying which teachers are not cut out for the profession and encouraging them to move to the private sector, this may lead to better outcomes for students as the effective teachers are retained while the least-effective teachers are winnowed out. On the other hand, if a school loses many of its teachers to schools in other districts, this suggests that its teachers are good enough at teaching that someone else is willing to hire them, and they are choosing disproportionately to flee a particular school or district due to its unique and possibly challenging characteristics. Students in these schools may particularly lose out, as their worst teachers may stay while those who have opportunities elsewhere may choose to pursue those opportunities. To incorporate this, we estimate a single OLS model containing all of the types of turnover separately; we would expect the most substantial negative coefficients to come from the fraction of teachers leaving for other districts[11]. In the semi-parametric specifications, we estimate different regressions for each type of turnover, allowing us to compare the responses to the different types throughout the distribution.

III.7 Aftermath Results

Table III.11 presents results from OLS regressions of math and reading test scores on future and one-year lagged fractions of teacher exits. A 10-percentage point increase in the fraction of teachers who leave at the end of the current year of observation is associated with a 0.004-standard deviation decline in math scores and a 0.002-standard deviation in math scores; the same increase in teacher turnover at the end of the previous year is associated with declines of 0.006 and 0.004 standard deviations, respectively. Both estimates for lagged turnover are statistically different from their future counterparts, implying that there is a relationship between the actual teacher departures and student performance, above and beyond any

[11]Of course, the Bridges (1991) "dance of the lemons" hypothesis implies that even bad teachers who want to continue teaching can find a way to do so, usually in schools with the fewest resources or the most underserved students, and teachers who leave for the private sector may do so because they are particularly skilled (and not only in teaching). These hypotheses, also perfectly plausible, would lead to opposite effects of the ones that we describe above.

effects of the working conditions that pushed the teachers to quit. If anything, the difference between the lagged coefficient and the future coefficient is likely an underestimate of the true effects of teacher turnover, as the current working conditions may be more salient to student performance than the prior year's working conditions would be. In any case, we proceed using the values of turnover from the summer before the observation.

Table III.12 decomposes the relationship by the departing teachers' destinations. For both math and reading scores, the responses to teacher turnover are driven by teachers who depart for other full-time teaching positions in the Michigan public schools, especially teachers who move to other districts; a 10-percentage point increase in teacher departures to other districts is associated with a 0.013-standard deviation decrease in math scores and a 0.009-standard deviation decrease in reading scores. The relationships between teacher retirements, teachers moving to part-time or administrative positions, and teachers moving to private-sector jobs, and student performance are either statistically insignificant or only marginally significant. This result supports one main hypothesis in this study: namely, that teachers' mobility outcomes reveal something about their quality, and that teachers who move to other districts are more likely to be effective (because more than one district is interested in hiring them) while teachers who leave the public school system are more likely to be ineffective (whether they learned that they were not a good fit for teaching and chose to leave, or their employers encouraged or forced them to leave). It is the loss of effective teachers that hurts students' success the most.

To interpret the figures, it is best to focus on the shape of the line and where it is significantly different from its value when no teachers leave. Drawing a horizontal line from the y-intercept of the graph and determining where it exits (and possibly later re-enters) the confidence region reveals which levels of teacher turnover are associated with significantly worse student outcomes than no turnover at all. Of course, it is rare for schools to lose more than half of their teachers in a single year, so the confidence intervals grow wider at the highest levels of turnover.

Figure III.1 contains the results for general types of mobility. Students exposed to more than 5% of their teachers leaving see declines in their math scores, but these declines are initially fairly modest before growing sharper toward the middle of the distribution. Students who have about 50% of their teachers leave have math scores about 0.05 standard deviations lower than those who have no teachers leave, though the confidence interval is somewhat broad. The declines for reading scores are a bit more modest, requiring about 10% of teachers to leave before any significant effects are seen, with the effects hitting a trough at around 0.03 standard deviations for 60% of teachers leaving before starting to move back toward the initial level at the very top of the distribution (again, with wide confidence intervals).

Figure III.2 presents the non-parametric results for math separately for different types of teacher turnover. The patterns generally match the OLS results. There is a sharp decline that comes from losing any teachers to cross-district moves, followed by a steady continued decline, while the decrease in math scores following in-district moves is slower-developing and the decline accompanying teachers leaving the profession is unclear due to very wide confidence intervals. Figure III.3 repeats the exercise for reading scores; the patterns are broadly similar. In general, the results show that losing more teachers is associated with worse outcomes, and that departures to other schools are most damaging. Schools can weather some in-district turnover, but large amounts of it signify problems in the district that lead to poor scores; meanwhile, even a small number of teachers leaving for other districts can have noticeable effects on student achievement.

These results suggest that firm-specific human capital is a significant driver of teachers' productivity, combined with innate ability. As teachers leave a school, abandoning the school-specific human capital that they have accumulated, students perform worse, as these teachers are replaced by teachers without any experience in the given school and who need time to learn how to work best with the students and staff in their new environment. However, as the relationships are larger for teachers who leave for other schools and smaller for teachers who leave for the private sector, it is likely that teachers learn about their innate ability to teach (and capacity to improve) over time on the job, and those who find out that they are poorly suited to the profession leave without doing harm to their former schools. Teachers who find that they have higher innate abilities to teach will continue teaching, and if they leave for greener pastures in other districts, their former schools will end up worse off.

Finally, we look at the relationship between Black teachers' turnover and the achievement of Black students. There is an extensive literature (see Tyson 2003, among many others) on how Black teachers are especially valuable to Black students, whether by serving as role models for children who look like them, practicing more culturally relevant pedagogy, or communicating more effectively with Black families. Table III.13 and Figure III.4 show the results of similar specifications to those above, looking exclusively at Black teachers and Black students. Black students perform slightly worse in math, with inconclusive results in reading, after more Black teachers leave, and this estimate is very similar to how they perform in years after which more Black teachers leave. At face value, one may take this as evidence against the importance of Black teachers to Black students, but it is important to consider alternative explanations. Michigan's public school system is quite segregated; recall from Table III.2 how most Black teachers take their first jobs in schools with large populations of Black students. Our variable looks at who leaves and not at who enters; if one Black teacher is replaced by another, the effects should be closer to regular turnover effects than to effects that take into account racial matching. Looking at the effects of Black teachers leaving on Black students' outcomes in suburban or rural schools (which are almost always majority-white in Michigan) may reduce the sample size significantly but more effectively capture the effects of losing these important teachers.

III.8 Conclusion

We find that teacher turnover reinforces many of the disparities in the American educational system. Poor and Black students already have less-experienced teachers than their wealthier white counterparts, and these teachers are more likely to leave those schools with more marginalized students. Students exposed to more teacher turnover, in turn, receive lower scores on standardized tests; this is more true of teacher turnover in the summer before the test date than the summer after it, implying that the effects cannot be attributed solely to environmental factors at the school that push teachers to leave. As such, students who are already disadvantaged are most likely to suffer academically as a result of instability in their schools' staffs. Other predictors of teacher turnover include teaching in a charter school, teaching in a school in an urban area, and having attended college outside of Michigan.

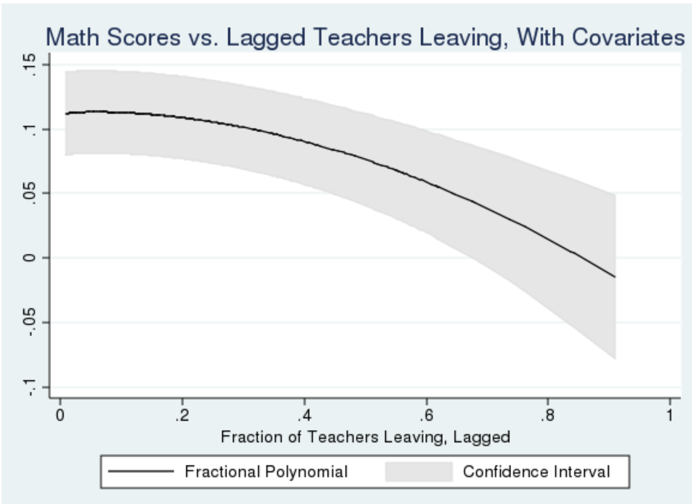
Looking beyond the raw fraction of teachers who leave a school and focusing on their destinations reveals important patterns of which teacher exits are most detrimental to student achievement. Students exposed to more teachers who retire, leave for the private sector, or move to part-time or administrative positions are largely unaffected by these departures. On the other hand, students in schools with more teachers moving to other full-time public school teaching positions, particularly those in other districts, see more significant drops in their test scores. The former set of teachers may well be leaving their jobs because they do not see themselves as effective teachers, so they choose to do something else, whether that involves scaling back their teaching commitments or leaving the profession entirely. The latter teachers remain committed to teaching and have been deemed worthy of another full-time teaching job by another district or school. It is these teachers who struggling schools cannot afford to lose.

The greatest policy conclusion from this study, then, is that schools should have the willingness and the resources to aggressively work to retain their most effective teachers, rather than allowing them to be poached by other districts. High retention bonuses, tied to a meaningful measure of effectiveness (whether measured by value added, principal observation, or some combination of the two), would provide incentives for wavering teachers to continue in their same schools. Considering that many of the schools that lose teachers have very strained budgets, state or local governments should provide additional funding targeted to this bonus pool. These efforts to retain teachers can improve student achievement while strengthening the ties of teachers to their schools and communities.

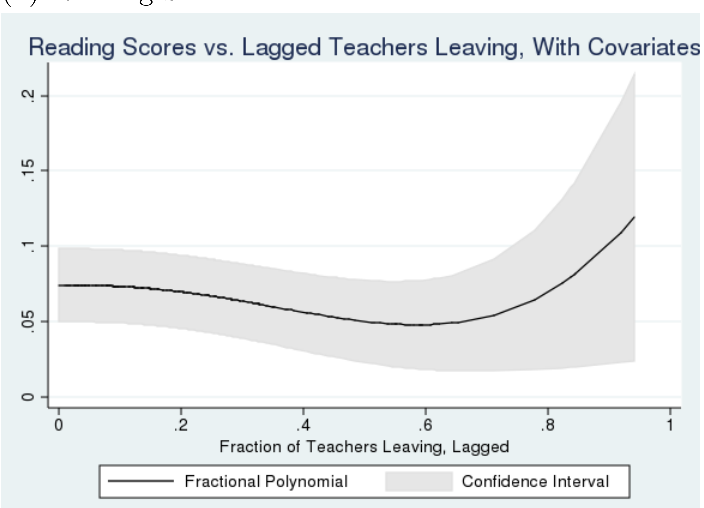
III.9 Supplemental Figures

Figure III.1.: Non-Parametric Effects of All Lagged Turnover on Test Scores

(a) Math Scores



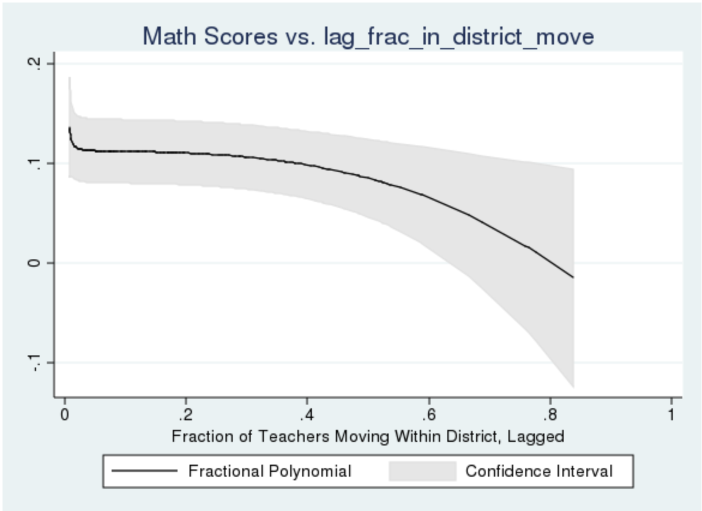
(b) Reading Scores



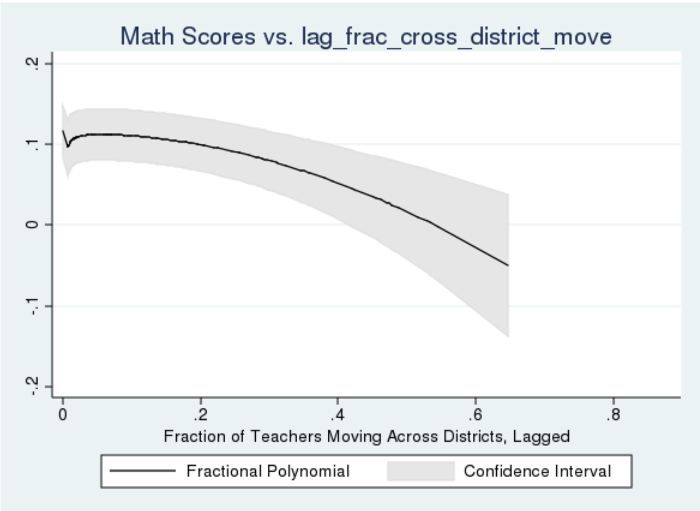
Fractional polynomial results shown. All regressions control for student and school characteristics, including the student's last observed test score. Fractions leaving include all teachers who stop teaching full-time at the given school except for retirements and temporary exits that are coded as parental leave. Standard errors clustered by school.

Figure III.2.: Non-Parametric Effects of Lagged Turnover on Math Scores, By Type

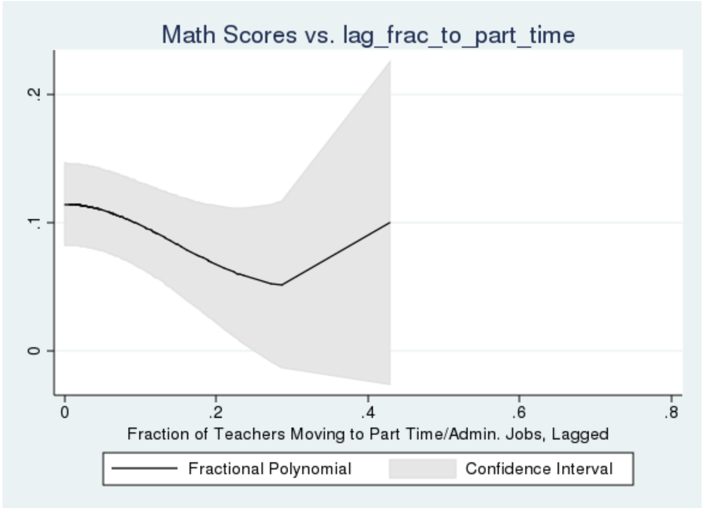
(a) In-District Move



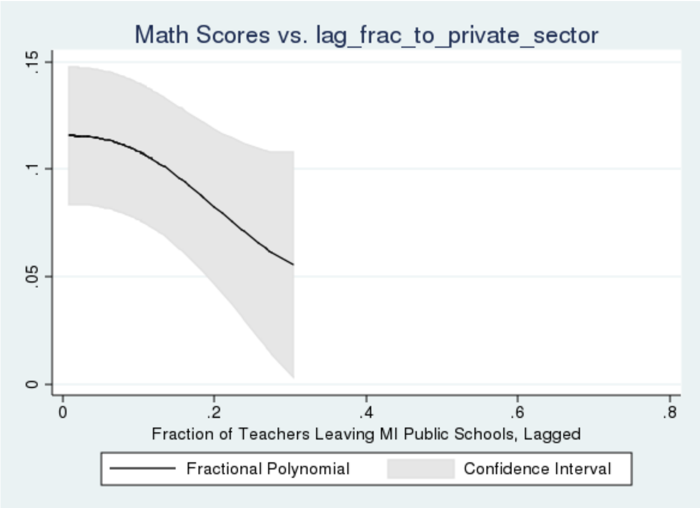
(b) Cross-District Move



(c) To Part-Time



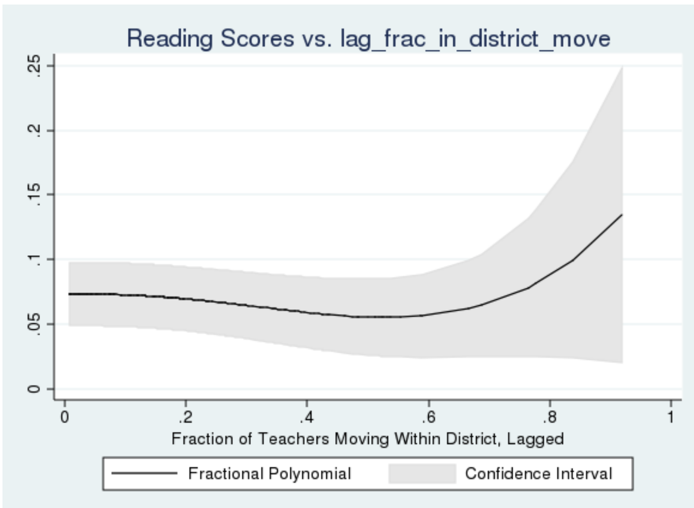
(d) To Private Sector



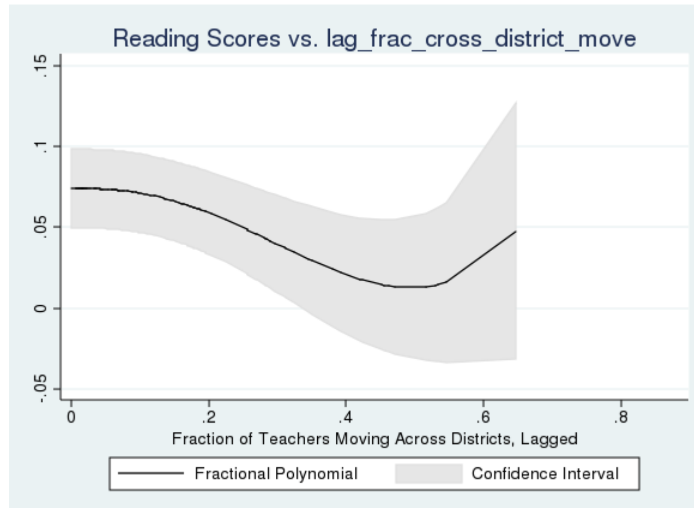
Fractional polynomial results shown. All regressions control for student and school characteristics, including the student's last observed test score. Standard errors clustered by school.

Figure III.3.: Non-Parametric Effects of Lagged Turnover on Reading Scores, By Type

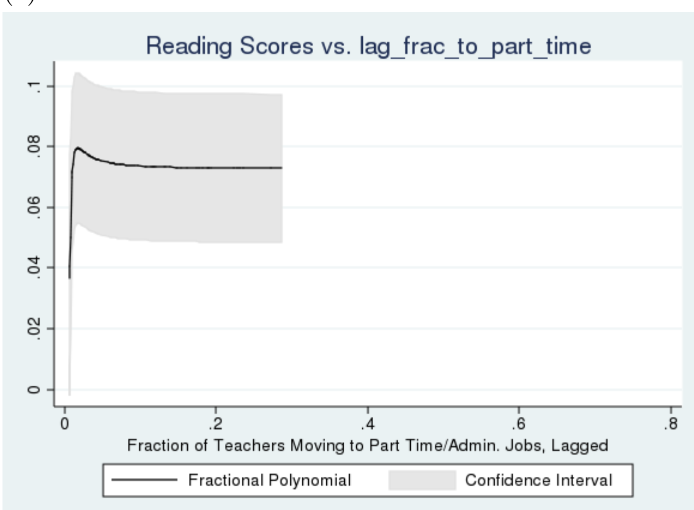
(a) In-District Move



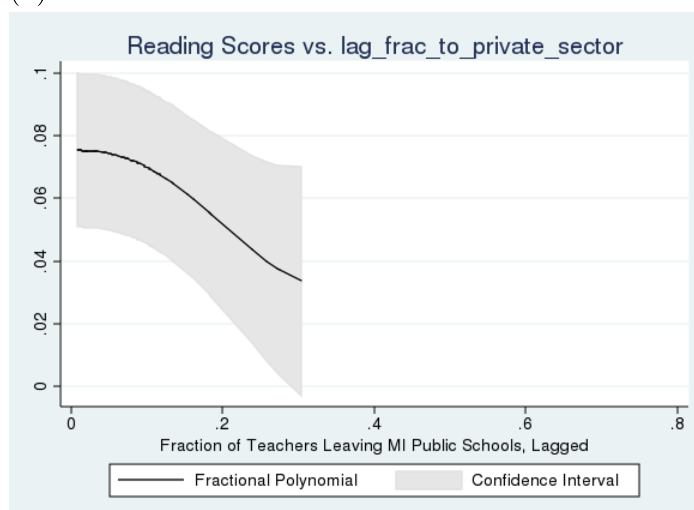
(b) Cross-District Move



(c) To Part-Time



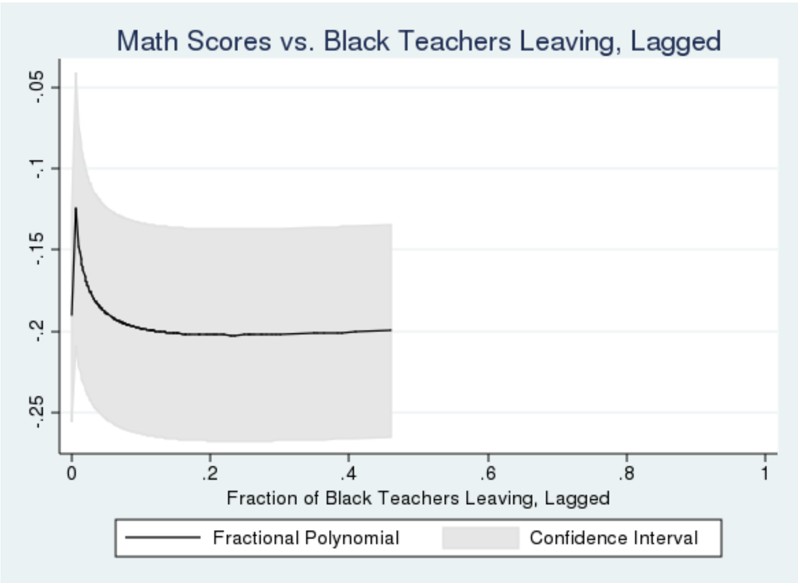
(d) To Private Sector



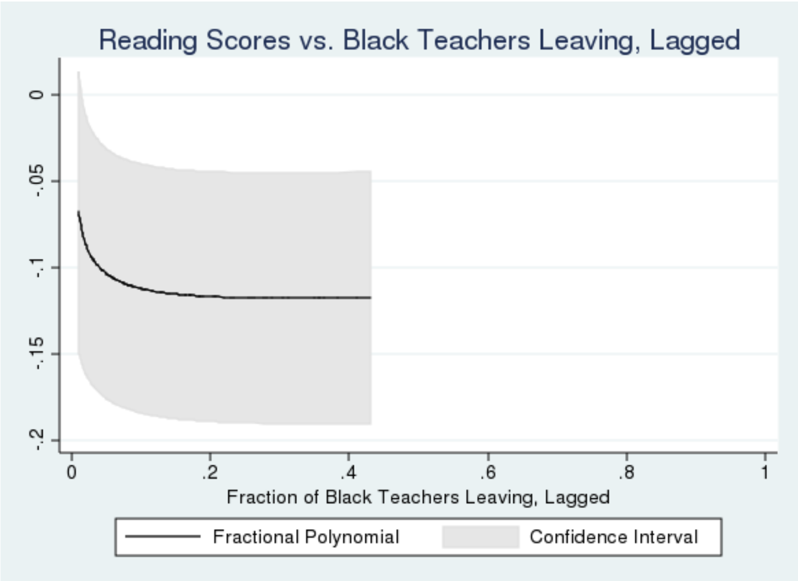
Fractional polynomial results shown. All regressions control for student and school characteristics, including the student's last observed test score. Standard errors clustered by school.

Figure III.4.: Non-Parametric Effects of Black Teacher Turnover on Black Students' Scores

(a) Math Scores



(b) Reading Scores



Fractional polynomial results shown. All regressions control for student and school characteristics, including the student's last observed test score. Standard errors clustered by school.

III.10 Supplemental Tables

Table III.1.: Distribution of Full-Time Teachers in Michigan Public Schools in 2016

	Fewest Poor Students	2^{nd}	3^{rd}	Most
Teacher is Black	0.012	0.012	0.037	0.171
Teacher is Hispanic	0.009	0.01	0.012	0.018
Teacher is Asian	0.009	0.004	0.006	0.008
Teacher is Highly Qualified	0.875	0.873	0.881	0.866
Teacher Teaches Math	0.084	0.081	0.069	0.058
Teacher Teaches Science	0.079	0.071	0.058	0.043
Teacher Teaches English	0.096	0.093	0.085	0.078
Teacher Teaches Social Studies	0.072	0.066	0.053	0.042
Teacher Teaches Special Ed.	0.128	0.154	0.184	0.162
Elementary Teacher	0.683	0.713	0.77	0.87
Secondary Teacher	0.317	0.287	0.23	0.13
Attended College in Michigan	0.635	0.589	0.586	0.535
Has Advanced Degree	0.707	0.632	0.555	0.522
Teacher Age	43.6	43.3	43.4	43.1
Years Since Hire	14.6	14.1	13.8	12.4
1-3 Years Experience	0.074	0.081	0.103	0.161
4-10 Years Experience	0.255	0.273	0.284	0.315
11-20 Years Experience	0.46	0.443	0.406	0.352
20+ Years Experience	0.212	0.203	0.207	0.172
Max Frac. FRPL Eligible	0.284	0.494	0.699	1
Number of Teachers	18,597	18,584	18,619	18,561
Number of Schools	656	816	969	986

Sample includes all full-time teachers in any Michigan public school for the 2015-2016 school year. Quartiles of poor students are defined by percent of students in the school who are eligible for free or reduced price lunch. Cutoffs for these quartiles are 28%, 49%, and 70% for the 25^{th}, 50^{th}, and 75^{th} percentiles, respectively. Data for attendance at a Michigan college are unavailable after 2012. Teachers who accepted their first full-time teaching job in the 2012 -2013 school year or later are missing for this statistic.

Table III.2.: Distribution of Initial Placements from 2004-2012

	Fewest Poor Students	2^{nd}	3^{rd}	Most
Teacher is Black	0.021	0.023	0.065	0.147
Teacher is Hispanic	0.01	0.007	0.01	0.023
Teacher is Asian	0.009	0.009	0.01	0.012
Attended Very Selective College	0.179	0.182	0.146	0.128
Teacher is Highly Qualified	0.84	0.86	0.854	0.869
Teacher Teaches Math	0.093	0.09	0.087	0.077
Teacher Teaches Science	0.073	0.072	0.058	0.061
Teacher Teaches English	0.11	0.103	0.084	0.086
Teacher Teaches Social Studies	0.072	0.062	0.052	0.058
Teacher Teaches Special Ed.	0.181	0.188	0.178	0.135
Elementary Teacher	0.64	0.666	0.763	0.808
Secondary Teacher	0.36	0.334	0.237	0.192
Attended College in Michigan	0.827	0.83	0.797	0.775
Has Advanced Degree	0.702	0.652	0.596	0.526
Teacher Age	30.2	30.1	30.9	3.4
Number of Teachers	5,235	5,235	5,235	5,233
Number of Schools	1,142	1,391	1,433	889

Sample includes all full-time teachers in any Michigan public school between the 2003-2004 school year and the 2011-2012 school year. The sample of teachers is restricted to those who accepted their first job in a school that remained open for at least five years thereafter. Quartiles of poor students are defined by percent of students in the school who are eligible for free or reduced price lunch. Cutoffs for these quartiles are 19%, 39%, and 66% for the 25^{th}, 50^{th}, and 75^{th} percentiles, respectively.

Table III.3.: Teacher Characteristics by Experience in 2016

	1 Year	5 Years	10 Years	20 Years
Teacher is Black	0.06	0.072	0.058	0.06
Teacher is Hispanic	0.025	0.017	0.014	0.012
Teacher is Asian	0.018	0.011	0.006	0.003
Teacher is Highly Qualified	0.842	0.858	0.844	0.872
Teacher Teaches Math	0.073	0.069	0.079	0.077
Teacher Teaches Science	0.059	0.048	0.056	0.062
Teacher Teaches English	0.096	0.092	0.075	0.102
Teacher Teaches Social Studies	0.051	0.045	0.061	0.075
Teacher Teaches Special Ed.	0.141	0.171	0.207	0.12
Elementary Teacher	0.757	0.787	0.75	0.751
Secondary Teacher	0.243	0.213	0.25	0.249
Has Advanced Degree	0.206	0.303	0.602	0.79
Teacher Age	31.4	35.5	40.9	49.4
Fraction of Poor Students	0.607	0.559	0.478	0.472
Fraction of Black Students	0.318	0.286	0.193	0.183
Number of Teachers	2,418	2,853	2,801	2,371
Number of Schools	1,322	1,626	1,604	1,400

Sample includes all full-time teachers in any Michigan public school for the 2015-2016 school year who were hired 1 year, 5 years, 10 years, or 20 years prior to the date of observation.

Table III.4.: Mobility Outcomes by Experience

	Year 2	Year 3	Year 4	Year 5
Full-Time Teacher in Same School	0.775	0.609	0.5	0.426
Full-Time Teacher in New District	0.018	0.091	0.134	0.161
Full-Time Teacher Elsewhere in Same District	0.057	0.081	0.094	0.1
Part-Time Teacher or Non-Teaching Staff	0.041	0.056	0.064	0.075
Left Michigan Public Schools	0.109	0.164	0.207	0.236

All outcomes are relative to the first year of teaching in Michigan public schools. The sample includes all teachers who accepted a full-time teaching job in any Michigan public school between the 2003-2004 school year and the 2011-2012 school year. The sample of teachers is restricted to those who accepted their first job in a school that remained open for at least five years thereafter.

Table III.5.: Mobility Outcomes by Cohort

	2004	2005	2006	2007	2008	2009	2010	2011	2012
Full-Time Teacher in Same School	0.435	0.438	0.458	0.442	0.445	0.452	0.381	0.378	0.321
Full-Time Teacher in New District	0.151	0.153	0.127	0.147	0.146	0.144	0.214	0.203	0.253
Full-Time Teacher Elsewhere in Same District	0.107	0.098	0.121	0.117	0.089	0.106	0.061	0.096	0.053
Part-Time Teacher or Non-Teaching Staff	0.065	0.074	0.074	0.083	0.085	0.068	0.08	0.078	0.079
Left Michigan Public Schools	0.241	0.236	0.219	0.21	0.235	0.228	0.262	0.245	0.294
Number of Teachers	3,425	3,536	3,266	2,514	1,987	1,817	1,371	2,227	1,235

All outcomes reflect the teachers' status five years after they are hired for their first full-time teaching job in the Michigan public schools. The sample includes all teachers who accepted a full-time teaching job in any Michigan public school between the 2003-2004 school year and the 2011-2012 school year. The sample of teachers is restricted to those who accepted their first job in a school that remained open for at least five years thereafter.

Table III.6.: Mobility Outcomes by Fraction Poor

	Fewest Poor Students	2^{nd}	3^{rd}	Most
Full-Time Teacher in Same School	0.534	0.489	0.415	0.273
Full-Time Teacher in New District	0.095	0.131	0.18	0.245
Full-Time Teacher Elsewhere in Same District	0.117	0.114	0.102	0.069
Part-Time Teacher or Non-Teaching Staff	0.071	0.068	0.074	0.084
Left Michigan Public Schools	0.184	0.198	0.23	0.329
Number of Teachers	5,232	5,228	5,234	5,229
Number of Schools	1,141	1,392	1,434	890

All outcomes are measured five years after the date of hire. The sample includes all teachers who accepted a full-time teaching job in any Michigan public school between the 2003-2004 school year and the 2011-2012 school year, conditional on the school remaining open for at least five years after the initial date of hire. Quartiles are defined by the percent of students eligible for free or reduced-price lunch at the school level. Cutoffs for these quartiles are 19%, 39%, and 66% for the 25^{th}, 50^{th}, and 75^{th} percentiles, respectively.

Table III.7.: Contributing Factors to Mobility, by Next Job Type

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	2.37***	1.09	1.24	1.35**
Fraction of Black Students	1.89***	2.49***	1.07	2.13***
Fraction of Hispanic Students	0.451***	0.264***	1.002	0.772
Fraction of English Learners	1.33	9.41***	1.23	1.74**
Fraction of Special Ed. Students	1.04	1.87***	1.5*	1.09
2nd Quintile Test Scores	0.863	1.42**	0.831	0.826**
3rd Quintile Test Scores	0.696***	1.25	0.81*	0.742***
4th Quintile Test Scores	0.596***	1.18	0.717**	0.624***
5th Quintile Test Scores	0.45***	1.29	0.579***	0.622***
First School is a Charter	1.61***	0.073***	1.65***	1.87***
First School is Urban	0.956	1.5***	1.11	1.1
First School is Town/Rural	0.785**	0.908	1.001	0.952
First School is Secondary	1.28***	0.428***	1.25***	1.28***
Avg. Frac. Enrollment Change	1.51***	0.969	1.36***	1.01
Local Unemployment Rate	1.01	0.996	1.01	0.995
Per-Pupil Expenditure ($1,000s)	0.977*	1.01	0.994	0.994
County Avg. Per-Pupil Exp.	0.967	1.05	1.2**	1.04
Teacher is Highly Qualified	1.06	0.881	0.724***	0.77***
Teacher's Age	0.975***	1.004	1.005	1.002
Teacher is Female	0.945	1.05	0.905	0.931
Teacher is Black	0.996	1.06	1.24*	0.841*
Teacher is Hispanic	0.79	0.955	0.945	0.811
Teaches Math/Science	1.11*	1.01	0.774***	1.1*
Teaches Special Education	2.08***	1.15*	1.66***	1.15**
Attended Selective College	1.07	1.001	1.26***	1.23***
Attended College in Michigan	1.11	1.05	0.842**	0.606***
Has Advanced Degree	1.03	1.14*	1.66***	1.09
Entered Teaching in 2004-05	0.963	0.91	1.19*	1.04
Entered Teaching in 2005-06	0.794***	1.09	1.27**	1.02
Entered Teaching in 2006-07	0.934	1.05	1.36***	0.95
Entered Teaching in 2007-08	0.846*	0.824*	1.27**	0.945
Entered Teaching in 2008-09	0.808*	0.931	0.95	0.928
Entered Teaching in 2009-10	1.18	0.701**	1.11	1.1
Entered Teaching in 2010-11	1.32***	1.02	1.38**	1.26**
Entered Teaching in 2011-12	1.86***	1.16	0.653**	0.338***
Number of Observations	21,336	21,336	21,336	21,336
Sample Mean	0.145	0.108	0.073	0.228

Multinomial logit odds ratios reported. Outcome variable is conditional on the teacher's first school remaining open five years after the date of hire. School/student characteristics are measured in the first school at the time of hire unless stated otherwise. Standard errors (available on request) are clustered at the school level. Missing indicators are included, not shown, for any variables that are ever missing.

Table III.8.: Contributing Factors to Mobility, by Title I Status of Next School

	To Non-Title I School	To Title I School	Private Sector
Frac. Subsidized Lunch Eligible	0.924	3.39***	1.57***
Fraction of Black Students	2.01***	2.27***	1.81***
Fraction of Hispanic Students	0.36***	0.393***	0.416***
Fraction of English Learners	3.23***	2.4***	1.43***
Fraction of Special Ed. Students	1.02	1.76***	2.13***
2nd Quintile Test Scores	0.941	1.05	0.645
3rd Quintile Test Scores	0.849	0.891	1.94***
4th Quintile Test Scores	0.821	0.745**	1.14
5th Quintile Test Scores	0.936	0.425***	0.829**
First School is a Charter	0.745***	0.817**	1.83***
First School is Urban	1.14*	1.16*	1.14**
First School is Town/Rural	0.738***	0.96	0.999
First School is Secondary	0.894	0.786***	1.22***
Avg. Frac. Enrollment Change	1.44***	1.44***	1.001
Local Unemployment Rate	0.985	1.02*	0.993
Per-Pupil Expenditure ($1,000s)	1.004	0.989	0.992
County Avg. Per-Pupil Exp.	1.1	0.89	1.02
Teacher is Highly Qualified	0.925	1.05	0.733***
Teacher's Age	0.977***	0.996	1.002
Teacher is Female	0.933	1.07	0.948
Teacher is Black	0.399***	1.37***	0.838*
Teacher is Hispanic	0.972	0.806	0.803
Teaches Math/Science	1.09	1.04	1.11*
Teaches Special Education	1.51***	1.68***	1.1
Attended Selective College	1.03	1.03	1.2***
Attended College in Michigan	1.08	1.06	0.587***
Has Advanced Degree	1.21**	0.989	1.09
Entered Teaching in 2004-05	0.916	0.955	1.04
Entered Teaching in 2005-06	0.891	0.966	1.06
Entered Teaching in 2006-07	0.937	0.971	0.918
Entered Teaching in 2007-08	0.712***	0.917	0.936
Entered Teaching in 2008-09	0.883	0.793*	0.89
Entered Teaching in 2009-10	1.23	0.809	1.05
Entered Teaching in 2010-11	1.3***	1.06	1.25**
Entered Teaching in 2011-12	1.91***	1.49**	0.31***
Number of Observations	19,097	19,097	19,097
Sample Mean	0.144	0.137	0.221

Multinomial logit odds ratios reported.Outcome variable is conditional on the teacher's first school remaining open five years after the date of hire. School/student characteristics are measured in the first school at the time of hire unless stated otherwise. Standard errors (available on request) are clustered at the school level. Missing indicators are included, not shown, for any variables that are ever missing.

Table III.9.: Contributing Factors to Mobility, Without Teacher Characteristics

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	2.36***	1.1	1.12	1.24*
Fraction of Black Students	1.76***	2.41***	1.32	2.62***
Fraction of Hispanic Students	0.463***	0.23***	1.06	0.925
Fraction of English Learners	1.26	9.49***	1.33	1.91**
Fraction of Special Ed. Students	1.28	2.02***	1.94***	1.23
2nd Quintile Test Scores	0.882	1.42**	0.841	0.838**
3rd Quintile Test Scores	0.723***	1.24	0.868	0.779***
4th Quintile Test Scores	0.637***	1.18	0.777*	0.669***
5th Quintile Test Scores	0.48***	1.28	0.64***	0.677***
First School is a Charter	1.52***	0.076***	1.5***	1.7***
First School is Urban	0.982	1.5***	1.15	1.11
First School is Town/Rural	0.774***	0.91	1.02	0.974
First School is Secondary	1.16**	0.42***	1.05	1.25***
Avg. Frac. Enrollment Change	1.51***	0.958	1.35***	0.997
Local Unemployment Rate	1.01	0.99	1.01	1.002
Per-Pupil Expenditure ($1,000s)	0.982	1.01	1.01	1.01
County Avg. Per-Pupil Exp.	0.95	1.05	1.23**	1.05
Entered Teaching in 2004-05	0.983	0.898	1.14	0.962
Entered Teaching in 2005-06	0.82**	1.07	1.16	0.904
Entered Teaching in 2006-07	1.002	1.03	1.29**	0.861**
Entered Teaching in 2007-08	0.909	0.811*	1.29**	0.909
Entered Teaching in 2008-09	0.837	0.924	0.899	0.838**
Entered Teaching in 2009-10	1.24*	0.703**	1.05	0.97
Entered Teaching in 2010-11	1.3***	1.03	1.26*	1.12
Entered Teaching in 2011-12	1.74***	0.699**	1.41**	1.3**
Number of Observations	21,336	21,336	21,336	21,336
Sample Mean	0.145	0.108	0.073	0.228

Multinomial logit odds ratios reported. Outcome variable is conditional on the teacher's first school remaining open five years after the date of hire. School/student characteristics are measured in the first school at the time of hire unless stated otherwise. Standard errors (available on request) are clustered at the school level. Missing indicators are included, not shown, for any variables that are ever missing.

Table III.10.: Contributing Factors to Mobility, by Pre/Post Recession

(a) Non-Recession Cohorts

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	2.63***	1.17	1.45*	1.46***
Fraction of Black Students	1.99***	2.58***	1.05	2.26***
Fraction of Hispanic Students	0.422***	0.275**	1.15	0.712
Fraction of English Learners	1.22	9.96***	1.43	1.68**
Fraction of Special Ed. Students	0.971	1.66**	1.42	1.05
2nd Quintile Test Scores	0.903	1.48**	0.819	0.897
3rd Quintile Test Scores	0.748**	1.32	0.934	0.798**
4th Quintile Test Scores	0.598***	1.2	0.769*	0.606***
5th Quintile Test Scores	0.502***	1.36*	0.689**	0.626***
First School is a Charter	1.69***	0.085***	1.72***	1.88***
First School is Urban	0.933	1.46***	1.08	1.08
First School is Town/Rural	0.762**	0.938	1.01	0.872
First School is Secondary	1.33***	0.44***	1.24**	1.38***
Avg. Frac. Enrollment Change	1.54***	1.08	1.42***	1.09
Local Unemployment Rate	1.02	1.004	1.03*	0.996
Per-Pupil Expenditure ($1,000s)	0.985	1.02	0.996	0.993
County Avg. Per-Pupil Exp.	0.966	1.01	1.21*	1.03
Teacher is Highly Qualified	1.06	0.843*	0.765***	0.78***
Teacher's Age	0.971***	1.004	1.005	1.001
Teacher is Female	0.937	1.02	0.898	0.95
Teacher is Black	0.946	1.01	1.28*	0.803**
Teacher is Hispanic	0.886	0.945	0.902	0.954
Teaches Math/Science	1.08	1.04	0.765**	1.13**
Teaches Special Education	2.34***	1.2**	1.69***	1.24***
Attended Selective College	1.07	0.993	1.37***	1.27***
Attended College in Michigan	1.12	1.06	0.812**	0.609***
Has Advanced Degree	1.02	1.12	1.68***	1.08
Entered Teaching in 2004-05	0.964	0.91	1.19*	1.04
Entered Teaching in 2005-06	0.801**	1.09	1.28**	1.02
Entered Teaching in 2006-07	0.929	1.04	1.36***	0.944
Entered Teaching in 2010-11	1.24*	0.966	1.25*	1.23**
Entered Teaching in 2011-12	2***	1.1	0.577***	0.313***
Number of Observations	16,173	16,173	16,173	16,173

Multinomial logit odds ratios reported. Outcome variable is measured after five years and is conditional on the teacher's first school remaining open five years after the date of hire. School/student characteristics are measured in the first school at the time of hire unless stated otherwise. Standard errors (available on request) are clustered at the school level. Missing indicators are included, not shown, for any variables that are ever missing. Non-recession cohorts are 2003-04 through 2006-07 and 2010-11 through 2011-12.

(b) Recession Cohorts

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	1.37	0.791	0.578	1.07
Fraction of Black Students	1.72**	2.18**	0.95	1.94***
Fraction of Hispanic Students	0.581	0.217**	0.52	1.04
Fraction of English Learners	1.83	8.57***	0.908	0.201
Fraction of Special Ed. Students	1.42	3.94**	1.36	1.14
2nd Quintile Test Scores	0.753*	1.26	0.827	0.681***
3rd Quintile Test Scores	0.559***	1.1	0.484***	0.642***
4th Quintile Test Scores	0.536***	1.12	0.474**	0.675**
5th Quintile Test Scores	0.3***	1.11	0.279***	0.618**
First School is a Charter	1.42**	0.041***	1.56**	1.81***
First School is Urban	1.01	1.57***	1.15	1.13
First School is Town/Rural	0.819	0.797	0.921	1.11
First School is Secondary	1.15	0.377***	1.35**	1.07
Avg. Frac. Enrollment Change	1.51***	0.533**	1.03	0.735**
Local Unemployment Rate	0.991	0.984	0.99	0.993
Per-Pupil Expenditure ($1,000s)	0.94	0.975	0.987	0.993
County Avg. Per-Pupil Exp.	0.965	1.19	1.21	1.08
Teacher is Highly Qualified	1.04	1.03	0.622***	0.745**
Teacher's Age	0.983***	1.004	1.003	1.002
Teacher is Female	0.968	1.23	0.93	0.881
Teacher is Black	1.15	1.27	1.16	0.963
Teacher is Hispanic	0.666	1.07	1.01	0.624
Teaches Math/Science	1.2	0.912	0.801	0.997
Teaches Special Education	1.39**	0.963	1.58**	0.876
Attended Selective College	1.06	1.01	1.04	1.09
Attended College in Michigan	1.08	1.01	0.938	0.598***
Has Advanced Degree	1.06	1.2	1.64***	1.1
Entered Teaching in 2007-08	0.598***	1.09	0.889	0.848
Entered Teaching in 2008-09	0.624***	1.26	0.794	0.812**
Number of Observations	5,163	5,163	5,163	5,163

Multinomial logit odds ratios reported. Outcome variable is measured after five years and is conditional on the teacher's first school remaining open five years after the date of hire. School/student characteristics are measured in the first school at the time of hire unless stated otherwise. Standard errors (available on request) are clustered at the school level. Missing indicators are included, not shown, for any variables that are ever missing. Recession cohorts are 2007-08 through 2009-10.

Table III.11.: Effects of All Turnover on Test Scores

	Math	Reading
Fraction Leaving After This Year	-0.036***	-0.019***
	(0.002)	(0.002)
Fraction Left Last Year	-0.057***	-0.038***
	(0.003)	(0.003)
P-value from Test of Equal Coefficients	0.029	0.013
Number of Observations	6,754,830	5,634,372

OLS results shown. All regressions control for student and school characteristics, including the student's last observed test score. Fractions leaving include all teachers who stop teaching full-time at the given school except for retirements and temporary exits that are coded as parental leave. Standard errors in parentheses, clustered by school.

Table III.12.: Effects of Types of Turnover on Test Scores

	Math	Reading
In-District Move	-0.041**	-0.026**
	(0.017)	(0.012)
Cross-District Move	-0.128***	-0.085***
	(0.03)	(0.025)
To Part-Time/Administration	-0.029	0.051
	(0.046)	(0.036)
To Private Sector	-0.063*	-0.023
	(0.035)	(0.028)
Retires	-0.011	-0.016
	(0.02)	(0.016)
Number of Observations	6,743,698	5,623,245

OLS results shown. All regressions control for student and school characteristics, including the student's last observed test score, and school fixed effects. Coefficients shown are on the fractions of teachers who exited in the given fashion after the previous school year. Standard errors in parentheses, clustered by school.

Table III.13.: Effects of Black Teacher Exits on Black Students' Scores

	Math	Reading
Fraction Leaving After This Year	-0.052**	-0.027
	(0.022)	(0.017)
Fraction Left Last Year	-0.045**	-0.024
	(0.019)	(0.016)
Number of Observations	1,154,865	976,892

OLS results shown. All regressions control for student and school characteristics, including the student's last observed test score. Fractions leaving include all teachers who stop teaching full-time at the given school except for retirements and temporary exits that are coded as parental leave. Standard errors in parentheses, clustered by school.

Bibliography

[1] Adnot, M., Dee, T., Katz, V., & Wyckoff, J. (2016). Teacher Turnover, Teacher Quality, and Student Achievement in DCPS. *Educational Evaluation and Policy Analysis*, 1-23.

[2] Altonji, J.G., Kahn, L.B., & Speer, J.D. (2016). Cashier or Consultant? Labor Market Conditions, Field of Study, and Career Success. *Journal of Labor Economics*, 361-401.

[3] Boyd, D., Lankford, H., & Loeb, S.W. (2005). The Draw of Home: How Teachers' Preferences for Proximity Disadvantage Urban Schools. *Journal of Policy Analysis and Management*, 113-132.

[4] Boyd, D., Lankford, H., Loeb, S., Ronfeldt, M., & Wyckoff, J. (2011). The Role of Teacher Quality in Retention and Hiring: Using Applications to Transfer to Uncover Preferences of Teachers and Schools. *Journal of Policy Analysis and Management*, 88-110.

[5] Boyd, D., Lankford, H., Loeb, S., & Wyckoff, J. (2013). Analyzing the Determinants of the Matching of Public School Teachers to Jobs: Disentangling the Preferences of Teachers and Employers. *Journal of Labor Economics*, 83-117.

[6] Bridges, E.M. (1991). Evaluation for Tenure and Dismissal. In J. Millman & L. Darling-Hammond, *The New Handbook of Teacher Evaluation: Assessing Elementary and Secondary Teachers* (pp. 147-157). Newbury Park, CA: Corwin Press.

[7] Clotfelter, C.T., Ladd, H.F., & Vigdor, J.L. (2007). Teacher Credentials and Student Achievement: Longitudinal Analysis with Student Fixed Effects. *Economics of Education Review*, 673-682.

[8] Emdin, C. (2016, August 27). Why Black Men Quit Teaching. *New York Times*.

[9] Frey, W., & University of Michigan Social Science Data Analysis Network (SSDAN). *New Racial Segregation Measures for States and Large Metropolitan Areas: Analysis of the 2005-2009 American Community Survey*. Accessed April 5, 2018. `https://www.psc.isr.umich.edu/dis/census/segregation.html`.

[10] Goldhaber, D., Gross, B., & Player, D. (2011). Teacher Career Paths, Teacher Quality, and Persistence in the Classroom: Are Public Schools Keeping Their Best?. *Journal of Policy Analysis and Management*, 57-87.

[11] Goldhaber, D., Lavery, L., & Theobald, R. (2015). Uneven Playing Field? Assessing the Teacher Quality Gap Between Advantaged and Disadvantaged Students. *Education Researcher*, 1-15.

[12] Hansen, M., Backes, B., & Brady, V. (2016). Teacher Attrition and Mobility During the Teach for America Clustering Strategy in Miami-Dade County Public Schools. *Educational Evaluation and Policy Analysis*, 495-516.

[13] Hanushek, E.A. & Rivkin, S.G. (2007). Pay, Working Conditions, and Teacher Quality. *The Future of Children*, 69-86.

[14] Hanushek, E.A., Rivkin, S.G., & Schiman, J.C. (2016). Dynamic Effects of Teacher Turnover on the Quality of Instruction. *Economics of Education Review*, 132-148.

[15] Kraft, M.A., Brunner, E.J., Dougherty, S.M., & Schwegman, D. (2018). *Teacher Accountability Reforms and the Supply of New Teachers*. Working paper.

[16] Lankford, H., Loeb, S., & Wyckoff, J. (2002). Teacher Sorting and the Plight of Urban Schools: A Descriptive Analysis. *Educational Evaluation and Policy Analysis*, 37-62.

[17] Levin, J. & Quinn, M. (2003). Missed Opportunities: How We Keep High-Quality Teachers Out of Urban Classrooms. *The New Teacher Project.*

[18] Loeb, S., Darling-Hammond, L., & Luczak, J. (2005). How Teaching Conditions Predict Teacher Turnover in California Schools. *Peabody Journal of Education*, 44-70.

[19] Lortie, D.C. (1977). Schoolteacher: A Sociological Study.

[20] Miller, L.C. (2012). Situating the Rural Teacher Labor Market in the Broader Context: A Descriptive Analysis of the Market Dynamics in New York State. *Journal of Research in Rural Education*, 1-31.

[21] Murnane, R.J. & Steele, J.L. (2007). What Is the Problem? The Challenge of Providing Effective Teachers for All Children. *The Future of Children*, 15-43.

[22] Oreopoulos, P., von Wachter, T., & Heisz, A. (2012). The Short and Long-Term Career Effects of Graduating in a Recession. *American Economic Journal: Applied Economics*, 1-29.

[23] Ronfeldt, M., Loeb, S., & Wyckoff, J. (2013). How Teacher Turnover Harms Student Achievement. *American Education Research Journal*, 4-36.

[24] Springer, M.G., Swain, W.A., & Rodriguez, L.A. (2015). Effective Teacher Retention Bonuses: Evidence from Tennessee. *Educational Evaluation and Policy Analysis*, 1-23.

[25] Stinebrickner, T.R. (2001). A Dynamic Model of Teacher Labor Supply. *Journal of Labor Economics*, 196-230.

[26] Sun, M., Saultz, A., & Ye, Y. (2017). Federal Policy and the Teacher Labor Market: Exploring the Effects of NCLB School Accountability on Teacher Turnover. *School Effectiveness and School Improvement*, 102-122.

[27] Teach for America (2018). *Detroit*. Accessed April 5, 2018. `https://detroit.teachforamerica.org/homepage`.

[28] Tyson, K. (2003). Notes from the Back of the Room: Problems and Paradoxes in the Schooling of Young Black Students. *Sociology of Education*, 326-343.

C Appendix Tables

Table III.14.: Contributing Factors to Mobility, by Pre-Service Qualifications

(a) Zero Qualifications

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	2.63**	3.26***	0.776	1.07
Fraction of Black Students	3.32**	1.45	3.4**	3.83***
Fraction of Hispanic Students	0.916	0.075**	14.6***	1.42
Fraction of English Learners	2.94	43.7***	0.454	4.44***
Fraction of Special Ed. Students	1.53	1.84	1.32	0.594
2nd Quintile Test Scores	0.62*	0.787	0.558*	0.678*
3rd Quintile Test Scores	0.54*	0.731	1.23	0.583**
4th Quintile Test Scores	0.772	1.17	1.13	0.727
5th Quintile Test Scores	0.554	1.08	0.471	0.636
First School is a Charter	1.59*	0.079***	1.86**	1.55**
First School is Urban	0.743	1.05	0.914	0.807
First School is Town/Rural	0.532**	0.729	0.98	0.914
First School is Secondary	0.887	0.461***	1.01	0.999
Avg. Frac. Enrollment Change	1.222**	0.948	1.02	1.04
Local Unemployment Rate	0.981	0.986	0.956	1.02
Per-Pupil Expenditure ($1,000s)	0.972	0.96	0.997	0.975
County Avg. Per-Pupil Exp.	0.938	1.03	0.884	1.14
Teacher's Age	0.965***	0.99	1.01	1.02
Teacher is Female	1.11	1.14	0.908	1.17
Teacher is Black	0.362***	1.07	2.6**	1.25
Teacher is Hispanic	1.19	0.519	1.44	0.599
Teaches Math/Science	1.15	0.91	0.553	1.02
Teaches Special Education	1.55**	1.17	1.01	0.896
Attended College in Michigan	0.983	0.862	0.62**	0.441***
Entered Teaching in 2004-05	0.839	1.05	0.891	1.3
Entered Teaching in 2005-06	1.34	1.42	0.984	1.61**
Entered Teaching in 2006-07	0.792	0.965	0.923	1.14
Entered Teaching in 2007-08	0.76	0.773	0.982	1.46*
Entered Teaching in 2008-09	0.874	0.915	1.4	1.1
Entered Teaching in 2009-10	1.34	0.653	1.29	1.2
Entered Teaching in 2010-11	1.95**	1.22	1.11	1.59*
Entered Teaching in 2011-12	1.49	1.23	1.58	1.66
Number of Observations	2,356	2,356	2,356	2,356

Multinomial logit odds ratios reported. Outcome variable is measured after five years and is conditional on the teacher's first school remaining open five years after the date of hire. Pre-service qualifications are master's degrees, bachelor's degrees from highly selective colleges, and classifications as "highly qualified" under NCLB.

(b) One Qualification

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	2.72***	0.891	1.52*	1.54***
Fraction of Black Students	1.83***	2.37***	1.04	2.18***
Fraction of Hispanic Students	0.37***	0.211***	0.923	0.802
Fraction of English Learners	1.23	9.35***	1.31	1.68**
Fraction of Special Ed. Students	1.06	2.21***	1.22	1.2
2^{nd} Quintile Test Scores	0.921	1.5**	0.942	0.9
3^{rd} Quintile Test Scores	0.753**	1.27	0.849	0.811**
4^{th} Quintile Test Scores	0.625***	1.12	0.761	0.615***
5^{th} Quintile Test Scores	0.488***	1.39*	0.724*	0.702***
First School is a Charter	1.58***	0.079***	1.72***	1.94***
First School is Urban	0.994	1.52***	1.09	1.14
First School is Town/Rural	0.798*	0.843	1.04	1.08
First School is Secondary	1.33***	0.429***	1.31**	1.33***
Avg. Frac. Enrollment Change	1.62***	0.868	0.861	
Local Unemployment Rate	1.01	0.9994	1.43***	0.987
Per-Pupil Expenditure ($1,000s)	0.973	1.02	1.005	1.01
County Avg. Per-Pupil Exp.	0.91	0.996	1.17	0.991
Teacher's Age	0.975***	1.01*	1.02***	1.004
Teacher is Female	0.948	1.03	0.921	0.895**
Teacher is Black	1.09	1.07	1.16	0.933
Teacher is Hispanic	0.751	0.894	0.979	0.817
Teaches Math/Science	1.16**	1.01	0.819	1.17**
Teaches Special Education	2.15***	1.08	2.41***	1.4***
Attended College in Michigan	1.13	1.22**	0.726***	0.453***
Entered Teaching in 2004-05	0.958	0.893	1.35**	1.05
Entered Teaching in 2005-06	0.787**	0.997	1.29*	1.05
Entered Teaching in 2006-07	0.918	1.01	1.56***	0.974
Entered Teaching in 2007-08	0.879	0.816	1.72***	1.01
Entered Teaching in 2008-09	0.744**	0.819	1.16	0.996
Entered Teaching in 2009-10	1.15	0.666**	1.2	1.16
Entered Teaching in 2010-11	1.3**	1.01	1.53**	1.33***
Entered Teaching in 2011-12	1.66***	0.607**	1.7***	1.19
Number of Observations	14,112	14,112	14,112	14,112

Multinomial logit odds ratios reported. Outcome variable is measured after five years and is conditional on the teacher's first school remaining open five years after the date of hire. Pre-service qualifications are master's degrees, bachelor's degrees from highly selective colleges, and classifications as "highly qualified" under NCLB.

(c) Two or More Qualifications

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	1.67*	0.823	0.746	1.05
Fraction of Black Students	1.88**	4.08***	0.864	1.96***
Fraction of Hispanic Students	0.829	0.76	0.609	0.963
Fraction of English Learners	1.11	4.82**	1.01	1.31
Fraction of Special Ed. Students	0.397*	0.89	1.41	0.769
2$^{\text{nd}}$ Quintile Test Scores	0.763	1.43	0.683*	0.627***
3$^{\text{rd}}$ Quintile Test Scores	0.568***	1.37	0.689*	0.653**
4$^{\text{th}}$ Quintile Test Scores	0.465***	1.33	0.603**	0.658**
5$^{\text{th}}$ Quintile Test Scores	0.337***	1.001	0.411***	0.512***
First School is a Charter	1.62***	0.051***	1.36**	2.09***
First School is Urban	0.865	1.65***	1.36**	1.09
First School is Town/Rural	0.858	1.23	0.863	0.676**
First School is Secondary	1.25*	0.406	1.19	1.37***
Avg. Frac. Enrollment Change	1.65***	1.29	1.59***	1.28
Local Unemployment Rate	1.02	0.991	1.04**	1.01
Per-Pupil Expenditure ($1,000s)	0.996	1.05*	0.962	0.989
County Avg. Per-Pupil Exp.	1.2	1.14	1.6***	1.22
Teacher's Age	0.978***	1.01	1.01	1.01**
Teacher is Female	0.848	1.06	0.906	0.904
Teacher is Black	1.11	0.972	0.971	0.615***
Teacher is Hispanic	0.642	1.23	0.638	1.08
Teaches Math/Science	0.913	0.989	0.633***	0.87
Teaches Special Education	2.24***	1.28*	2.03***	1.28**
Attended College in Michigan	1.09	0.918	0.799*	0.591***
Entered Teaching in 2004-05	1.01	0.916	1.13	0.872
Entered Teaching in 2005-06	0.67**	1.21	1.56**	0.895
Entered Teaching in 2006-07	1.02	1.09	1.37	0.849
Entered Teaching in 2007-08	0.793	0.862	0.911	0.804
Entered Teaching in 2008-09	0.918	1.23	0.709	0.732*
Entered Teaching in 2009-10	1.11	0.805	1.03	0.887
Entered Teaching in 2010-11	1.07	0.922	1.5*	1.18
Entered Teaching in 2011-12	1.93***	1.31	1.26	0.995
Number of Observations	4,868	4,868	4,868	4,868

Multinomial logit odds ratios reported. Outcome variable is measured after five years and is conditional on the teacher's first school remaining open five years after the date of hire. Pre-service qualifications are master's degrees, bachelor's degrees from highly selective colleges, and classifications as "highly qualified" under NCLB.

Table III.15.: Contributing Factors to Two-Year Mobility, by Next Job Type

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	2.04**	0.97	1.04	1.44**
Fraction of Black Students	1.04	1.96***	0.949	1.65***
Fraction of Hispanic Students	0.291*	0.518	0.935	1.05
Fraction of English Learners	1.7	4.31***	0.99	0.917
Fraction of Special Ed. Students	0.475*	1.54	1.31	0.952
2nd Quintile Test Scores	0.632***	1.03	0.742**	0.777***
3rd Quintile Test Scores	0.619**	1.14	0.732**	0.814**
4th Quintile Test Scores	0.507***	0.947	0.795	0.657***
5th Quintile Test Scores	0.427***	1.12	0.86	0.648***
First School is a Charter	1.31	0.104***	0.99	1.54***
First School is Urban	1.06	1.76***	1.06	1.07
First School is Town/Rural	1.25	1.14	1.07	1.04
First School is Secondary	1.2	0.403***	1.06	1.09
Avg. Frac. Enrollment Change	0.722	0.949	0.989	0.455***
Local Unemployment Rate	1.001	0.997	0.976*	0.984*
Per-Pupil Expenditure ($1,000s)	0.924*	1.01	0.989	0.992
County Avg. Per-Pupil Exp.	0.995	0.989	0.844	0.966
Teacher is Highly Qualified	0.643**	0.983	0.485***	0.596***
Teacher's Age	0.998	1.01	1.02***	1.01***
Teacher is Female	0.888	1.16*	1.03	0.89**
Teacher is Black	1.56**	0.902	1.62***	0.807**
Teacher is Hispanic	0.624	0.538*	1.1	0.882
Teaches Math/Science	1.44**	0.959	0.87	1.06
Teaches Special Education	1.5**	0.818**	0.904	0.819**
Attended Selective College	1.03	0.948	1.08	1.1
Attended College in Michigan	0.842	0.978	0.713***	0.619***
Has Advanced Degree	1.17	0.931	0.966	1.05
Entered Teaching in 2004-05	0.959	0.636***	1.62***	0.985
Entered Teaching in 2005-06	1.02	0.952	1.69***	1.06
Entered Teaching in 2006-07	0.891	1.17	1.92***	1.14
Entered Teaching in 2007-08	0.751	0.797	1.76***	0.997
Entered Teaching in 2008-09	0.813	0.892	2.39***	1.16
Entered Teaching in 2009-10	1.82**	1.21	3.24***	0.989
Entered Teaching in 2010-11	1.11	1.17	4.66***	1.38***
Entered Teaching in 2011-12	3.65***	1.1	1.52*	0.395***
Number of Observations	21,298	21,298	21,298	21,298
Sample Mean	0.018	0.057	0.041	0.109

Multinomial logit odds ratios reported. Outcome variable is conditional on the teacher's first school remaining open five years after the date of hire. School/student characteristics are measured in the first school at the time of hire unless stated otherwise. Standard errors (available on request) are clustered at the school level. Missing indicators are included, not shown, for any variables that are ever missing.

Table III.16.: Contributing Factors to Three-Year Mobility, by Next Job Type

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	2.17***	1.26	1.75***	1.59***
Fraction of Black Students	1.5***	2.14***	1.21	1.79***
Fraction of Hispanic Students	0.334***	0.468	0.569	0.687*
Fraction of English Learners	1.42	5.48***	1.59	1.44*
Fraction of Special Ed. Students	0.789	1.24	1.73**	0.988
2nd Quintile Test Scores	0.72***	1.19	0.899	0.815***
3rd Quintile Test Scores	0.711***	1.2	0.899	0.84**
4th Quintile Test Scores	0.576***	1.09	0.85	0.654***
5th Quintile Test Scores	0.463***	1.32*	0.857	0.67***
First School is a Charter	1.33***	0.091***	1.05	1.58***
First School is Urban	0.905	1.64***	1.14	1.09
First School is Town/Rural	0.794*	1.08	1.19	0.961
First School is Secondary	1.14*	0.431***	1.14	1.09
Avg. Frac. Enrollment Change	1.01	0.968	1.001	0.755***
Local Unemployment Rate	1.01	0.996	0.977**	0.993
Per-Pupil Expenditure ($1,000s)	0.989	1.02*	0.999	0.993
County Avg. Per-Pupil Exp.	1.03	1.09	0.891	0.99
Teacher is Highly Qualified	0.94	0.904	0.58***	0.677***
Teacher's Age	0.98***	1.004	1.02***	1.004*
Teacher is Female	0.9*	1.15**	1.03	0.923*
Teacher is Black	1.1	1.08	1.17	0.892
Teacher is Hispanic	1.1	0.878	1.23	1.05
Teaches Math/Science	1.35***	0.996	0.875	1.08
Teaches Special Education	1.66***	0.99	1.13	0.845**
Attended Selective College	0.982	0.947	1.16*	1.2***
Attended College in Michigan	1.04	1.04	0.817**	0.646***
Has Advanced Degree	1.26***	1.05	1.3***	0.967
Entered Teaching in 2004-05	0.986	0.916	1.13	0.953
Entered Teaching in 2005-06	0.908	1.22*	1.1	1.15*
Entered Teaching in 2006-07	0.924	1.1	1.34**	1.04
Entered Teaching in 2007-08	0.789**	0.858	1.25	0.987
Entered Teaching in 2008-09	0.79*	1.003	1.2	0.893
Entered Teaching in 2009-10	1.01	1.12	1.83***	1.08
Entered Teaching in 2010-11	0.785*	1.08	1.76***	1.21**
Entered Teaching in 2011-12	1.94***	1.48*	0.552***	0.311***
Number of Observations	21,292	21,292	21,292	21,292
Sample Mean	0.091	0.081	0.056	0.164

Multinomial logit odds ratios reported. Outcome variable is conditional on the teacher's first school remaining open five years after the date of hire. School/student characteristics are measured in the first school at the time of hire unless stated otherwise. Standard errors (available on request) are clustered at the school level. Missing indicators are included, not shown, for any variables that are ever missing.

Table III.17.: Contributing Factors to Four-Year Mobility, by Next Job Type

	New District	Same District	Part-Time	Private Sector
Frac. Subsidized Lunch Eligible	2.44***	1.09	1.9***	1.41***
Fraction of Black Students	1.52***	2.2***	1.03	1.85***
Fraction of Hispanic Students	0.327***	0.268***	0.941	0.682
Fraction of English Learners	1.35	8.55***	1.04	1.66***
Fraction of Special Ed. Students	1.06	1.5*	1.3	0.993
2^{nd} Quintile Test Scores	0.765***	1.28*	0.942	0.763***
3^{rd} Quintile Test Scores	0.611***	1.13	0.857	0.696***
4^{th} Quintile Test Scores	0.537***	0.993	0.678**	0.6***
5^{th} Quintile Test Scores	0.445***	1.19	0.739*	0.608***
First School is a Charter	1.51***	0.079***	1.46***	1.85***
First School is Urban	0.899	1.55***	1.02	1.1
First School is Town/Rural	0.766**	1.11	0.998	1.05
First School is Secondary	1.22***	0.441***	1.05	1.24***
Avg. Frac. Enrollment Change	1.01	0.894	1.004	0.751***
Local Unemployment Rate	1.01	1.001	0.977*	0.997
Per-Pupil Expenditure ($1,000s)	0.99	1.02	0.993	0.9995
County Avg. Per-Pupil Exp.	1.003	1.14	1.16	0.986
Teacher is Highly Qualified	0.997	0.89	0.645***	0.714***
Teacher's Age	0.977***	1.005	1.01**	1.002
Teacher is Female	0.956	1.13**	1.09	0.941
Teacher is Black	1.01	1.05	1.28**	0.894
Teacher is Hispanic	0.979	0.816	1.39	0.923
Teaches Math/Science	1.26***	1.03	0.871	1.1*
Teaches Special Education	1.97***	1.1	1.47***	1.06
Attended Selective College	0.996	1.03	1.21**	1.16***
Attended College in Michigan	1.06	1.001	0.732***	0.602***
Has Advanced Degree	1.09	1.02	1.33***	1.04
Entered Teaching in 2004-05	0.977	0.987	1.02	1.1
Entered Teaching in 2005-06	0.773***	1.21*	1.32**	1.12
Entered Teaching in 2006-07	0.9	1.12	1.36**	1.1
Entered Teaching in 2007-08	0.792**	0.856	1.26*	0.918
Entered Teaching in 2008-09	0.751**	1.002	1.52***	0.999
Entered Teaching in 2009-10	0.98	0.896	1.32	1.15
Entered Teaching in 2010-11	1.06	1.17	1.38**	1.27
Entered Teaching in 2011-12	1.87***	1.37	0.638**	0.35***
Number of Observations	21,298	21,298	21,298	21,298
Sample Mean	0.134	0.094	0.064	0.207

Multinomial logit odds ratios reported. Outcome variable is conditional on the teacher's first school remaining open five years after the date of hire. School/student characteristics are measured in the first school at the time of hire unless stated otherwise. Standard errors (available on request) are clustered at the school level. Missing indicators are included, not shown, for any variables that are ever missing.