# ESSAYS IN THE ECONOMICS OF EDUCATION

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This dissertation applies econometric methods to estimate the effects of education-related policies in three distinct contexts. Each policy that I examine is implemented at a different level of government, and each has repercussions for different levels and sectors of education in the United States.

In the first chapter, I examine the effect of a federal policy on state education spending. The federal policy that I study is the State Fiscal Stabilization Fund, which was designed to support state education spending during the Great Recession. I use two identification strategies to estimate how this aid program affected states' spending decisions, by sector (K-12 versus higher education) and recession intensity. First, using pre-recession data on each state's education spending as a share of funds, I estimate how federal aid altered spending relative to expected levels given coinciding changes in state revenues. Second, I exploit the program's spending requirements to estimate the effects of its maintenance-of-effort provisions and total-spending targets on states' spending decisions. I find that federal aid substantially increased state education spending during the treatment period, with states spending about all of the funds on education initially, and about 40% after three years. This high rate is partly explained by states' compliance with the program's spending requirements, which set a binding floor on many states' spending on higher education. As a result, higher education spending fell sharply after the program ended.

In the second chapter, I estimate the effect of state occupational licensing policies on vocational higher education. I estimate the effects of different combinations of registration, training, and certification requirements on markets for vocational higher education, focusing on pharmacy technicians. Given recent attention to the growth of the for-profit

college sector, I am particularly interested in how certain licensing requirements may affect for-profit relative to public colleges. Results show that registration requirements on their own have little effect. However, when states require the completion of a training program, college program completions increase substantially, with larger increases in the for-profit sector despite its higher prices. When states require the passage of an examination, professional certifications increase. In both cases, private entities such as for-profit colleges and private certifying bodies have taken on outsize roles, compared to publicly funded and subsidized community colleges and board-written exams.

In the third chapter, my coauthors[1] and I examine the effect of a single public school district's health policies on student outcomes. Public schools are an important setting for public health policy, and increased efforts to connect students with care may pay off not only in terms of immediate quality-of-life improvements but also in terms of other health outcomes and academic achievement. We estimate the effects of increased follow-up efforts within the New York City (NYC) Public School's School Vision Program on a variety of student outcomes. With data from the NYC Office of School Health, we observe students' first and second vision screenings, physical fitness results, visits to the school nurse, attendance, and performance on 3rd grade standardized tests. We exploit a policy rule that targeted increased follow-up efforts to students with a worst visual acuity score of 20/70 or worse, which allows us to implement a regression-discontinuity design. We find robust effects of follow-up efforts on vision outcomes: confirmed eye-exams, wearing glasses in the next screening, passing the next screening, and having substantially improved visual acuity scores in the next screening. Our findings on health and academic outcomes are relatively small and inconsistent compared to those vision findings.

[1]Coauthors on this chapter are Maria Fitzpatrick (Cornell University) and Sophia Day, Kevin Konty, and Jasmina Spasojevic (New York City Office of School Health).

## BIOGRAPHICAL SKETCH

Martha Christine Johnson earned her Bachelor of Arts in Economics from the University of Michigan in 2013. She worked in policy research in Washington, D.C. for three years before enrolling as a doctoral student at Cornell University in 2016. As a student within both the Department of Economics and the Department of Policy Analysis and Management, she concentrated her studies in public and labor economics. Her research focuses on education and health policy topics.

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## TABLE OF CONTENTS

## LIST OF TABLES

## LIST OF FIGURES

CHAPTER 1

# FUNDING SCHOOLS DURING FISCAL CRISES: THE EFFECTS OF FEDERAL RECESSIONARY AID ON STATE SUPPORT FOR PUBLIC EDUCATION

## 1.1 Introduction

States and localities provide most funding for public education, from elementary to higher education, but the federal government plays a substantial role in supporting education spending during crises. For example, the federal government has provided over $200 billion dollars in aid to states for education costs related to the COVID-19 pandemic.[1] A decade earlier, it provided what was at the time an unprecedented amount of nearly $100 billion dollars in education aid during the Great Recession.[2] This aid is a form of insurance against unexpected revenue shortfalls. States face more constraints on spending than the federal government due to balanced-budget rules, yet during recessions their revenues fall while their need for funds rises.[3] To prevent cuts to education spending, the federal government provides fiscal relief, protecting public investment in students' future productivity and the current education workforce. Unlike traditional forms of insurance, however, Congress designs federal aid anew each time a fiscal crisis strikes. Estimating the causal effects of prior aid packages, therefore, can inform the design of federal policy in future crises.

In this paper, I study the effects of the largest education component of federal aid to states during the Great Recession, the State Fiscal Stabilization Fund (SFSF). Totaling about $50 billion, this aid was designed to keep state spending at pre-recession levels in public K-12 and higher education.[4] Because money is fungible, federal aid need not

[1] https://oese.ed.gov/offices/education-stabilization-fund
[2] https://www2.ed.gov/about/offices/list/oig/recoveryact.html
[3] https://www.brookings.edu/articles/state-and-local-budgets-and-the-great-recession/
[4] https://www.ed.gov/open/plan/financial-transparency-sfsf-arra

necessarily increase states' education spending dollar-for-dollar [30, 36, 38]. However, aid programs often include spending requirements that, if followed, would lead to more aid spent on education as intended [2, 54]. I estimate the causal effect of the SFSF on states' spending decisions, compare effects on K-12 versus higher education spending, and explore heterogeneity by recession intensity.

If the federal government shares the same preferences over public spending as state governments, then federal recessionary aid should be unrestricted [54].[5] But with the SFSF, the federal government made several policy choices to influence how states spent federal funds. First, these funds were explicitly set aside for states to spend on public education, rather than any area of spending at states' discretion. Second, the aid was required to be spent within the time-frame of state fiscal years 2009-2011, not saved for potential future needs. Third, states were to maintain their own spending at at a prescribed level rather than use federal funds to reduce spending from their own sources. Finally, the federal government prescribed a target level of spending and required that extra federal funds not required to reach that target be distributed to high-poverty school districts. With these restrictions, the federal government clearly intended to direct funds according to its own preferences.

These restrictions and other features of the program prompt several questions about its causal effects on states' education spending. To what extent did states spend federal funds on education? Did states follow the spending requirements, and if so, were they binding? The K-12 and higher education sectors are affected differently by recession, and states generally have more discretion over higher-education spending. How did the effects of federal aid differ between K-12 and higher education spending? States less affected by the recession may have tried more to save or reallocate federal funds. How

[5]Like [54], when I refer to governments' preferences, I mean to refer to the priorities of these levels of government in response to constituents' preferences. Any consideration of government preference, however, must, as [8] note, take into account the political process.

did effects differ across states by recession intensity? Lastly, what happened after the aid and requirements expired in 2012?

Answering these questions involves estimating how the SFSF altered state support for public education relative to what it would have been otherwise, in the same economic and policy context. This is an important parameter, but difficult to identify. All states received aid, leaving no obvious control group from which to estimate counterfactual spending. Moreover, there were other policy shocks and economic shocks taking place during this time period, the effects of which may be difficult to isolate from the effects of the aid. However, certain aspects of the program facilitate identification. First, the intensity of the recession varied across states while the "intensity" of this federal aid did not, which allows for disentangling the effects of the recession from the effects of the aid. Second, the spending requirements involved specific spending thresholds, the arbitrary nature of which makes it plausible to consider bunching of observations around those thresholds as a reflection of causal responses to the spending requirements. Finally, the program ended abruptly in 2012, whereas the economic recovery was gradual and varied across states. This makes it more likely that similarly abrupt changes in state spending are attributable to the expiration of the program rather than confounding economic factors.

To analyze the effects of the SFSF on state spending, I compiled data at the state-year level covering years 2002-2016 from various sources. Data on state revenue and spending in broad categories comes from the National Association of State Budget Officers' state fiscal surveys. Data on state K-12 education spending, including spending from SFSF funds, comes from National Center for Education Statistics' Common Core of Data. Data on state higher education spending, including from SFSF funds, comes from the State Higher Education Officer Organization's State Support for Higher Education Database.

With this data, I use two identification strategies to estimate causal effects of the SFSF on state spending. In the first, I simulate each state's counterfactual spending in the absence of the aid based on that state's pre-recession share of funds spent on education. Projecting that share of spending forward, I allow state funds to decline with tax revenues and education spending to decline as a result. I then estimate the effect of the SFSF by comparing actual state spending to this projected counterfactual spending. This strategy assumes that the pre-recession relationship between state funds and educational expenditure would continue into the recession years in the absence of the aid. While it is impossible to fully test this assumption, I provide evidence of its plausibility and discuss how results change under alternative assumptions about counterfactual spending.

In the second strategy, I exploit the spending thresholds of the program. I compare the distributions of state spending in relation to the relevant thresholds before and after the requirements applied, testing for characteristics of a binding threshold. I consider the characteristics of a binding threshold to be (1) bunching at the threshold while the requirements apply and (2) an increase in the number of states spending below the threshold after the requirement expires. This strategy assumes that the spending thresholds were arbitrary enough that, if not for the program, I would not observe state spending distributions consistent with these characteristics.

Results show that the federal aid substantially increased state support for public education during the treatment period 2009-2011. It increased K-12 spending by 5% and higher-education spending by nearly 7%, relative to spending in the absence of the aid. The dollar-amounts of these effects sum to about 100% of the SFSF funds actually distributed to states by the Department of Education, suggesting a full pass-through. However, after the program ended, higher-education spending fell sharply and even K-12 spending fell in some states. As a result, the overall pass-through rate of federal funds fell to less than 50% by 2014. Declines in education spending post-SFSF are attributable to the loss of federal funds, but spending requirements played a role as well. The floor on own-spending as well as the target threshold for total-spending were binding on higher education spending for many states, but neither were binding on K-12 spending.

Analyses of heterogeneous effects across states reveal that the estimated effects from both strategies are concentrated in states most negatively impacted by the recession. Interacting treatment with levels of budget shortfall in 2009, I find that federal aid increased K-12 spending by 7% and increased higher education spending by 10% in states with the largest budget shortfalls. The program's spending requirements were also more binding on states with larger budget shortfalls. These results are consistent with smaller-shortfall states being more able to substitute federal funds for state funds while meeting the program's spending requirements, as predicted by a simple model of the state's spending decision.

Several studies have touched on the effects of federal aid for education during the Great Recession more generally, but no academic work to my knowledge has focused on the SFSF in particular. In addition, papers in this literature focus only on one sector of public education, either K-12 or higher education, not both together. For example, many have examined the effects of the Great Recession on K-12 school district spending [4, 6, 18, 29, 50]. Others have done the same for the effect of the recession on higher education [5, 27]. Of these, only [29] examines the direct effect of the SFSF, estimating district-level outcomes in a panel model with state-by-year fixed effects. They find no effect on staffing or total spending but a one-to-one negative effect of SFSF funds on state revenues in districts that received those funds. In contrast to [29], I estimate effects at the state level rather than district level within state-years. Doing so allows me to assess the role of SFSF spending requirements, which are set at the state level, and to compare spending in the K-12 versus higher-education sectors. In general, I contribute to this literature by estimating the causal effects of the SFSF on state education spending and by estimating how the aid interacted with states' spending preferences between K-12 and higher education during recessions.

In addition to providing insight into the effects of an understudied program, this paper contributes to the literature on intergovernmental transfers by examining the role of spending requirements. Research on intergovernmental transfers is often explicitly interested in the phenomenon of flypaper effects of different types, which occur when states do not treat federal funds as fungible [38]. These papers typically ignore spending requirements such as maintenance-of-effort, arguing that they should not matter due to lax enforcement or because the aid is less than what states would spend [20, 44]. If states do respond to maintenance-of-effort and other such spending requirements, however, it is a possible explanation for the varying sizes of flypaper effect estimates in this literature.

## 1.2 The State Fiscal Stabilization Fund

In early 2009, states were beginning to feel the effects of the recession on revenues. Some states were experiencing budget shortfalls, and some were already planning or beginning to cut education spending [45]. The federal government had previously provided a $10 billion-dollar stabilization fund to states following the 2001 recession. This time, they provided a stabilization fund five times larger, specifically to support education spending. Through the SFSF, Congress awarded states $48.6 billion to support public K-12 and higher education. This was part of a larger package of education aid that also included funds allocated to specific programs, including Pell Grant, Individuals with Disabilities Education Act, Title I, Race to the Top, and other smaller programs. Each state's SFSF allocation amount was based entirely on a weighted average of its school-age (5 to 24) population and total population. States could spend the funds during fiscal years 2009 through 2011. Congress instructed states to spend the money quickly, and guidance advised: "Invest one-time ARRA funds thoughtfully to minimize the 'funding cliff'" [1].

### 1.2.1 Spending Requirements

States had significant flexibility over how to distribute their SFSF aid across years or between the K-12 and higher education sectors, but the program included other spending constraints. The following bullets are taken directly from the Department of Education's online guidance:

- States must use their allocations to help restore for FY 2009, 2010, and 2011 support for public elementary, secondary, and postsecondary education to the greater of the FY 2008 or FY 2009 level. The funds needed to restore support for elementary and secondary education must be run through the state's primary elementary and secondary education funding formulae. The funds for higher education must go to [institutions of higher education].
- As part of the state's application, each governor must include an assurance that the state will maintain the same level of support for elementary, secondary, and postsecondary education in FY 2009 through FY 2011 as it did in FY 2006. However, the statute authorizes the Department to waive this maintenance-of-effort requirement under certain conditions.
- If any SFSF funds remain after the state has restored state support for elementary and secondary education and higher education, the state must award the funds to LEAs on the basis of the relative Title I shares but not subject to Title I program requirements.

### 1.2.2 Examples

A couple of simplified examples based on Montana and Missouri can help illustrate these spending requirements. In these examples, Montana received more than enough federal funds to keep total spending at the target levels, while Missouri did not receive enough.

Suppose Montana received \$150 million through the SFSF. It spent \$1.3 billion on K-12 and \$170 million on higher education in 2006, so these were its own-spending floors during 2009-2011. It spent \$1.4 billion on K-12 and \$200 million on higher education in 2009, so these were its total-spending targets during 2009-2011. Suffering little revenue loss during the recession, Montana maintained its own-spending on K-12 at \$1.4 billion during 2010 and 2011, but reduced own-spending on higher education to \$170 million in 2010 and 2011. It used \$60 million of its \$150 million SFSF allocation to increase total-spending on higher education to the target of \$200 million in 2010 and 2011, leaving \$90 million to distribute to K-12 Title-I school districts.

Now suppose Missouri received \$1 billion through SFSF. It spent \$7.5 billion on K-12 and \$900 million on higher education in 2006, so these were its own-spending floors during 2009-2011. It spent \$8.5 billion on K-12 and \$1 billion on higher education in 2009, so these were its total-spending targets during 2009-2011. Suffering large revenue losses from the recession, Missouri reduced its own-spending on K-12 to \$8 billion and own-spending on higher education to \$900 million in 2010 and 2011. It used \$600 million of its \$1 billion SFSF allocation to bring K-12 and higher education spending exactly to the target levels in 2010, but didn't have enough left over in 2011 to do so, using the remaining \$400 million to spend \$8.35 billion on K-12 and \$950 million on higher education in 2011.

In both examples, the states seemed to followed the SFSF spending requirements. They did not reduce their own-spending below the floor, and they used the SFSF dollars to increase spending to the target level as much as possible. As long as Montana sent extra funds to Title-I districts, it followed all requirements. And as long as Missouri failed to reach the target spending levels in 2009-2011 because of revenue shortfalls, it also followed requirements. The case of Missouri shows why the total spending threshold is a "target" rather than a "floor", because the SFSF may not be enough to restore total spending to that level even when the state maintains its own-spending at its 2006 level.

### 1.2.3 Enforcement

Missouri and Montana may appear to have followed the requirements, but the law did not include penalties for states not following them. Instead, accountability mechanisms were limited to assurances and reporting. Each state needed to apply for their SFSF allocation. As part of the application, each state needed to describe its own-spending floor and total-spending targets and provide an assurance that the requirements would be followed, signed by its governor. Then, states that received funds were required to report back to the Department of Education each year describing how funds were spent, the estimated number of jobs saved or created, and the estimated tax increases averted.

Given the combination of assurance, reporting, and lack of penalty, whether or not states decided to follow the requirements likely depended on their expectation of future consequences as well as how closely their own preferences aligned with the federal government's preferences. In the next section I discuss how we might expect states to spend federal aid with and without spending requirements, given their preferences between education and other categories of spending.

## 1.3 Federal Aid and State Spending in a Simple Choice Model

The federal government wanted states to spend their SFSF allocations on public education. However, because money is fungible and allocation amounts were small relative to states' typical education spending levels, states could have used federal dollars to replace state dollars they would have spent regardless of federal aid.[6] Effectively, states could have treated federal aid as general revenue. When states treat federal grant dollars differently from other income, by spending it at a higher rate on its intended spending area for example, public finance economists term this a "flypaper effect." Such effects are surprising because they go against the predictions of a simple consumer-choice model, where the state government is the consumer and the goods are categories of state spending.

Figure 1.1 illustrates the expected effects, in such a model, of federal aid to a state for education during a recession. Education spending is quantified along the $X$-axis, while all other state spending is along the $Y$-axis. Prior to the recession, the state has chosen bundle $A$, where, given its preferences over public education and all other goods, its utility is maximized subject to its budget constraint. Due to the recession's effects on tax revenues, the state's budget constraint shifts inward, and the state chooses to spend at bundle $B$. Observing or predicting this detrimental effect of the recession on state education spending, equal to $ES1$ minus $ES2$, the federal government steps in, providing the state with $G$, exactly the amount needed to restore education spending to its prior level ($G = ES1 - ES2$). However, the state uses $G$ to move not to bundle $F$ (full flypaper effect) but to bundle $C$, which lies between $A$ and $B$ and has the state's preferred spending mix. The result is an increase in education spending, but not to the extent intended by federal policymakers.

[6]SFSF allocations, for states to spend on K-12 and higher education over 2009-2011, were equal to 10% of the amount each state spent on K-12 education in 2008, on average.

Now suppose the federal government expects that states might substitute federal funds for state funds, so they impose a spending floor on state spending from their own funds and a higher floor on total spending. If it sets these floors at $ES2$ (recession spending) and $ES1$ (pre-recession spending), respectively, and states find it in their best interest to follow federal spending requirements, then they would choose bundle $F$. In that case, the effect of federal aid on state spending would look like a full flypaper effect, but it would be fully explained by the state following spending requirements.

In reality, however, the federal government cannot set spending floors aligning precisely with $ES1$ and $ES2$, and it cannot set aid allocations for each state to equal the difference between $ES1$ and $ES2$ either. Instead, as described in the previous section, it allocated the same amount of aid (on a per-capita basis) to each state, set the spending floor for own spending at an arbitrary low level of spending, and set a spending target at the pre-recession level. In setting a target rather than floor on total spending, it acknowledged that the aid may not be enough in all states to fill the gap. On the other hand, some states would receive more than enough aid to achieve the target level. This could result in different effects of aid by recession intensity if, for example, states with larger gaps spend more of the federal funds on education in order to reach the target threshold.

Figure 1.2 describes a similar scenario to Figure 1.1 for two states with the same level of spending pre-recession, but different levels of revenue shortfall during the recession. The top state is unaffected by the recession; their budget constraint and chosen bundle before and during the recession is the same, $A/B$. The bottom state experiences significant revenue losses; their budget constraint shifts inward and their bundle changes from $A$ to $B$. Both states receive the same amount of federal aid, but the state below the target threshold may spend all of their federal funds trying to reach that threshold, choosing bundle $C/F$, while the state already above the target threshold may treat federal funds as general revenue, choosing bundle $C$. The result in that case is that the same amount of federal aid caused education spending to increase by more in states with worse budget shortfalls.

These simple models provide a way of interpreting and understanding the effects that I estimate. As I examine how the federal aid during the Great Recession affected states' own and total spending and whether the spending requirements affected states' spending choices, I investigate whether the design of the SFSF had the intended effect of moving states from bundle $B$ in Figure 1.1 toward bundle $F$ rather than bundle $C$. One reason I expect to find different effects by recession intensity is that states not suffering from budget shortfalls are more able to substitute federal for state funds while meeting spending requirements, as shown in the top panel of Figure 1.2.

## 1.4 Data

Below I describe my data sources and how I used the available data to construct my measures of states' own and total spending and states' available funds.

### 1.4.1 Data Sources

I compiled data at the state-year level describing revenue and spending from the following sources: the National Association of State Budget Officers (NASBO), the National Center for Education Statistics' Common Core of Data (CCD), and the State Higher Education Officer Organization's State Support for Higher Education Database (SSDB).

- NASBO data describe state expenditures and revenues in broad categories, including both K12 and higher education. These data are self-reported by state budget offices in NASBO's State Fiscal Survey.

- CCD data include K-12 finances at the state and district level. State-level data are sourced from the Census Bureau's National Public Education Financial Survey, and district-level data are sourced from the Census Bureau's Annual Survey of School System Finances.
- SSDB data describe state funding for higher education. These data are sourced from Illinois State University's Grapevine Survey and the State Higher Education Officer Organization's State Higher Education Finance (SHEF) Survey (the Grapevine and SHEF surveys were merged to form the SSDB in 2010).
- Finally, I collected data from various PDFs and spreadsheets on SFSF allocations from the US Department of Education website, and I use data on state budget shortfalls in 2009 from the Center on Budget and Policy Priorities (CBPP).

### 1.4.2 Own and Total State Spending on Education

The most important analysis variables are state spending from state revenue and state spending from the SFSF on public K-12 and public higher education. With these, I create two versions of state spending each for K-12 and higher education: own spending, which includes only state spending from state sources; and total spending, which equals own spending plus spending from the SFSF.

For states' own spending on K-12, I use the variable *r3* "state revenues" from the CCD state fiscal survey. This variable captures funds received by school districts from state governments. For state spending from the SFSF on public K-12 education, I use the variable *arrate5* from the same survey, which captures "total current expenditures for public elementary-secondary education from ARRA funds." Unfortunately, the survey did not separate out the SFSF portion of these ARRA funds, but SFSF accounts for the majority of ARRA funds allocated for public K-12 education.[7]

For state spending on higher education from state revenue, I use the data item "State Support for Public Higher Education Including ARRA" from the SSDB. For state spending on higher education from the SFSF, I use the data item "ARRA Funds for the Operations of Higher Education" from the SSDB. Unlike for K-12, there were no other ARRA programs that supplemented state funding, so this variable captures only SFSF funds.

Table 1.1 summarizes average state spending on K-12 and higher education before during and after the SFSF treatment period, in millions of constant $2012 dollars. During 2009-2011, the average state spent about $5.6 billion on public K-12, $1.5 billion on public higher education, and an additional $288 million from SFSF on K-12 and $67 million from SFSF on higher education. States' own K-12 spending increased across these three periods, while own higher education spending decreased.

### 1.4.3 State Funds and Budget Shortfalls

Other important variables for my analysis are the measures of state funds, which I use to construct counterfactual spending in each sector, and the measure of states' budget shortfalls, which I use to compare effects of the SFSF by recession intensity. This is the most relevant measure of recession intensity for this analysis, because it reflects the fiscal challenge facing states just as they began to make decisions about how to spend SFSF funds.

The measure of state funds comes from NASBO. The NASBO data reports expenditures from the following sources: general funds, bond funds, other state sources, and federal funds. For example, the data report K-12 expenditure overall, but also K-12 ex-

[7] This is also the variable used by [29] to capture SFSF funds.

penditure from each category of funds separately. Comparing NASBO K-12 expenditure to my measure of state spending on K-12 from the CCD, I find that my CCD measure of state spending on K-12 aligns closely with NASBO K-12 expenditures from all non-federal sources (Panel A, Figure 1.3). Comparing NASBO higher education expenditure to my measure of state spending on higher education from the SSDB, I found that the latter aligns closely with NASBO higher education expenditures from general funds (Panel B, Figure 1.3).

The measure of budget shortfall is each state's total shortfall in state fiscal year 2009 as a percentage of the projected general fund budget that year. Shortfalls are equal to the difference between projected or planned spending and available revenues. These data come from the CBPP's reporting after collecting the information from various state sources [46]. Based on the distribution of states by budget shortfall, I group states into categories of small, moderate, and large shortfalls as shown in Figure 1.4). Five states had zero budget shortfall, another five had shortfalls equal to less than 4% of the budget, and the rest had shortfalls of at least 6%. In order to have a category of states relatively unaffected, I assign all these states with less than 4% shortfall to the small shortfall category. The moderate category then contains the 17 states with shortfalls between 6% and 10% inclusive, and the large category contains the 23 states with shortfalls larger than 10%.

The average values of state revenues and funds reported in Table 1.1 before, during, and after the SFSF treatment period show how state finances suffered as a result of the recession. Both sales and personal income tax revenues declined in real terms, leading to a decline in state general funds, but not total state funds, at least between these broad time periods.

## 1.5 Identification Strategies and Assumptions

Estimating the causal effect of the SFSF requires some assumption about spending in its absence. I use two different strategies, each with different underlying assumptions, that address different questions about the effect of the federal aid on state spending.

### 1.5.1 Counterfactual Spending Strategy

Because money is fungible, it is unreasonable to assume that all funds distributed to states were spent as the federal government intended. A simple model predicts that states treat federal aid for education like any other general revenue and spend only a fraction of it on education. It also is unreasonable to assume that state spending just prior to the SFSF would have continued during the treatment period, because that period coincided with deepening of the Great Recession and its impacts on state revenues and spending. What is needed, therefore, is a plausible assumption about how state education spending would have been impacted by the recession.

In my counterfactual spending strategy, I assume that education spending would have fallen in proportion to state funds. In other words, I assume that states continue to spend the same share of state funds on education during the treatment period that they spent in the years leading up to it. Therefore, when I compare actual state education spending with federal funds to this measure of counterfactual spending, the parameter I estimate is how the SFSF altered state spending relative to how we would have expected states to spend given the effect of the recession on their available funds.

There are a few reasons to think this assumption is reasonable. The strongest argument comes from observing whether state funds and education spending move proportionally in the pre-treatment period. Using data from 1995 to 2008, Figure 1.5 plots annual percent changes in K-12 spending and state funds (Panel A) as well as in higher-education spending and general funds (Panel B). It is clear that these variables move together, even during and after the 2001 recession, which supports the assumption that they would have continued to do so into the treatment period. Second, states were already showing signs of cutting education spending prior to the passage of the federal aid package in response to budget shortfalls. As of late 2008, 16 states had proposed or implemented cuts to K-12 and 21 states had done so for higher education [45]. Indeed, the fact that education was vulnerable to spending cuts as a result of revenue shortfalls was the reason behind the SFSF. Finally, I show that my estimated counterfactual spending performs quite well in predicting states' own spending in other years.

To implement this strategy, I first calculate each state's education spending as a share of state funds during the four years prior to the treatment period as shown in Equation 1.1, where $s$ indexes states and $t$ state fiscal years. I do this separately for each sector. For K-12, $EdSpend_{st}$ is equal to state revenues received by public schools, and $StateFunds_{st}$ includes general funds, bond funds, and other state funds. For higher-education spending, $EdSpend_{st}$ is equal to state appropriations for public higher education, and $StateFunds_{st}$ includes state general funds.[8]

$$EdShare_s = \frac{1}{4} \times \sum_{t=2005}^{2008} \frac{EdSpend_{st}}{StateFunds_{st}} \tag{1.1}$$

Next, I multiply state funds in all years by this value as in Equation 1.2, generating a state-year panel of education spending for each sector with a constant education-spending share over time in each state-sector combination. This is the counterfactual

[8]It is important I do not include other state funds in the measure of state funds from which state appropriations are drawn, because many states report higher education operating funds such as tuition revenues in this category, which are not part of appropriations.

spending series that I compare against both states' own spending and their total spending.

$$\widehat{EdSpend}_{st} = EdShare_s \times StateFunds_{st} \tag{1.2}$$

Finally, I estimate the difference between actual (observed) and counterfactual spending in each state and year by stacking the panels of observed and counterfactual state-year education spending and estimating the model in Equation 1.3. Here, I use the natural log of education spending, because, while dollar amounts of spending and federal aid vary widely across states by size, we would expect percent-changes in spending to be similar, which are approximated by the coefficients in this model.[9] In addition to $s$ and $t$, the data are indexed by $i$, which represents whether the observation is actual or counterfactual.

$$\begin{aligned} Log(EdSpend)_{ist} = \beta_0 + \beta_1 Actual_{is} + \beta_2 Actual_{is} \times SFSF_t \\ + \beta_3 Actual_{is} \times PostSFSF_t + \theta_{st} + \varepsilon_{ist} \end{aligned} \tag{1.3}$$

I estimate this model using data from 2006-2014, which includes three years before, during, and after the treatment period. The coefficients of interest are $\beta_2$ and $\beta_3$, which capture the average annual percent-difference in each state between actual versus counterfactual spending during 2009-2011 ($SFSF_t$) and during 2012-2014 ($PostSFSF_t$). The parameter $\beta_1$ captures this difference during 2006-2008, which should be close to zero. State-by-year fixed effects, $\theta_{st}$ are crucial in isolating those effects of interest; they capture differences in spending across states within years and across years within states that are common between the actual and counterfactual series. Because of how the counterfactual is constructed, that includes the state-specific effects of the recession on state funds and education spending. The model attributes the remaining variation to the effect of federal

[9] An alternative way to handle the variation in state size would be to convert all variables to per-student by dividing by enrollment. This would require choosing some base year to measure enrollment so as not to incorporate the enrollment effects of the recession.

aid.

The goal of this model is to estimate the causal effect of the SFSF. The assumption underlying this interpretation is that, in the absence of that aid, states' education spending would decline in proportion to the relevant state funds. The main threat to internal validity is the possibility that other coinciding determinants of state spending are not captured by the counterfactual. For example, the same bill that included the SFSF also included federal aid to states for Medicaid, unemployment benefits, and infrastructure, and other aid for education. This aid would bias estimates upward if it spilled over into education in the same way that the SFSF may itself have spilled over into other areas of spending. It would not bias estimates, however, if that aid simply covered new costs associated with the recession (as may have been the case for Medicaid and unemployment benefits) or was used only for its intended purposes (as [44] find for infrastructure aid).[10] The next strategy sidesteps the issue of coinciding federal aid programs by examining states' response to spending rules that were unique to the SFSF.

### 1.5.2 Spending Threshold Strategy

My second strategy estimates effects of the SFSF on states' spending decisions without relying on the assumption that states would otherwise continue to spend the same share of available funds on education during the recession as they did before. Instead, I exploit the arbitrary spending thresholds included in the program's requirements along with the abrupt ending of those requirements in 2012. Using this approach, we can observe directly whether or not the spending thresholds were binding.

We can think of spending requirements like laws that constrain prices, such as mini-

[10]On the other hand, if the Great Recession created new pressures on state budgets that federal aid did not cover and that would have reduced education spending beyond the effect of declining revenues, this would bias estimates downward because counterfactual spending would be too high.

mum wage laws or rent ceilings. Such policies only have a causal effect on wages or rent if they are binding—that is, employers would set wages lower than the minimum if not for the law. Similarly, the spending requirements of the SFSF only had a causal effect on state spending if they were binding—if states would have chosen to spend less than the floor on own-spending, for example. How can we observe whether or not a threshold is binding? If a minimum wage is binding, we would expect to see many firms setting wages exactly at that level while the law is in effect but below that level when it is not in effect. Similarly, if the spending thresholds of the SFSF were binding, we would expect to see many states spending near the thresholds while the requirements were in place and more states spending below the thresholds after the requirements were removed.

Based on this intuition, I test for binding spending thresholds by measuring the bunching of states near the thresholds and the number of states spending above the thresholds in 2010-2011 versus 2012-13.[11] I first normalize each state's education spending in each sector and year as a percentage of the relevant threshold. The spending thresholds from the SFSF correspond to the state's past spending levels in K-12 and higher education. The floor on own spending during 2009 to 2011 is the state's 2006 spending, so I divide each state's own spending by their spending level in 2006, as in Equation 1.4. The target for total spending during 2009 to 2011 is the state's maximum of 2008 spending and 2009 spending, so I divide each state's total spending by the higher of that state's own spending in 2008 or 2009, as in Equation 1.5.

$$OwnEdSpend\%_{st} = \frac{OwnEdSpend_{st}}{OwnEdSpend_s^{2006}} \times 100 \tag{1.4}$$

$$TotalEdSpend\%_{st} = \frac{TotalEdSpend_{st}}{\max(OwnEdSpend_s^{2008}, OwnEdSpend_s^{2009})} \times 100 \tag{1.5}$$

[11] I use more years than only 2011 and 2012 for statistical power, and I use only 2010-2011 instead of 2009-2011, because, due to the timing of fiscal years and budget planning, state fiscal year 2010 (which begins July 2009) is the first year that states could set spending levels with the SFSF rules in mind.

I use these measures of spending relative to the threshold to test whether the distribution of state spending is consistent with binding thresholds. That is, I test whether state spending is bunched near the thresholds while the requirements are in effect but not afterward (Equation 1.6), and I test whether more states spend above the threshold while the requirements are in effect than afterward (Equation 1.7). The outcomes $Bunched_{st}$ and $Above_{st}$ are indicators for $OwnEdSpend\%_{st}$ or $TotalEdSpend\%_{st}$ being within 5-percentage-points of 100 and being greater than or equal to 100, respectively. The independent variable $SFSF_t$ is an indicator for observations during 2010-2011 as opposed to 2012-2013. The parameters of interest are $\beta_1^b$ and $\beta_1^a$, which represent the change in the probability of state spending being near the threshold and above the threshold during the time the SFSF spending requirements apply as opposed to just afterward. I consider a threshold to be binding if both of these parameters are greater than zero.

$$Bunched_{st} = \beta_0^b + \beta_1^b SFSF_t + \varepsilon_{st}^b \tag{1.6}$$

$$Above_{st} = \beta_0^a + \beta_1^a SFSF_t + \varepsilon_{st}^a \tag{1.7}$$

The assumption underlying this strategy is that there is no other reason why state spending would be bunched and above the thresholds to a greater extent during 2010-2011 than during 2012-2013, such that we would observe these patterns in the absence of the federal aid. The main reason apart from the SFSF why we might expect spending to differ between these two periods is, of course, the Great Recession. It is possible that the recession may have caused multiple states to choose to keep spending at pre-recession levels while revenues are low, neither increasing nor decreasing spending, and this would cause bunching at the total spending threshold more often in 2010-2011 than in 2012-13. However, while this would meet one characteristic of a binding threshold it would not meet the other—more states spending below the threshold in 2012-2013. Instead, we would expect the recession to cause states to spend more in those years. As for the spending floor on states' education spending from their own funds, there is no reason I can think of to believe that states would be near that threshold during 2010-2011 before falling below it in 2012-2013.

## 1.6 Results

I present the results below first from the counterfactual spending strategy then from the spending threshold strategy. In each section, I compare effects by sector (K-12 versus higher education) and across states by recession intensity (level of 2009 budget shortfall).

### 1.6.1 Differences Between Actual and Counterfactual Spending

I first present figures that illustrate the difference between actual and counterfactual spending from 2001-2016. Each graph contains two series of logged spending. The line with hollow markers corresponds to counterfactual spending, the result of applying the education-share of state funds from 2005-2008 to all years. The line with solid markers corresponds to actual spending. The horizontal line in each figure is drawn at the relevant spending threshold, and the years of the SFSF 2009-2011 are shaded gray.

By construction, the lines coincide most closely during 2005 to 2008, the years upon which the counterfactual is based. Before and after those years, the predicted-spending line increases or decreases with changes in state revenue. Therefore, the difference between the lines after 2008 represents the estimated effect of the SFSF on state spending under the assumption that states would have continued to spend the same fraction of revenue in its absence. Result tables present regression estimates corresponding to these observable differences, testing their statistical significance while allowing for correlation in the error term within each state (state-level clusters).

**K-12 Spending**

In Figure 1.6, Panel A includes these result figures for states' own and total spending on K-12, and Panel B the same for higher education. The difference between the figures to the left and right is that the actual series on the right includes the state spending identified as SFSF funds in the data. Considering K-12 spending from states' own funds, we see that it is closely aligned with counterfactual spending during and after the treatment period. This is consistent with the SFSF having no effect on states' spending from their own funds, on average. States did not decrease their spending more than would be expected from their declines in revenue, and certainly did not decrease spending all the way to the spending floor imposed by the program, at least not on average. Considering total spending, on the right, the addition of federal aid increased states' spending on K-12 above the predicted levels during the treatment period. While counterfactual spending fell below the target spending threshold, actual total spending with federal aid was above the target threshold from 2009-2011. After the aid expired in 2012, spending dropped slightly to lie just at the target threshold before rising in tandem with predicted spending after 2012.

The figures suggest that on average, federal aid increased total but not own state spending on K-12. The estimates in the first row of Table 2 confirm the statistical significance of that finding. States' spending on public K-12 education from all sources during the period of the SFSF from 2009-2011 was about 5% higher relative to what it would have been in the absence of federal aid. Other estimates in this row are negative and not statistically distinguishable from zero, indicating that there was no detectable effect of federal aid on states' spending from their own funds nor on their total spending after the federal aid expired.

### Higher Education Spending

Panel B of Figure 1.6 contains the corresponding results figure for higher education spending. The results are similar to those for K-12 spending during the treatment period. Actual own spending coincides with counterfactual spending during the SFSF treatment period, but actual total spending is higher during those years. Own spending falls nearly all the way to the spending floor in 2010, as predicted by falling general fund revenues. Total spending, however, stays very close to the target threshold during 2009-2011. Unlike what we observed for K-12 spending, states' spending on higher education did not pick back up to the predicted levels as revenues began increasing after 2010. Instead, own spending on higher education remained low through 2013, causing a gap through the end of the years shown. As a result, what we see in the right-hand figure of Panel B is that the SFSF had the effect of delaying the drop in spending on public education; rather than dropping in 2009 and 2010 as predicted by falling revenues, it dropped in 2012.

The figures suggest that on average, federal aid increased total state spending on higher education during the period of the SFSF but caused states to spend less on that sector from their own funds after the aid expired. The regression estimates in the second row of Table 2 confirm that total spending was about 7% higher during 2009-2011, but about 9% lower during 2012-2014 than it would have been had states continued spending the same share of revenues as they did before the recession. The effect of the SFSF on states' own spending during 2009-2011 is positive but standard errors do not allow rejection of no effect, on average.

### Heterogeneity by Recession Intensity

Figure 1.7 plots the same series as Panel A of Figure 1.6 separately for three levels of recession intensity. While these categories were determined based on budget shortfalls in mid-2009, they predict larger declines in funds throughout 2009-2011. This is visible in the trends of the counterfactual series, which dips lower in Panel C, much closer to the spending floor.

In the overall results, predicted and actual own spending on K-12 aligned closely during and after the SFSF treatment period. Considering states separately by the size of their 2009 budget shortfall reveals that this is somewhat less true for states with the smallest shortfalls. As expected, these states reduced their own spending somewhat in response to federal aid. This effect began in the last year of the SFSF and continued after the aid expired in 2012.

Now considering the effect of the SFSF on total spending in the figures to the right, federal aid increased spending during the SFSF treatment period for states in all categories of budget shortfall. For states with smaller shortfalls, federal aid boosted spending above the predicted level, despite the predicted level already being above the target spending threshold of the program. For states with moderate shortfalls, federal aid enables these states to spend above the target threshold, and for states with large shortfalls, federal aid increased spending just barely to the target spending threshold.

Panel (b) of Table 1.2 presents the regression estimates corresponding to these results. Relative to counterfactual spending, in states with the smallest budget shortfalls in 2009, the SFSF had the effect of reducing states' own spending on public K-12 education. The estimate is negative but not statistically significant during the period of the SFSF, but grows to a statistically significant 7% after the SFSF expired. Other effects on states' own spending on K-12 are both small and not statistically significant. Estimates for the effect of the SFSF on states' total spending on K-12 are all positive and larger than 3% during 2009-2011, but only about 7% for states with the largest budget shortfalls. The results for K-12 spending by budget shortfall are consistent with the predictions of the simple model in which states choose to spend more of the federal aid on education when doing so is necessary to reach the target threshold of the program.

Figure 1.8 presents the results for higher-education spending by budget shortfall. Considering states' own spending on higher education, actual and counterfactual spending align nearly perfectly in states with small and moderate shortfalls during the treatment period. In states with the largest shortfalls, own spending is just slightly higher than predicted. This is the opposite pattern to K-12, but it may have to do with the effect of the spending floor; only in this category do we see average spending come so close to that floor. After the expiration of the SFSF, actual and predicted spending diverge in these figures. In states with small shortfalls, this occurs because predicted spending rises more quickly, whereas in states with larger shortfalls there is also an immediate drop-off in own spending in 2012. Turning to total spending, federal aid clearly boosted spending during the treatment period, but much more so in states with the largest shortfalls, where counterfactual spending fell the most.

The regression estimates in Table 1.2 Panel (b) corresponding to these results reflect what we observe in the figures. During the treatment period, effects on own spending are statistically indistinguishable from zero, but the point estimate is about 4% (with a large standard error) in states with large budget shortfalls, which suggests that the spending floor may have been binding for some states in this category. Effects on total spending from 2009-2011 are between 3 and 5% among states with small or moderate shortfalls (though not statistically significant at conventional levels), but a substantial 10% among states with the largest shortfalls. After the SFSF expired, states' own and total spending on public higher education fell by 9 to 10% relative to the counterfactual across all budget-shortfall categories.

### Robustness to an Alternative Counterfactual

The counterfactual strategy assumes that state education spending moves in proportion to state funds, which is equivalent to assuming that the elasticity of education spending with respect to state funds is one. As a robustness check, I instead predict counterfactual education spending using each state's estimated elasticity in the pre-treatment data.

To create this alternative counterfactual, I estimate the model shown in Equation 1.8 on data from 1995-2008, separately for K-12 and higher education spending. In this model, I interact state fixed effects with logged state funds and allow each state to have its own intercept, so that I estimate unique parameters $\beta_{0s}$ and $\beta_{1s}$ for each state $s$. I then use these estimates to predict logged education spending in all years as shown in Equation 1.9. These are then the counterfactual values that I stack onto the observed values in order to estimate the difference between actual and counterfactual spending using Equation 1.3.

$$Log(EdSpend)_{st} = \beta_{0s} + \beta_{1s} Log(StateFunds)_{st} + \varepsilon_{st} \tag{1.8}$$

$$Log(\widehat{EdSpend})_{st} = \hat{\beta}_{0s} + \hat{\beta}_{1s} Log(StateFunds)_{st} \tag{1.9}$$

The results from this version of the analysis, as shown in Table 1.3 are very close to the main results, especially in the overall results. The increases in K-12 and higher-education spending during 2009-2011 are still between 5 and 7%. The decline in higher-education spending is slightly smaller, 7% instead of 9% relative to the counterfactual, but still highly statistically significant.

The results by budget shortfall also are quite similar. The results follow the same pattern of larger effects in states with larger budget shortfalls, except for K-12 spending in states with small budget shortfalls, where the estimated effect on total spending is larger than the other categories. This category includes only 10 states, so it is not surprising that it would be different. Moreover, looking at the results figures reveals that the actual and counterfactual series do not align well in the years leading up to treatment, unlike in the other figures (see Appendix Figure A.1).

The results after implementing this alternative counterfactual lend greater confidence to the robustness of the results and to the counterfactual identification strategy. Next I turn to the results from the second identification strategy, which examines the distribution of state spending around the required spending thresholds and does not rely on any assumption about the relationship between states' revenue and spending.

### 1.6.2 Were Spending Requirements Binding?

I consider the spending thresholds of the SFSF to be binding on states' own or total spending if more states spent close to the threshold while the requirement is in place than after it expires, and more states spend above the threshold while the requirement than after it expires. I first present visual evidence, comparing state spending distributions side-by-side, then I discuss the regression estimates corresponding to the models in Equations 1.6 and 1.7.

Figures 1.9 and 1.10 contain histograms of states' own and total spending, respectively, on K-12 (Panel A) and higher education (Panel B) in 2010-2011 (left) versus 2012-2013 (right). Values on the X-axis are normalized as a percentage of the relevant threshold, with a vertical line at 100%, where spending is equal to the threshold. Each bar represents the number of state-year observations in 2-percentage-point bins.

### Floor on Own-Spending (Maintenance-of-Effort)

First considering states' own K-12 spending in Figure 1.9 Panel A, there is no visual evidence that the floor on K-12 own-spending was binding. There were more states beneath, not above, this spending floor while the requirement was in place than afterward. Moreover, there is no clear bunching around the threshold in 2010-2011. This suggests that most states were already choosing to spend more than this spending floor, such that the spending floor had no effect on states' spending from their own funds. However, turning to the distributions of states' own higher-education spending in Panel B, there is visual evidence to suggest that this threshold was binding for some states. There is bunching around the threshold in 2010-2011, and there are more states to the left of the threshold in 2012-2013. While the own-spending floor was not binding on states' K-12 spending, it may have been preventing some states from spending less than they otherwise would on higher education.

Table 1.4 presents estimates corresponding to Figure 1.9 in the "Own-Spending" columns and "Overall Results" panel. The estimates suggest binding requirements if both the bunched and above estimates are statistically significantly greater than zero. This indicates a higher probability of each characteristic of a binding threshold during 2010-2011 relative to 2012-2013. The estimates for states' own-spending on K-12 are positive for the bunched outcome but negative for above the threshold during the treatment period. States were 9 percentage-points more likely to spend near the threshold, but 8 percentage-points less likely to spend above the threshold during 2010-11. The estimates for higher education, however, are both positive. They suggest that the own-spending floor was binding for about 12-14% of states. Both outcomes were between 12 and 14 percentage-points higher during 2010-2011 than 2012-2013.

### Target for Total Spending

Turning to the distributions of total spending shown in Figure 1.10, we again see stronger evidence for binding spending thresholds on higher education spending than on K-12 spending. States' total spending on K-12 was centered around the total spending threshold during 2010-2011, but this was still the case after the spending requirements no longer applied in 2012-2013 (Panel A). This suggests that the threshold may have simply coincided with the level that states were choosing to spend at over the entire period 2010-2013. In Panel B, however, there is bunching near the threshold during 2010-2011 followed by a left-ward shift of the distribution such that more states spent below the threshold in 2012-2013.

The estimates in Table 1.10 reflect what we see in the Figure 8. There is no difference in bunching around the threshold for states' K-12 total spending, although states were 11-percentage-points more likely to spend above that threshold during the treatment period. But for higher education, states were both 21-percentage-points more likely to be near the threshold and 17-percentage-points more likely to spend above the threshold during the treatment period than just afterward.

### Heterogeneity by Recession Intensity

Table 1.10 also contains estimates for heterogeneity in the effects of the spending requirements by budget shortfall. Here we see that the requirements were not binding on K-12 spending for any category of states by shortfall. For higher education spending, own-spending and total-spending thresholds were more likely to be binding among states with moderate or large shortfalls. Statistical power declines when splitting the sample this way, but the estimates for total-spending among states with large shortfalls remain highly statistically significant.

## 1.7 Discussion

### 1.7.1 Flypaper Effects

Results so far have focused on the causal effect of the aid states received through the SFSF and of its spending requirements on states' spending decisions. What do these results suggest about the propensity for states to spend federal aid allocated for education on education? To more directly answer this question, I compare the percentage of each states' SFSF allocation spent on education (based on the estimates) to the percentage of their total budget spent on education pre-SFSF. If the estimates imply that states spent 100% of their allocation on education, this would be a full flypaper effect. Under the null hypothesis of no flypaper effect, states would spend a similar amount of their allocation on education as the percentage of their entire budget on education. Anything in between would be a partial flypaper effect.

Table 1.5 presents the estimates for states' propensity to spend their SFSF allocation on education, compared to their propensity to spend on education from all funds. Education spending here combines spending on public K-12 and public higher education. The "SFSF received" amounts are based on actual allocations distributed to states by the Department of Education to spend on either sector. The "SFSF spent on education" amounts are calculated from the estimates (percent changes) multiplied by the counterfactual spending amounts.

The results in Panel A suggest a full flypaper effect overall. States spent about 100% of their SFSF allocations on education during the treatment period. As expected based on the pattern of results by recession intensity presented in the previous sections, states with larger budget shortfalls spent a higher proportion of federal funds on education.

The explanation for this pattern that I put forward is the role of spending requirements. States with smaller budget shortfalls did not need to spend all federal funds to achieve the federal government's target threshold, and they would not fall below the own-spending floor by reducing own spending somewhat below the counterfactual. States with larger budget shortfalls, on the other hand, did need to spend all federal funds to achieve the target threshold and may have even needed to increase their own spending above what they would have otherwise spent in order to meet the own-spending floor. Even given the effects of the spending requirements, however, the estimate of 151% for states with the largest budget shortfall is surprising given the pressure these states' budgets were under during this time period.

Over the longer time horizon, as shown in in Panel B of Table 1.5, the flypaper effect dissipates for states with smaller budget shortfalls. Recall from the previous sections that K-12 spending in states with small shortfalls fell below the counterfactual trend post-SFSF, and this was true in all categories for higher education spending. However, the estimates imply that states with large budget shortfalls still spent 90% of the SFSF on education. The fading flypaper effect that I observe for most states is similar to what [36] finds when examining the effect of federal Title I grants on state and local spending—an initial flypaper effect but substantial crowding out over a three-year horizon.

### 1.7.2 Why Do Effects Differ by Sector?

One of the contributions of this analysis is to examine both K-12 and higher education spending and test whether and to what extent an aid program like the SFSF affects states' spending in these sectors differently. The SFSF was designed to support both these sectors, and the same spending floor and target applied to each separately. However, K-12 and higher education are financed differently and affected differently by recessions, suggesting the possibility of different effects. The results show that spending on higher education uniquely fell after the program expired, and that the spending requirements of the program were binding for higher education spending but not K-12 spending.

These results are consistent with other work that has shown higher education spending to be more vulnerable to budget cuts when revenues fall [9]. My results show that, even though federal aid made up for the declines in revenue, allowing states to maintain higher education spending during the worst of the recession from 2009-2011, states still spent less on higher education afterward, even while revenues rebounded. One reason for this may be the fact that enrollments rose during the Great Recession, providing an alternative stream of revenue to public colleges and universities (assuming costs did not rise commensurate with rising enrollments). This, combined with increases in tuition, may have led states to cut or simply not raise spending sufficiently to maintain the level of spending post-SFSF that federal aid had allowed them to reach during the treatment period.

On the K-12 side, that sector may have been less vulnerable to spending cuts because of the strength of states' commitment to funding public K-12 schools through predetermined funding formulae. Spending may have been vulnerable within a certain margin, but not a sufficiently wide margin that would have caused the SFSF spending floor and target to be binding in the same way that they were for higher education spending.

## 1.8 Conclusion

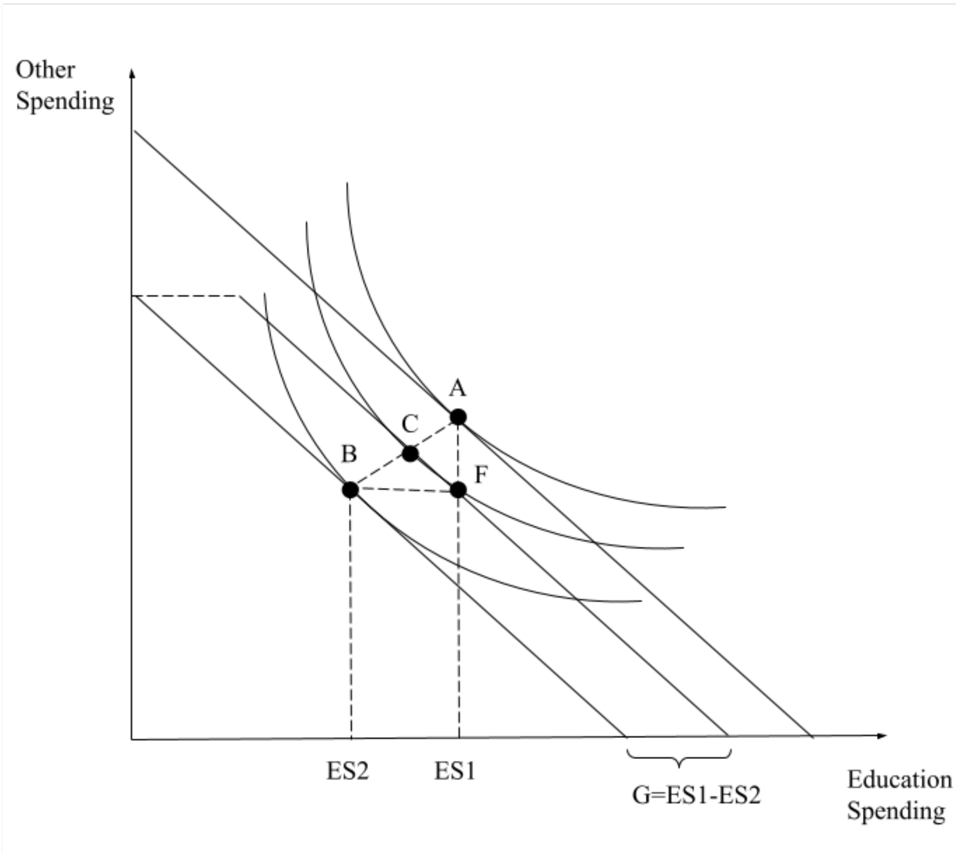
In this paper I study the $50 billion-dollar aid package that the federal government provided to states to stabilize their spending on public K-12 and higher education in the aftermath of the Great Recession. I use two identification strategies to estimate how this aid package actually affected states' spending, including the role of its spending requirements.

Results show that the federal aid did bolster spending in both sectors during the treatment period 2009-2011. It increased K-12 spending by 5% and higher-education spending by nearly 7% relative to spending in the absence of the aid, with larger effects in states with larger budget shortfalls. Spending requirements were followed and were binding during the treatment period on many states' higher education spending. After the program ended, and aid and requirements expired, higher education spending fell but not K-12 spending did not. These results suggest that federal recessionary aid can positively affect state support for public education in the short run, especially when combined with spending floors and targets. By stabilizing state support for public education, federal aid can indirectly support the positive effects of state education spending on K-12 student achievement [39] and college attainment [17] through higher per-student resources and lower college tuition. However, federal aid and requirements should be gradually eased away in order to avoid sharp declines in state spending and associated negative consequences for students.

The estimated increases in spending during 2009-2011 sum to about 100% of the actual SFSF funds distributed to states by the Department of Education, suggesting a full pass-through or full flypaper effect. However, looking at a longer time horizon including the three years after the program ended reveals that higher-education spending fell sharply and even K-12 spending fell in some states in 2012. As a result, the overall pass-through rate of federal funds fell to less than 50% by 2014. Declines in education spending post-SFSF are attributable to the loss of federal funds, but spending requirements played a role as well. The floor on own-spending as well as the target threshold for total-spending were binding on higher education spending for many states. These provisions are common in federal aid programs, and these results suggest that they should considered more often as a mechanism behind flypaper effects in the economic literature studying this phenomenon.

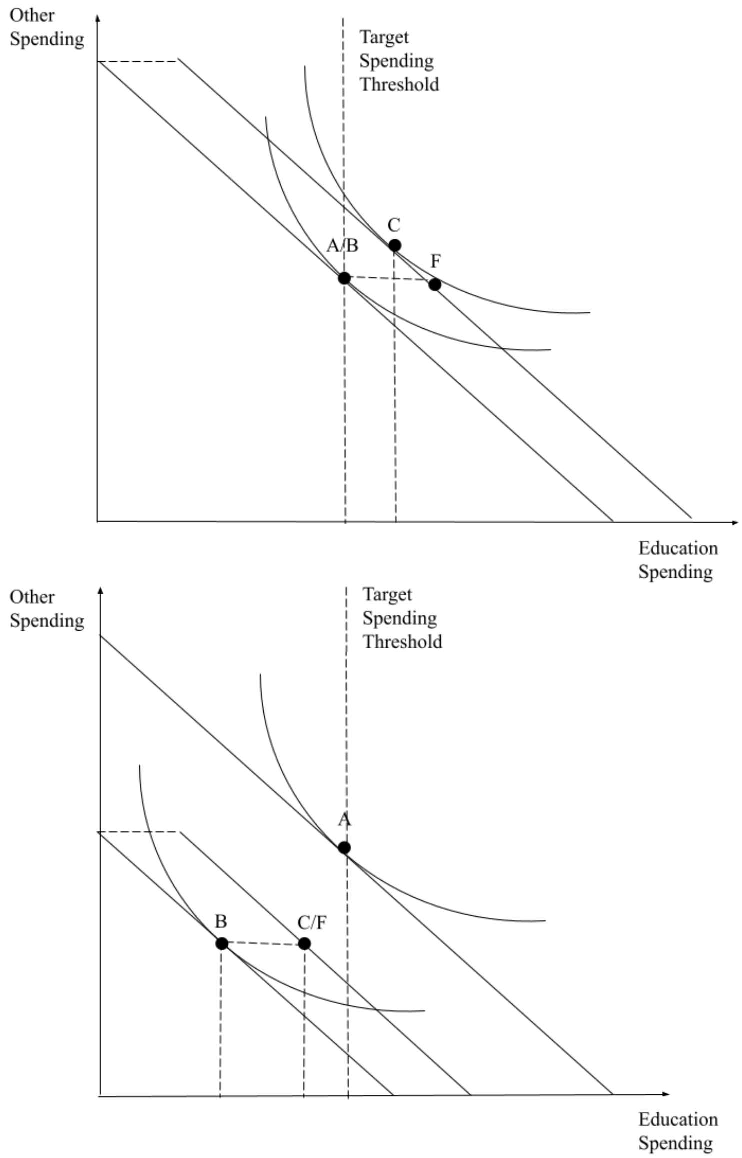
## Figures

Figure 1.1: The Effect of Federal Aid on State Education Spending




Notes: This figure illustrates the expected effects of federal aid to a state for education during a recession in a simple choice model. Prior to the recession, the state has chosen bundle *A*, where its utility is maximized subject to its budget constraint. Due to the recession's effects on tax revenues, the state's budget constraint shifts inward, and the state chooses to spend at bundle *B*. Observing or predicting this detrimental effect of the recession on state education spending, equal to $ES1$ minus $ES2$, the federal government steps in, providing the state with $G$, exactly the amount needed to restore education spending to its prior level ($G = ES1 - ES2$). However, the state uses $G$ to move not to bundle *F* (full flypaper effect) but to bundle *C*, which lies between *A* and *B* and has the state's preferred spending mix.

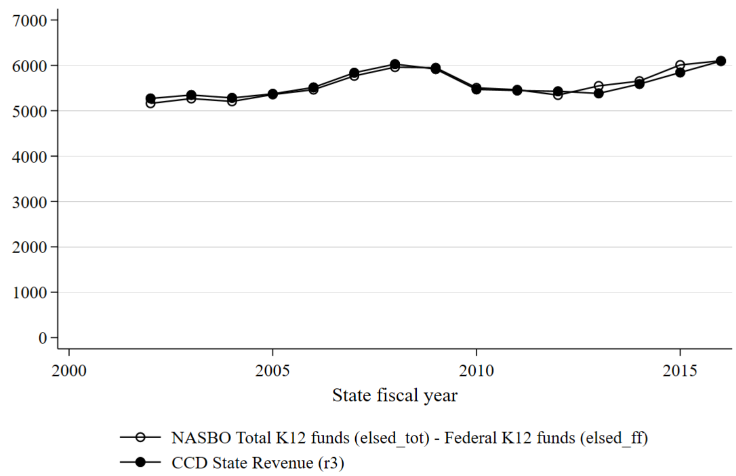
Figure 1.2: The Effect of Federal Aid on State Education Spending in States with



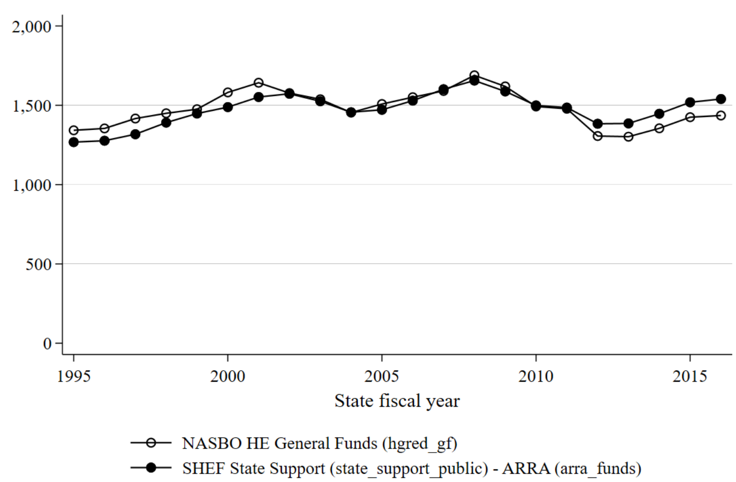

Notes: This figure models a similar scenario to Figure 1.1 for two states with the same level of spending pre-recession, but different levels of revenue shortfall during the recession. The top state is unaffected by the recession; their budget constraint and chosen bundle before and during the recession is the same, $A/B$. The bottom state is affected by significant revenue losses; their budget constraint shifts inward and their bundle changes from $A$ to $B$. Both states receive the same amount of federal aid, but the state below the target threshold may spend all of their federal funds trying to reach that threshold, choosing bundle $C/F$, while the state already above the target threshold may treat federal funds as general revenue, choosing bundle $C$. The result in that case is that the same amount of federal aid caused education spending to increase by more in states with worse budget shortfalls.

Figure 1.3: Comparison of State Educational Expenditure Variables by Data Source

Panel A. K-12

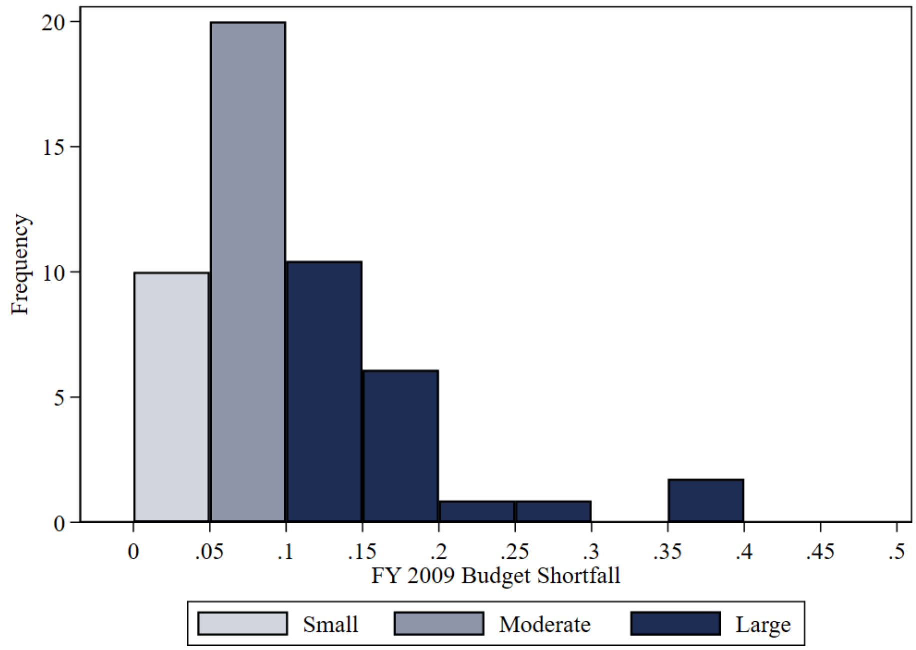



Panel B. Higher Education




Notes: Values are expressed in millions of constant 2012 dollars. The figures show the alignment between measures of state spending from different data sources. Panel A shows the alignment between (1) the CCD variable capturing state revenue received by school districts and (2) the NASBO variables capturing state K-12 expenditures from non-federal funds. Panel B shows the alignment between (1) the SHEF variable capturing state support for public higher education excluding ARRA funds and (2) the NASBO variable capturing state higher education expenditures from general funds. Sources: NCES-CCD, NASBO, SHEF-SSDB.

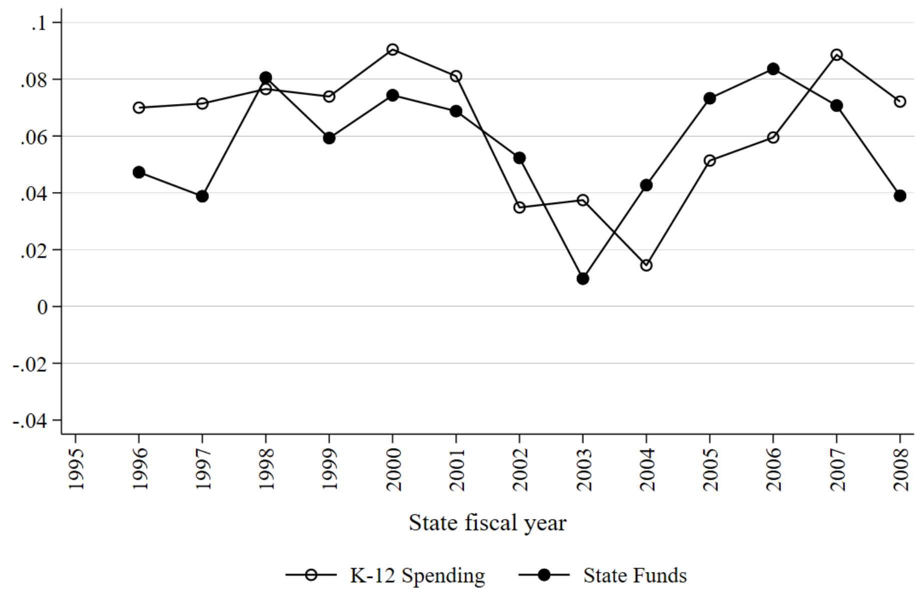
Figure 1.4: Frequency Histogram of States by Budget Shortfalls in SFY 2009, Illustrating the Shortfall Categories Used for Heterogeneity Analyses



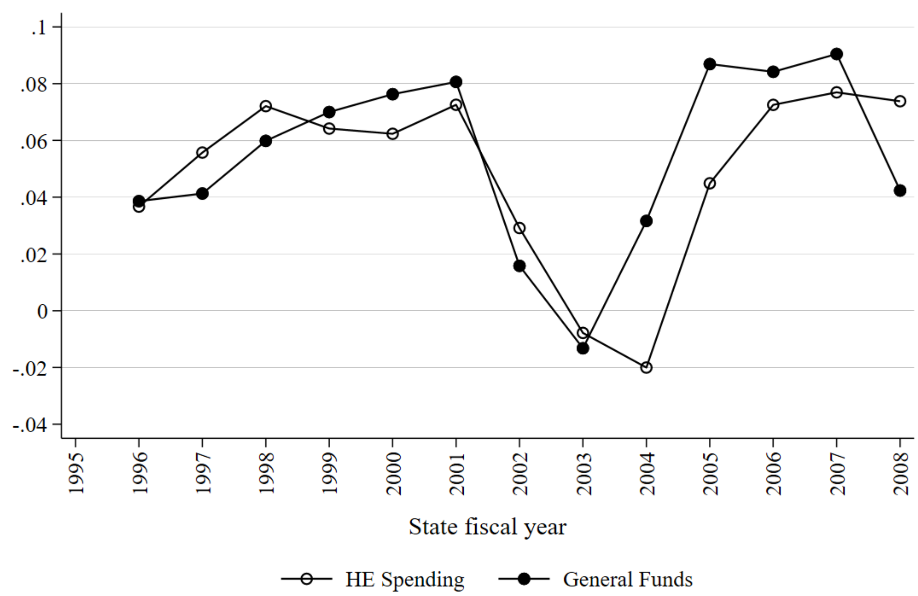

Notes: The figure shows the number of states by size of FY 2009 budget shortfall. The measure of budget shortfall is each state's total shortfall in state fiscal year 2009 as a percentage of the projected general fund budget that year. Shortfalls are equal to the difference between projected or planned spending and available revenues.
Source: These data come from the CBPP's reporting after collecting the information from various state sources [46].

Figure 1.5: Annual Percent Changes Education Spending and State Funds, 1995-2008

Panel A. K-12 Spending and State Funds

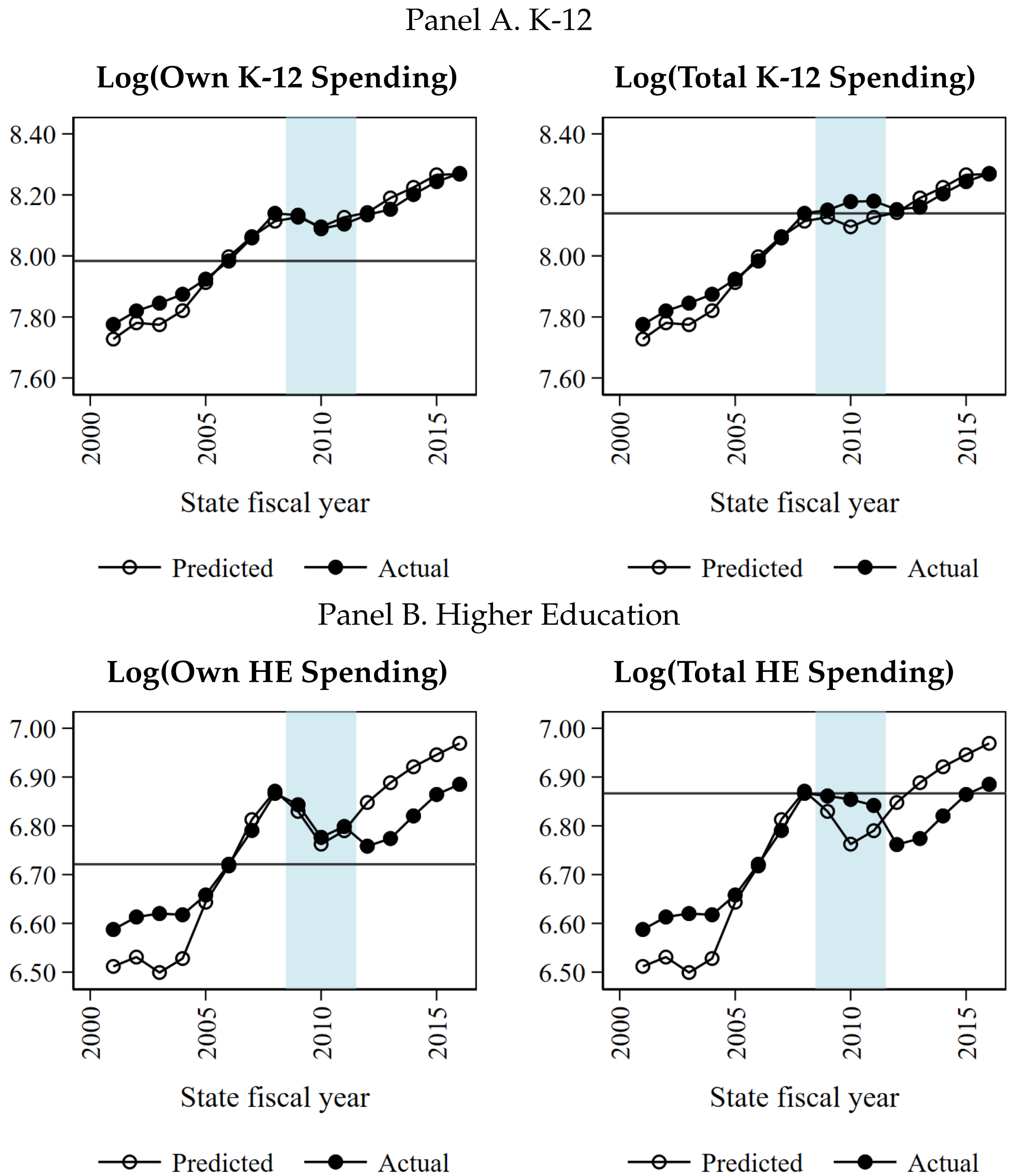



Panel B. Higher-Education Spending and General Funds



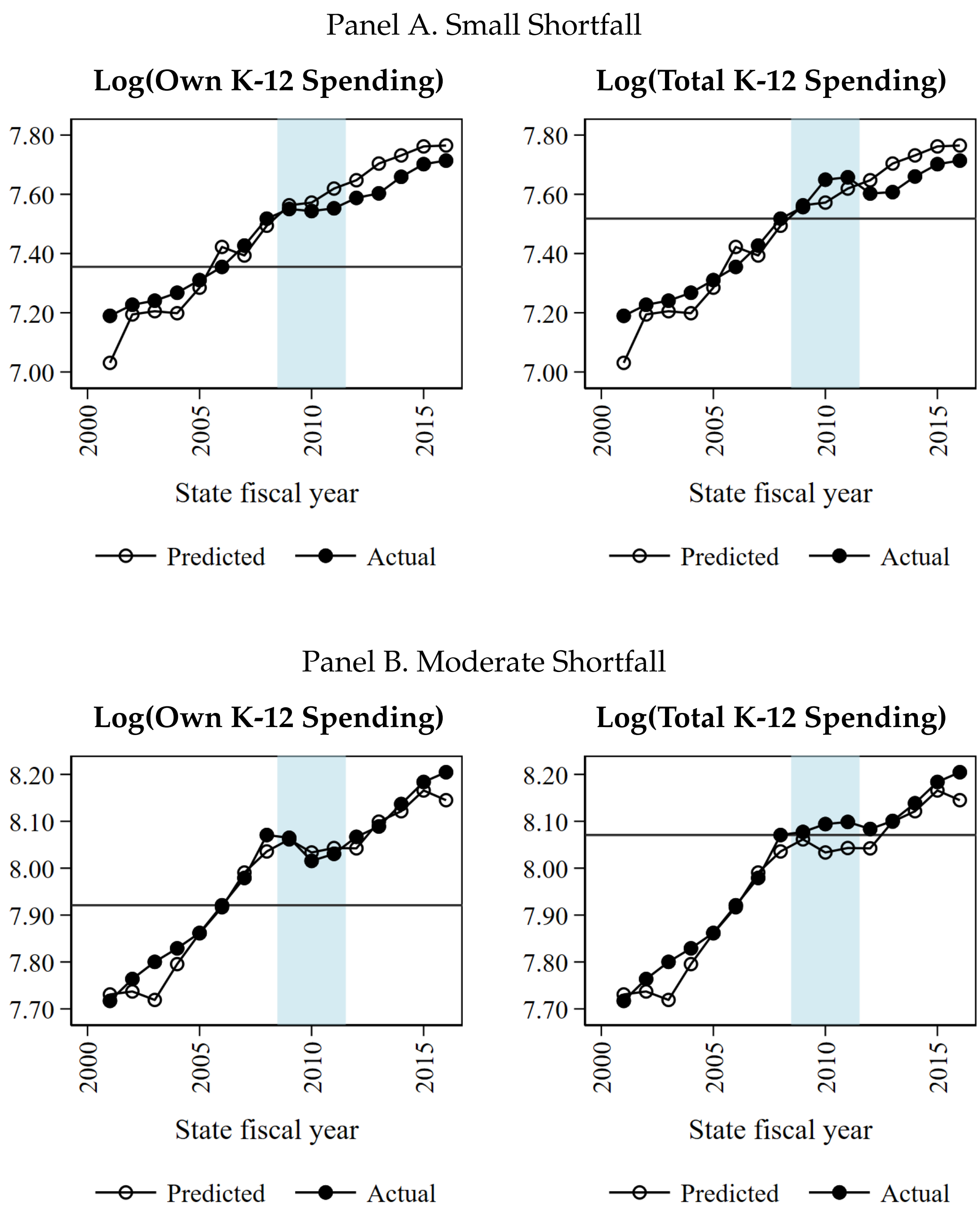

Notes: The figures plot the annual percent changes in state K-12 spending and higher education (HE) in the years leading up to the Great Recession. It shows that these percent changes align closely with percent changes in the relevant state funds. Sources: NCES-CCD, NASBO, SHEF-SSDB.

Figure 1.6: Overall Spending Effects Relative to Counterfactual, SFY 2001-2016

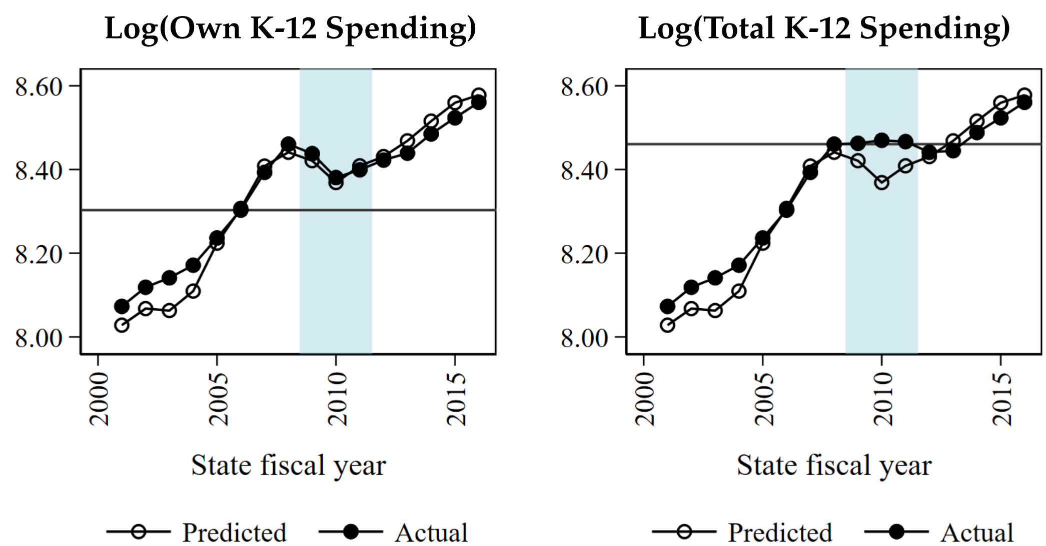



Notes: Each graph contains two series of logged spending. The line with hollow markers corresponds to counterfactual spending, the result of applying the education-share of state funds from 2005-2008 to all years. The line with solid markers corresponds to actual spending. The horizontal line in each figure is drawn at the relevant spending threshold (for own or total spending), and the years of the SFSF 2009-2011 are shaded gray.
Sources: Author's calculations using data from NCES-CCD, NASBO, SHEF-SSDB.

Figure 1.7: K-12 Spending Effects Relative to Counterfactual,
by Size of 2009 Budget Shortfall, 2001-2016



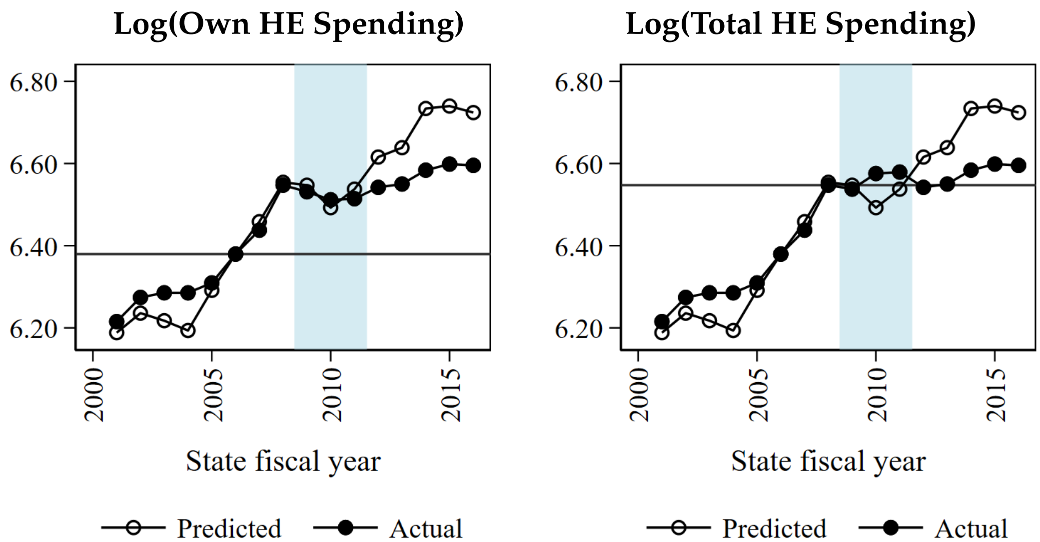
Panel C. Large Shortfall



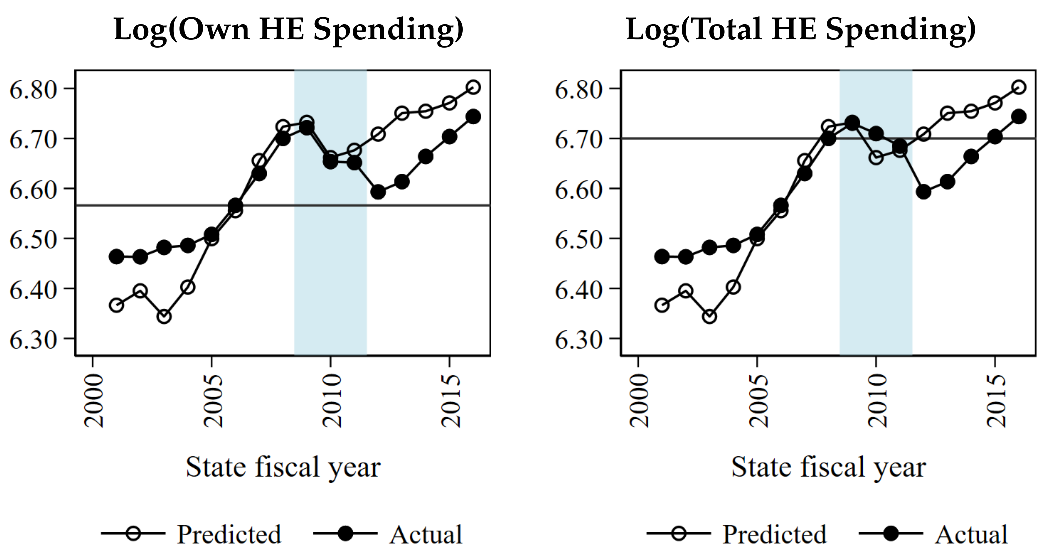

Notes: Each graph contains two series of logged spending. The line with hollow markers corresponds to counterfactual spending, the result of applying the education-share of state funds from 2005-2008 to all years. The line with solid markers corresponds to actual spending. The horizontal line in each figure is drawn at the relevant spending threshold (for own or total spending), and the years of the SFSF 2009-2011 are shaded gray.
Sources: Author's calculations using data from NCES-CCD, NASBO, SHEF-SSDB.

Figure 1.8: Higher-Education Spending Effects Relative to Counterfactual, by Size of 2009 Budget Shortfall, 2001-2016

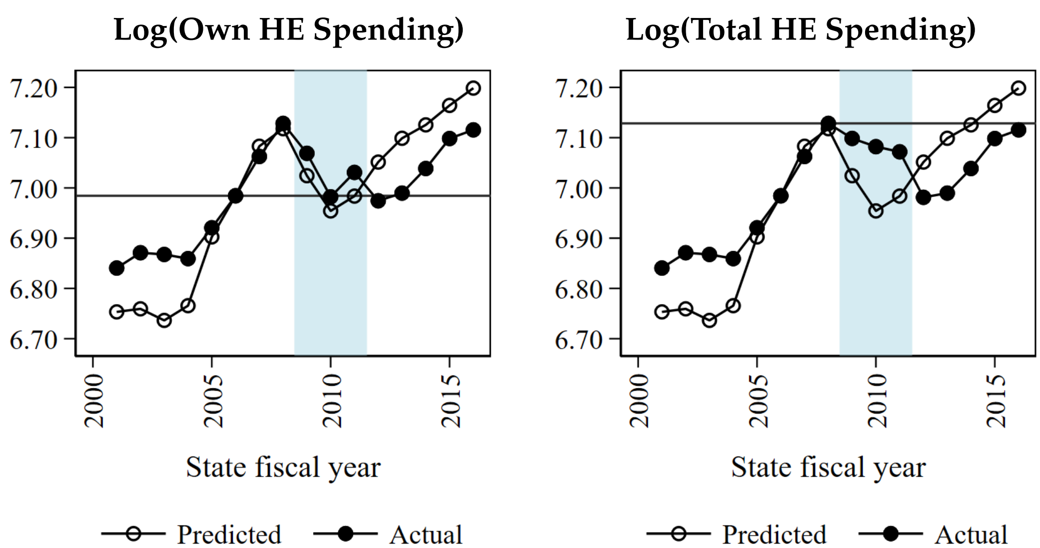
Panel A. Small Shortfall




Panel B. Moderate Shortfall



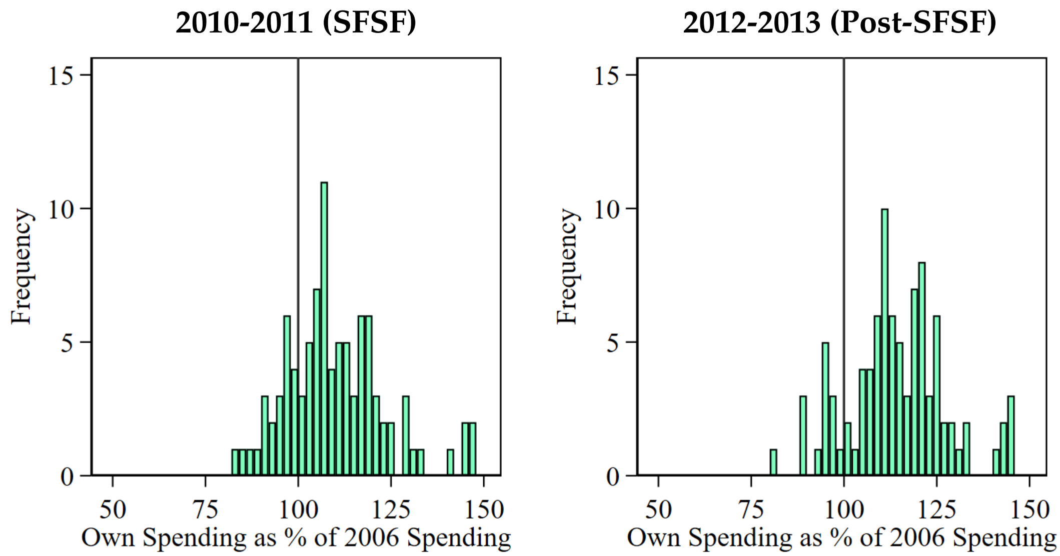
Panel C. Large Shortfall



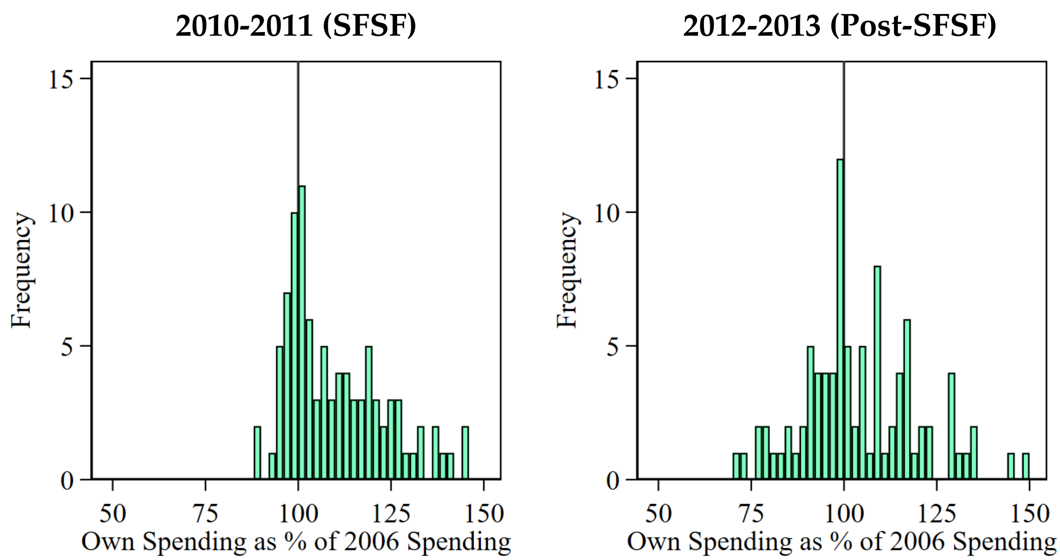

Notes: Each graph contains two series of logged spending. The line with hollow markers corresponds to counterfactual spending, the result of applying the education-share of state funds from 2005-2008 to all years. The line with solid markers corresponds to actual spending. The horizontal line in each figure is drawn at the relevant spending threshold (for own or total spending), and the years of the SFSF 2009-2011 are shaded gray.
Sources: Author's calculations using data from NCES-CCD, NASBO, SHEF-SSDB.

Figure 1.9: Bunching of Own Spending by Sector in 2010-2011 vs. 2012-2013

Panel A. K-12



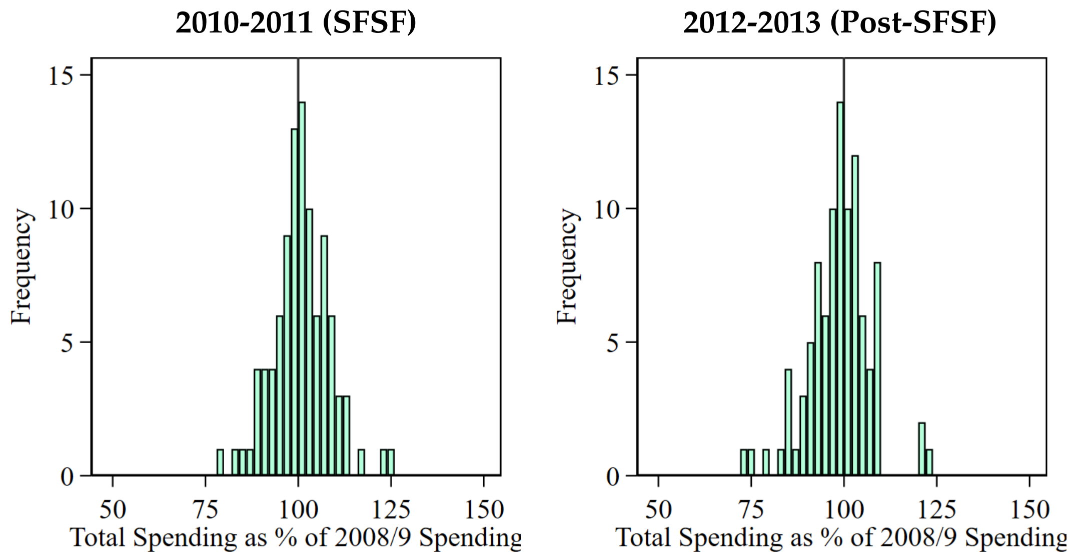

Panel B. Higher Education



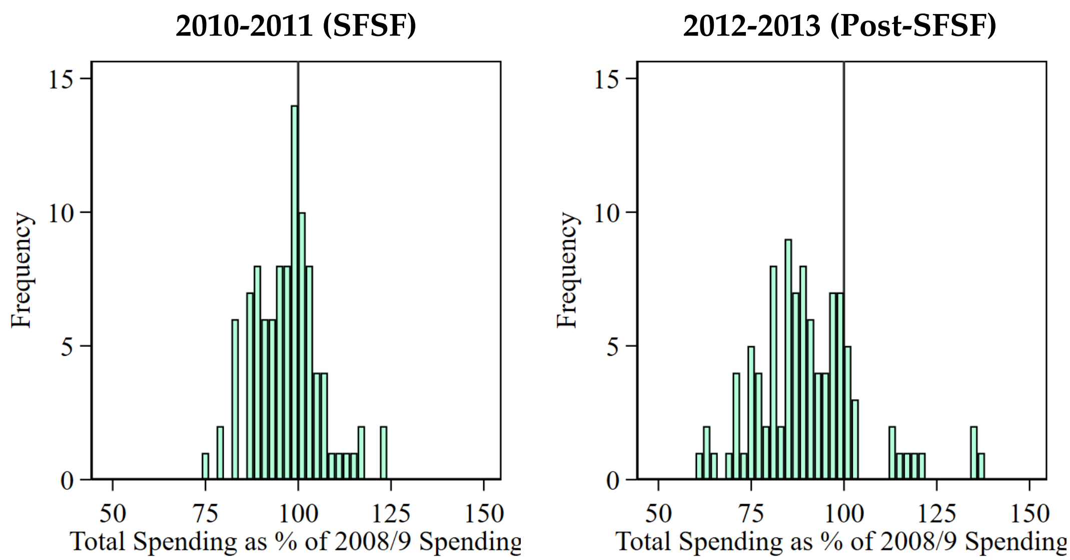

Notes: Each graph contains histograms of states' own spending on K-12 (Panel A) and higher education (Panel B) in 2010-2011 (left) versus 2012-2013 (right). Values on the X-axis are normalized as a percentage of the relevant threshold, with a vertical line at 100%, where spending is equal to the threshold. Each bar represents the number of state-year observations in 2-percentage-point bins.
Sources: Author's calculations using data from NCES-CCD and SHEF-SSDB.

Figure 1.10: Bunching of Total Spending by Sector in 2010-2011 vs. 2012-2013

Panel A. K-12




Panel B. Higher Education




Notes: Each graph contains histograms of states' total spending on K-12 (Panel A) and higher education (Panel B) in 2010-2011 (left) versus 2012-2013 (right). Values on the X-axis are normalized as a percentage of the relevant threshold, with a vertical line at 100%, where spending is equal to the threshold. Each bar represents the number of state-year observations in 2-percentage-point bins.
Sources: Author's calculations using data from NCES-CCD and SHEF-SSDB.

## Tables

Table 1.1: Average Annual State Revenue and Expenditure (Millions of Constant 2012 $) on Public K-12 and Higher Education Before, During, and After the SFSF Period, 2002-2016

| | Pre (2002-2008) | SFSF (2009-2011) | Post (2012-2016) |
|---|---|---|---|
| State Finances | | | |
| Sales tax revenue | 4,435 | 4,152 | 4,349 |
| Personal income tax revenue | 5,293 | 5,120 | 6,120 |
| Total general funds | 13,078 | 12,873 | 14,168 |
| Total state funds | 22,809 | 24,013 | 25,010 |
| SFSF Allocations | | | |
| SFSF - Education | | 272 | |
| SFSF - Gov. Services | | 60 | |
| SFSF - Total | | 332 | |
| State Education Spending | | | |
| K12 spending from SFSF | | 288 | 22 |
| HE spending from SFSF | | 67 | 0 |
| K12 own spending | 5,523 | 5,612 | 5,668 |
| HE own spending | 1,545 | 1,524 | 1,455 |
| K12 own spending (% state funds) | 23% | 22% | 22% |
| HE own spending (% general funds) | 13% | 13% | 11% |

Notes: This table reports averages at the state-year level in constant 2012 dollars. SFSF=State Fiscal Stabilization Fund. HE=Higher Education.
Sources: State Finances statistics are calculated from NASBO data, SFSF Allocations are the distributed amounts reported by the US Department of Education, and State Education Spending statistics are calculated from NCES and SSDB data.

Table 1.2: Effects Relative to Counterfactual by Sector and 2009 Budget Shortfall

| | Log(Total Spending) | | Log(Own Spending) | |
|---|---|---|---|---|
| | SFSF (2009-2011) | Post (2012-2014) | SFSF (2009-2011) | Post (2012-2014) |
| (a) Overall Results | | | | |
| K-12 | 0.051*** | -0.016 | -0.01 | -0.025 |
| | (0.019) | (0.021) | (0.019) | (0.021) |
| Higher Education | 0.066*** | -0.093*** | 0.02 | -0.094*** |
| | (0.017) | (0.019) | (0.018) | (0.019) |
| (b) By Budget Shortfall | | | | |
| K-12 | | | | |
| Small | 0.040 | -0.068* | -0.033 | -0.074** |
| | (0.030) | (0.037) | (0.027) | (0.037) |
| Moderate | 0.035 | 0.011 | -0.018 | 0.001 |
| | (0.027) | (0.029) | (0.028) | (0.030) |
| Large | 0.067** | -0.014 | 0.006 | -0.023 |
| | (0.033) | (0.036) | (0.032) | (0.036) |
| Higher Education | | | | |
| Small | 0.047* | -0.095** | 0.002 | -0.095** |
| | (0.027) | (0.045) | (0.020) | (0.045) |
| Moderate | 0.031 | -0.101** | -0.002 | -0.101*** |
| | (0.024) | (0.033) | (0.025) | (0.033) |
| Large | 0.100*** | -0.085*** | 0.043 | -0.088*** |
| | (0.029) | (0.028) | (0.032) | (0.028) |

Notes: This table presents the estimates corresponding to $\beta_2$ and $\beta_3$ in Equation 1.3, where each set of SFSF and Post estimates is from a different regression. The outcomes are logged total and own spending, so that the estimates can be interpreted as a percent change relative to counterfactual spending in each sector. Standard errors clustered at the state level are in parentheses. Stars denote statistical significance:
$*p < 0.10, **p < 0.05, ***p < 0.01$.

Table 1.3: Robustness: Effects Relative to Alternative Counterfactual by Sector and 2009 Budget Shortfall

| | Log(Total Spending) | | Log(Own Spending) | |
|---|---|---|---|---|
| | SFSF (2009-2011) | Post (2012-2014) | SFSF (2009-2011) | Post (2012-2014) |
| (a) Overall Results | | | | |
| K-12 Education | 0.057*** | -0.003 | -0.003 | -0.012 |
| | (0.019) | (0.023) | (0.018) | (0.024) |
| Higher Education | 0.065*** | -0.073*** | 0.018 | -0.074*** |
| | (0.010) | (0.015) | (0.011) | (0.015) |
| (b) By Budget Shortfall | | | | |
| K-12 | | | | |
| Small | 0.082** | 0.004 | 0.009 | -0.002 |
| | (0.034) | (0.050) | (0.031) | (0.050) |
| Moderate | 0.040 | 0.019 | -0.013 | 0.009 |
| | (0.030) | (0.035) | (0.030) | (0.035) |
| Large | 0.059* | -0.023 | -0.001 | -0.032 |
| | (0.030) | (0.038) | (0.030) | (0.038) |
| Higher Education | | | | |
| Small | 0.056** | -0.062** | 0.011 | -0.062** |
| | (0.022) | (0.031) | (0.016) | (0.031) |
| Moderate | 0.061*** | -0.068** | 0.028* | -0.068** |
| | (0.015) | (0.029) | (0.017) | (0.029) |
| Large | 0.072*** | -0.081*** | 0.015 | -0.083*** |
| | (0.016) | (0.022) | (0.020) | (0.022) |

Notes: This table presents the estimates corresponding to $\beta_2$ and $\beta_3$ in Equation 1.8, where each set of SFSF and Post estimates is from a different regression. The outcomes are logged total and own spending, so that the estimates can be interpreted as a percent change relative to counterfactual spending in each sector. Standard errors clustered at the state level are in parentheses. Stars denote statistical significance: $*p < 0.10, **p < 0.05, ***p < 0.01$.

Table 1.4: Probability of Spending Near and Above the Spending Thresholds in 2010-2011 Relative to 2012-2013, by Sector and 2009 Budget Shortfall

| | Own-Spending | | Total-Spending | |
|---|---|---|---|---|
| | Bunched at threshold | Above threshold | Bunched at threshold | Above threshold |
| (a)Overall Results | | | | |
| K-12 | 0.090** | -0.080** | 0.000 | 0.110** |
| | (0.041) | (0.031) | (0.059) | (0.055) |
| Higher Education | 0.120** | 0.140*** | 0.210*** | 0.170*** |
| | (0.051) | (0.048) | (0.053) | (0.045) |
| (b)By Budget Shortfall | | | | |
| K-12 | | | | |
| Small | 0.000 | 0.000 | -0.450*** | 0.450*** |
| | (0.000) | (0.000) | (0.152) | (0.113) |
| Moderate | 0.147 | -0.147** | 0.265** | 0.059 |
| | (0.102) | (0.070) | (0.113) | (0.126) |
| Large | 0.087 | -0.065 | 0.000 | 0.000 |
| | (0.074) | (0.072) | (0.113) | (0.089) |
| Higher Education | | | | |
| Small | 0.15 | -0.05 | 0.05 | 0.15 |
| | (0.103) | (0.152) | (0.152) | (0.145) |
| Moderate | 0.176 | 0.294** | 0.324*** | 0.000 |
| | (0.112) | (0.120) | (0.103) | (0.085) |
| Large | 0.065 | 0.109 | 0.196** | 0.304*** |
| | (0.114) | (0.083) | (0.093) | (0.087) |

Notes: This table presents the estimates corresponding to $\beta_1^b$ in Equation 1.6 ("Bunched at threshold" columns) and $\beta_1^a$ in Equation 1.7 ("Above threshold" columns). The outcomes are indicators equal to zero or one, so that estimates can be interpreted as a percentage-point change in the probability of the outcome during versus after the spending requirements applied. Standard errors in parentheses. Stars denote statistical significance: $*p < 0.10, **p < 0.05, ***p < 0.01$.

Table 1.5: Estimated % of SFSF Spent on Education During SFSF and Post-SFSF Compared to % of Budget Spent on Education Pre-SFSF (Implied Flypaper Effects), by 2009 Budget Shortfall

| | \$SFSF Spent on Education | \$SFSF Received | %SFSF Spent on Education | %Budget Spent on Education |
|---|---|---|---|---|
| Panel A. Flypaper effect during SFSF (2009-2011) | | | | |
| Overall | 1031 | 951 | 108%* | 11% |
| By Budget Shortfall | | | | |
| Small | 516 | 709 | 73%* | 10% |
| Moderate | 474 | 664 | 71% | 11% |
| Large | 1912 | 1270 | 151%* | 11% |
| Panel B. Flypaper effect during SFSF and Post-SFSF period (2009-2014) | | | | |
| Overall | 386 | 951 | 41% | 11% |
| By Budget Shortfall | | | | |
| Small | 0 | 709 | 0% | 10% |
| Moderate | 157 | 664 | 24% | 11% |
| Large | 1138 | 1270 | 90% | 11% |

Notes: This table converts the estimates presented in prior tables to dollar terms, then calculates the magnitude of these dollar increases as a share of each state's SFSF funds received from the Department of Education. If states spent a much larger share of those funds on education than the share of their budget spent on education pre-SFSF (last column), then this implies a flypaper effect. Panel B extends the analysis over a longer time horizon. A star (*) indicates that the share of the budget spent on education is not within the 95% confidence interval of the share of SFSF funds spent on education.

CHAPTER 2

# OCCUPATIONAL LICENSING IN HEALTHCARE AND THE MARKET FOR VOCATIONAL HIGHER EDUCATION: EVIDENCE FROM PHARMACY TECHNICIANS

## 2.1 Introduction

In today's economy, many low-skill workers seek 'blue scrubs' jobs in the growing healthcare sector. As demand for healthcare assistant and technician positions has grown, states have implemented new licensing laws and added requirements such as the completion of training programs and passage of certification exams. These requirements may cause workers to seek expensive college credentials for relatively low-wage positions. As states add licensing requirements for a broader set of occupations, it is important to understand their repercussions in higher education markets, where for-profit colleges compete with state-funded community colleges in offering vocational training programs.

In this paper, I estimate the effect of new licensing requirements for pharmacy technicians, a low-skill healthcare profession, on the market for formal credentials, including college completions by sector and professional certifications. Most empirical economics research on occupational licensing tests theoretical predictions for the effect of licensing on wages, labor supply, or quality of services. For example, [40] were the first to study labor market effects of licensing across all occupations; [43] study licensing and teacher quality. In contrast, my research question addresses theoretical predictions about rents accruing to the providers of training and certification [48] and the importance of accounting for different licensing requirements [31]. My hypothesis is that colleges, especially for-profit colleges, benefit from licensing, and that competency requirements like training and examination have stronger effects on college-going than registration requirements alone.

I selected the pharmacy technician profession as a representative low-level healthcare occupation for which there is sufficient variation in licensing laws and college training programs over my analysis period (1997 to 2018). To separately estimate the effects of different licensing requirements, I collected and categorized state laws and regulations of pharmacy technicians since the 1990s. With this state-year variation in licensing requirements, I use a difference-in-difference identification strategy and event study models to estimate the effects of different types of requirements on the flow of new college completions and new national certifications. Data on college program completions are from the Department of Education's Integrated Postsecondary Education Database (IPEDS), which includes all higher education institutions eligible for federal aid programs. Certification data are publicly available from the Pharmacy Technician Certification Board (PTCB), which is the largest professional certifier for this profession and began offering certifications in 1995.

Trends in occupational licensing and in higher education motivate this research, as does recent empirical work on these topics. The White House Council of Economic Advisers drew attention in a 2015 report to the importance of licensing as US employment shifts toward licensed sectors like healthcare [11]. Survey questions added to the 2015 Census contributed to a new wave of empirical research on the potential distortionary effect of licensing on labor markets [for example, 7, 41]. Meanwhile, the explosive growth of for-profit colleges during the 2000s and 2010s has sparked research into whether they are productive or predatory [25]. For-profit colleges and community colleges are the main suppliers of vocational higher education, but for-profit colleges charge much higher tuition for similar or worse labor-market and loan-default outcomes [3, 12]. Given concerns that for-profit colleges practice deceptive marketing and charge higher tuition to capture federal student aid [14, 15, 42], it may be the case that for-profit colleges capture rents from occupational licensing. Rather than studying the potential distortionary effects of licensing on labor markets, therefore, I estimate the effect of licensing requirements on markets for higher education: the creation of new programs in the public and for-profit sectors, the number of students completing those programs, and program prices.

Data constraints and the task of accurately characterizing variation in licensing laws across states and years led me to focus on one occupation. Starting from a list of low-skill occupations included in the National Conference of State Legislature's Occupational Licensing Database, I selected pharmacy technician because it met key criteria: it is well-defined by college programs and state regulations; it is consistently defined over time in data sources; and it experienced changing licensing requirements across many states over the analysis period from 1997-2018. I collected current pharmacy technician training requirements from a variety of websites aimed at workers, then confirmed current regulations and tracked their legislative and regulatory history using a legal research database. I coded licensing requirements along three dimensions: registration, defined as filling out an application with the Board of Pharmacy; training, defined as completing a board-approved training program (including employer-provided training); and certification, defined as passing a state-written or other board-approved examination such as PTCB certification.

I find that different kinds of licensing requirements have different effects on the demand for formal credentials. Registration on its own has little effect. However, when states require the completion of a training program, college program completions increase substantially, even though on-the-job employer-sponsored training programs meet the requirement. When states require the passage of an examination, PTCB certifications increase. In both cases, private entities—for-profit colleges and private certifying bodies—have taken on outsize roles, compared to publicly funded and subsidized community colleges and board-written exams.

Larger increases in for-profit completions occur despite the much higher tuition charged by for-profit colleges: for-profit pharmacy technician training programs charged on average about $15,000 in tuition in 2017-2018 compared to $3,000 for community college programs. However, the average for-profit program graduated 60 students per year, while the average public program graduated only 18 students. This difference likely reflects binding enrollment caps in public programs—many community colleges cap programs at 20 students. Because states do not expand public programs to meet demand created by their own licensing laws, therefore, for-profit colleges are significant beneficiaries of licensing laws with training requirements.

Professional certification requirements have gradually replaced board-written exams. The PTCB and other certifying bodies like it therefore benefit from licensing in a similar way to colleges. Certification is not nearly as expensive as a college program (nor as costly to provide), but the fee is over $100 to take the examination. While a few states require both the completion of a training program and obtaining certification, for the most part these two requirements act as substitutes, alternative ways for workers to demonstrate competence prior to obtaining their official pharmacy technician license.

This paper contributes to the literature on occupational licensing and the economics of higher education. First, I add to our understanding of the costs and benefits of occupational licensing by considering a neglected player: the institutions that sell credentials that qualify workers for licensure. I show that certain licensing requirements significantly bolster demand for for-profit colleges and national certifying bodies, which financially benefit from licensing. It remains a question how much value workers, employers, and consumers gain from these credentials. In two resume audit studies that estimate the value of sub-baccalaureate credentials from different college sectors, both found no statistically-significant difference in callbacks for healthcare jobs between resumes listing for-profit credentials versus community-college credentials, relative to no postsecondary credentials at all [23, 26]. One possible explanation is that the postsecondary credentials simply fulfilled licensing requirements, and the license itself was the qualification that mattered to employers.

Second, I offer additional evidence in a new context that for-profit colleges are more responsive to demand than community colleges. [32] find that for-profit college program enrollment follows employment and wage growth more so than public college program enrollment. [55] exploits a change in the occupational regulation of dental assistants that leads to increased demand for vocational training in higher education. She finds that for-profit programs expand their capacity to meet increased demand, while public programs remain the same size. Putting together these results, one could argue that when states add licensing regulations that require training, they should also consider adding additional funding for the expansion of community college training.

## 2.2 Background on Pharmacy Technician Licensing

### 2.2.1 Pharmacy Technicians

Pharmacy technicians are assistants to pharmacists. They work in stand-alone pharmacies and in hospitals, helping pharmacists prepare medications, process requests, and handle insurance claims. They may also stock shelves and operate the cash register behind the pharmacy counter. Pharmacy technician is the largest health-support occupation after nursing assistant, home health aide, and medical assistant. Getting a job as a pharmacy technician traditionally required no more than a high school education, but state licensing regulation and the number of relevant college training programs have increased dramatically in the past twenty-five years. The fraction of pharmacy technicians with some college education or more has increased from little more than half in the late

1990s to nearly 80% in 2018 (figure 1). The annual salary of a pharmacy technician was about $32,000, or just a thousand dollars less than the median annual individual income in the United States in 2018.

### 2.2.2 Licensing Requirements

Many types of regulations affect pharmacy technicians, including scope of practice, staffing, and licensing regulations. Scope of practice regulations limit what health support staff can do by defining what can only be done by a pharmacist. Staffing regulations specify minimum ratios of pharmacist to support staff and required levels of supervision. I focus on licensing regulations, which require pharmacy technicians to meet specified requirements before they can legally work in the role.

### 2.2.3 Categorizing and Tracking Licensing Requirements

I gathered information about current pharmacy technician licensing requirements from multiple sources including the National Council of State Legislatures, the Pharmacy Technician Certification Board (PTCB), the Emily Jerry Foundation, and the Pharmacy Times. I confirmed or corrected the requirements by checking the state's relevant statutes and administrative code using LexisNexis and/or checking the state's Board of Pharmacy website where the statutes and code were unclear. Finally, I used LexisNexis to track the timing of changes in licensing requirements backward to 1997, the first year of my outcome data. Some states' archived laws and regulations do not go back as far as 1997, in which case I used information available from state websites and administrative materials to estimate the year a given licensing requirement was implemented.

After collecting licensing information for each state and assessing patterns, I coded licensing requirements on three dimensions: registration, training, and certification. The registration dimension indicates whether or not the state requires workers to apply to an agency for registration, licensure, or a permit in order to legally work as a pharmacy technician. Registration is essentially the minimal licensing requirement. States that register pharmacy technicians may also require training or certification or both. I define a training requirement as a requirement that pharmacy technicians complete a board-approved training program or training program with a board-specified curriculum.[1] I define a certification requirement as a requirement that pharmacy technicians pass a board-approved examination. The PTCB examination is preferred by most state boards, but some allow various alternatives. Many states provide a grace period of time between registering with the board of pharmacy and completing training and/or certification requirements so that workers can gain some experience as trainees before becoming fully licensed.

States' regulation of pharmacy technicians has expanded on all three dimensions over my analysis period from 1997 to 2018. Figure 2.2 compares licensing requirements in the first year of my data, 1997, to the last year, 2018. In 1997, a minority of states required pharmacy technicians to register with the government, the minimal licensing requirement. But as of 2018, only a handful of states did not require registration. The number of states requiring training grew with the number requiring registration. Certification is a more recent phenomenon, with the PTCB only beginning to offer certification in 1995. In 1997, only a few states required certification, but about half did so by 2018. Figure 2.3 Panel A shows how the number of states adopting each requirement grew in the years between 1997 and 2018. Panel B additionally shows how states' combination of the three types of requirements have changed over time. Beginning around 2007, the number of states requiring only registration began declining as states added either training or certification requirements.

[1] Many states' laws or regulations state that employers should train pharmacy technicians in their duties—this would not meet my definition of a training requirement, as the training is at the full discretion of the employer.

## 2.3 Discussion of Hypotheses

I test multiple hypotheses for the effects of registration, training, and certification requirements on workers' acquisition of formal credentials. I expect registration alone to have little effect on the outcomes I consider, because it is essentially an administrative hurtle rather than competency requirement. However, registration requirements may increase the expected value of credentials because they may signal the professionalization and importance of the occupation.

Training requirements are likely to increase demand for college programs because completing these programs is usually sufficient, though not necessary, to meet a training requirement. In terms of completions by sector, I expect for-profit colleges to be more responsive to demand [25]. On the other hand, community colleges, as public entities, may coordinate with the licensing board to offer enough training for new workers.

Certification requirements should increase PTCB certifications because this certification is widely accepted by states; some states in fact require this exact certification, so we would expect some effect as long as workers stay in the profession. To the extent that training and certification are complementary in the labor market, training requirements may also lead more workers to obtain certification and vice versa.

## 2.4 Data and Empirical Strategy

Estimating the causal effect of licensing is hindered by its potential endogeneity with supply and demand of healthcare workers. In a cross section of states, those that license a particular occupation may also have higher demand for that occupation and therefore more students enrolling in the relevant training regardless of licensing requirements. The same pattern may hold within a single time series, where licensing requirements are added at the same time that demand for the occupation rises. A solution to these problems is to use a longitudinal panel of states, occupations, or both, so that states or occupations without changes in licensing can provide an estimate for counterfactual trends. I use a panel of all 50 states from 1997 to 2018 to estimate the causal effect of occupational licensing requirements for pharmacy technicians in a difference-in-difference framework. I also add an occupation comparison in a triple-difference model as a robustness check.

### 2.4.1 Data Sources

#### Higher Education Data

I use data on college program completions and prices from the Integrated Postsecondary Education Data System (IPEDS), which includes all colleges whose students receive federal aid. I identify relevant programs using a crosswalk between occupation codes and CIP (classification of instructional programs) codes. Outcomes variables include the number of colleges offering pharmacy technician programs, enrollment and completion in those programs, and program prices. Colleges must report completions for every program (by 6-digit CIP code), while enrollment is only reported at the college level. I identify programs simply by counting the number of colleges with a positive number of completions reported under the pharmacy technician CIP code. Prices are available for the vast majority of programs, but colleges that charge different prices by program are required only to report the prices of their six largest programs. In my sample, the exact price is available for about 90% of pharmacy technician programs.

### Certification Data

I use data on the number of new certifications each year from the Pharmacy Technician Certification Board (PTCB). Their website includes a database of certificate-holders with the date they were first certified and their location. I collapse this data to the state-year level to create a variable for the flow of new certifications. As noted above, states that require a board-approved examination appear to always approve and often explicitly require PTCB's Certified Pharmacy Technician examination. However, many states allow alternatives, sometimes even examinations that conclude a college program.

## 2.4.2 Empirical Strategy

I use a difference-difference estimation strategy, in which I compare outcomes across states and years. My identifying variation comes from variation in states' pharmacy technician licensing requirements and variation in the timing of when each state announced registration, training, and certification requirements. I separately estimate the effects of the following licensing requirement combinations: registration only; registration and training only; registration and certification only; and registration, training, and certification.[2] Interpreting the resulting estimates as causal requires assuming that, for any given state adding a new licensing requirement in a particular year, other states provide an accurate counterfactual for how the outcome would change in that state in the absence of the new licensing requirement. This assumption would be violated if the state adding the requirement is in a different trajectory in terms of the outcomes (differential trends), or if the state adding the requirement is implements another change that affects the outcome (policy shock).

[2]Table 2.1 reports the treatment years in my data for each state and licensing requirement.

In my context, factors correlated with both new licensing requirements and the growth of pharmacy technician training programs in colleges and/or professional certification could cause bias from differential trends or policy shocks. One potential source of bias from differential trends is that state with higher demand for pharmacy technicians tend to add new licensing requirements. A source of bias from policy shocks is that states passing new licensing requirements also change other aspects of pharmacy technician regulation at the same time, such as expand scope of practice. Another shock could be the opening of a for-profit college. These would positively bias my estimates, because the estimates would capture increases in training and certification resulting from these factors in addition to the effect of licensing requirements.

My empirical strategy addresses these concerns in a few ways. First, I use event studies to test for differential trends leading up to the addition of new licensing requirements. If demand for pharmacy technicians was ramping up in states that add licensing requirements, these event studies would show this in the five years leading up to the new requirement. Second, I use an additional occupation comparison in a robustness check. This additional control group could difference out both differential trends and policy shocks if they are broader than the pharmacy technician occupation. For example, the state passes a new healthcare regulation law, or a new for-profit college opens offering vocational training for various healthcare positions. Finally, my strategy tests whether licensing requirements have different effects according to their precise requirements. Even if licensing laws for pharmacy technicians are generally correlated with the growth of this occupation and everything that comes with that, this would not explain why state choosing training requirements as competency requirements see different outcomes relative to states choosing certification requirements as competency requirements.

The models below estimate the average treatment effect of each licensing requirement combination. For outcomes with many zeroes in my data (program completions and programs offerings), I use Poisson Quasi-Maximum-Likelihood-Estimation (QMLE) with robust standard errors, which is similar to a log-linear model but can handle counts of zero [21, 47, 51]. I use a simple log-linear model for the remaining outcomes. For both models, I interpret all coefficients as percent changes, which is a local approximation. I cluster standard errors at the state level to allow errors to be correlated over time within states.

**Difference-in-Difference Model**

I use the difference-in-difference model described in Equation 2.1. Observations are uniquely identified by state and year.

$$\begin{aligned} ln(Y_{st}) = \beta_0 &+ \beta_1 \text{R-only}_{st} + \beta_2 \text{R-and-T-only}_{st} + \beta_3 \text{R-and-C-only}_{st} \\ &+ \beta_4 \text{R-and-T-and-C}_{st} + \delta_t + \mu_s + \varepsilon_{st} \end{aligned} \tag{2.1}$$

The parameter of interest $\beta_1$ represents the differential percent change in outcome $Y$ when state $s$ adds a particular licensing requirement. Year fixed effects $\delta_t$ control for changes in outcome $Y$ that are common across states, and state fixed effects $\mu_s$ control for these fixed differences over the sample period.

While the above model is estimated at the state level, I also estimate the effect of licensing requirements on completions at the program level. For these analyses I use the following model, where $c$ indexes colleges. The identifying variation is still at the level of state-year.

$$\begin{aligned} ln(Y_{cst}) = \beta_0 &+ \beta_1 \text{R-only}_{st} + \beta_2 \text{R-and-T-only}_{st} + \beta_3 \text{R-and-C-only}_{st} \\ &+ \beta_4 \text{R-and-T-and-C}_{st} + \delta_t + \gamma_c + \varepsilon_{cst} \end{aligned} \tag{2.2}$$

In Equation 2.2, $\gamma_c$ is an individual program fixed effect parameter. Here $\beta_1$ through $\beta_4$ represents the differential percent change in $Y$ in a pharmacy technician program within a particular state when that state implements a particular new licensing requirement, relative to such programs in other states.

**Event-Study Model**

Even with a panel of states, estimating the causal effect of changes in occupational licensing is hindered by the possibility that states change their licensing requirements in response to state-and-occupation-specific changes in the labor market or education market that affect the outcomes of interest. This would violate the assumption of common trends in outcomes between treated and untreated states that is necessary for causal identification in a difference-in-difference framework.

Below is an event-study model for the state-level difference-in-difference specification estimating the effect of a new training requirement. The model includes controls for the other licensing requirement combinations that do not include training, so that the effect of training is captured by the $\beta_1$ coefficients. I also estimate versions of this model replacing the Training term with Registration or Certification.

$$\begin{aligned} ln(Y_{st}) = \beta_0 + \sum_{\tau=-5,\tau\neq-1}^{5} \beta_{1\tau}\text{Training}_{st}^{\tau} + \beta_2\text{R-only}_{st} \\ + \beta_3\text{R-and-C-only}_{st} + \delta_t + \gamma_s + \varepsilon_{st} \end{aligned} \tag{2.3}$$

The interpretation of the parameters in Equation 2.3 are the same as in Equation 2.1, except there are now several parameters estimating the effect of training requirements in each year relative to the year before the policy change was announced ($\tau = -1$). If the outcome $Y$ was trending similarly in treatment and control states prior to the announcement of the training requirement, then the estimates for $\beta_{st}^{\tau}$ with $\tau$ less than zero should be statistically indistinguishable from zero.

For observations more than five years before or after the treatment year, I set $\tau$ equal to $-5$ or 5 such that $\beta_{st}^{-5}$ and $\beta_{st}^{5}$ actually average the differential percent changes in all years before and after the endpoints of the event window. For this reason, the coefficients from $\beta_{st}^{-4}$ to $\beta_{st}^{4}$ are more important for testing parallel trends and identifying the short-run effects of licensing requirements in these models.

**Triple-difference Model Specification**

As a robustness check, I also estimate triple-difference models, which add in an occupation comparison between pharmacy technicians and medical assistants. The triple-difference specification at the state level is shown in Equation 2.4. Observations are uniquely identified by occupation $o$, state $s$, and year $t$, and occupations are limited to pharmacy technicians and medical assistants.

$$\begin{aligned} ln(Y_{ost}) = \beta_0 + \beta_1\text{R-only}_{ost} + \beta_2\text{R-and-T-only}_{ost} + \beta_3\text{R-and-C-only}_{ost} \\ + \beta_4\text{R-and-T-and-C}_{ost} + \delta_{st} + \gamma_{ot} + \mu_{os} + \varepsilon_{ost} \end{aligned} \tag{2.4}$$

In Equation 2.4 the parameters of interest $\beta_1$ through $\beta_4$ represent the differential percent change in outcome $Y$ for pharmacy technicians relative to medical assistants when state $s$ adds a particular licensing requirement, relative to the same change in $Y$ for pharmacy technicians relative to medical assistants in other states. The three fixed effect parameters control for confounding trends in a given state over time (state-by-year fixed effects $\delta_{st}$), trends in the pharmacy technician occupation over time (occupation-by-year fixed effects $\mu_{ot}$), and fixed differences in outcomes for pharmacy technicians and medical assistants in different states (occupation-by-state fixed effects $\mu_{os}$).

For program-level analyses I use the following model. The identifying variation is still at the level of state-occupation-year.

$$
\begin{aligned}
ln(Y_{ocst}) = \beta_0 + \beta_1 \text{R-only}_{ost} + \beta_2 \text{R-and-T-only}_{ost} + \beta_3 \text{R-and-C-only}_{ost} \\
+ \beta_4 \text{R-and-T-and-C}_{ost} + \delta_{st} + \gamma_{ot} + \mu_{os} + \theta_{oc} + \varepsilon_{ocst}
\end{aligned}
\tag{2.5}
$$

In Equation 2.5, each combination of $c$ and $o$ is an individual program, so $\theta_{oc}$ is an individual program fixed effect parameter. Here $\beta_1$ through $\beta_4$ represents the differential percent change in $Y$ in a pharmacy technician program relative to the same change in medical assistant programs when the state implements a particular new licensing requirement.

## 2.5 Results

### 2.5.1 Overall Effects on Completions and Certifications

Table 2.2 reports the main estimates for the the effect of pharmacy technician licensing requirements on college program completions, number of programs, and certifications. Here, each column is a different regression, and each row contains the coefficients estimating parameters $\beta_1$ to $\beta_4$ in the difference-in-difference model (Equation 2.1). Each coefficient can be interpreted as the percent-change in the outcome before versus after a state adopts the combination of licensing requirements in that row.

What we see in these overall results is that each combination of licensing requirements generally has the expected impact on the markets for college training and professional certification. Registration on its own does not have a detectable effect on these outcomes. Registration and certification only has positive estimates across all outcomes, but only the effect on PTCB certifications in column (3) is statistically significantly different from zero. The coefficient 0.29 suggests that certifications increase by about 30% after a state adopts this combination of requirements. After a state adopts registration, training, and certification, certifications increase even more on average, by over 60%.

In contrast, when a state adopts registration and training only, the estimate for certifications drops to 0.03, while the estimates for college completions and college programs are far larger and statistically significantly greater than zero. When a state adopts registration and training requirements, the estimates in columns (1) and (2) imply that pharmacy technician program completions increase by about 76% and the number of new programs increases by about 26%. Interestingly, the estimated effects of all three requirements on these college outcomes are inconsistent and noisy, but the effect on completions is similar in size to the corresponding estimated effect on certifications.

These estimates can only be interpreted as causal under the assumption that there would be no difference between the outcomes of the treatment and control groups in the absence of the new licensing requirements. The event studies test the plausibility of this assumption by testing for differences between the treatment and control groups leading up to the new licensing requirements. Figure 2.4 presents the event studies corresponding to each of the statistically significant effects in Table 2.2. In Panels A and B, the figure plots the coefficients and 95% confidence intervals from regressions corresponding to Equation 2.3, which indicate the percent-change in completions or programs in states that implemented training requirements relative to the year prior to implementation. In Panel C, the figure plots the coefficients from a version of Equation 2.3 focusing on the implementation of certification requirements. While Panels A and C show parallel trends leading up to the new requirement, Panel B does not, suggesting that we should not interpret the difference-in-difference estimate in column (2) of Table 2.2 as causal. Rather than causing a jump in the number of pharmacy technician programs, the positive estimate in the table was driven by a preexisting longer-term pattern in the growth of such programs in treated states.

Given that training requirements appear to increase completions of college pharmacy technician training programs, the next sections take a closer look at this response in the higher education market, beginning with overall effects by sector.

### 2.5.2 Effects in Public Versus For-Profit Colleges

Next I examine the relative magnitude of the effect of new licensing requirements in the public versus for-profit college sectors. Comparing the size of estimates in each sector may shed light on whether the public sector is able to meet demand for training as a result of these public regulations, or whether the for-profit sector is more responsive to that demand. Combined with average tuition in each sector, the estimates can also shed light on how much tuition money flows to each sector as a result of licensing laws.

Table 2.3 formally compares for-profit versus public pharmacy technician programs. Some characteristics are measured at the college level (Panel A) and others at the program level (Panel B). In Panel A, we see that, among colleges offering pharmacy technician programs, for-profit colleges are less likely to be degree-granting, much less likely to have large enrollments of at least 1,000 students, and more likely to be in an urban location. Looking to Panel B, we see that the most significant differences are in the number of graduates (3 times higher in for-profit programs), the percentage of non-white students (65% in for-profit programs compared to 44% in public programs), and the tuition (over 5 times higher at $15,000 in for-profit programs compared to $2,650 in public programs). The tuition difference would suggest that students should choose public programs all else equal. For-profit programs are also somewhat longer. However, for-profit programs may be more conveniently located in cities and have more slots available (capacity).

Table 2.4 reports the estimated effects of each combination of licensing requirements on public and for-profit completions and programs. Comparing columns (1) and (2), the estimated effect of registration and training requirements on for-profit completions is larger than public completions (0.92 compared to 0.31), but both are strongly statistically significant. The coefficient estimating the effect of registration, training and certification requirements is also large and positive for for-profit completions and marginally significant, but insignificant for public completions. In contrast, the coefficients estimating the effect of registration and training in programs are similarly sized between columns (3) and (4). Therefore, this evidence suggests that the effects of licensing requirements are stronger and larger at for-profit colleges and programs. The pre-treatment mean of for-profit completions is also quite a bit larger (185 for-profit completions compared to 65 public completions), indicating that the already larger percent-increase translates into an even larger numerical increase relative to public completions.

The event studies in Figure 2.5 also reflect a stronger effect of training requirements on for-profit sector completions. The effect on public completions is delayed by a few years, which lends more support to the hypothesis of a more responsive effect in the for-profit sector. As for the overall results for programs, Panels C and D of Figure 2.5 show relatively flat effects in the years after the training requirements were implemented as well as some pre-trends in these outcomes leading up to the treatment year.

### 2.5.3 Program-level Effects

The previous estimates were at the state level. Now I turn to the results from estimating the program-level model in Equation 2.2. These results may differ from the state-level results if state-level completion effects are driven by new programs rather than increases within individual programs, or if state-level completion effects are concentrated within a few programs within each state.

What we see in Table 2.5 is that estimates at the program level are largest and much more statistically significant in for-profit programs. In fact the coefficients in column (2) estimating the effects of registration with additional training or certification requirements on completions within public programs are negative. Turning to the corresponding event studies in Figure 2.6, these negative coefficients are driven by a negative pre-trend rather than a negative post-treatment effect. In general, these program level analyses further support the finding of stronger effects in for-profit programs.

### 2.5.4 Program Prices

In a competitive market, increased demand for training due to new training requirements would lead to increased prices for training. Table 2.6 reports the estimated effects of licensing requirements on college program prices, overall and by sector. Surprisingly, here all of the coefficients in the last two rows, estimating the effects of requirements including training on overall, public, and for-profit prices are statistically indistinguishable from zero and in fact negative in sign. However, the minimal licensing requirement of registration only here has a positive statistically significant effect on for-profit program prices of 11% from a base price of about $14,620.

## 2.6 Robustness

In this section I weaken the assumptions of the analysis and test the robustness of the results in three different ways. First, I add the medical-assistant control group, which allows me to control for state-by-year, occupation-by-year, and state-occupation factors that could bias the difference-in-difference estimates. Second, I relax the functional form assumption behind using Poisson count models and instead estimate linear models after transforming outcome data using the inverse hyperbolic sine function. Third, I examine more closely the identifying variation of my main difference-in-difference estimates using Bacon decomposition.

### 2.6.1 Triple-Difference Model

Difference-in-difference estimates for the effect of pharmacy technician licensing requirements rely on exogenous variation in the timing of those requirements' adoption and implementation across states. This timing may not be exogenous to training and certification outcomes if it occurs in response or alongside other changes in healthcare markets that would affect these outcomes on their own and are not attributable to licensing requirements. By adding another healthcare occupation in a triple-difference model, I can control for such factors by isolating only the difference in outcomes between two healthcare occupations.

I use medical assistants as the occupation control group because of this occupation's similarity to pharmacy technicians, and because it remains an unlicensed occupation in the vast majority of states. Figure 2.8 shows that trends in program offerings and program completions have been broadly similar across the pharmacy technician and medical assistant training. Pharmacy technician and medical assistant programs are also similar in terms of program length, academic calendar, and completer demographics, and tuition, though medical assistant programs are significantly larger. Both medical assistant and pharmacy technician programs are also disproportionately female and nonwhite, compared to non-pharmacy-technician programs. These similarities support the use of medical assistant programs as a comparison program for triple-difference models.

Tables 2.7 through 2.10 are versions of Tables 2.2 through 2.6 that report coefficients from regressions based on the triple-difference specification from Equations 2.4 (state-level) and 2.5 (program-level). The results are very similar to the difference-in-difference specification, lending confidence to the interpretation of those estimates as resulting from pharmacy-technician licensing requirements rather than differential trends in markets for healthcare vocational training across states. One difference worth noting is that the positive effect of registration on for-profit program tuition from Table 2.6 is not present in Table 2.10.

### 2.6.2 Functional Form

I use Poisson or count models in the main specification because of the high frequency of zeros in pharmacy technician completions and programs across many state-years in my sample. Another way of handling zeros is to use the inverse-hyperbolic-sine (IHS) function. This is similar to a log transformation, and can be interpreted similarly, but it is defined for values of zero. Tables B.1 through B.3 are versions of Tables 2.2 through 2.5 that report coefficients from linear regressions using the IHS transformation on outcomes. Again, the results are similar to the difference-in-difference specification. However, some of the coefficients, though similar, are no longer statistically significant. The main qualitative results hold and continue to show a stronger response in the for-profit sector.

### 2.6.3 Variation in Treatment Timing

Several recent papers have considered how difference-in-difference estimates can be biased or misleading in designs with different treatment dates across treated units, as in my context [10, 16, 34, 53]. There are two aspects of these designs that can cause issues. First, earlier-treated units are used as control units for later-treated units in the difference-in-difference regression. This can cause bias if there are dynamic treatment effects, violating the assumption of parallel trends between control units and later-treated units. Second, even if there are not dynamic treatment effects, the difference-in-difference estimate is a variance-weighted average treatment effect where more weight is given to the treated units treated in the middle of the analysis period, because they have higher variance. This is problematic if treatment effects differ between the earlier, middle, and later treated units.

The event studies presented earlier help with these issues. First, they check for potential violation of the parallel trends assumption. Second, they avoid using earlier-treated units as controls for later-treated units and limit the outsize weight of middle-treated units by limiting the focus to the event window and estimating coefficients by event-time. Another way to check whether the difference-in-difference regression estimates suffer from biases due to the variation in treatment timing is to perform a Bacon-decomposition [35]. This decomposition reveals, for a given difference-in-difference regression, the weight given to comparisons of each combination of always-treated, never-treated, and sometimes-treated units. Doing this for each type of licensing requirement reveals that comparisons between never-treated and sometimes treated units provide the majority of the identifying variation for the effect of training requirements (71%), about half of the identifying variation for the effect of certification requirements (51%). This is reassuring because it means these estimate are not relying on comparisons with other treated units. However, only 14% of the identifying variation for the effect of registration requirements is from comparisons between never-treated and sometimes treated units, which suggests these estimates should be interpreted with greater caution.

## 2.7 Discussion of Results

The results presented above suggest that the specific combinations of requirements included in pharmacy technician licensing regulations matter for their effects on the market for vocational higher education. In particular, licensing regulations that require workers to complete a board-approved training program cause more workers to enroll in and complete sub-baccalaureate training programs offered at public and for-profit colleges. This entails costs to these workers through opportunity cost and tuition costs, and significant revenues to colleges, in particular for-profit colleges. Consider a worker who seeks a job as a pharmacy technician. If her state requires completion of a board approved training program, she might postpone employment for nine months to complete a pharmacy technician college program, incurring an opportunity cost of about \$21,000 (three-fourths of an annual salary of \$28,000), and tuition cost of either \$3,000 at a community college or \$15,000 at a for-profit college. Even if her annual salary increased by 7% to \$30,000, it would take 15 years for the increased wages to cover the training cost. Meanwhile, estimates suggest that a typical for-profit program may enroll an additional 100 students per year, making \$1,500,000 in additional annual tuition revenues.

Despite large differences in tuition, I find that for-profit completions increase by more than public completions. One reason why students enroll at higher rates in more-expensive for-profit programs may be enrollment caps in community college programs. A simple search of community college websites reveals that enrollment caps of about 20 students are common for pharmacy technician programs. In fact, the average public program in my data had 18 graduates per year in the year prior to a new training requirement, compared to about 60 graduates in the average for-profit program. [55] similarly finds that for-profit dental assistant programs expand to meet increased demand while public programs do not. Other research has shown that when community colleges receive the public funding that allows them to expand capacity, they crowd out for-profit enrollment [13].

While I have shown that the requirement to complete a board-approved training program does lead many students to enroll in and complete college programs, state laws and regulations do not only approve of training through college programs. States allow and often encourage employers themselves to develop training program curricula and submit them for approval by boards of pharmacy. Some states also pre-approve any training program accredited by the American Society of Health Pharmacists, which accredits many college-based programs but also programs offered by Walmart and CVS. Because many non-college training programs are also approved by boards of pharmacy, the number of pharmacy technicians becoming licensed each year in a given state with training requirements is more than the number graduating from that state's college pharmacy technician programs. For example, Florida, which began implementing training requirements in 2008, the number of graduates from college programs was 3,119 in 2013 while the number of newly licensed pharmacy technicians was 5,572 according to state data.

## 2.8 Conclusion

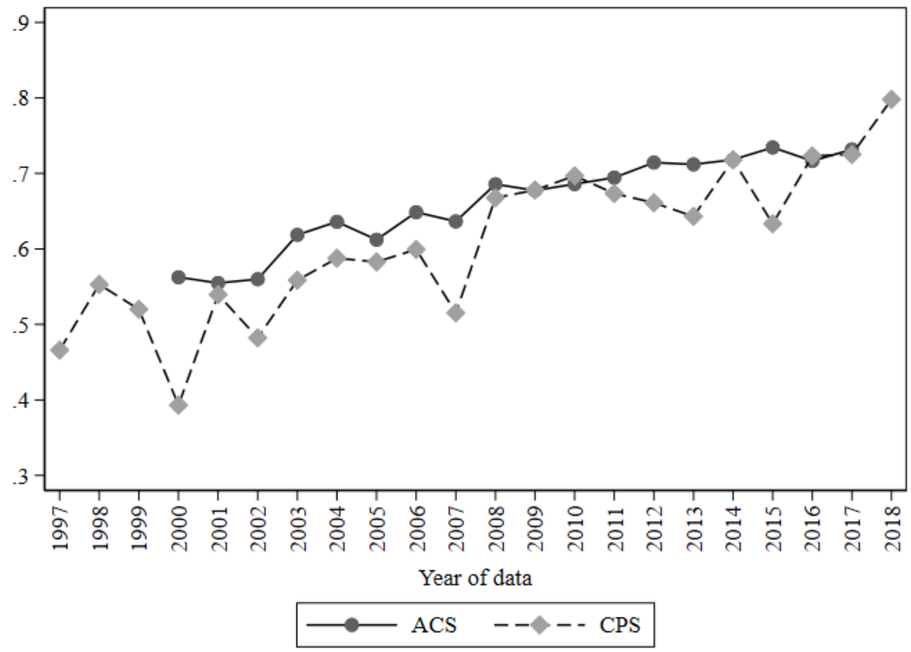
This paper provides estimates for the impacts of different types of licensing requirements for pharmacy technicians on markets for higher education. I find evidence that training requirements shift demand for college training outward, increasing the number of program completions in both the public and for-profit sectors, but by more in the for-profit sector.

Licensing requirements are intended to safeguard consumers by solving an information problem. However some are wary of interest groups lobbying for increasingly costly requirements, usually citing incumbent workers who are exempted from new requirements. This analysis suggests that colleges represent another interest group that benefits from licensing requirements that push workers into college training programs.

The welfare effects of different licensing requirements ultimately depend not only on the cost of training, but the compensation for these workers in the labor market and consumer surplus from new information on worker quality. If certification requirements provide the same information as college credentials, the results of these analyses suggest that certification may be a less costly alternative to training requirements for workers. And if public colleges provide as valuable training as for-profit colleges, the higher price tag of for-profit programs suggests that state governments can ease the costs of meeting training requirements by expanding public program offerings.

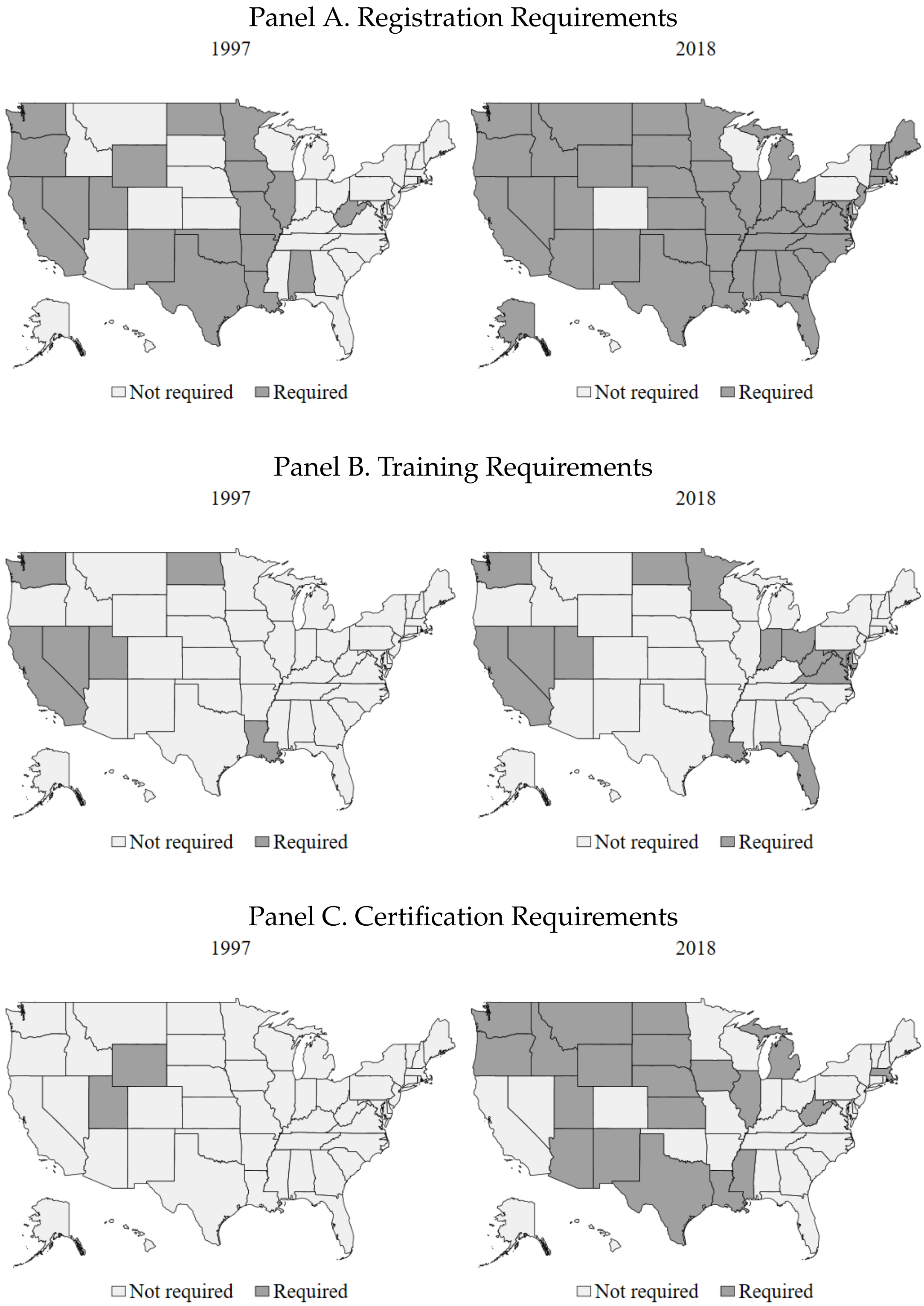
# Figures

Figure 2.1: Fraction of Pharmacy Technicians with Some College or More, 1997-2018




Note: ACS=American Community Survey. CPS=Current Population Survey. Pharmacy technicians are identified in the ACS and CPS as non-pharmacist health workers in drug stores, which excludes pharmacy technicians working in hospitals. Some college or more includes 1- and 2-year college programs as well as unfinished college programs.
Source: IPUMS-USA and IPUMS-CPS

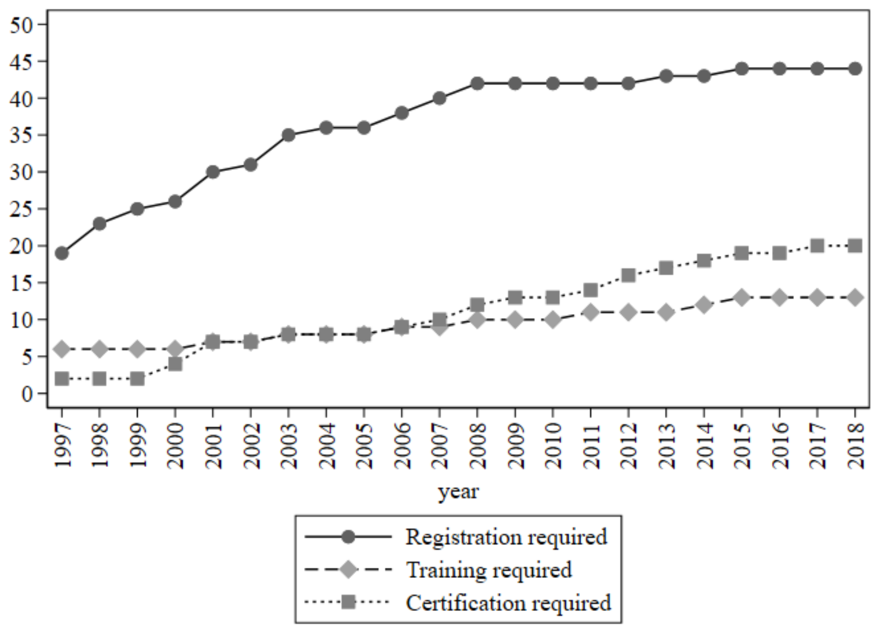
Figure 2.2: State Pharmacy Technician Licensing Requirements in 1997 versus 2018



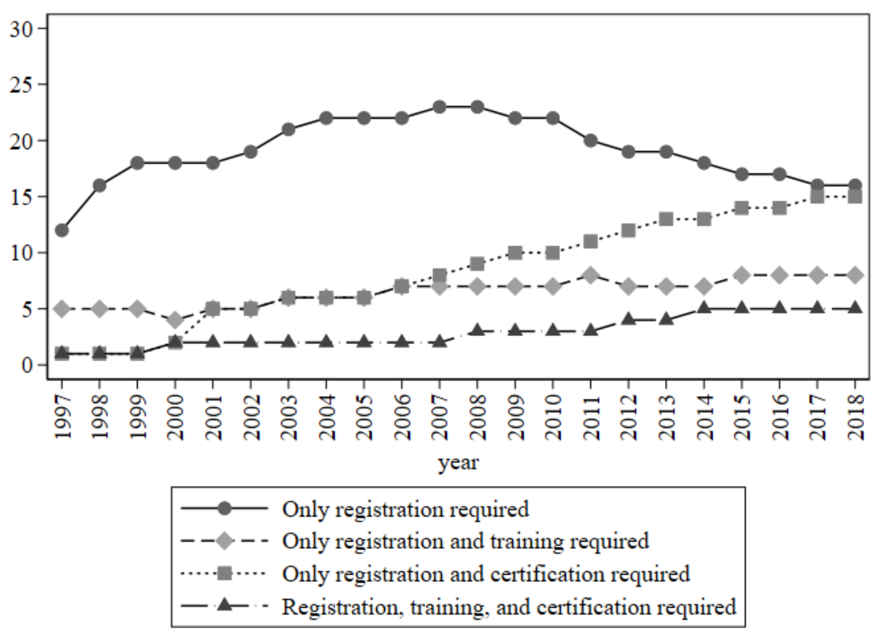

Note: Requirements are not mutually exclusive. States that require training and/or certification always require registration as well.
Source: Author's collection and coding of state laws and regulations.

Figure 2.3: Number of States with Each Pharmacy Technician Licensing Requirement or Combination of Requirements, 1997-2018

Panel A. Individual Requirements



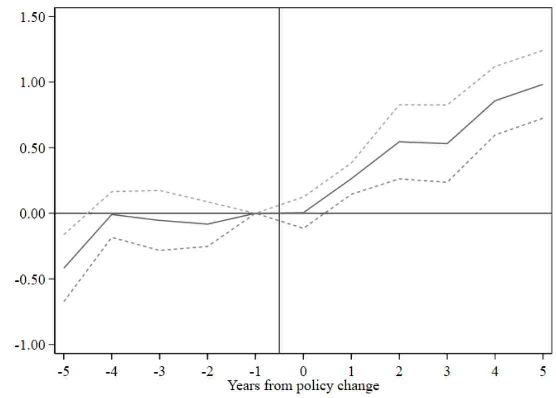

Panel B. Combinations of Requirements



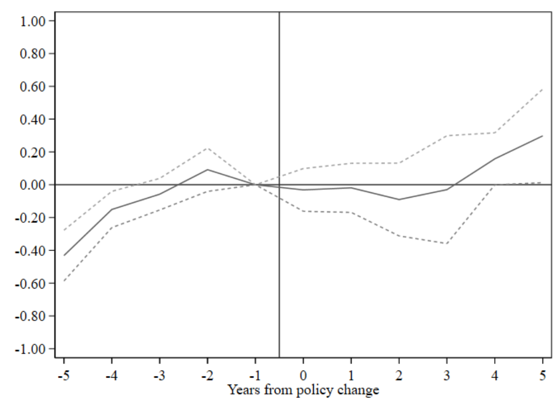

Note: The two panels show two different ways of categorizing states' licensing requirements, with Panel B incorporating combinations of requirements. States that require training and/or certification always require registration as well. Requirements have generally increased over time. The declines in series in Panel B reflect states moving from fewer to more requirements.
Source: Author's collection and coding of state laws and regulations.

Figure 2.4: Event Studies for the Overall Effect of Training and Certification Requirements on Completions, Programs, and Certifications

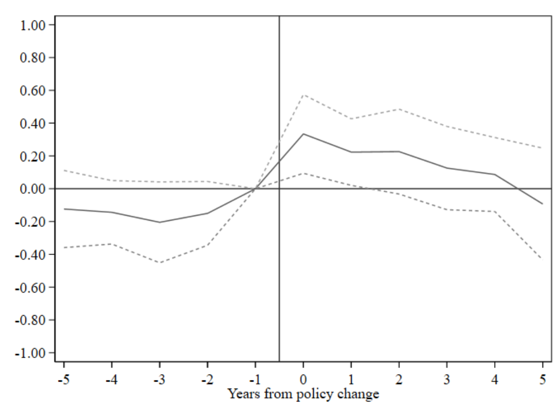
Panel A. Effect of Training Requirements on Completions




Panel B. Effect of Training Requirements on Programs



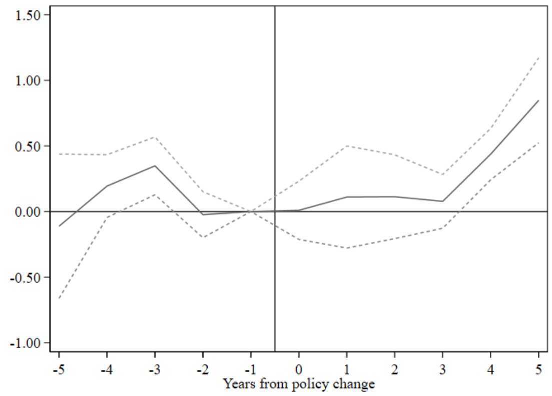

Panel C. Effect of Certification Requirements on Certifications



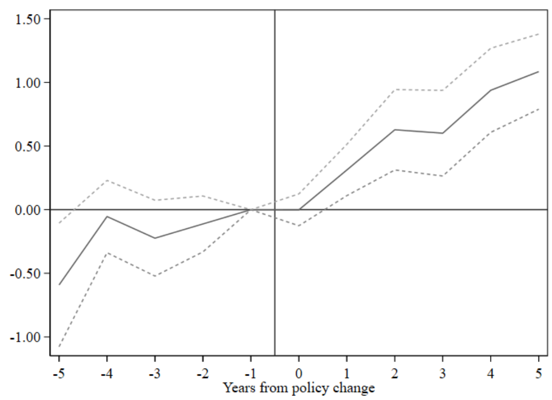

Note: The figure plots coefficients and 95% confidence intervals from regressions corresponding to Equation 2.1, which indicate the percent-change in the outcome in states that implemented training requirements (in combination with registration and/or certification requirements) relative to the year prior to implementation. The coefficients for -5 and 5 years from the policy change are averages of the difference in the outcome for years 5 years or more from the policy change.
Source: Outcome data are from IPEDS and PTCB, and treatment data are from the author's collection and coding of state laws and regulations.

Figure 2.5: Event Studies for the Effects of Licensing Requirements on Completions and Programs, by Sector

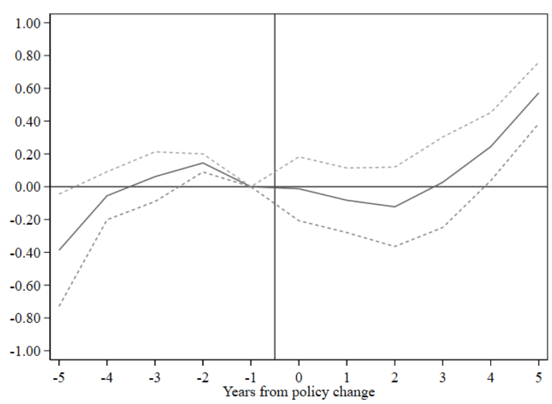
Panel A. Effect of Training Requirements on Public Sector Completions



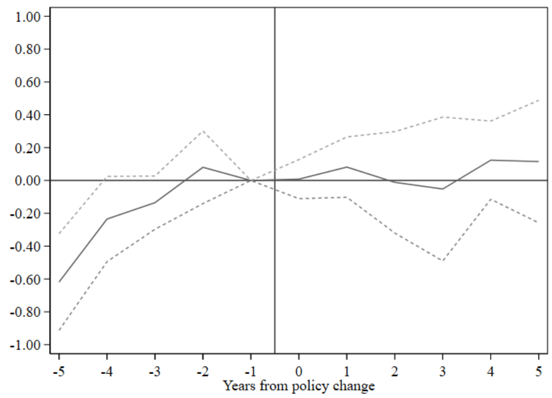

Panel B. Effect of Training Requirements on For-profit Sector Completions




Panel C. Effect of Training Requirements on Public Sector Programs



Panel D. Effect of Training Requirements on For-profit Sector Programs




Note: The figure plots coefficients and 95% confidence intervals from regressions corresponding to Equation 2.1, which indicate the percent-change in the outcome in states that implemented training requirements (in combination with registration and/or certification requirements) relative to the year prior to implementation. The coefficients for -5 and 5 years from the policy change are averages of the difference in the outcome for years 5 years or more from the policy change.
Source: Outcome data are from IPEDS, and treatment data are from the author's collection and coding of state laws and regulations.

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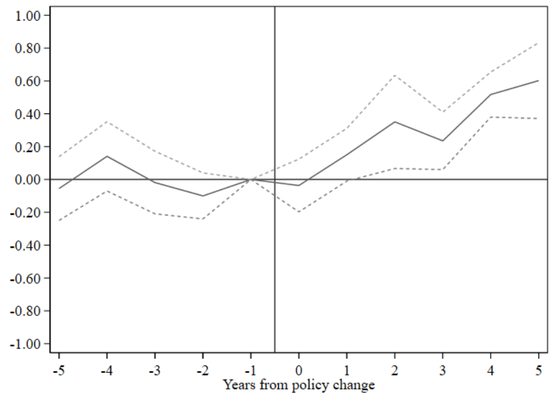
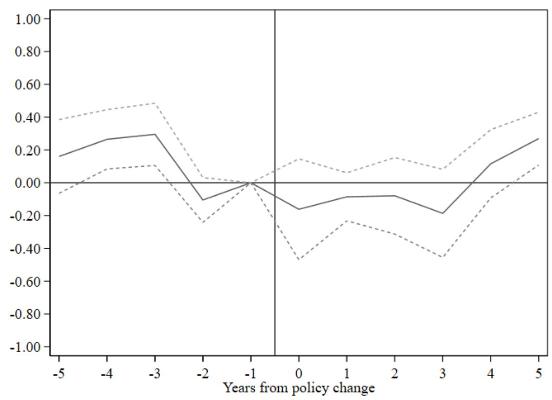
Figure 2.6: Event Studies Corresponding to Table 6 Results

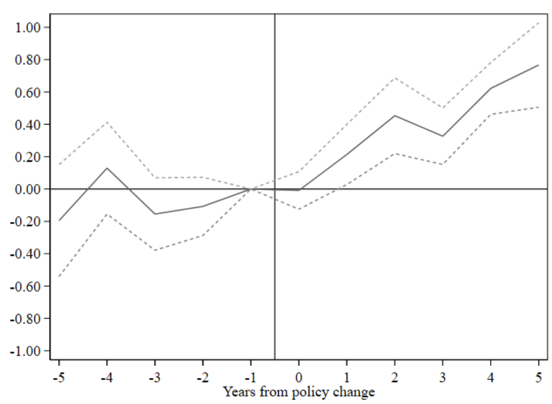
Panel A. Effect of Training Requirements on Overall Completions




Panel B. Effect of Training Requirements on Public Sector Completions

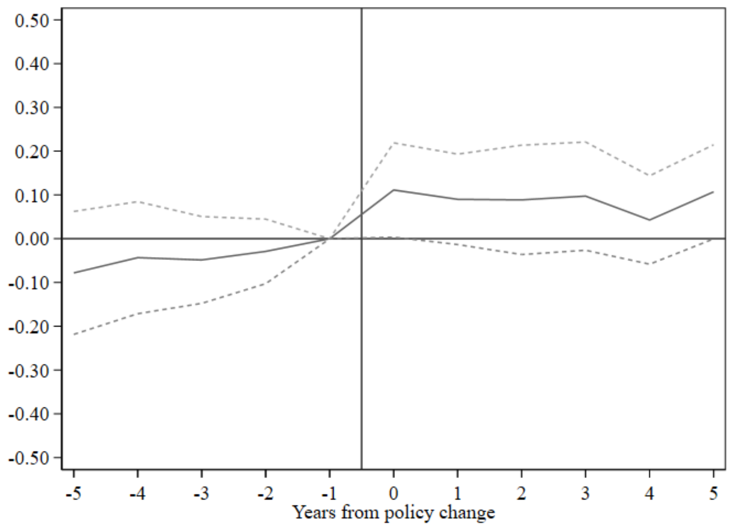



Panel C. Effect of Training Requirements on For-profit Sector Completions




Note: The figure plots coefficients and 95% confidence intervals from regressions corresponding to Equation 2.1, which indicate the percent-change in the outcome in states that implemented training requirements (in combination with registration and/or certification requirements) relative to the year prior to implementation. The coefficients for -5 and 5 years from the policy change are averages of the difference in the outcome for years 5 years or more from the policy change.
Source: Outcome data are from IPEDS, and treatment data are from the author's collection and coding of state laws and regulations.

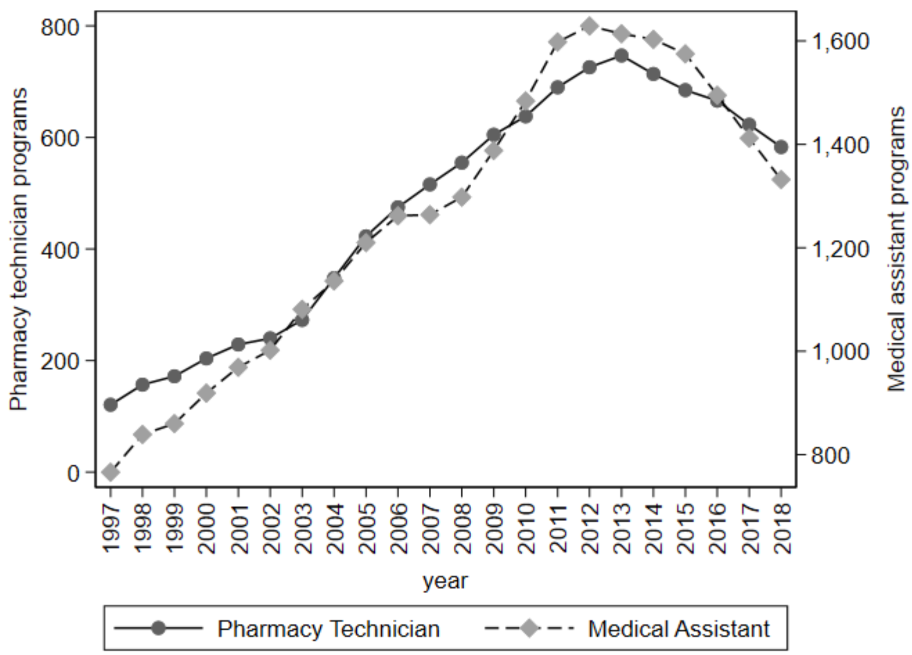
Figure 2.7: Event Study Estimates Corresponding to Table 7 Results



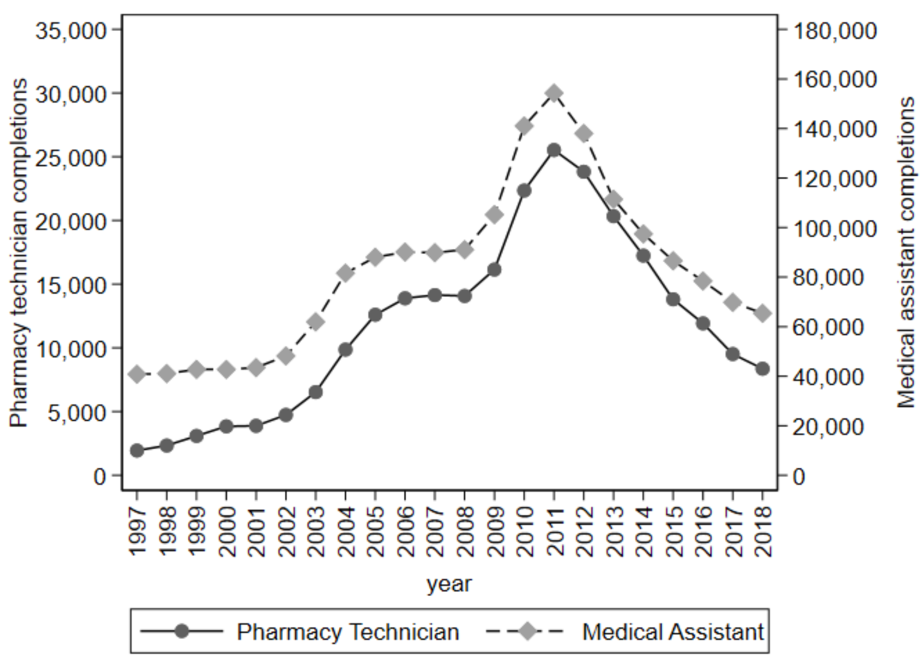

Note: The figure plots coefficients and 95% confidence intervals from regressions corresponding to Equation 2.1, which indicate the percent-change in the outcome in states that implemented training requirements (in combination with registration and/or certification requirements) relative to the year prior to implementation. The coefficients for -5 and 5 years from the policy change are averages of the difference in the outcome for years 5 years or more from the policy change.
Source: Outcome data are from IPEDS, and treatment data are from the author's collection and coding of state laws and regulations.

Figure 2.8: Programs and Completions for Pharmacy Technician and Medical Assistant Training in the IPEDS Universe, 1997-2018

Panel A. Programs




Panel B. Completions




Note: The figure shows the similar trends in the growth of pharmacy technician programs and completions to that of medical assistant programs and completions, which is the occupation used in the triple-difference specification.
Source: IPEDS

## Tables

Table 2.1: Pharmacy Technician Licensing Requirement Treatment Years

| State | Registration | Training | Certification |
|---|---|---|---|
| Illinois | 1989 | 2001 | 2010 |
| Washington | 1990 | 1990 | 2008 |
| California | 1991 | 1991 | |
| North Dakota | 1993 | 1993 | 2012 |
| Nevada | 1993 | 1993 | |
| Wyoming | 1993 | | 2014 |
| Arkansas | 1993 | | |
| Oklahoma | 1994 | | |
| West Virginia | 1995 | 1995 | 2014 |
| Utah | 1996 | 1996 | 2004 |
| Rhode Island | 1996 | 2003 | |
| Alabama | 1996 | 2020 | |
| Louisiana | 1997 | 1997 | 2005 |
| Minnesota | 1997 | 2011 | |
| Texas | 1997 | | 2001 |
| Oregon | 1997 | | 2006 |
| Iowa | 1997 | | 2010 |
| New Mexico | 1997 | | 2010 |
| Missouri | 1997 | | |
| Alaska | 1998 | | |
| South Carolina | 1998 | | |
| Tennessee | 1998 | | |
| Connecticut | 1998 | | |
| Idaho | 1999 | | 2009 |
| Maine | 1999 | | |
| New Hampshire | 2000 | | |
| Virginia | 2001 | 2001 | 2001 |
| Montana | 2001 | 2001 | 2002 |
| Massachusetts | 2001 | 2001 | |
| North Carolina | 2001 | | |
| Mississippi | 2002 | | 2011 |
| Kansas | 2003 | 1999 | 2017 |

Continued on next page

Table 2.1: Pharmacy Technician Licensing Requirement Treatment Years
(Continued from previous page)

| State | Registration | Training | Certification |
|---|---|---|---|
| Arizona | 2003 | 2003 | 2004 |
| Indiana | 2003 | 2003 | |
| Vermont | 2003 | | |
| South Dakota | 2004 | | 2014 |
| Maryland | 2006 | 2006 | |
| New Jersey | 2006 | | |
| Nebraska | 2007 | | |
| Georgia | 2007 | | |
| Florida | 2008 | 2008 | |
| Kentucky | 2008 | | |
| Michigan | 2013 | 2013 | |
| Ohio | 2015 | 2015 | |
| Count | 44 | 21 | 18 |

Note: The table includes only states with registration requirements in place as of 2018. States are ordered by year of registration requirement.
Source: Author's collection and coding of state laws and regulations.

Table 2.2: Estimated Effects of Licensing Requirements on Pharmacy Technician College Program Completions, Number of Programs Offered, and PTCB Certifications (% Change)

| | (1)<br>Completions | (2)<br>Programs | (3)<br>Certifications |
|---|---|---|---|
| Registration only | 0.10 | -0.07 | 0.07 |
| | (0.16) | (0.12) | (0.11) |
| Registration and Certification only | 0.26 | 0.08 | 0.29** |
| | (0.25) | (0.15) | (0.13) |
| Registration and Training only | 0.76*** | 0.26** | 0.03 |
| | (0.19) | (0.12) | (0.12) |
| Registration, Training, and Certification | 0.58 | -0.04 | 0.63*** |
| | (0.36) | (0.32) | (0.20) |
| Pre-treat. Mean | 254 | 15 | 535 |
| R-squared | 0.938 | 0.757 | 0.949 |
| N | 1,100 | 1,100 | 1,100 |

Notes: Each column contains coefficients from a regression following the difference-in-difference specification in Equation 2.1, which include state and year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS and PTCB. Treatment data is from author's collection and coding of state laws and regulations.

Table 2.3: Public Versus For-profit Pharmacy Technician Colleges and Programs, 1997-2018

Panel A. Public versus For-profit Colleges that offer Pharmacy Technician Programs

| Variable | (1) Public Colleges | (2) For-profit Colleges | (3) Difference |
|---|---|---|---|
| Primarily subbaccalaureate | 0.99 | 0.98 | -0.01 |
| Degree-granting | 0.81 | 0.62 | -0.19** |
| Enrollment ≥ 1,000 | 0.82 | 0.12 | -0.70*** |
| Urban | 0.65 | 0.90 | 0.25*** |
| Observations | 4,563 | 5,345 | 9,908 |

Panel B. Public versus For-profit Pharmacy Technician Programs

| Variable | (1) Public Programs | (2) For-profit Programs | (3) Difference |
|---|---|---|---|
| Less than 2 years | 0.80 | 0.78 | -0.02 |
| 2 to 3 years | 0.20 | 0.21 | 0.01 |
| 4-plus years | 0.00 | 0.01 | 0.00 |
| Academic calendar | 0.85 | 0.26 | -0.59*** |
| Number of completions | 10.44 | 29.69 | 19.25*** |
| Female % of completions | 0.82 | 0.79 | -0.03*** |
| Nonwhite % of completions | 0.44 | 0.65 | 0.20*** |
| Contact hours per year | 894 | 1,029 | 135*** |
| Tuition | 2,648 | 15,026 | 12,378*** |
| Observations | 6,166 | 6,313 | 12,479 |

Notes: This table compares the average characteristics of public versus for-profit pharmacy-technician programs. Some characteristics are measured at the college level (Panel A) and others at the program-level (Panel B). Non-profit college programs are omitted because there are very few. Tuition is in 2018 dollars. * $p < .10$, ** $p < .05$, *** $p < .01$

Source: IPEDS

Table 2.4: Estimated Effects of Licensing Requirements on Pharmacy Technician College Program Completions and Number of Programs Offered, by Sector (% Change)

| | (1) Public Completions | (2) For-profit Completions | (3) Public Programs | (4) For-profit Programs |
|---|---|---|---|---|
| Registration only | 0.02 | 0.13 | -0.13 | 0.28 |
| | (0.14) | (0.27) | (0.11) | (0.23) |
| Registration and Certification only | -0.15 | 0.38 | -0.09 | 0.60 |
| | (0.22) | (0.35) | (0.15) | (0.38) |
| Registration and Training only | 0.31*** | 0.92*** | 0.32*** | 0.30* |
| | (0.11) | (0.25) | (0.11) | (0.17) |
| Registration, Training, and Certification | -0.21 | 0.93* | -0.13 | 0.28 |
| | (0.21) | (0.52) | (0.20) | (0.70) |
| Pre-treat. Mean | 65 | 185 | 6 | 9 |
| R-squared | 0.875 | 0.924 | 0.586 | 0.730 |
| N | 1,012 | 1,034 | 1,012 | 1,034 |

Notes: Each column contains coefficients from a regression following the difference-in-difference specification in Equation 2.1, which include state and year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS. Treatment data is from author's collection and coding of state laws and regulations.

Table 2.5: Estimated Effects of Licensing Requirements on Completions Within Pharmacy Technician College Programs, Overall and by Sector (% Change)

| | (1) Overall | (2) Public | (3) For-profit |
|---|---|---|---|
| Registration only | 0.19* | 0.12 | 0.14 |
| | (0.11) | (0.09) | (0.19) |
| Registration and Certification only | 0.19 | -0.12 | 0.20 |
| | (0.12) | (0.13) | (0.22) |
| Registration and Training only | 0.32* | -0.12 | 0.46*** |
| | (0.17) | (0.14) | (0.17) |
| Registration, Training, and Certification | 0.24 | -0.32* | 0.44** |
| | (0.19) | (0.17) | (0.20) |
| Pre-treat. Mean | 14 | 9 | 17 |
| R-squared | 0.663 | 0.545 | 0.653 |
| N | 10,189 | 4,455 | 5,376 |

Notes: Each column contains coefficients from a regression following the difference-in-difference specification in Equation 2.2, which include college and year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS. Treatment data is from author's collection and coding of state laws and regulations.

Table 2.6: Estimated Effects of Licensing Requirements on Pharmacy Technician College Program Prices (% Change)

| | (1) Overall | (2) Public | (3) For-profit |
|---|---|---|---|
| Registration only | 0.09 | 0.03 | 0.11** |
| | (0.12) | (0.05) | (0.05) |
| Registration and Certification only | -0.03 | -0.05 | 0.10* |
| | (0.18) | (0.07) | (0.05) |
| Registration and Training only | -0.07 | -0.07 | -0.06 |
| | (0.13) | (0.06) | (0.06) |
| Registration, Training, and Certification | -0.07 | -0.03 | -0.11 |
| | (0.28) | (0.13) | (0.07) |
| Pre-treat. Mean | 10,262 | 3,585 | 14,620 |
| R-squared | 0.691 | 0.697 | 0.582 |
| N | 946 | 864 | 715 |

Notes: Each column contains coefficients from a regression following the difference-in-difference specification in Equation 2.1, which include state and year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS. Treatment data is from author's collection and coding of state laws and regulations.

Table 2.7: Relative to Medical Assistant: Estimated Effects of Licensing Requirements on Pharmacy Technician College Program Completions and Number of Programs Offered (% Change)

| | (1) Completions | (2) Programs |
|---|---|---|
| Registration only | 0.22 | 0.10 |
| | (0.14) | (0.11) |
| Registration and Certification only | 0.36** | 0.05 |
| | (0.17) | (0.18) |
| Registration and Training only | 0.75*** | 0.30** |
| | (0.15) | (0.12) |
| Registration, Training, and Certification | 0.65** | 0.04 |
| | (0.32) | (0.29) |
| Pre-treat. Mean | 254 | 15 |
| R-squared | 0.991 | 0.841 |
| N | 2,170 | 2,170 |

Notes: Each column contains coefficients from a regression following the triple-difference specification in Equation 2.4, which include occupation-by-state, occupation-by-year, and state-by-year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS. Treatment data is from author's collection and coding of state laws and regulations.

Table 2.8: Relative to Medical Assistant: Estimated Effects of Licensing Requirements on Pharmacy Technician College Program Completions and Number of Programs Offered, by Sector (% Change)

| | (1) Public Completions | (2) For-profit Completions | (3) Public Programs | (4) For-profit Programs |
|---|---|---|---|---|
| Registration only | 0.33** | 0.19 | 0.15 | 0.27 |
| | (0.14) | (0.26) | (0.14) | (0.22) |
| Registration and Certification only | -0.04 | 0.41 | -0.11 | 0.44 |
| | (0.24) | (0.26) | (0.19) | (0.39) |
| Registration and Training only | 0.57*** | 0.87*** | 0.35*** | 0.28* |
| | (0.12) | (0.20) | (0.12) | (0.15) |
| Registration, Training, and Certification | -0.20 | 1.08** | -0.15 | 0.37 |
| | (0.22) | (0.47) | (0.20) | (0.61) |
| Pre-treat. Mean | 65 | 185 | 6 | 9 |
| R-squared | 0.955 | 0.990 | 0.701 | 0.816 |
| N | 1,986 | 2,024 | 1,986 | 2,024 |

Notes: Each column contains coefficients from a regression following the triple-difference specification in Equation 2.4, which include occupation-by-state, occupation-by-year, and state-by-year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS. Treatment data is from author's collection and coding of state laws and regulations.

Table 2.9: Relative to Medical Assistant: Estimated Effects of Licensing Requirements on Completions Within Pharmacy Technician College Programs, Overall and by Sector (% Change)

| | (1) Overall | (2) Public | (3) For-profit |
|---|---|---|---|
| Registration only | 0.18* | 0.11 | 0.13 |
| | (0.09) | (0.09) | (0.17) |
| Registration and Certification only | 0.27*** | -0.08 | 0.26* |
| | (0.08) | (0.14) | (0.14) |
| Registration and Training only | 0.27** | 0.09 | 0.39*** |
| | (0.12) | (0.15) | (0.12) |
| Registration, Training, and Certification | 0.20 | -0.43** | 0.53*** |
| | (0.14) | (0.18) | (0.17) |
| Pre-treat. Mean | 35 | 18 | 46 |
| R-squared | 0.784 | 0.621 | 0.759 |
| N | 37,540 | 14,148 | 21,443 |

Notes: Each column contains coefficients from a regression following the triple-difference specification in Equation 2.5, which include occupation-by-state, occupation-by-year, and state-by-year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS. Treatment data is from author's collection and coding of state laws and regulations.

Table 2.10: Relative to Medical Assistant: Estimated Effects of Licensing Requirements on Pharmacy Technician College Program Prices (% Change)

| | (1) Overall | (2) Public | (3) For-profit |
|---|---|---|---|
| Registration only | -0.02 | 0.04 | 0.02 |
| | (0.11) | (0.04) | (0.03) |
| Registration and Certification only | -0.07 | 0.08 | 0.03 |
| | (0.15) | (0.08) | (0.05) |
| Registration and Training only | -0.03 | 0.06 | -0.08* |
| | (0.13) | (0.07) | (0.04) |
| Registration, Training, and Certification | -0.16 | 0.20 | -0.10 |
| | (0.22) | (0.13) | (0.08) |
| Pre-treat. Mean | 10,262 | 3,585 | 14,620 |
| R-squared | 0.897 | 0.932 | 0.885 |
| N | 1,868 | 1,650 | 1,430 |

Notes: Each column contains coefficients from a regression following the triple-difference specification in Equation 2.4, which include occupation-by-state, occupation-by-year, and state-by-year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS. Treatment data is from author's collection and coding of state laws and regulations.

CHAPTER 3

# THE EFFECTS OF TARGETED FOLLOW-UP IN SCHOOL HEALTH PROGRAMS ON STUDENT HEALTH AND ACHIEVEMENT: LESSONS FROM THE NEW YORK CITY SCHOOL VISION PROGRAM

## 3.1 Introduction

Public schools are an important setting for public health policy, as the COVID-19 pandemic has made evident. Screenings for vision and hearing are some of the most common and long-standing public-health measures in schools [37]. Increasingly, public schools have taken steps to connect more students with care, whether by increasing follow-up efforts with families or by directly providing treatment in school [49]. Investing in child health now is potentially an investment in future health and human capital, and increased efforts to connect students with treatment may pay off not only in terms of immediate quality-of-life improvements, but also in terms of other health outcomes and academic achievement [22]. In this paper, we estimate the effects of increased follow-up efforts within the New York City (NYC) Public School's School Vision Program (SVP) on students' vision treatment and outcomes, other health outcomes, absence, and academic outcomes.

It is important to measure the impacts of school health programs like the SVP, through which screenings are provided by governmental mandate to all students in certain grade levels every year. However, by nature of its universality, there is no control group of students who do not receive vision screenings against which to measure subsequent outcomes. Suppose we wanted to test the hypothesis that correcting vision problems improves academic progress because students can read the board better during class. Even with detailed administrative data describing student characteristics and outcomes over time, comparing students by vision correction will likely be biased. The factors, observed or unobserved, that lead some students who need glasses not to wear them, such as financial resources, are likely to also affect academic outcomes. What is needed to estimate the causal effects of a program like the SVP is variation in some aspect of the vision screening, follow-up, or treatment process that is unrelated to other factors that would affect academics and other outcomes of interest. Our identification strategy exploits a such a process within the SVP, which targets follow-up efforts by vision screening results.

In the SVP's screening and follow-up procedures, there are two important visual acuity levels: 20/40 and 20/70. Students whose worst result is 20/40 or worse fail the screening and are sent home with a letter. Students whose worst result is 20/70 or worse are prioritized for additional follow-up efforts. These policy thresholds are conducive to an empirical strategy like a regression-discontinuity design, comparing the outcomes of students just above and just below the cutoff to estimate the causal effects of the policy. We focus on the follow-up cutoff of 20/70. Students both above and below this cutoff are likely to benefit from vision treatment and be similar along other dimensions, but there is a stark difference in one important dimension, follow-up efforts. Therefore, subsequent stark differences on either side of this cutoff in vision treatment and subsequent outcomes may plausibly be attributable to these follow-up efforts.

There is little existing research estimating the causal effects of school vision programs, but existing work suggests the potential for positive impacts on students' short- and long-run health and academic outcomes. For example, [33] find, through a randomized-control trial, that one year of wearing eyeglasses substantially improved test scores among primary-school students in rural China. More broadly, [19] find that providing Medicaid (which typically covers eye care) to low-income children in the United States improved rates of high school and college completion. Focusing on vision-treatment outcomes, [28] find, through a small randomized-control trial in the NYC schools, that providing free eyeglasses among other supports increased rates of glasses-wearing from 22% to 47%. Other studies of vision programs like [49] and [52] discuss vision treatment outcomes and how they relate to levels of follow-up and referral in different program models, but do not perform causal analyses.

We contribute to this literature on child health and vision screening by providing causal estimates of the effect of vision-screening follow-up efforts on a large population of students in NYC. With administrative data from the NYC Office of School Health, we link students' first vision screenings to subsequent vision screenings, the forms students returned after seeing optometrists, and a variety of subsequent health and academic outcomes, including BMI, school nurse visits, attendance, retention, and standardized test scores. We find robust positive effects on vision outcomes: confirmed eye-exams, wearing glasses in the next screening, passing the next screening, and having substantially improved visual acuity scores in the next screening. However, our findings on health and academic outcomes are relatively small and inconsistent compared to those vision findings. This may in part be due to imperfect data and outcome measures, especially for academic outcomes, which could only be observed a few years after vision screenings in the first year of standardized testing.

## 3.2 New York City School Vision Program

The primary purpose of the SVP is to ensure that vision problems are caught early and corrected.[1] New York State requires districts to screen students for vision problems in grades K, 1, 3, and 5, as well as within the first six months of enrollment for students in other grades, which in NYC includes Pre-Kindergarten (Pre-K). The School Vision Program Office is the part of the Office of School Health (OSH) within the Department of

[1] This section draws substantially from [24], our descriptive study of the NYC SVP.

Health and Mental Health responsible for the Pre-K, K, and 1st grade screenings, while the Department of Education conducts screenings for students of other ages.

Several teams within OSH coordinate to provide vision screenings and, in certain circumstances, to follow-up with families and perform eye exams. Teams coordinate with principals and administrators to schedule screenings and with school staff to screen each child. They also record detailed data about the screenings, follow-up, and vision outcomes of students. In grades K-1, screeners test for distance vision using one of three tests (Snellen, tumbling E, or Lea Symbol chart) and for near vision with the Rosenbaum pocket chart. Visual acuity scores for both distance and near vision range from 20/20 to 20/200, where failing is considered 20/40 or worse in either eye. Amblyopia, also known as lazy eye, is a disease where the brain does not recognize the sight from one eye. Children are defined as failing the screening because of being at risk of amblyopia if they have a difference or two lines or more between their right and left eyes on the distance-vision test.

While the vision screening offers a measure of a child's vision, eye exams by optometrists are necessary to determine a child's vision prescription and need for corrective eyewear. Students who fail the in-school vision screening or for some reason cannot be tested are sent home with a 1) a letter advising the guardian to take the child to an eye doctor, 2) an eye report and recommendation form (E12s) for the eye doctor to fill out, 3) a list of public facilities that will enroll children without insurance, and 4) a pre-addressed, postage paid envelope in which the parent can return the completed eye form to the OSH. It is the responsibility of the guardian to obtain the eye exam and return the form to the OSH. A separate team conducts intensive follow-up of all students whose vision is scored 20/70 or worse or who are at risk of amblyopia, or any Pre-K student who failed (regardless of score).

This follow-up starts with a call immediately following the failed screening, which is followed by calls every two weeks to encourage and assist the parent in obtaining eye care for the student. If attempts to call are not successful, the follow-up team sends a letter. This also may be followed by substantial efforts to work with school nurses or other school staff to reach families, particularly if the letter is returned undeliverable. The follow-up unit also assists families with finding ophthalmologists and navigating health insurance questions. Each of these points of contact are recorded in the OSH database. A separate data entry team is responsible for helping to record any information on forms returned with exam results.

These policies and procedures inspire our hypotheses and empirical strategy and provide us with the data to implement our analysis. We test the hypothesis that increased follow-up efforts cause more students to obtain eye exams and receive any necessary vision treatment. We exploit the 20/70 visual acuity cutoff that triggers increased follow-up efforts to implement a causal identification strategy. Finally, we focus on students in grades K-1 who are not at risk of amblyopia, which gives us a large sample of students for whom the 20/70 acuity cutoff meaningfully determines follow-up efforts. We further discuss our analysis sample and data in the next section.

### 3.3 Data

We use multiple administrative student-level datasets available through the NYC OSH. Student vision and other health outcomes come from the OSH's Automated Student Health Records (ASHR) data system, which includes SVP, BMI, and nurse-visit data. Student absence, grade retention, and standardized-test data are sourced from the Department of Education (DOE) data system. We use student demographic data from both the ASHR and DOE systems.

Our main analysis sample is based on the SVP screened population. Here, we first describe our sample selection choices, then compare our sample to the population of all Kindergarteners in the NYC Public Schools. From the screened population, we include students in K or 1st grade who were screened two years in a row with non-missing visual acuity scores, so that we can observe changes in vision outcomes.[2] We exclude students meeting the criteria for amblyopia risk as well as students attending community schools, for whom the follow-up cutoff does not apply.

Next, we restrict the sample to students for whom we observe demographic characteristics and academic outcomes. Demographic characteristics come from the ASHR demographic file of actively enrolled students and include self-identified race/ethnicity and eligibility for school lunch. Academic outcomes are grade repetition and 3rd grade standardized test scores (students in earlier grades are not given these tests), both of which come from the DOE. Less than 1% of students lack demographic characteristics. However, the requirement to observe 3rd grade standardized test scores (both ELA and Math) more severely restricts our sample: we keep about 83% of students initially screened between 2011-12 and 2015-16 for whom we observe 3rd grade test scores in 2014-15 through 2018-19.

To this sample we add BMI and nurse-visit data from ASHR and absence information from DOE data. While BMI and absence data are missing for some students, we do not drop these students from our main analysis, only from analyses involving those outcomes. The BMI sample is about 85% of the main sample, and the absence sample is about 94% of the main sample. The nurse-visit analyses include the entire sample, but this is a feature of that data only including students that visited the school nurse. That is, we cannot identify "no nurse visits" and risk some false negatives by assuming that any student not observed in the nurse visit data in a given schoolyear did not visit the school

[2]Sixty-five percent of all students screened in K-1 meet this criterion. For students observed more than twice, we keep the first two years observed.

nurse that year.

In Table 3.1, we compare our main analysis sample to all enrolled Kindergarteners in NYC public schools over the same period. Observations are at the student level, and the years of the table correspond to the school years of these students' initial vision screenings (2011-12 to 2015-16). The demographic composition of our analysis sample is broadly similar to that of all enrolled Kindergarteners, though the differences (reported in the far-right column) are all statistically significant at the 95% level, except for the fraction of other race/ethnicity. The differences greater than 1 percentage-point (pp) are: our analysis sample is about 1.5pp higher fraction female, 2.4pp smaller fraction Black, and 2.3pp higher fraction Asian. These differences likely result from our requirements that students in our sample be observed in SVP data for two consecutive years and then observed again in 3rd grade standardized test score data.

Table 3.2 summarizes our other analysis variables. The main sample is 99% Kindergarteners at their initial screening (the remaining are students that repeat 1st grade). At the time of their initial screening, 84% of students pass the screening. About 5% are wearing glasses, observable in the screening data because visual acuity scores are entered in a separate field if the student wears glasses during the screening. About 4% of students receive follow-up (efforts in addition to the initial letter sent home with students who fail the screening) and 6% confirm a follow-up eye exam. In subsequent vision screenings the following year, 4% of the sample are wearing glasses who were not previously, 8% pass the screening having failed previously, and 3% have visual acuity scores that improved by at least 2 lines. Of interest in this study is whether these differences between screening years is driven by the SVP's follow-up efforts.

Below these vision outcomes in Table 3.2, we report summary statistics for other outcomes which may be indirectly affected by follow-up efforts and subsequent vision treatment. In general, we hypothesize that improved vision may lead to improvements such as improved physical fitness (imperfectly measured by BMI), fewer visits to a school nurse, higher attendance, and improved academic performance (measured by on-time grade progression and 3rd grade standardized test scores). We define outcomes for fitness, nurse visits, and attendance using two years of observations for each student, as for vision improvement. Using these measures, on average, regardless of vision screening results or follow-up, about 58% of students' BMI decreased, about 5% entered the obese BMI category ("Newly obese") and about 4% left it ("No longer obese"). Visits to the school nurse declined among 5% of students, and specifically vision-related visits declined for 1% of students. Absence rates declined for 59% of students, 5% became chronically absent (missing 10% or more of school days), and 9% became no longer chronically absent. We define academic outcomes more simply, not relative to the initial screening year. About 6% of the sample repeated 1st or 2nd grade, and, by construction of the proficiency variable, about 75% of students obtained proficiency in ELA and Math in 3rd grade.

## 3.4 Identifying the Effect of Follow-up Efforts

To identify the effects of SVP follow-up efforts, we make use of the program mandate that all children in our sample who have a visual acuity score of 20/70 or worse in at least one eye in their initial vision screening should receive follow-up. We use this mandate to set up a regression discontinuity design, often used to estimate the effect of a treatment that increases sharply at a specific value. In this case, that treatment is follow-up efforts, which increase sharply at the 20/70 vision score. The vision score does not have as much variability as would be desired in a typical regression discontinuity design, as the vision outcome scores only take on eight values. Also, students are not evenly distributed across values; 85% of students have eyesight of at least 20/30. However, our large sample of students across several school years provides sufficient power to estimate discontinuities.

Our empirical specification compares the outcomes of students who score just below or above the cutoff for follow-up, i.e., we compare students with scores of 20/50 or 20/60 to those with scores of 20/70 or 20/80 in their first vision screening. The simplest version of the model is a comparison of means between these two groups, as shown in Equation 3.1:

$$Y_i = \beta_0 + \beta_1 \mathbb{1}(X_i \geq 70) + \varepsilon_i, \text{ where } X_i \in [50, 80] \tag{3.1}$$

On the left-hand side, $Y_i$ represents the outcome of interest for student $i$, such as an indicator for receiving follow-up after the initial vision screening or obtaining proficiency in mathematics in 3rd grade. On the right-hand side, this outcome is modeled as a local linear function of $X_i$, the 'denominator' of student i's worst visual acuity score in her initial vision screening, in the range of visual acuity scores from 20/50 to 20/80. The parameter $\beta_0$ estimates the mean of the outcome among students with $X_i < 70$, and $\beta_1$, the parameter of interest, estimates the difference in the mean of the outcome among students with $X_i \geq 70$, students who should receive follow-up. Under the assumption that no other factor affecting the outcome differs across this cutoff, we can interpret $\beta_1$ as the causal intent-to-treat effect of follow-up efforts within the regression sample. In three additional specifications, discussed below, we control for potential relationships between visual acuity scores and outcomes that could bias the estimate in this simple model.

In Equation 3.2, we add a linear control for visual acuity score, so that $\beta_2$ captures the average difference in the outcome for a unit increase in students' worst visual acuity score:

$$Y_i = \beta_0 + \beta_1 \mathbb{1}(X_i \geq 70) + \beta_2 X_i + \varepsilon_i, \text{ where } X_i \in [50, 80] \tag{3.2}$$

For example, students with high values of $X$ in their initial screening are likely to have lower values of $X$ in their subsequent screening (regression to the mean), which would result in a negative relationship between $X$ and the probability of passing the screening in the following year. The addition of this linear control for $X$ leaves $\beta_1$ to capture only the non-linear component of the relationship between $X$ and $Y$, the discontinuity at the cutoff. However, there are two shortcomings of this specification. First, a single linear trend across the full range of $X$ may capture a great deal of the true effect of interest and attenuate the estimate of $\beta_1$ toward zero. Second, a single linear trend will not capture changes in a linear relationship between $X$ and $Y$ at the cutoff. Therefore, we also estimate models allowing for different linear trends above and below the cutoff, as in Equation 3.3:

$$Y_i = \beta_0 + \beta_1 \mathbb{1}(X_i \geq 70) + \beta_2 X_i \times \mathbb{1}(X_i \geq 70) + \beta_3 X_i \times \mathbb{1}(X_i < 70) + \varepsilon_i \tag{3.3}$$

Here, $\beta_1$ captures the change in the level of Y as before. The parameter $\beta_2$ now captures the change in the slope, or linear relationship between $X$ and $Y$, above the cutoff, relative to the corresponding relationship below the cutoff, which is captured by $\beta_3$. We estimate this for the same values of $X$ as above ($X_i \in [50, 80]$) as well as for a broader range of $X$ incorporating all vision scores that would fail the vision screening.

To summarize our empirical strategy, we compare the outcomes of students close to but on different sides of the follow-up cutoff in worst visual acuity score. We control for changes in outcomes that vary linearly with this score and allow the nature of that relationship to vary across specifications to determine whether results are robust to specification choice. Our underlying assumption in interpreting the estimates of this comparison is that, after controlling for the effect of score itself, there is nothing else different about these two groups other than their exposure to follow-up efforts by the SVP staff. We test this assumption by adding controls for student and school characteristics and by estimating effects relative to a control group of students who should be unaffected by the follow-up cutoff. In all specifications, we cluster standard errors at the school level, allowing the errors in the model to be correlated across students within a school, where screenings take place.

### 3.4.1 First-Stage Results

To illustrate the nature of our identifying variation, in Figure 3.1 Panel A we present rates of follow-up by initial screening result. In this, as in later figures, we use a student's worst acuity score across right and left eyes, near and distance vision, in their initial vision screening, to categorize each student in the sample. In the figure, one can see that rates of follow-up increase dramatically, from about 15% to over 90%, as scores move from 20/60 and below to 20/70 and above. The estimated first-stage effect, reported in column 1 of Table 3.3, is about 0.82 (82 percentage points) across specifications. Though follow-up rates are not 100%, the program rules are clearly influencing follow-up efforts by staff.

## 3.5 Results

Given that visual acuity is a strong predictor of receiving follow-up efforts from SVP staff, we next estimate the effect of these follow-up efforts on other student outcomes in reduced-form, intent-to-treat analyses. Below, we have grouped these results by outcome categories: vision, BMI and nurse visits, absence, and academic outcomes. In each section, we first present and describe the visual results in Figures 3.1 to 3.4, then turn to interpreting coefficients from our four model specifications presented in Tables 3.3-3.6.

### 3.5.1 Vision Outcomes

The goal of the SVP's follow-up efforts is to identify and correct vision problems early in children's lives. To determine the success of follow-up program, we first examine to what extent increases in follow-up correspond to increases in confirmed eye exams, which means that the follow-up efforts successfully nudged families to arrange eye appointments, attend these appointments, and send the E12S form back to the school with the eye-exam results. Then, we use data from the student's vision screening in the following year to observe glasses-wearing and improved vision screening results. The glasses outcome we focus on is the rate of 'new' glasses: students wearing glasses in their second screening who did not wear them in the initial screening. For improved vision, we look at both rates of passing the second screening and rates of meaningful improvement in the worst visual acuity score: an improvement of at least 2 lines.

Figure 3.1 presents the relationship between worst initial visual acuity score and these vision outcomes graphically in panels B-E. There is a discrete increase in the rates of confirmed eye exams in Panel B for children with scores of 20/70 or worse relative to those with better vision acuity scores: the rate increases from about 30% among students at 20/60 to about 50% among students at 20/70, the first level triggering increased follow-up efforts. There is an increase in the rate of new glasses at the second screening in Panel C from about 30% to 40% at the cutoff. The implied effect may be reduced, however, by taking into account the apparent relationship between visual acuity and new glasses in this range: the rate of new glasses increases steadily for worse visual acuity scores from 20/40 to 20/70. There is no apparent discontinuity in rates of passing the second vision screening at the cutoff in Panel D, but rather the sharp decline in pass rate flattens out at that point in the distribution. Finally, we see a clear jump in rates of vision improvement in Panel E from about 50% to 65% above the cutoff.

To get more precise estimates of the sizes of these effects, in Table 3.3 we present reduced form estimates of the changes in outcomes across the threshold for follow-up. This table and the following results tables report the coefficient on the main treatment variable "Above Cutoff," or the value of $\beta_1$ in the equations above. This coefficient estimates the change in the value of each student outcome attributable to having had a worst visual acuity score above the follow-up cutoff. The specifications in Panels A through C correspond to the models in Equations 3.1 through 3.3, and Panel D applies the Equation 3.3 specification to a broader range of vision scores. Each column corresponds to a different vision outcome, beginning with the first-stage follow-up outcome in column (1).

The estimates for the eye-exam outcome in column (2) vary from 0.17 to 0.23 across specifications, higher among the specifications controlling for a linear relationship between scores and exams. Estimates for the new-glasses outcome in column (3) vary from 0.02 to 0.17, this time lower among specifications controlling for the linear relationship between the score and new glasses. Comparing these estimates to the observed discontinuities in Figure 3.1, the Panel-B estimates appear to parsimoniously capture the observed discontinuity while controlling for the relationship between score and outcome, so we will focus on that specification. The Panel-B estimate for exams is 0.23 and for glasses 0.08. Turning to the vision-improvement outcomes, we estimate a 0.10 increase in the rate of students passing the vision screening and a 0.15 increase in the rate of substantial vision improvement.

Taken together, these estimates suggest that the follow-up prioritization policy using the 20/70 cutoff increased children's rates of reported follow-up eye exams by about 70% (0.23 divided by an average rate of 0.32 below the cutoff), rates of wearing glasses by about 30% (0.08 divided by 0.24), rates of passing their subsequent screening by about 25% (0.10 divided by 0.42), and rates of substantial vision improvement by about 30% (0.15 divided by 0.45). Because the estimated effects on glasses-wearing and vision improvement are about one-tenth or more than the effect on follow-up, the results imply that for every 10 students that received additional follow-up from SVP staff, there was at least one more student wearing glasses and with improved vision one year later.

### 3.5.2 BMI and Nurse Visits

Given that the follow-up efforts lead to improvements in vision one year after screening, we now turn to examine the effects of follow-up on a wider range of health outcomes, beginning with physical fitness (BMI) and health concerns during the school day (nurse visits). As with the new glasses and vision improvement outcomes described above, we define these outcomes using each student's BMI and number of nurse visits in the years of their initial and subsequent vision screening. Our physical fitness outcomes are indicators for decreased BMI, newly obese, and no longer obese. Our nurse visit outcomes are indicators for decreased nurse visits of any type and decreased vision-related nurse visits, including visits for headaches and dizziness. The graphical relationships between worse visual acuity and these outcomes are presented in Figure 3.2, Panels A through E. Unlike for the vision outcomes, there are no obvious discontinuities at the follow-up cutoff (20/70) across these five figures. There is a slight decrease in the rate of a new obesity status at 20/70 in Panel B, but the rate jumps upward by much more than that decrease at 20/80. Also note the smaller range of the Y-axis in Panels B-E and the large 95% confidence intervals relative to the height of the bars, which indicate the lack of precision in estimating the differences in these means.

This lack of an effect of follow-up efforts on these health measures is confirmed in the Table 3.4 regression results. Focusing on the results in Panel B, coefficients for BMI outcomes in the first three columns are in the hypothesized directions (more student with decreases in BMI, fewer students newly obese, more students no longer obese), but none are statistically significant at conventional levels. The estimates for both nurse-visit outcomes in columns (4) and (5) are also close to zero and statistically insignificant. As expected from the graphical results, we do not find evidence an effect of SVP follow-up carrying over to these other short-term and imperfect measures of health outcomes.

### 3.5.3 Absence Outcomes

The next set of outcomes we examine relate to attendance. Low rates of attendance may be a sign of poor health or lack of engagement with school. One way of looking at SVP follow-up efforts is as outreach from the school district demonstrating its care for students' well-being. Because of these efforts, students and families may feel more connected to the school and attend more regularly. The specific absence outcomes we consider are: (1) absence rate declined between the initial and second screening year; (2) newly chronically absent in the second screening year (defined as missing 10% or more of enrolled days), and no longer chronically absent that year. The corresponding graphs are presented in Figure 3.3, Panels A-C. As with the previous category of outcomes, there are no clear discontinuities apparent in these figures.

The regression results in Table 3.5 Panel B are small and statistically insignificant. The largest estimate in Panel B is a 0.02 decline in the probability that a student's absence rate decreased between initial and second screening (column 1), which is opposite to the hypothesized improvement in attendance from follow-up efforts. The estimates for chronic absence are close to zero and inconsistent across specifications. We do not find evidence of an effect of SVP follow-up on these absence outcomes.

### 3.5.4 Academic Outcomes

Finally, we look at how SVP's follow-up efforts impacted academic outcomes, including repeating a grade and performance on standardized tests in 3rd grade, the first grade that students take such tests. Figure 3.4 presents the relationship between students' worst initial visual acuity score and the probability of repeating grades 1 or 2 (Panel A), the rate of ELA proficiency in 3rd grade (Panel B), and the rate of Math proficiency in 3rd grade

(Panel C). Proficiency in these subjects is defined as scoring above the 25th percentile in that subject and schoolyear. There is a positive relationship between worse vision and grade repetition in Panel A across the full range of visual acuity scores, which flattens out slightly around the cutoff. In Panels B and C, there is a weak negative relationship between worse vision and ELA/Math proficiency which flattens out below the cutoff, closer to 20/50. These graphs do not rule out a possible effect, but once again are not as clearly suggestive as the vision outcome graphs.

If SVP's follow-up efforts improved academic outcomes within the next few school years, we might expect to see a negative effect in column (1) of Table 3.6, for the "repeated a grade" outcome, and positive effects in columns (2) and (3) for the ELA and Math proficiency outcomes. There are negative coefficients in Panel B ranging from -0.02 to -0.05 across all three columns, but all have standard errors that rule out statistical significance of these effects at conventional levels. The flattening of the negative relationship between vision and ELA/Math proficiency observed in the graphs only leads to a positive coefficient in the specification that includes 20/40 through 20/60 in the sample (Panel D).

### 3.5.5 Heterogeneous Effects

The above results are average effects across all students in our sample. We also estimate effects within subgroups of students by eligibility for free or reduced-price lunch and by race and ethnicity. Follow-up efforts may have stronger effects among students from lower-income families and racial groups; less-advantaged parents may be less likely to successfully set up appointments and access treatment without the help from SVP follow-up staff. On the other hand, less-advantaged parents may also be more difficult for follow-up staff to reach and help. In general, we do not find that effects are very different across student subgroups, as evident in Figures 3.5 and 3.6.[3]

In results by eligibility for free/reduced-price lunch, the same increase in follow-up rates leads to slightly higher rates of eye-exams, glasses, and vision improvement among ineligible (higher-income) families. Examining vision outcomes by race/ethnicity, follow-up translates to somewhat smaller increases in rates of confirmed eye-exams among Black and White students (relative to Hispanic and Asian students) and lower rates of glasses-wearing among Black and Asian students. Measures of vision improvement, however, are more consistent across these groups. This limited evidence suggests that the SVP's policy of targeted follow-up may have a stronger effect among more-advantaged rather than less-advantaged students and families.

## 3.6 Robustness Checks

To test the robustness of our results, here we discuss two additional sets of results. The first set of results include controls for student and school characteristics. We add school fixed effects, which control for any characteristic of schools, observed or unobserved, that affects both vision screening results and outcomes. We also add student-level controls proxying for students' relative economic and social disadvantages: race/ethnicity and eligibility for free-or-reduced school lunch. The second set of results add a control group of students who should be unaffected by the follow-up cutoff, composed of students flagged as being at risk of amblyopia and students attending community schools. By adding these students to our analysis, we estimate the differential effect of the cutoff among students for whom the cutoff affects follow-up.

[3]Full results by student subgroup are reported in Appendix Tables C.5 through C.8.

### 3.6.1 Student and School Controls

By including controls, we account for student and school characteristics that may be discontinuous at the cutoff, which would be problematic for our analysis if the characteristics are correlated with the outcomes. It is difficult to think of reasons why student characteristics would be different between the 20/60 and 20/70 screening results, but it is possible to think of this occurring due to problems within the screening or data input process. For example, if those performing the screening or inputting the data are aware of the cutoff for increased follow-up efforts, some may err on the side of recording 20/70 if they fail to record a student's result due to some disruption. Or it is possible that some staff may err on the side of recording 20/60 to reduce staff workload. These may correlate with student characteristics if they are more likely to occur at certain schools or for students who have difficulty with the screening process.

In fact, not all our observable student characteristics are smooth across the 20/70 follow-up cutoff. Hispanic students (54% of sample) are overrepresented above the cutoff relative to below it by 5 percentage-points, while Black students (23% of sample) are underrepresented by 4 percentage-points above the cutoff. To the extent that race/ethnicity is correlated with any of our outcomes, our results that do not control for this difference are somewhat biased. However, many of our estimates for vision outcomes are themselves larger than these differences in student composition, and moreover our heterogeneity analyses show that our main results hold within student subgroups by race/ethnicity. Here we show that our results also hold when we add controls for student and school characteristics.

The versions of Tables 3.3 through 3.6 with the addition of controls are in the Appendix as Tables C.1 through C.4. The tables include the coefficients on the indicators for each race/ethnicity category relative to Hispanic and for free or reduced-price lunch eligibility (FRPL), which estimate the correlation between these student characteristics and the outcome (averaged across all vision screening results, whether above or below the cutoff). These coefficients show the expected relationships for vision, BMI, and academic outcomes. Students eligible for FRPL have more negative changes in vision and BMI outcomes between first and second screenings and worse subsequent academic outcomes, relative to students from higher-income families. Similarly, students identifying as Black or Hispanic have worse outcomes in these categories relative to those identifying as White or Asian. The only consistent pattern in the nurse-visit and absence outcome categories is that Asian students are more likely to have absence improvements between their first and second screenings.

Including these student controls as well as school fixed effects has little effect on the main coefficients of interest in each table. For vision outcomes, the estimates in Table C.1 are remarkably like those reported in Table 3.3, with the only exception being the coefficient on the "New glasses" outcome, which is no longer statistically significant in the Panel B specification. However, its magnitude is still 0.04 in Panel B, and it retains statistical significance at the 1% level in Panels A and C. As in the specifications without controls, there are no clear discontinuities estimated for the other outcome categories in Tables C.2 to C.4, and very few estimates are meaningfully different from the Tables 3.3-3.6 results. One perhaps notable difference is that the negative coefficients for academic outcomes without controls (Panel B of Table 3.6) go to zero after the controls are added (Panel B of Table 3.6).

Overall, our findings so far are robust to the addition of controls for student and school characteristics. This lends confidence to our interpretation of the estimates as resulting from the SVP's follow-up efforts rather than from the characteristics of students who have worse visual acuity scores in the analyzed range around the 20/70 cutoff.

### 3.6.2 Addition of Unaffected Control Group

By adding a control group of students who are unaffected by the follow-up cutoff, we account for the possibility of systematic or random features of the vision screening and acuity scores or outcome data that could explain our results. This could include systematic non-linear relationships between visual acuity score and outcomes, or randomness in our samples that causes misleading results. The new model includes an indicator for being affected by the follow-up cutoff, denoted as D in the model below:

$$Y_i = \beta_0 + \beta_1 \mathbb{1}(X_i \geq 70) \times D_i + \beta_2 \mathbb{1}(X_i \geq 70) + \beta_3 D + \varepsilon_i, \text{ where } X_i \in [50, 80] \tag{3.4}$$

Here, $\beta_1$, the parameter of interest in Equation 3.4, represents the differential effect of a visual acuity score above the cutoff among those students for whom the cutoff triggers increased follow-up efforts. The parameter $\beta_2$ captures the effect of being above the cutoff among the control group of students, and $\beta_3$ captures the effect of being in the treated group but below the cutoff. We also estimate versions of Equation 3.4 corresponding to the Panel B-D specifications.

The versions of Tables 3.3-3.6 using this specification are Tables 3.7-3.10, which report the coefficient on the interaction between being above the cutoff and being affected by the follow-up prioritization policy. The results in Table 3.7 for vision outcomes confirm our original results. All coefficients in Panel B, for example, are within 2 percentage-points of the corresponding coefficients in Table 3, except for vision improvement, which is attenuated from 0.15 in Table 3.3 to 0.08 in Table 3.7 and still statistically significant. In the other outcome categories, focusing on Panel B as before, there is a new negative effect on nurse-visit improvement in Table 3.8, suggesting that students above the follow-up go on to have relatively more nurse visits, rather than fewer, as hypothesized. And the coefficients for ELA and Match proficiency in Table 3.10 remain statistically insignificant but turn positive. We interpret the package of results across outcomes and specifications as supportive of the immediate positive effects of follow-up on improving vision outcomes, but inconclusive about its effects on more distant, potentially less accurately measured outcomes.

## 3.7 Discussion and Conclusion

In this paper we study the impacts of NYC Public Schools SVP on a wide variety of student outcomes. With data from OSH, we can observe students over time, from their first and second vision screenings, to their physical fitness results, visits to the school nurse, attendance, and performance on 3rd grade standardized tests. We hypothesize that the resources expended in the SVP to identify vision concerns and connect students with vision treatment may not only result in improved vision, but also in other health and academic outcomes.

To test this hypothesis, we implement a regression-discontinuity empirical strategy. We exploit a policy rule that targeted increased follow-up efforts to students with a worst visual acuity score of 20/70 or worse. After confirming in the data that students with worst scores of 20/70 received follow up at a rate 0.82 higher than students with worst scores of 20/60, we proceed to compare the other outcomes of interest across this cutoff to see if we could detect in the data the ripple effects of SVP follow-up efforts. Using a variety of specifications, we find robust effects on vision outcomes: confirmed eye-exams, wearing glasses in the next screening, passing the next screening, and having substantially improved visual acuity scores in the next screening.

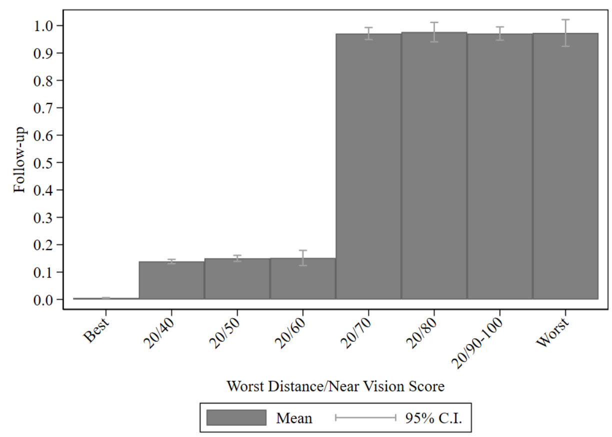
Our findings on health and academic outcomes are relatively small and inconsistent compared to those vision findings. However, just because we fail to identify effects in these other areas does not mean they do not exist. We are constrained by our data and our estimation strategy to a very specific set of outcomes for a specific set of students. For example, our only measures of physical health are BMI and school nurse visits, but these measures may not be the right ones. We also only look at immediate effects, differences in BMI and nurse visits between students' first and second vision screenings, which may be too soon to detect anything. For academic outcomes, ideally we would be able to detect how well students understand their lessons before versus after wearing glasses. Instead, we can only test for effects a few years later in 3rd grade standardized test results.

We focus specifically on the increased follow-up efforts targeted to students with visual acuity scores of 20/70 or worse. Our results therefore do not speak to the effects of the vision screening program overall, including the letters sent home for all students who fail the screening for any reason (including having acuity scores of 20/40 or worse), or the follow-up efforts provided to students at risk of amblyopia. The SVP has also expanded to providing direct care to many students in recent years, which is likely to have much stronger effects than follow-up alone, especially for very under-resourced students and families.

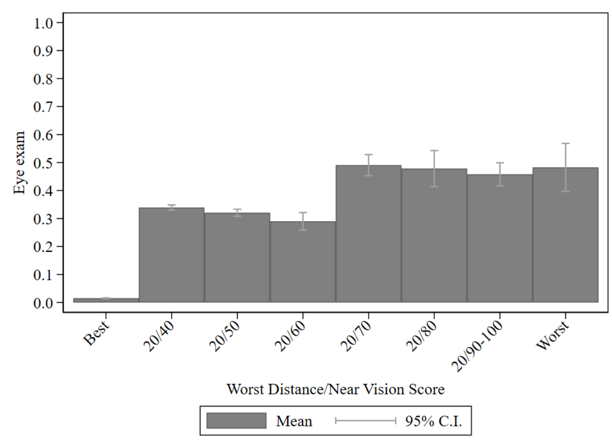
# Figures

Figure 3.1: Vision Outcomes by Worst Visual Acuity Score During Initial Screening

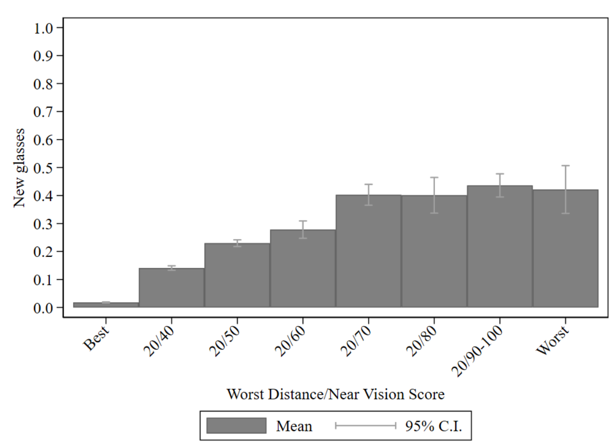
Panel A. Follow-up



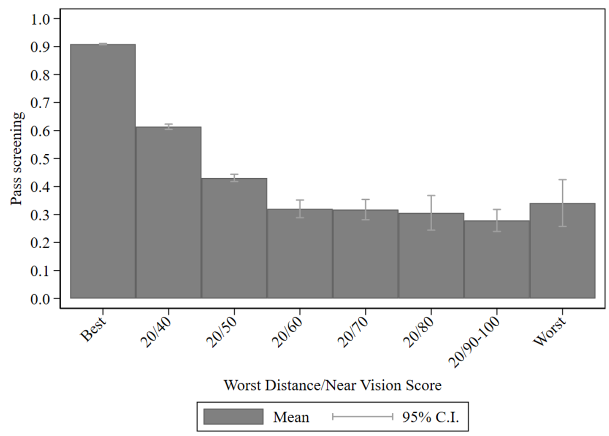

Panel B. Eye-exam



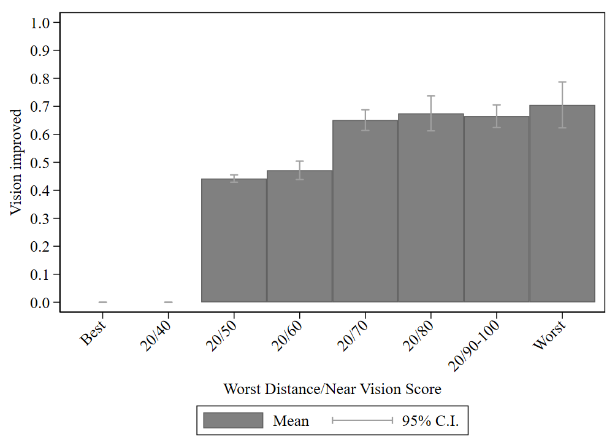

Panel C. New Glasses



Panel D. Pass Screening



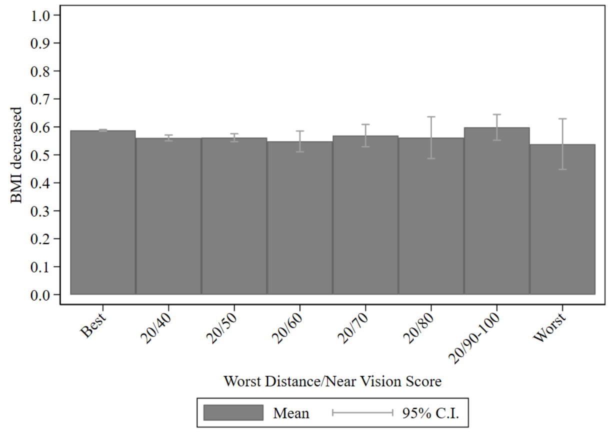

Panel E. Vision Improvement



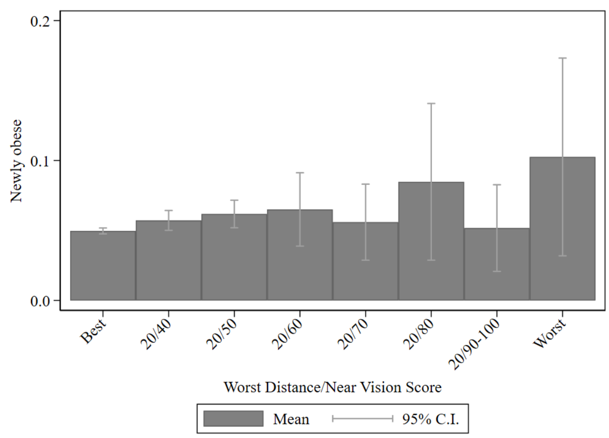

Notes: These figures present the means of each vision outcome for the full sample (N=259,177) by initial worst visual acuity scores. The differences in the average heights of the 20/50 to 20/60 bars versus the 20/70 to 20/80 bars correspond to the estimates reported in result tables.

Figure 3.2: BMI and Nurse Visit Outcomes by Worst Visual Acuity Score During Initial Screening

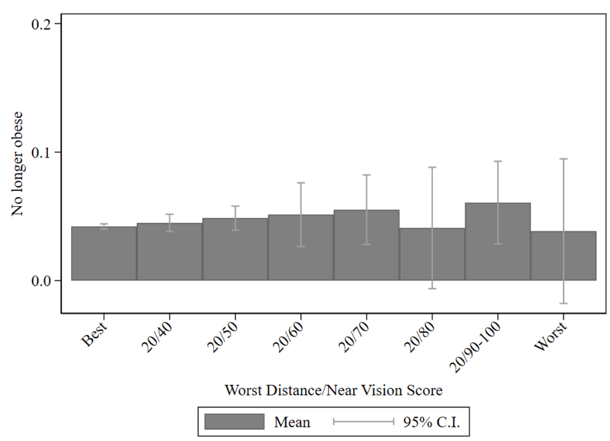
Panel A. BMI Decreased




Panel B. Newly Obese

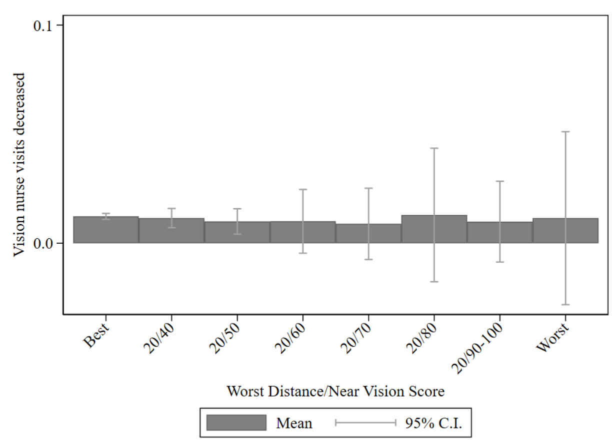



Panel C. No Longer Obese



Panel D. Nurse Visits Decreased

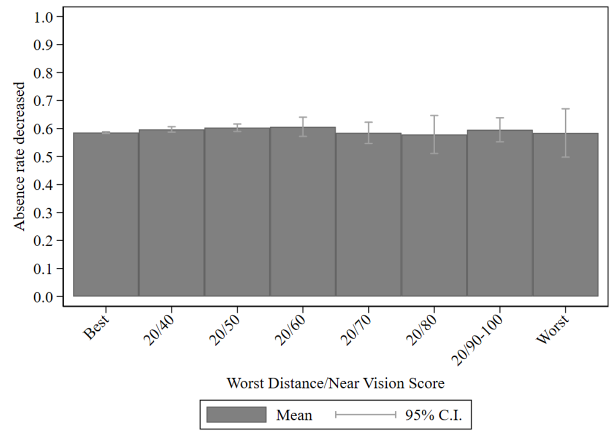

Panel E. Vision Nurse Visits Decreased



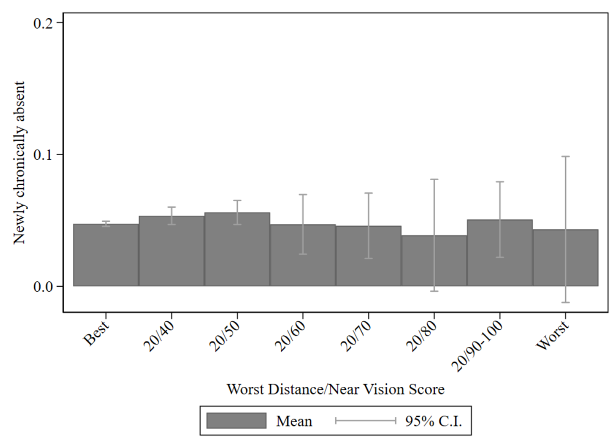

Notes: These figures present the means of each outcome in the relevant sample by initial worst visual acuity scores. The differences in the average heights of the 20/50 to 20/60 bars versus the 20/70 to 20/80 bars correspond to the estimates reported in result tables.

Figure 3.3: Absence Outcomes by Worst Visual Acuity Score During Initial Screening

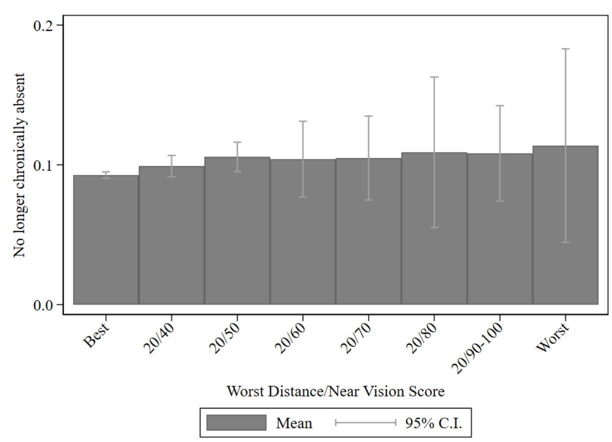
Panel A. Absence Rate Decreased




Panel B. Newly Chronically Absent



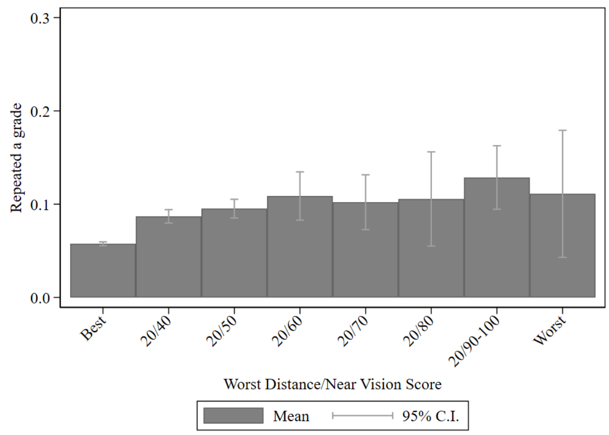

Panel C. No Longer Chronically Absent



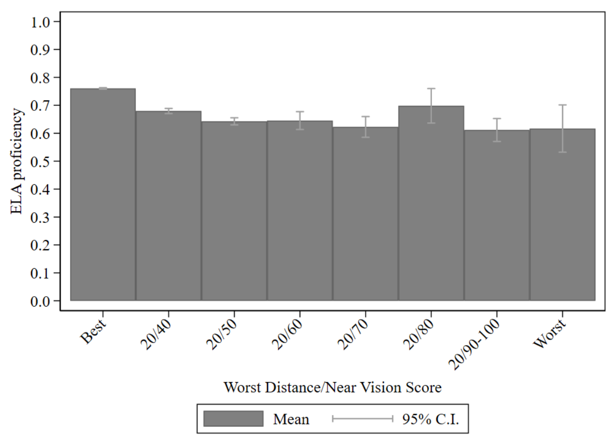

Notes: These figures present the means of each outcome in the relevant sample by initial worst visual acuity scores. The differences in the average heights of the 20/50 to 20/60 bars versus the 20/70 to 20/80 bars correspond to the estimates reported in result tables.

Figure 3.4: Academic Outcomes by Worst Visual Acuity Score During Initial Screening

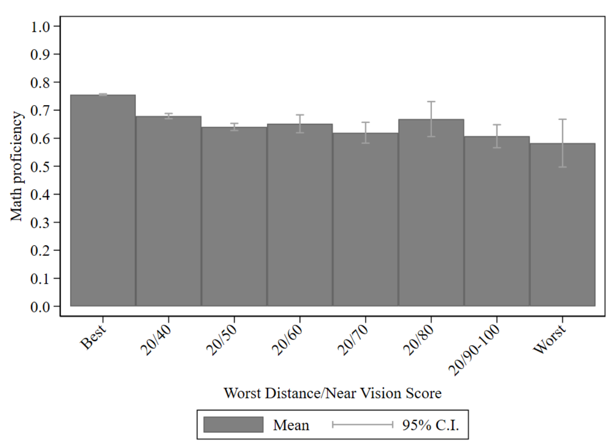
Panel A. Repeated a Grade




Panel B. ELA Proficiency



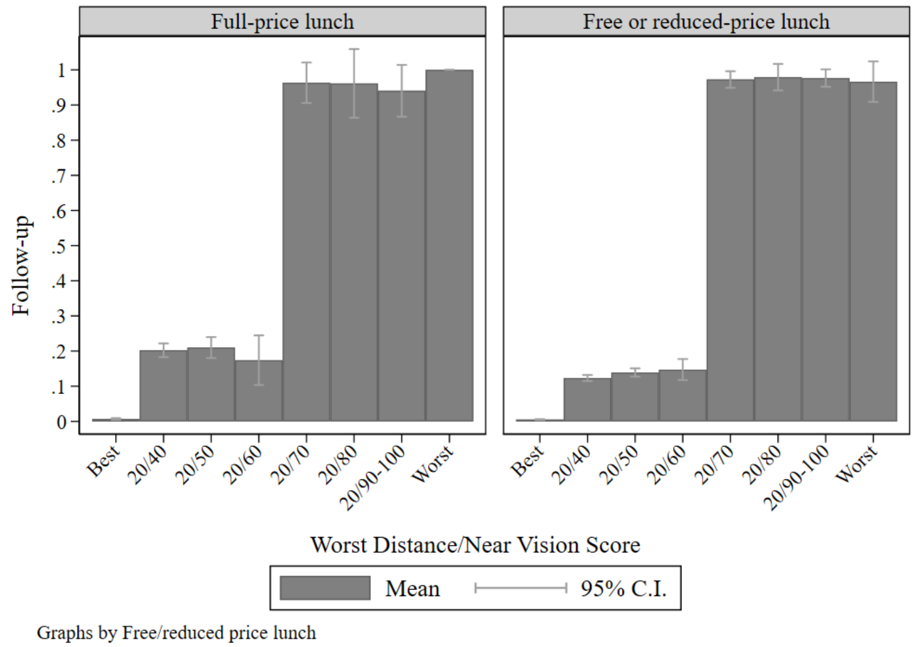

Panel C. Math Proficiency



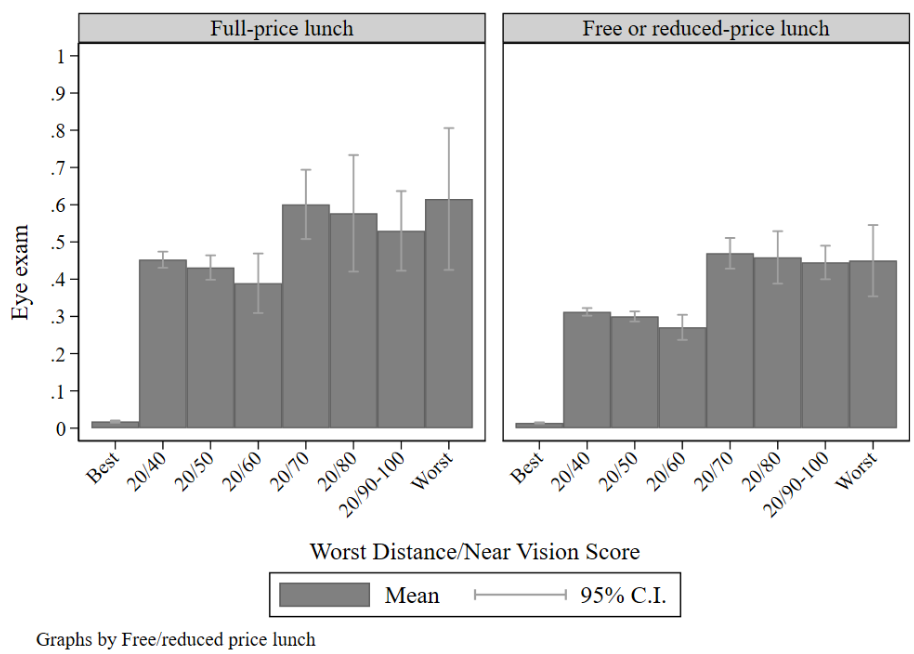

Notes: These figures present the means of each outcome in the relevant sample by initial worst visual acuity scores. The differences in the average heights of the 20/50 to 20/60 bars versus the 20/70 to 20/80 bars correspond to the estimates reported in result tables.

Figure 3.5: Vision Outcomes by Worst Visual Acuity Score at Initial Screening and by FRPL

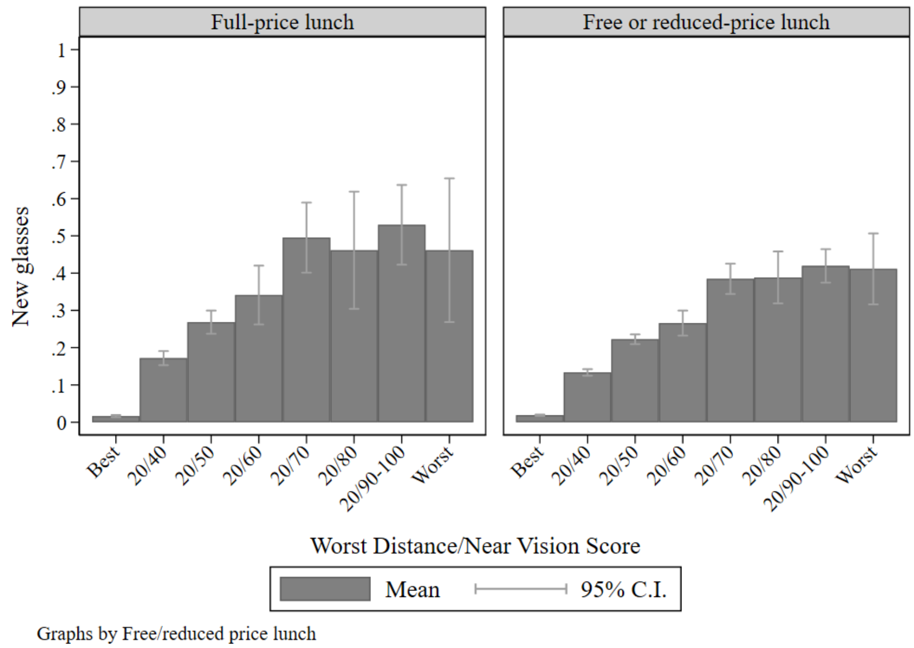
Panel A. Follow-up



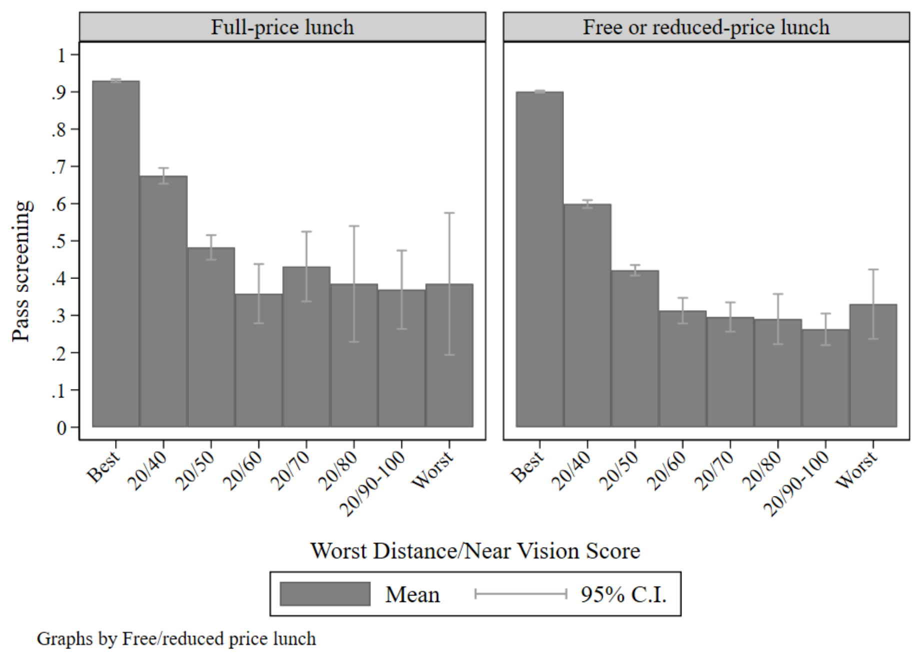

Panel B. Eye-exam



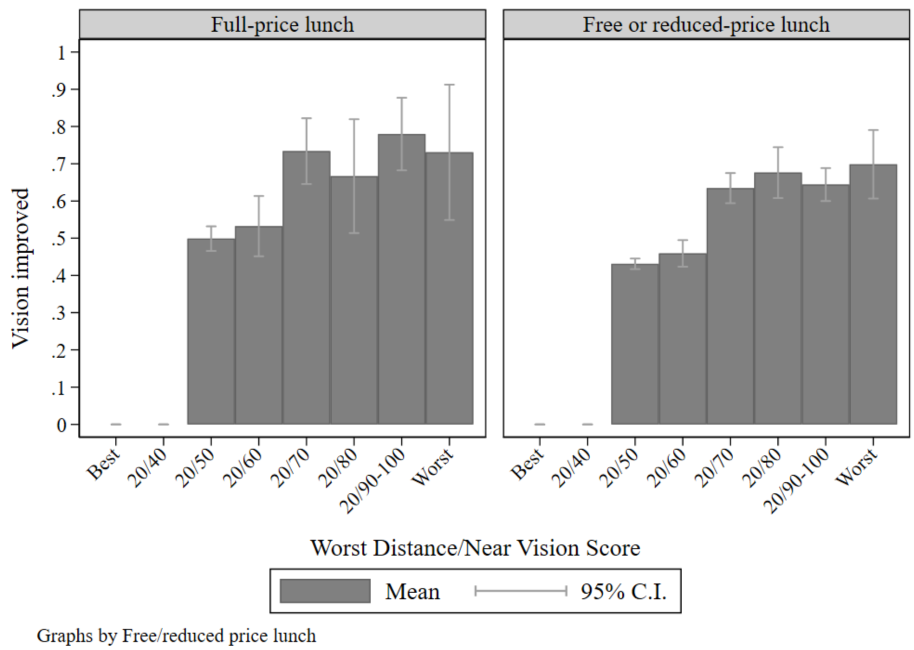
Panel C. New Glasses




Panel D. Pass Screening



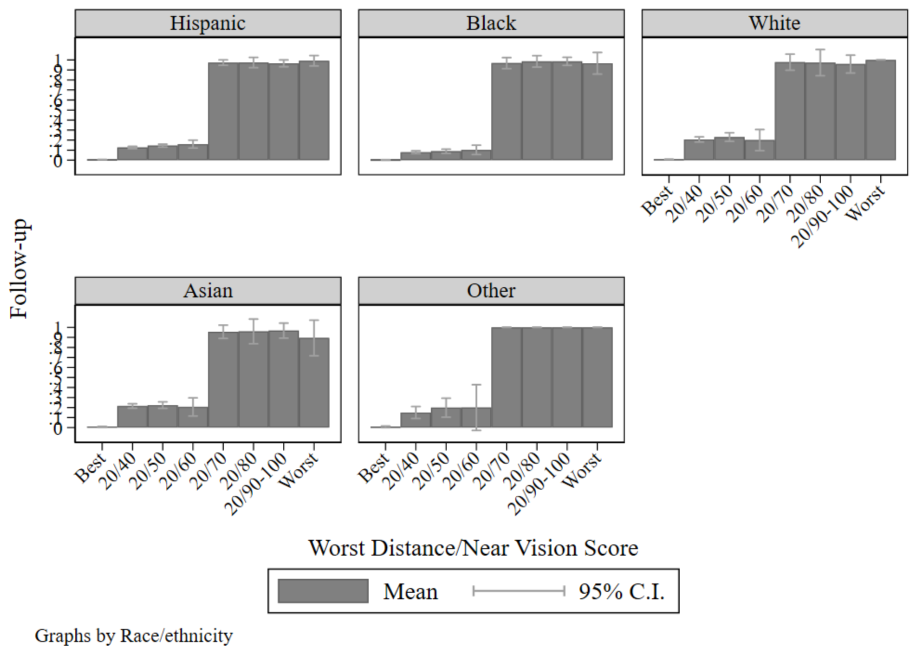
Panel E. Vision Improved



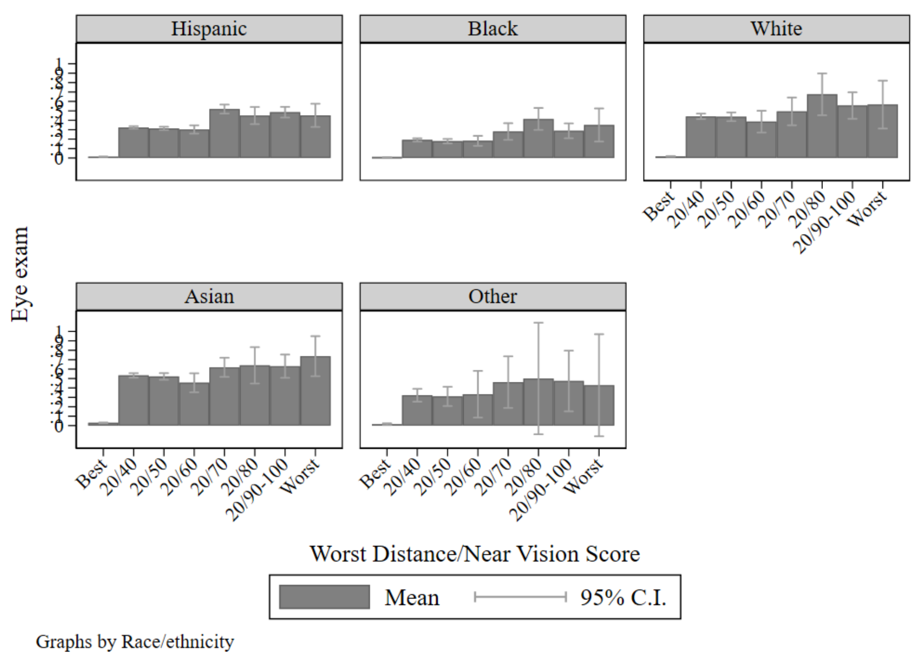

Notes: These figures present the means of each vision outcome for students in the full sample (N=259,177) and in each demographic category by initial worst visual acuity scores. The differences in the average heights of the 20/50 to 20/60 bars versus the 20/70 to 20/80 bars correspond to the estimates reported in relevant result tables.

Figure 3.6: Vision Outcomes by Worst Visual Acuity Score at Initial Screening and by Race/Ethnicity

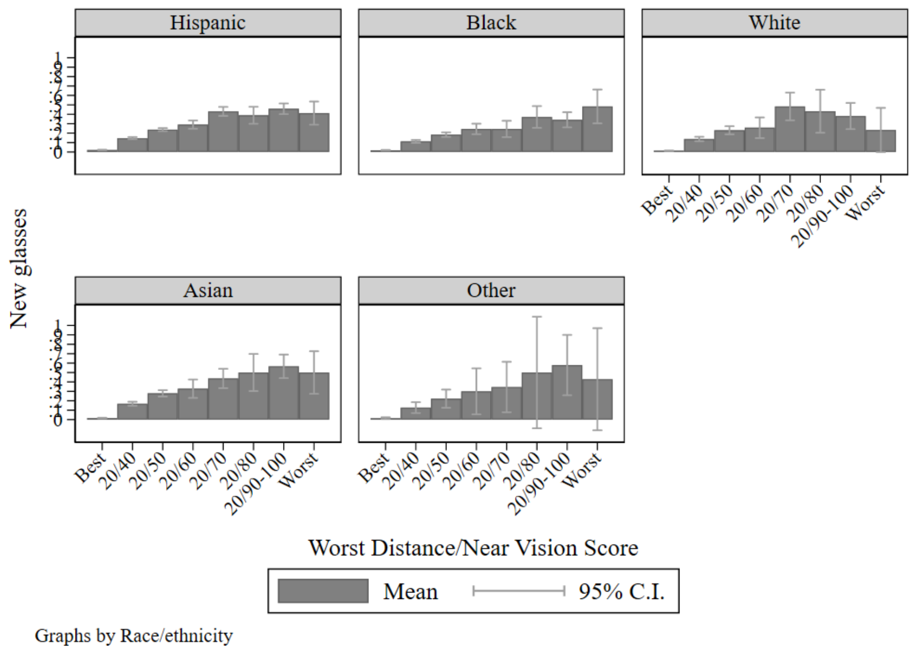
Panel A. Follow-up



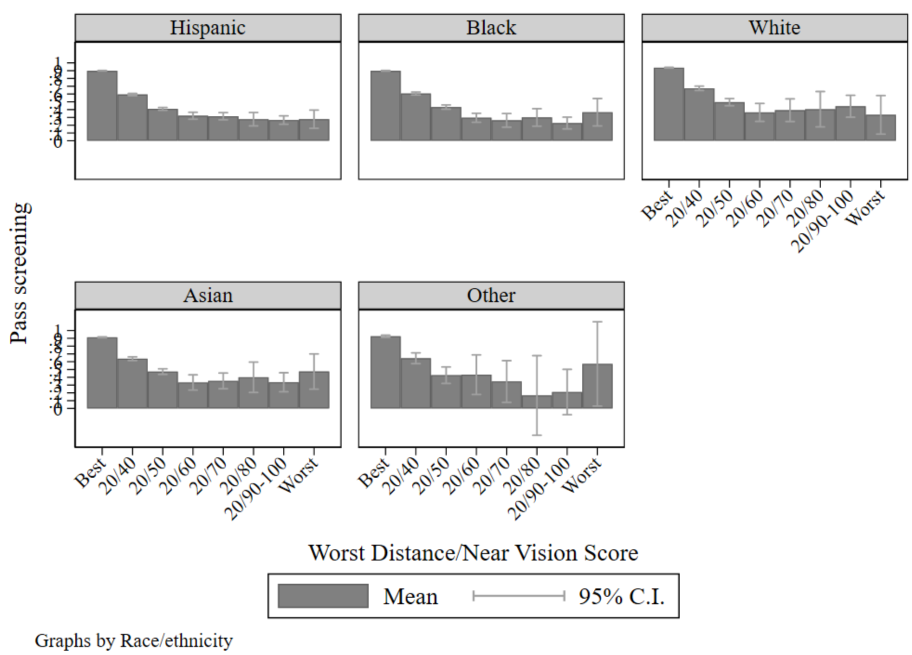

Panel B. Eye-exam



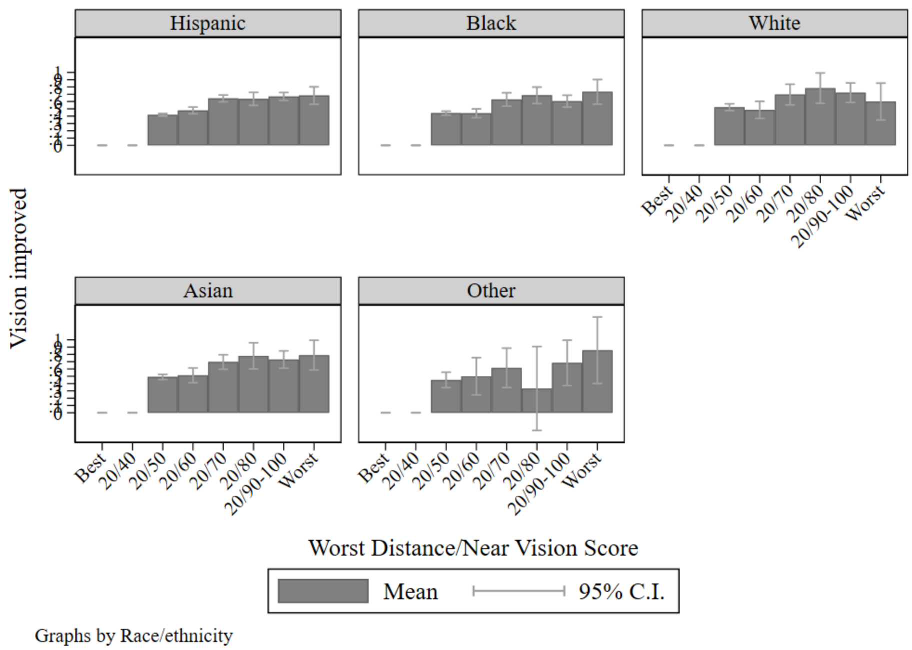
Panel C. New Glasses




Panel D. Pass Screening



Panel E. Vision Improved




Notes: These figures present the means of each vision outcome for students in the full sample (N=259,177) and in each demographic category by initial worst visual acuity scores. The differences in the average heights of the 20/50 to 20/60 bars versus the 20/70 to 20/80 bars correspond to the estimates reported in relevant result tables.

## Tables

Table 3.1: Student Demographics in Analysis Sample Compared to All Enrolled Kindergarteners, 2011-12 to 2015-16

| | Analysis Sample | | All Kindergarteners | | |
|---|---|---|---|---|---|
| | N | % | N | % | Diff. |
| Female | 129,643 | 50.0 | 205,586 | 48.5 | 1.55 |
| Male | 129,534 | 50.0 | 218,574 | 51.5 | -1.55 |
| Hispanic | 105,517 | 40.7 | 175,779 | 41.4 | -0.73 |
| Black | 58,962 | 22.8 | 106,460 | 25.1 | -2.35 |
| White | 43,823 | 16.9 | 68,252 | 16.1 | 0.82 |
| Asian | 45,057 | 17.4 | 64,054 | 15.1 | 2.28 |
| Other race/ethnicity | 5,818 | 2.2 | 9,615 | 2.3 | -0.03 |
| FRPL eligibility | 190,459 | 73.5 | 315,261 | 74.3 | -0.84 |
| Total | 259,177 | 100.0 | 424,160 | 100.0 | - |

Notes: The analysis sample is composed of students for whom we observe two consecutive years of grade K-1 vision screenings in as well as grade 3 standardized test results. The sample of all Kindergarteners includes those in the ASHR system who are indicated as actively enrolled. Observations are unique by student. Years 2011-12 to 2015-16 refer to years of initial vision screening. All differences in the right-most column are statistically significant at the 95% confidence level except for "Other race/ethnicity." FRPL=Free or reduced-price lunch.

Table 3.2: Summary Statistics for Analysis Samples, 2011-12 to 2015-16

| | N | % |
|---|---|---|
| Grade level at time of screening | | |
| Kindergarten | 255,964 | 98.8 |
| First Grade | 3,213 | 1.2 |
| Vision outcomes at time of screening | | |
| Wearing glasses | 11,922 | 4.6 |
| Pass vision screening | 216,542 | 83.6 |
| Worst Vision Acuity | | |
| Best | 221,959 | 85.6 |
| 20/40 | 21,010 | 8.1 |
| 20/50 | 11,227 | 4.3 |
| 20/60 | 1,793 | 0.7 |
| 20/70 | 1,351 | 0.5 |
| 20/80 | 464 | 0.2 |
| 20/90-100 | 1,112 | 0.4 |
| Worst | 261 | 0.1 |
| Around cutoff (20/50 to 20/80) | 14,835 | 5.7 |
| School Vision Program outcomes | | |
| Received follow-up | 9,201 | 3.6 |
| Confirmed eye exam | 16,095 | 6.2 |
| Vision outcomes in following year | | |
| New glasses | 11,404 | 4.4 |
| New pass | 21,895 | 8.4 |
| Vision improved | 7,931 | 3.1 |
| Total Vision Sample | 259,177 | 100.0 |
| | | |
| BMI outcomes | | |
| BMI decreased | 128,180 | 58.4 |
| Newly obese | 11,220 | 5.1 |
| No longer obese | 9,419 | 4.3 |
| Total BMI Sample | 219,561 | 100.0 |
| | | |
| Nurse visits outcomes | | |
| Nurse visits decreased | 14,332 | 5.5 |
| Vision-related nurse visits decreased | 3,136 | 1.2 |
| Total Nurse Visits Sample | 259,177 | 100.0 |
| | | |
| Absence outcomes | | |
| Absence rate decreased | 143,567 | 58.7 |
| Newly chronically absent | 11,809 | 4.8 |
| No longer chronically absent | 22,982 | 9.4 |
| Total Absence Sample | 244,494 | 100.0 |
| | | |
| Academics outcomes | | |
| Repeated 1st or 2nd Gr. | 16,224 | 6.3 |
| ELA proficiency | 193,398 | 74.6 |
| Math proficiency | 192,283 | 74.2 |
| Total Academic Sample | 259,177 | 100.0 |

Notes: The analysis sample is composed of students for whom we observe two consecutive years of grade K-1 vision screenings in as well as grade 3 standardized test results. This includes some students repeating grades K or 1. The BMI and Absence samples are subsets of the analysis sample due to data availability. Observations are unique by student. Years 2011-12 to 2015-16 refer to years of initial vision screening. Worst visual acuity refers to the student's acuity score in the left or right eye and for either the far or near vision tests. Students fail the screening if their worst acuity score is 20/40 or worse. Vision-related nurse visits include nurse visits explicitly for vision issues but also for headaches or dizziness, which could result from difficulty seeing. ELA and Math proficiency refer to scoring above the 25th percentile in these grade 3 standardized tests.

Table 3.3: Intent-to-Treat Estimates of the Effect of Follow-up on Vision Outcomes

| | (1) Follow-up | (2) Eye exam | (3) New glasses | (4) Pass screening | (5) Vision improved |
|---|---|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trends | | | | | |
| Above Cutoff | 0.822*** | 0.171*** | 0.166*** | -0.101*** | 0.211*** |
| | (0.006) | (0.012) | (0.012) | (0.011) | (0.012) |
| R-Squared | 0.386 | 0.014 | 0.015 | 0.005 | 0.019 |
| Observations | | | 14,835 | | |
| Panel B. Regression using 20/50 to 20/80, single trend | | | | | |
| Above Cutoff | 0.817*** | 0.229*** | 0.082*** | 0.095*** | 0.151*** |
| | (0.018) | (0.027) | (0.025) | (0.026) | (0.027) |
| Vision Score | 0.002 | -0.027** | 0.039*** | -0.093*** | 0.028** |
| | (0.008) | (0.011) | (0.010) | (0.011) | (0.012) |
| R-squared | 0.386 | 0.015 | 0.016 | 0.009 | 0.020 |
| Observations | | | 14,835 | | |
| Panel C. Regression using 20/50 to 20/80, trends above and below cutoff | | | | | |
| Above Cutoff | 0.818*** | 0.231*** | 0.076*** | 0.108*** | 0.150*** |
| | (0.019) | (0.027) | (0.025) | (0.026) | (0.028) |
| Vision Score X Above Cutoff | 0.004 | 0.018 | -0.050* | 0.099*** | -0.005 |
| | (0.013) | (0.029) | (0.028) | (0.028) | (0.028) |
| Vision Score | 0.002 | -0.031*** | 0.049*** | -0.111*** | 0.029** |
| | (0.010) | (0.011) | (0.011) | (0.012) | (0.013) |
| R-squared | 0.386 | 0.015 | 0.017 | 0.010 | 0.020 |
| Observations | | | 14,835 | | |
| Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff | | | | | |
| Above Cutoff | 0.805*** | 0.213*** | 0.021 | 0.200*** | -0.405*** |
| | (0.011) | (0.018) | (0.016) | (0.017) | (0.017) |
| Vision Score X Above Cutoff | -0.009** | 0.011 | -0.067*** | 0.158*** | -0.333*** |
| | (0.004) | (0.010) | (0.009) | (0.009) | (0.009) |
| Vision Score | 0.009*** | -0.021*** | 0.079*** | -0.166*** | 0.345*** |
| | (0.003) | (0.005) | (0.004) | (0.005) | (0.005) |
| R-squared | 0.373 | 0.193 | 0.116 | 0.162 | 0.399 |
| Observations | | | 37,218 | | |

Notes: Each column and panel contain the estimates from a different regression, where outcomes vary by column and specifications vary by panel. The main treatment variable "Above Cutoff" is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. Panels B through D also include "Vision Score", which is a discrete variable ranging from 1-7 corresponding to vision scores 20/40, 20/50, 20/60, 20/70, 20/80, 20/90, and 20/100+. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.4: Intent-to-Treat Estimates of the Effect of Follow-up on BMI and Nurse Visit Outcomes

| | (1) BMI decreased | (2) Newly obese | (3) No longer obese | (4) Nurse visits decreased | (5) Vision nurse visits decreased |
|---|---|---|---|---|---|
| **Panel A. Regression using 20/50 to 2080, trends** | | | | | |
| Above Cutoff | 0.008 | 0.000 | 0.003 | -0.007 | -0.000 |
| | (0.015) | (0.006) | (0.006) | (0.005) | (0.003) |
| R-Squared | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | | 12,435 | | 14,835 | |
| **Panel B. Regression using 20/50 to 20/80, single trend** | | | | | |
| Above Cutoff | 0.033 | -0.016 | 0.004 | 0.008 | -0.002 |
| | (0.030) | (0.014) | (0.013) | (0.013) | (0.006) |
| Vision Score | -0.012 | 0.008 | -0.000 | -0.007 | 0.001 |
| | (0.013) | (0.006) | (0.006) | (0.005) | (0.002) |
| R-squared | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | | 12,435 | | 14,835 | |
| **Panel C. Regression using 20/50 to 20/80, trends above and below cutoff** | | | | | |
| Above Cutoff | 0.034 | -0.012 | 0.001 | 0.007 | -0.001 |
| | (0.030) | (0.014) | (0.014) | (0.013) | (0.006) |
| Vision Score X Above Cutoff | 0.006 | 0.026 | -0.017 | -0.007 | 0.004 |
| | (0.033) | (0.019) | (0.015) | (0.012) | (0.006) |
| Vision Score | -0.013 | 0.003 | 0.003 | -0.006 | 0.000 |
| | (0.014) | (0.007) | (0.007) | (0.006) | (0.003) |
| R-squared | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | | 12,435 | | 14,835 | |
| **Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff** | | | | | |
| Above Cutoff | 0.017 | -0.014 | -0.001 | -0.006 | 0.001 |
| | (0.020) | (0.009) | (0.008) | (0.008) | (0.004) |
| Vision Score X Above Cutoff | 0.006 | 0.001 | -0.004 | 0.004 | 0.002 |
| | (0.011) | (0.005) | (0.004) | (0.004) | (0.002) |
| Vision Score | -0.002 | 0.004* | 0.004 | -0.001 | -0.001 |
| | (0.005) | (0.002) | (0.002) | (0.002) | (0.001) |
| R-squared | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | | 31,280 | | 37,218 | |

Notes: Each column and panel contain the estimates from a different regression, where outcomes vary by column and specifications vary by panel. The main treatment variable "Above Cutoff" is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. Panels B through D also include "Vision Score", which is a discrete variable ranging from 1-7 corresponding to vision scores 20/40, 20/50, 20/60, 20/70, 20/80, 20/90, and 20/100+. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.5: Intent-to-Treat Estimates of the Effect of Follow-up on Absence

| | (1) Absence rate decreased | (2) Newly chronically absent | (3) No longer chronically absent |
|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trends | | | |
| Above Cutoff | -0.020 | -0.011** | 0.000 |
| | (0.013) | (0.005) | (0.008) |
| R-Squared | 0.000 | 0.000 | 0.000 |
| Observations | | 13,923 | |
| Panel B. Regression using 20/50 to 20/80, single trend | | | |
| Above Cutoff | -0.024 | 0.008 | 0.001 |
| | (0.029) | (0.012) | (0.018) |
| Vision Score | 0.002 | -0.009* | -0.001 |
| | (0.012) | (0.005) | (0.008) |
| R-squared | 0.000 | 0.000 | 0.000 |
| Observations | | 13,923 | |
| Panel C. Regression using 20/50 to 20/80, trends above and below cutoff | | | |
| Above Cutoff | -0.025 | 0.008 | 0.002 |
| | (0.030) | (0.012) | (0.018) |
| Vision Score X Above Cutoff | -0.009 | 0.002 | 0.006 |
| | (0.030) | (0.013) | (0.019) |
| Vision Score | 0.003 | -0.009 | -0.002 |
| | (0.014) | (0.006) | (0.008) |
| R-squared | 0.000 | 0.000 | 0.000 |
| Observations | | 13,923 | |
| Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff | | | |
| Above Cutoff | -0.029 | -0.009 | -0.009 |
| | (0.018) | (0.008) | (0.011) |
| Vision Score X Above Cutoff | -0.002 | 0.001 | -0.002 |
| | (0.010) | (0.004) | (0.006) |
| Vision Score | 0.005 | -0.000 | 0.005 |
| | (0.005) | (0.002) | (0.003) |
| R-squared | 0.000 | 0.000 | 0.000 |
| Observations | | 35,071 | |

Notes: Each column and panel contain the estimates from a different regression, where outcomes vary by column and specifications vary by panel. The main treatment variable "Above Cutoff" is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. Panels B through D also include "Vision Score", which is a discrete variable ranging from 1-7 corresponding to vision scores 20/40, 20/50, 20/60, 20/70, 20/80, 20/90, and 20/100+. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.6: Intent-to-Treat Estimates of the Effect of Follow-up on Academic Outcomes

| | (1) Repeated a grade | (2) ELA proficiency | (3) Math proficiency |
|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trends | | | |
| Above Cutoff | 0.006 | -0.001 | -0.010 |
| | (0.008) | (0.012) | (0.012) |
| R-Squared | 0.000 | 0.000 | 0.000 |
| Above Cutoff | | 14,835 | |
| Panel B. Regression using 20/50 to 20/80, single trend | | | |
| Above Cutoff | -0.019 | -0.035 | -0.048* |
| | (0.017) | (0.029) | (0.029) |
| Vision Score | 0.012* | 0.016 | 0.018 |
| | (0.007) | (0.012) | (0.012) |
| R-squared | 0.000 | 0.000 | 0.000 |
| Observations | | 14,835 | |
| Panel C. Regression using 20/50 to 20/80, trends above and below cutoff | | | |
| Above Cutoff | -0.020 | -0.026 | -0.043 |
| | (0.017) | (0.029) | (0.029) |
| Vision Score X Above Cutoff | -0.010 | 0.073*** | 0.037 |
| | (0.018) | (0.026) | (0.028) |
| Vision Score | 0.014* | 0.003 | 0.011 |
| | (0.008) | (0.013) | (0.013) |
| R-squared | 0.000 | 0.001 | 0.000 |
| Observations | | 14,835 | |
| Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff | | | |
| Above Cutoff | -0.012 | 0.040** | 0.034** |
| | (0.011) | (0.018) | (0.017) |
| Vision Score X Above Cutoff | -0.001 | 0.023** | 0.017* |
| | (0.006) | (0.010) | (0.009) |
| Vision Score | 0.009*** | -0.028*** | -0.027*** |
| | (0.003) | (0.005) | (0.005) |
| R-squared | 0.001 | 0.002 | 0.002 |
| Observations | | 37,218 | |

Notes: Each column and panel contain the estimates from a different regression, where outcomes vary by column and specifications vary by panel. The main treatment variable "Above Cutoff" is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. Panels B through D also include "Vision Score", which is a discrete variable ranging from 1-7 corresponding to vision scores 20/40, 20/50, 20/60, 20/70, 20/80, 20/90, and 20/100+. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.7: Intent-to-Treat Estimates of the Effect of Follow-up on Vision Outcomes, Relative to Students Who Should be Unaffected by the Follow-up Cutoff

| | (1) Follow-up | (2) Eye exam | (3) New glasses | (4) Pass screening | (5) Vision improved |
|---|---|---|---|---|---|
| **Panel A. Regression using 20/50 to 2080, no trends** | | | | | |
| Above Cutoff X Treated | 0.815*** | 0.196*** | 0.064*** | 0.051*** | 0.053*** |
| | (0.007) | (0.015) | (0.014) | (0.015) | (0.015) |
| R-squared | 0.685 | 0.034 | 0.015 | 0.014 | 0.047 |
| Observations | | | 27,758 | | |
| **Panel B. Regression using 20/50 to 20/80, single trend** | | | | | |
| Above Cutoff X Treated | 0.843*** | 0.245*** | 0.061* | 0.116*** | 0.082** |
| | (0.020) | (0.035) | (0.033) | (0.034) | (0.034) |
| Vision Score X Treated | -0.014 | -0.023 | 0.001 | -0.029* | -0.015 |
| | (0.009) | (0.015) | (0.014) | (0.015) | (0.015) |
| R-squared | 0.686 | 0.034 | 0.016 | 0.018 | 0.048 |
| Observations | | | 27,758 | | |
| **Panel C. Regression using 20/50 to 20/80, trends above and below cutoff** | | | | | |
| Above Cutoff X Treated | 0.844*** | 0.251*** | 0.095*** | 0.055 | 0.099** |
| | (0.021) | (0.040) | (0.036) | (0.037) | (0.039) |
| Vision Score X Above Cutoff X Treated | 0.003 | 0.022 | -0.001 | 0.012 | 0.015 |
| | (0.014) | (0.034) | (0.034) | (0.034) | (0.036) |
| Vision Score X Treated | -0.014 | -0.029 | -0.017 | 0.000 | -0.025 |
| | (0.011) | (0.019) | (0.018) | (0.019) | (0.020) |
| R-squared | 0.686 | 0.034 | 0.017 | 0.019 | 0.048 |
| Observations | | | 27,758 | | |
| **Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff** | | | | | |
| Above Cutoff X Treated | 0.887*** | 0.224*** | 0.051* | 0.187*** | -0.372*** |
| | (0.023) | (0.034) | (0.028) | (0.031) | (0.035) |
| Vision Score X Above Cutoff X Treated | 0.038*** | 0.006 | -0.004 | 0.114*** | -0.183*** |
| | (0.013) | (0.018) | (0.016) | (0.017) | (0.019) |
| Vision Score X Treated | -0.042*** | -0.014 | 0.006 | -0.080*** | 0.230*** |
| | (0.013) | (0.016) | (0.012) | (0.015) | (0.017) |
| R-squared | 0.649 | 0.027 | 0.045 | 0.066 | 0.345 |
| Observations | | | 55,247 | | |

Notes: Each column and panel contain the estimates from a different regression, where outcomes vary by column and specifications vary by panel. The main treatment variable "Above Cutoff" is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. Panels B through D also include "Vision Score", which is a discrete variable ranging from 1-7 corresponding to vision scores 20/40, 20/50, 20/60, 20/70, 20/80, 20/90, and 20/100+. These estimates incorporate a control group to the analysis, composed of students unaffected by the follow-up cutoff, either because they attend a community school or determined to be at risk of amblyopia. "Treated" is equal to 1 if a student is affected by the follow-up cutoff and equal to 0 otherwise. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.8: Intent-to-Treat Estimates of the Effect of Follow-up on BMI and Nurse Visits, Relative to Students Who Should be Unaffected by Follow-up Cutoff

| | (1) BMI decreased | (2) Newly obese | (3) No longer obese | (4) Nurse visits decreased | (5) Vision nurse visits decreased |
|---|---|---|---|---|---|
| **Panel A. Regression using 20/50 to 2080, no trends** | | | | | |
| Above Cutoff X Treated | 0.025 | -0.001 | 0.003 | -0.010 | 0.000 |
| | (0.018) | (0.007) | (0.007) | (0.006) | (0.003) |
| R-squared | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | | | 23,293 | | |
| **Panel B. Regression using 20/50 to 20/80, single trend** | | | | | |
| Above Cutoff X Treated | 0.041 | -0.025 | 0.005 | -0.031** | -0.007 |
| | (0.040) | (0.018) | (0.017) | (0.015) | (0.007) |
| Vision Score X Treated | -0.007 | 0.012 | -0.001 | 0.010 | 0.003 |
| | (0.017) | (0.008) | (0.008) | (0.006) | (0.003) |
| R-squared | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 |
| Observations | | | 23,293 | | |
| **Panel C. Regression using 20/50 to 20/80, trends above and below cutoff** | | | | | |
| Above Cutoff X Treated | 0.063 | -0.040** | 0.014 | -0.028* | -0.009 |
| | (0.044) | (0.020) | (0.021) | (0.016) | (0.007) |
| Vision Score X Above Cutoff X Treated | 0.030 | 0.005 | -0.004 | -0.002 | 0.001 |
| | (0.039) | (0.022) | (0.018) | (0.014) | (0.007) |
| Vision Score X Treated | -0.022 | 0.019* | -0.005 | 0.009 | 0.004 |
| | (0.022) | (0.010) | (0.011) | (0.008) | (0.004) |
| R-squared | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 |
| Observations | | | 23,293 | | |
| **Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff** | | | | | |
| Above Cutoff X Treated | 0.058 | -0.042*** | 0.009 | -0.070*** | -0.008 |
| | (0.036) | (0.016) | (0.017) | (0.016) | (0.006) |
| Vision Score X Above Cutoff X Treated | 0.017 | -0.015* | 0.003 | -0.028*** | -0.003 |
| | (0.019) | (0.009) | (0.009) | (0.009) | (0.003) |
| Vision Score X Treated | -0.017 | 0.019*** | -0.002 | 0.033*** | 0.004 |
| | (0.016) | (0.007) | (0.008) | (0.008) | (0.003) |
| R-squared | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 |
| Observations | | | 46,404 | | |

Notes: Each column and panel contain the estimates from a different regression, where outcomes vary by column and specifications vary by panel. The main treatment variable "Above Cutoff" is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. Panels B through D also include "Vision Score", which is a discrete variable ranging from 1-7 corresponding to vision scores 20/40, 20/50, 20/60, 20/70, 20/80, 20/90, and 20/100+. These estimates incorporate a control group to the analysis, composed of students unaffected by the follow-up cutoff, either because they attend a community school or determined to be at risk of amblyopia. "Treated" is equal to 1 if a student is affected by the follow-up cutoff and equal to 0 otherwise. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.9: Intent-to-Treat Estimates of the Effect of Follow-up on Absence, Relative to Students Who Should be Unaffected by Follow-up Cutoff

| | (1) Absence rate decreased | (2) Newly chronically absent | (3) No longer chronically absent |
|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trends | | | |
| Above Cutoff X Treated | -0.012 | -0.005 | -0.012 |
| | (0.015) | (0.006) | (0.010) |
| R-squared | 0.000 | 0.000 | 0.000 |
| Observations | | 26,127 | |
| Panel B. Regression using 20/50 to 20/80, single trend | | | |
| Above Cutoff X Treated | -0.049 | 0.016 | -0.032 |
| | (0.037) | (0.016) | (0.024) |
| Vision Score X Treated | 0.018 | -0.010 | 0.010 |
| | (0.016) | (0.007) | (0.011) |
| R-squared | 0.000 | 0.000 | 0.000 |
| Observations | | 26,127 | |
| Panel C. Regression using 20/50 to 20/80, trends above and below cutoff | | | |
| Above Cutoff X Treated | -0.044 | 0.014 | -0.035 |
| | (0.042) | (0.017) | (0.027) |
| Vision Score X Above Cutoff X Treated | -0.002 | -0.000 | 0.001 |
| | (0.037) | (0.016) | (0.024) |
| Vision Score X Treated | 0.016 | -0.009 | 0.011 |
| | (0.021) | (0.009) | (0.013) |
| R-squared | 0.000 | 0.000 | 0.000 |
| Observations | | 26,127 | |
| Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff | | | |
| Above Cutoff X Treated | -0.049 | -0.003 | -0.059*** |
| | (0.034) | (0.015) | (0.022) |
| Vision Score X Above Cutoff X Treated | -0.010 | -0.007 | -0.026** |
| | (0.018) | (0.008) | (0.012) |
| Vision Score X Treated | 0.020 | 0.001 | 0.027*** |
| | (0.015) | (0.007) | (0.010) |
| R-squared | 0.000 | 0.000 | 0.001 |
| Observations | | 52,098 | |

Notes: Each column and panel contain the estimates from a different regression, where outcomes vary by column and specifications vary by panel. The main treatment variable "Above Cutoff" is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. Panels B through D also include "Vision Score", which is a discrete variable ranging from 1-7 corresponding to vision scores 20/40, 20/50, 20/60, 20/70, 20/80, 20/90, and 20/100+. These estimates incorporate a control group to the analysis, composed of students unaffected by the follow-up cutoff, either because they attend a community school or determined to be at risk of amblyopia. "Treated" is equal to 1 if a student is affected by the follow-up cutoff and equal to 0 otherwise. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3.10: Intent-to-Treat Estimates of the Effect of Follow-up on Academics, Relative to Students who Should be Unaffected by Follow-up Cutoff

| | (1) Repeated a grade | (2) ELA proficiency | (3) Math proficiency |
|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trends | | | |
| Above Cutoff X Treated | -0.000 | 0.028** | 0.018 |
| | (0.009) | (0.014) | (0.013) |
| R-squared | 0.000 | 0.011 | 0.018 |
| Observations | | 27,758 | |
| Panel B. Regression using 20/50 to 20/80, single trend | | | |
| Above Cutoff X Treated | 0.003 | 0.036 | 0.025 |
| | (0.022) | (0.035) | (0.035) |
| Vision Score X Treated | -0.002 | -0.004 | -0.004 |
| | (0.009) | (0.016) | (0.016) |
| R-squared | 0.001 | 0.012 | 0.018 |
| Observations | | 27,758 | |
| Panel C. Regression using 20/50 to 20/80, trends above and below cutoff | | | |
| Above Cutoff X Treated | 0.001 | 0.033 | 0.018 |
| | (0.024) | (0.038) | (0.037) |
| Vision Score X Above Cutoff X Treated | -0.010 | 0.058* | 0.023 |
| | (0.022) | (0.030) | (0.031) |
| Vision Score X Treated | 0.000 | -0.010 | -0.003 |
| | (0.012) | (0.018) | (0.018) |
| R-squared | 0.001 | 0.012 | 0.018 |
| Observations | | 27,758 | |
| Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff | | | |
| Above Cutoff X Treated | 0.006 | 0.128*** | 0.128*** |
| | (0.019) | (0.030) | (0.029) |
| Vision Score X Above Cutoff X Treated | 0.007 | 0.056*** | 0.061*** |
| | (0.011) | (0.017) | (0.016) |
| Vision Score X Treated | -0.004 | -0.061*** | -0.065*** |
| | (0.009) | (0.014) | (0.014) |
| R-squared | 0.003 | 0.006 | 0.006 |
| Observations | | 55,247 | |

Notes: Each column and panel contain the estimates from a different regression, where outcomes vary by column and specifications vary by panel. The main treatment variable "Above Cutoff" is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. Panels B through D also include "Vision Score", which is a discrete variable ranging from 1-7 corresponding to vision scores 20/40, 20/50, 20/60, 20/70, 20/80, 20/90, and 20/100+. These estimates incorporate a control group to the analysis, composed of students unaffected by the follow-up cutoff, either because they attend a community school or determined to be at risk of amblyopia. "Treated" is equal to 1 if a student is affected by the follow-up cutoff and equal to 0 otherwise. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

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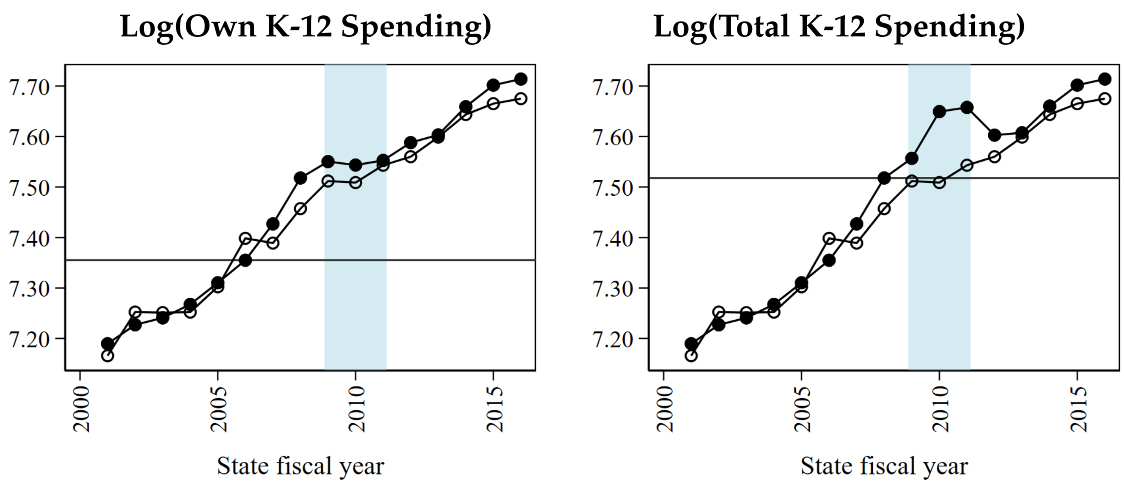
APPENDIX A

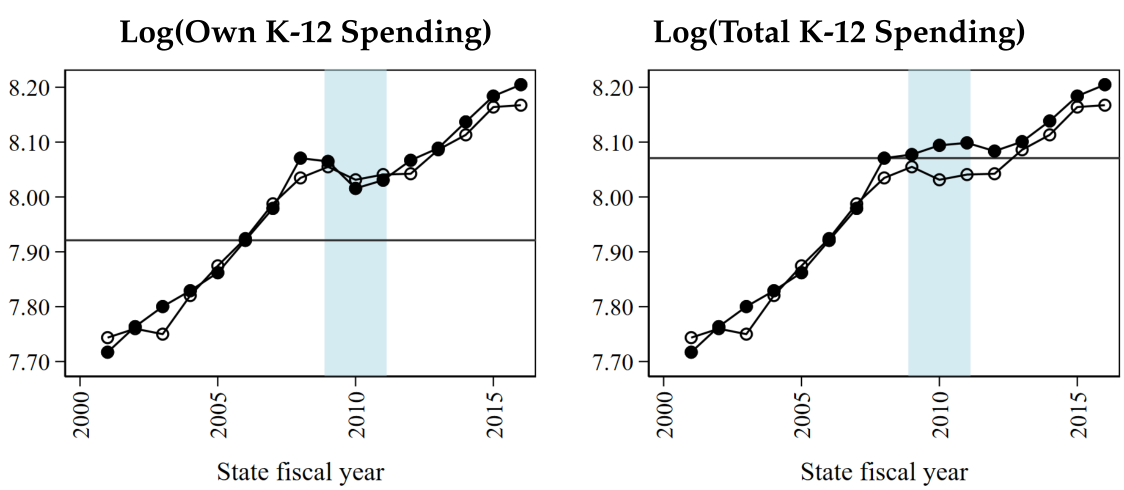
## CHAPTER 1 APPENDIX

Figure A.1: Robustness: K-12 Spending Effects Relative to Alternative Counterfactual, by Size of 2009 Budget Shortfall, 2001-2016

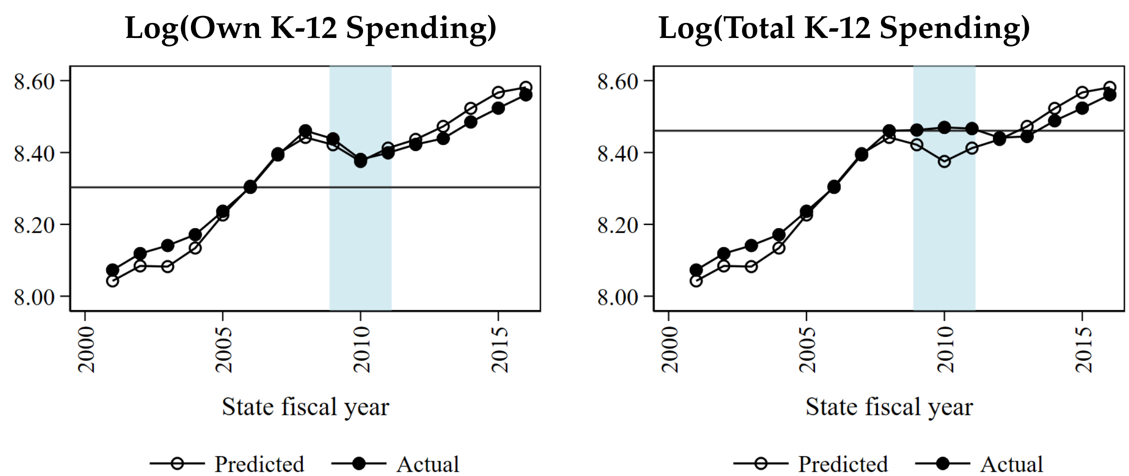
Panel A. Small Shortfall




Panel B. Moderate Shortfall



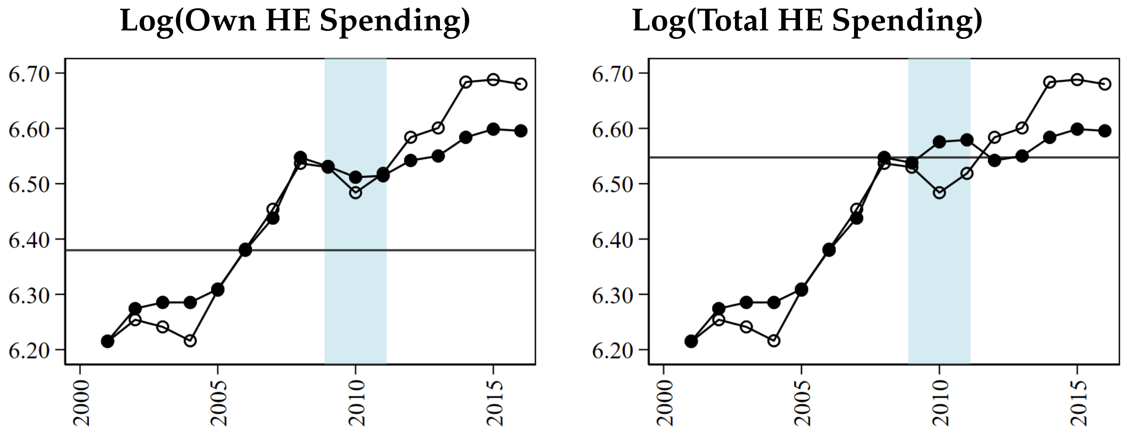
Panel C. Large Shortfall



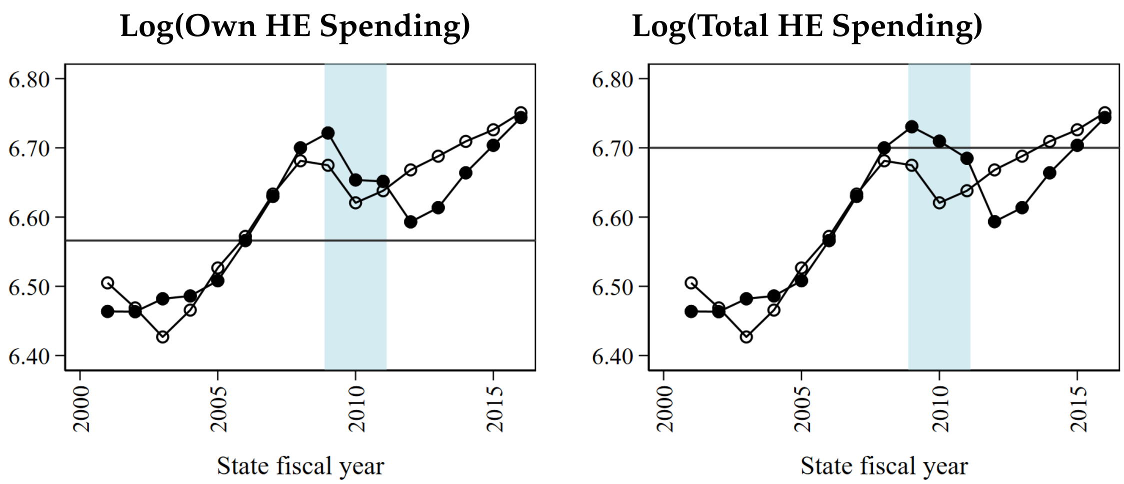

Notes: Each graph contains two series of logged spending. The line with hollow markers corresponds to counterfactual spending, the result of applying the education-share of state funds from 2005-2008 to all years. The line with solid markers corresponds to actual spending. The horizontal line in each figure is drawn at the relevant spending threshold (for own or total spending), and the years of the SFSF 2009-2011 are shaded gray.
Sources: Author's calculations using data from NCES-CCD, NASBO, SHEF-SSDB.

Figure A.2: Robustness: Higher-Ed Spending Effects Relative to Alternative Counterfactual, by Size of 2009 Budget Shortfall, 2001-2016

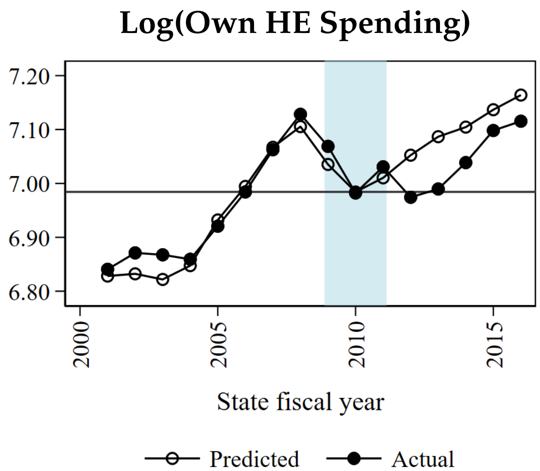
Panel A. Small Shortfall

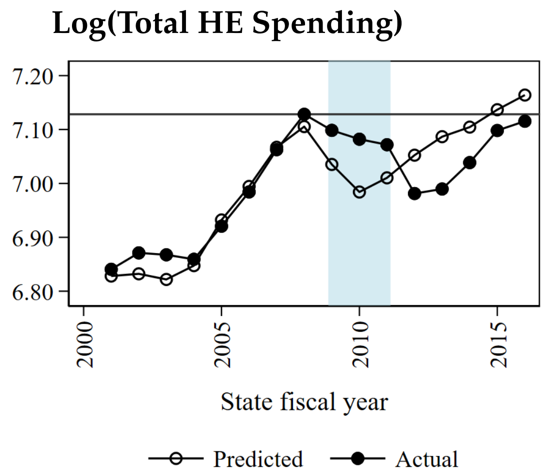



Panel B. Moderate Shortfall



Panel C. Large Shortfall




Notes: Each graph contains two series of logged spending. The line with hollow markers corresponds to counterfactual spending, the result of applying the education-share of state funds from 2005-2008 to all years. The line with solid markers corresponds to actual spending. The horizontal line in each figure is drawn at the relevant spending threshold (for own or total spending), and the years of the SFSF 2009-2011 are shaded gray.
Sources: Author's calculations using data from NCES-CCD, NASBO, SHEF-SSDB.

APPENDIX B

## CHAPTER 2 APPENDIX

Table B.1: Linear Model with IHS-Transformed Outcomes: Estimated Effects of Licensing Requirements on Pharmacy Technician College Program Completions, Number of Programs Offered, and PTCB Certifications (% Change)

| | (1)<br>Completions | (2)<br>Programs | (3)<br>PTCB Certifications |
|---|---|---|---|
| Registration only | -0.29 | -0.16 | -0.05 |
| | (0.29) | (0.14) | (0.07) |
| Registration and Certification only | -0.52 | -0.12 | 0.23** |
| | (0.36) | (0.15) | (0.09) |
| Registration and Training only | 1.07** | 0.47** | -0.05 |
| | (0.50) | (0.18) | (0.11) |
| Registration, Training, and Certification | 0.95 | 0.51 | 0.46*** |
| | (0.83) | (0.40) | (0.15) |
| Pre-treat. Mean | 254 | 15 | 535 |
| R-squared | 0.825 | 0.897 | 0.953 |
| N | 1,100 | 1,100 | 1,100 |

Notes: Each column contains coefficients from a regression following the difference-in-difference specification in Equation 2.1, which include state and year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS and PTCB. Treatment data is from author's collection and coding of state laws and regulations.

Table B.2: Linear Model with IHS-Transformed Outcomes: Estimated Effects of Licensing Requirements on Pharmacy Technician College Program Completions and Number of Programs Offered, by Sector (% Change)

| | (1)<br>Public<br>Completions | (2)<br>For-profit<br>Completions | (3)<br>Public<br>Programs | (4)<br>For-profit<br>Programs |
|---|---|---|---|---|
| Registration only | -0.28 | -0.08 | -0.14 | -0.03 |
| | (0.26) | (0.40) | (0.11) | (0.20) |
| Registration and Certification only | -0.16 | -0.37 | 0.00 | -0.02 |
| | (0.34) | (0.51) | (0.15) | (0.26) |
| Registration and Training only | 0.48 | 1.09** | 0.34** | 0.45** |
| | (0.50) | (0.54) | (0.17) | (0.17) |
| Registration, Training, and Certification | 0.27 | 1.53 | 0.21 | 0.54 |
| | (0.77) | (1.00) | (0.33) | (0.41) |
| Pre-treat. Mean | 65 | 185 | 6 | 9 |
| R-squared | 0.832 | 0.787 | 0.888 | 0.836 |
| N | 1,100 | 1,100 | 1,100 | 1,100 |

Notes: Each column contains coefficients from a regression following the difference-in-difference specification in Equation 2.1, which include state and year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS. Treatment data is from author's collection and coding of state laws and regulations.

Table B.3: Linear Model with IHS-Transformed Outcomes: Estimated Effects of Licensing Requirements on Completions Within Pharmacy Technician College Programs, Overall and by Sector (% Change)

| | (1)<br>Overall | (2)<br>Public | (3)<br>For-profit |
|---|---|---|---|
| Registration only | 0.22 | 0.16 | 0.25 |
| | (0.13) | (0.15) | (0.27) |
| Registration and Certification only | 0.18 | -0.10 | 0.49* |
| | (0.14) | (0.23) | (0.27) |
| Registration and Training only | 0.21 | -0.24 | 0.63 |
| | (0.29) | (0.19) | (0.38) |
| Registration, Training, and Certification | -0.03 | -0.65*** | 0.74 |
| | (0.27) | (0.18) | (0.58) |
| Pre-treat. Mean | 14 | 9 | 17 |
| R-squared | 0.593 | 0.623 | 0.548 |
| N | 10,293 | 4,509 | 5,426 |

Notes: Each column contains coefficients from a regression following the difference-in-difference specification in Equation 2.2, which include state and year fixed effects. Standard errors in parentheses are clustered at the state level. The pre-treatment means report the average of the outcome across states in the year prior to the first training requirement (among states with training requirements). * $p < .10$, ** $p < .05$, *** $p < .01$.

Sources: Outcome data is from IPEDS. Treatment data is from author's collection and coding of state laws and regulations.

APPENDIX C

# CHAPTER 3 APPENDIX

Table C.1: Intent-to-Treat Estimates of the Effect of Follow-up on Vision Outcomes, with Controls

| | (1) Follow-up | (2) Eye exam | (3) New glasses | (4) Pass screening | (5) Vision improved |
|---|---|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trend | | | | | |
| Above Cutoff | 0.822*** | 0.162*** | 0.157*** | -0.097*** | 0.219*** |
| | (0.007) | (0.012) | (0.012) | (0.012) | (0.013) |
| Black | -0.021*** | -0.061*** | -0.031*** | -0.007 | 0.005 |
| | (0.008) | (0.012) | (0.011) | (0.013) | (0.014) |
| White | 0.023 | 0.032* | -0.044** | 0.047** | 0.056*** |
| | (0.015) | (0.019) | (0.018) | (0.019) | (0.020) |
| Asian | 0.028** | 0.114*** | 0.004 | 0.038** | 0.060*** |
| | (0.013) | (0.016) | (0.015) | (0.016) | (0.015) |
| Other | 0.043* | -0.016 | -0.040 | 0.014 | 0.009 |
| | (0.025) | (0.032) | (0.031) | (0.034) | (0.035) |
| Free/reduced price lunch | -0.024** | -0.068*** | -0.040*** | -0.044*** | -0.040*** |
| | (0.010) | (0.013) | (0.012) | (0.013) | (0.013) |
| R-Squared | 0.447 | 0.176 | 0.101 | 0.086 | 0.099 |
| Observations | | | 14,835 | | |
| Panel B. Regression using 20/50 to 20/80, single trend | | | | | |
| Above Cutoff | 0.811*** | 0.173*** | 0.036 | 0.134*** | 0.183*** |
| | (0.020) | (0.026) | (0.027) | (0.028) | (0.030) |
| Vision Score | 0.005 | -0.005 | 0.056*** | -0.108*** | 0.017 |
| | (0.008) | (0.011) | (0.011) | (0.012) | (0.013) |
| Black | -0.021*** | -0.061*** | -0.031*** | -0.008 | 0.005 |
| | (0.008) | (0.012) | (0.011) | (0.013) | (0.014) |
| White | 0.023 | 0.032* | -0.043** | 0.046** | 0.056*** |
| | (0.015) | (0.019) | (0.018) | (0.019) | (0.020) |
| Asian | 0.028** | 0.114*** | 0.004 | 0.037** | 0.060*** |
| | (0.013) | (0.016) | (0.015) | (0.016) | (0.015) |
| Other | 0.043* | -0.016 | -0.040 | 0.014 | 0.009 |
| | (0.025) | (0.032) | (0.031) | (0.033) | (0.035) |
| Free/reduced price lunch | -0.024** | -0.068*** | -0.040*** | -0.044*** | -0.040*** |
| | (0.010) | (0.013) | (0.012) | (0.012) | (0.013) |
| R-squared | 0.447 | 0.176 | 0.103 | 0.091 | 0.099 |
| Observations | | | 14,835 | | |

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | | | New | Pass | Vision |
| | Follow-up | Eye exam | glasses | screening | improved |
| Panel C. Regression using 20/50 to 20/80, trends above and below cutoff | | | | | |
| Above Cutoff | 0.812*** | 0.175*** | 0.033 | 0.144*** | 0.181*** |
| | (0.021) | (0.026) | (0.027) | (0.029) | (0.031) |
| Vision Score X Above Cutoff | 0.006 | 0.016 | -0.029 | 0.091*** | -0.015 |
| | (0.015) | (0.029) | (0.030) | (0.029) | (0.030) |
| Vision Score | 0.004 | -0.008 | 0.061*** | -0.124*** | 0.020 |
| | (0.010) | (0.011) | (0.012) | (0.013) | (0.014) |
| Black | -0.021*** | -0.061*** | -0.031*** | -0.009 | 0.005 |
| | (0.008) | (0.012) | (0.011) | (0.013) | (0.014) |
| White | 0.023 | 0.032* | -0.043** | 0.046** | 0.056*** |
| | (0.015) | (0.019) | (0.018) | (0.019) | (0.020) |
| Asian | 0.028** | 0.114*** | 0.004 | 0.037** | 0.060*** |
| | (0.013) | (0.016) | (0.015) | (0.016) | (0.015) |
| Other | 0.043* | -0.016 | -0.041 | 0.015 | 0.009 |
| | (0.025) | (0.032) | (0.031) | (0.034) | (0.035) |
| Free/reduced price lunch | -0.024** | -0.068*** | -0.040*** | -0.044*** | -0.040*** |
| | (0.010) | (0.013) | (0.012) | (0.013) | (0.013) |
| R-squared | 0.447 | 0.176 | 0.103 | 0.092 | 0.099 |
| Observations | | | 14,835 | | |
| Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff | | | | | |
| Above Cutoff | 0.794*** | 0.180*** | 0.005 | 0.206*** | -0.406*** |
| | (0.011) | (0.016) | (0.016) | (0.017) | (0.018) |
| Vision Score X Above Cutoff | -0.012** | 0.007 | -0.062*** | 0.155*** | -0.334*** |
| | (0.005) | (0.010) | (0.010) | (0.010) | (0.010) |
| Vision Score | 0.013*** | -0.011** | 0.081*** | -0.165*** | 0.346*** |
| | (0.003) | (0.004) | (0.004) | (0.005) | (0.005) |
| Black | -0.017*** | -0.064*** | -0.027*** | -0.003 | -0.000 |
| | (0.005) | (0.007) | (0.006) | (0.008) | (0.005) |
| White | 0.022** | 0.026** | -0.042*** | 0.043*** | 0.013 |
| | (0.009) | (0.011) | (0.009) | (0.012) | (0.008) |
| Asian | 0.041*** | 0.122*** | 0.002 | 0.033*** | 0.021*** |
| | (0.008) | (0.010) | (0.008) | (0.009) | (0.006) |
| Other | 0.028* | -0.003 | -0.027 | 0.024 | 0.003 |
| | (0.015) | (0.019) | (0.017) | (0.021) | (0.013) |
| Free/reduced price lunch | -0.028*** | -0.060*** | -0.040*** | -0.044*** | -0.018*** |
| | (0.006) | (0.008) | (0.006) | (0.008) | (0.005) |
| R-squared | 0.363 | 0.141 | 0.085 | 0.092 | 0.363 |
| Observations | | | 37,218 | | |

Notes: Each reported estimate is from a regression of the outcome indicated in the column header on the treatment variable "Above Cutoff," the listed controls for student characteristics, and school fixed effects. The omitted race category is Hispanic and omitted FRPL category is full-price lunch. The specification in Panels A-C run these regressions only on students whose initial visual acuity scores were near to this cutoff, while Panel D includes students with better vision in order to better control for the "pre" trend by visual acuity. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table C.2: Intent-to-Treat Estimates of the Effect of Follow-up on BMI and Nurse Visits, with Controls

| | (1) BMI decreased | (2) Newly obese | (3) No longer obese | (4) Nurse visits decreased | (5) Vision nurse visits decreased |
|---|---|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trend | | | | | |
| Above Cutoff | 0.008 | -0.000 | 0.003 | -0.006 | 0.000 |
| | (0.015) | (0.007) | (0.006) | (0.005) | (0.003) |
| Black | 0.004 | -0.016* | -0.008 | -0.012** | -0.002 |
| | (0.016) | (0.008) | (0.007) | (0.006) | (0.002) |
| White | -0.016 | -0.002 | -0.009 | -0.002 | 0.008 |
| | (0.022) | (0.009) | (0.008) | (0.009) | (0.005) |
| Asian | 0.015 | -0.005 | -0.005 | -0.008 | 0.002 |
| | (0.015) | (0.008) | (0.007) | (0.008) | (0.003) |
| Other | 0.037 | 0.003 | -0.009 | -0.006 | -0.005 |
| | (0.041) | (0.020) | (0.016) | (0.017) | (0.005) |
| Free/reduced price lunch | -0.028* | 0.013** | 0.010* | -0.007 | -0.000 |
| | (0.015) | (0.007) | (0.005) | (0.006) | (0.003) |
| R-Squared | 0.085 | 0.069 | 0.082 | 0.090 | 0.083 |
| Observations | | | 12,435 | | |
| Panel B. Regression using 20/50 to 20/80, single trend | | | | | |
| Above Cutoff | 0.035 | -0.018 | 0.007 | -0.001 | -0.004 |
| | (0.032) | (0.016) | (0.014) | (0.013) | (0.006) |
| Vision Score | -0.013 | 0.008 | -0.002 | -0.002 | 0.002 |
| | (0.014) | (0.007) | (0.006) | (0.006) | (0.003) |
| Black | 0.004 | -0.016* | -0.008 | -0.012** | -0.002 |
| | (0.016) | (0.008) | (0.007) | (0.006) | (0.002) |
| White | -0.016 | -0.002 | -0.009 | -0.002 | 0.008 |
| | (0.022) | (0.009) | (0.008) | (0.009) | (0.005) |
| Asian | 0.015 | -0.005 | -0.005 | -0.008 | 0.002 |
| | (0.015) | (0.008) | (0.007) | (0.008) | (0.003) |
| Other | 0.037 | 0.003 | -0.009 | -0.006 | -0.005 |
| | (0.041) | (0.020) | (0.016) | (0.017) | (0.005) |
| Free/reduced price lunch | -0.028* | 0.013** | 0.010* | -0.007 | -0.000 |
| | (0.015) | (0.007) | (0.005) | (0.006) | (0.003) |
| R-squared | 0.085 | 0.069 | 0.082 | 0.090 | 0.083 |
| Observations | | | 12,435 | | |

| | (1) BMI decreased | (2) Newly obese | (3) No longer obese | (4) Nurse visits decreased | (5) Vision nurse visits decreased |
|---|---|---|---|---|---|
| Panel C. Regression using 20/50 to 20/80, trends above and below cutoff | | | | | |
| Above Cutoff | 0.034 | -0.014 | 0.005 | -0.001 | -0.003 |
| | (0.032) | (0.016) | (0.014) | (0.013) | (0.006) |
| Vision Score X Above Cutoff | -0.009 | 0.027 | -0.013 | -0.008 | 0.006 |
| | (0.035) | (0.021) | (0.016) | (0.012) | (0.006) |
| Vision Score | -0.011 | 0.003 | 0.001 | -0.001 | 0.001 |
| | (0.015) | (0.007) | (0.007) | (0.006) | (0.003) |
| Black | 0.004 | -0.016* | -0.008 | -0.012** | -0.002 |
| | (0.016) | (0.008) | (0.007) | (0.006) | (0.002) |
| White | -0.016 | -0.002 | -0.009 | -0.002 | 0.008 |
| | (0.022) | (0.009) | (0.008) | (0.009) | (0.005) |
| Asian | 0.015 | -0.005 | -0.005 | -0.008 | 0.002 |
| | (0.015) | (0.008) | (0.007) | (0.008) | (0.003) |
| Other | 0.037 | 0.003 | -0.009 | -0.006 | -0.005 |
| | (0.041) | (0.020) | (0.016) | (0.017) | (0.005) |
| Free/reduced price lunch | -0.028* | 0.013** | 0.010* | -0.007 | -0.000 |
| | (0.015) | (0.007) | (0.005) | (0.006) | (0.003) |
| R-squared | 0.085 | 0.070 | 0.082 | 0.090 | 0.083 |
| Observations | | | 12,435 | | |
| Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff | | | | | |
| Above Cutoff | 0.008 | -0.012 | -0.002 | -0.012 | 0.000 |
| | (0.020) | (0.009) | (0.008) | (0.008) | (0.004) |
| Vision Score X Above Cutoff | -0.002 | 0.005 | -0.004 | 0.003 | 0.001 |
| | (0.011) | (0.005) | (0.005) | (0.004) | (0.002) |
| Vision Score | 0.003 | 0.003 | 0.003 | 0.001 | -0.000 |
| | (0.005) | (0.002) | (0.002) | (0.002) | (0.001) |
| Black | -0.001 | -0.007 | -0.008** | -0.005 | -0.000 |
| | (0.010) | (0.005) | (0.004) | (0.004) | (0.002) |
| White | -0.000 | -0.005 | -0.004 | 0.007 | 0.005* |
| | (0.012) | (0.005) | (0.005) | (0.005) | (0.003) |
| Asian | 0.029*** | -0.009* | -0.002 | -0.009* | -0.001 |
| | (0.010) | (0.005) | (0.004) | (0.005) | (0.002) |
| Other | 0.016 | -0.013 | -0.003 | -0.002 | -0.009** |
| | (0.023) | (0.009) | (0.009) | (0.010) | (0.004) |
| Free/reduced price lunch | -0.029*** | 0.014*** | 0.006* | -0.005 | -0.003 |
| | (0.008) | (0.003) | (0.003) | (0.004) | (0.002) |
| R-squared | 0.048 | 0.032 | 0.037 | 0.054 | 0.039 |
| Observations | | | 31,280 | | |

Notes: Each reported estimate is from a regression of the outcome indicated in the column header on the treatment variable "Above Cutoff," the listed controls for student characteristics, and school fixed effects. The omitted race category is Hispanic and omitted FRPL category is full-price lunch. The specification in Panels A-C run these regressions only on students whose initial visual acuity scores were near to this cutoff, while Panel D includes students with better vision in order to better control for the "pre" trend by visual acuity. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table C.3: Intent-to-Treat Estimates of the Effect of Follow-up on Absence, with Controls

| | (1) Absence rate decreased | (2) Newly chronically absent | (3) No longer chronically absent |
|---|---|---|---|
| **Panel A. Regression using 20/50 to 2080, no trend** | | | |
| Above Cutoff | -0.027** | -0.010* | 0.000 |
| | (0.013) | (0.006) | (0.008) |
| Black | -0.013 | -0.005 | -0.005 |
| | (0.014) | (0.007) | (0.010) |
| White | 0.022 | 0.006 | 0.002 |
| | (0.019) | (0.009) | (0.013) |
| Asian | 0.038** | -0.013** | 0.012 |
| | (0.016) | (0.006) | (0.010) |
| Other | -0.017 | -0.016 | -0.021 |
| | (0.038) | (0.015) | (0.021) |
| Free/reduced price lunch | 0.009 | 0.004 | 0.014* |
| | (0.013) | (0.007) | (0.008) |
| R-Squared | 0.073 | 0.068 | 0.075 |
| Observations | | 13,923 | |
| **Panel B. Regression using 20/50 to 20/80, single trend** | | | |
| Above Cutoff | -0.054* | 0.013 | -0.010 |
| | (0.032) | (0.013) | (0.020) |
| Vision Score | 0.013 | -0.011* | 0.005 |
| | (0.014) | (0.006) | (0.008) |
| Black | -0.013 | -0.005 | -0.005 |
| | (0.014) | (0.007) | (0.010) |
| White | 0.022 | 0.006 | 0.002 |
| | (0.019) | (0.009) | (0.013) |
| Asian | 0.038** | -0.013** | 0.012 |
| | (0.016) | (0.006) | (0.010) |
| Other | -0.017 | -0.016 | -0.020 |
| | (0.038) | (0.015) | (0.021) |
| Free/reduced price lunch | 0.009 | 0.004 | 0.014* |
| | (0.013) | (0.007) | (0.008) |
| R-squared | 0.073 | 0.068 | 0.075 |
| Observations | | 13,923 | |

| | (1) | (2) | (3) |
|---|---|---|---|
| | | Newly | No longer |
| | Absence rate | chronically | chronically |
| | decreased | absent | absent |
| **Panel C. Regression using 20/50 to 20/80, trends above and below cutoff** | | | |
| Above Cutoff | -0.056* | 0.014 | -0.008 |
| | (0.032) | (0.013) | (0.020) |
| Vision Score X Above Cutoff | -0.009 | 0.004 | 0.016 |
| | (0.031) | (0.014) | (0.020) |
| Vision Score | 0.014 | -0.012* | 0.002 |
| | (0.015) | (0.007) | (0.009) |
| Black | -0.013 | -0.005 | -0.005 |
| | (0.014) | (0.007) | (0.010) |
| White | 0.022 | 0.006 | 0.002 |
| | (0.019) | (0.009) | (0.013) |
| Asian | 0.038** | -0.013** | 0.012 |
| | (0.016) | (0.006) | (0.010) |
| Other | -0.017 | -0.016 | -0.020 |
| | (0.038) | (0.015) | (0.021) |
| Free/reduced price lunch | 0.009 | 0.004 | 0.014* |
| | (0.013) | (0.007) | (0.008) |
| R-squared | 0.073 | 0.068 | 0.075 |
| Observations | | 13,923 | |
| **Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff** | | | |
| Above Cutoff | -0.039** | -0.006 | -0.012 |
| | (0.019) | (0.008) | (0.011) |
| Vision Score X Above Cutoff | -0.001 | 0.000 | -0.002 |
| | (0.010) | (0.004) | (0.006) |
| Vision Score | 0.007 | -0.000 | 0.005* |
| | (0.005) | (0.002) | (0.003) |
| Black | -0.016* | 0.003 | -0.006 |
| | (0.009) | (0.004) | (0.006) |
| White | 0.016 | 0.004 | 0.004 |
| | (0.012) | (0.005) | (0.007) |
| Asian | 0.038*** | -0.007* | -0.001 |
| | (0.010) | (0.004) | (0.006) |
| Other | 0.004 | -0.003 | 0.000 |
| | (0.019) | (0.009) | (0.013) |
| Free/reduced price lunch | 0.016** | 0.014*** | 0.019*** |
| | (0.008) | (0.004) | (0.005) |
| R-squared | 0.037 | 0.035 | 0.037 |
| Observations | | 35,071 | |

Notes: Each reported estimate is from a regression of the outcome indicated in the column header on the treatment variable "Above Cutoff," the listed controls for student characteristics, and school fixed effects. The omitted race category is Hispanic and omitted FRPL category is full-price lunch. The specification in Panels A-C run these regressions only on students whose initial visual acuity scores were near to this cutoff, while Panel D includes students with better vision in order to better control for the "pre" trend by visual acuity. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table C.4: Intent-to-Treat Estimates of the Effect of Follow-up on Academic Outcomes, with Controls

| | (1)<br>Repeated<br>a grade | (2)<br>ELA<br>proficiency | (3)<br>Math<br>proficiency |
|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trend | | | |
| Above Cutoff | 0.010 | 0.004 | -0.002 |
| | (0.008) | (0.012) | (0.012) |
| Black | 0.012 | 0.022 | -0.002 |
| | (0.009) | (0.013) | (0.014) |
| White | -0.029*** | 0.072*** | 0.134*** |
| | (0.009) | (0.017) | (0.017) |
| Asian | -0.039*** | 0.195*** | 0.226*** |
| | (0.008) | (0.014) | (0.015) |
| Other | 0.016 | 0.094*** | 0.095*** |
| | (0.022) | (0.031) | (0.033) |
| Free/reduced price lunch | 0.035*** | -0.100*** | -0.092*** |
| | (0.006) | (0.012) | (0.012) |
| R-Squared | 0.118 | 0.137 | 0.157 |
| Observations | | 14,835 | |
| Panel B. Regression using 20/50 to 20/80, single trend | | | |
| Above Cutoff | -0.001 | 0.004 | -0.002 |
| | (0.018) | (0.029) | (0.029) |
| Vision Score | 0.005 | 0.000 | 0.000 |
| | (0.007) | (0.012) | (0.012) |
| Black | 0.012 | 0.022 | -0.002 |
| | (0.009) | (0.013) | (0.014) |
| White | -0.029*** | 0.072*** | 0.134*** |
| | (0.009) | (0.017) | (0.017) |
| Asian | -0.039*** | 0.195*** | 0.226*** |
| | (0.008) | (0.014) | (0.015) |
| Other | 0.016 | 0.094*** | 0.095*** |
| | (0.022) | (0.031) | (0.033) |
| Free/reduced price lunch | 0.035*** | -0.100*** | -0.092*** |
| | (0.006) | (0.012) | (0.012) |
| R-squared | 0.118 | 0.157 | 0.137 |
| Observations | | 14,835 | |

| | (1) Repeated a grade | (2) ELA proficiency | (3) Math proficiency |
|---|---|---|---|
| Panel C. Regression using 20/50 to 20/80, trends above and below cutoff | | | |
| Above Cutoff | -0.003 | 0.012 | 0.000 |
| | (0.018) | (0.029) | (0.029) |
| Vision Score X Above Cutoff | -0.013 | 0.067** | 0.021 |
| | (0.019) | (0.026) | (0.028) |
| Vision Score | 0.008 | -0.012 | -0.004 |
| | (0.008) | (0.013) | (0.013) |
| Black | 0.012 | 0.021 | -0.002 |
| | (0.009) | (0.013) | (0.014) |
| White | -0.029*** | 0.072*** | 0.134*** |
| | (0.009) | (0.017) | (0.017) |
| Asian | -0.039*** | 0.195*** | 0.226*** |
| | (0.008) | (0.014) | (0.015) |
| Other | 0.016 | 0.094*** | 0.095*** |
| | (0.022) | (0.031) | (0.033) |
| Free/reduced price lunch | 0.035*** | -0.101*** | -0.092*** |
| | (0.006) | (0.012) | (0.012) |
| R-squared | 0.118 | 0.138 | 0.157 |
| Observations | | 14,835 | |
| Panel D. Regression using 20/40 to 20/100+, trends above and below cutoff | | | |
| Above Cutoff | -0.001 | 0.027 | 0.026 |
| | (0.011) | (0.017) | (0.016) |
| Vision Score X Above Cutoff | 0.004 | 0.007 | 0.002 |
| | (0.006) | (0.010) | (0.009) |
| Vision Score | 0.005* | -0.016*** | -0.017*** |
| | (0.003) | (0.004) | (0.004) |
| Black | 0.009 | 0.006 | -0.019** |
| | (0.005) | (0.008) | (0.008) |
| White | -0.022*** | 0.066*** | 0.103*** |
| | (0.005) | (0.010) | (0.010) |
| Asian | -0.036*** | 0.177*** | 0.215*** |
| | (0.005) | (0.009) | (0.010) |
| Other | -0.004 | 0.031 | 0.059*** |
| | (0.011) | (0.019) | (0.019) |
| Free/reduced price lunch | 0.040*** | -0.102*** | -0.094*** |
| | (0.003) | (0.007) | (0.007) |
| R-squared | 0.085 | 0.111 | 0.134 |
| Observations | | 37,218 | |

Notes: Each reported estimate is from a regression of the outcome indicated in the column header on the treatment variable "Above Cutoff," the listed controls for student characteristics, and school fixed effects. The omitted race category is Hispanic and omitted FRPL category is full-price lunch. The specification in Panels A-C run these regressions only on students whose initial visual acuity scores were near to this cutoff, while Panel D includes students with better vision in order to better control for the "pre" trend by visual acuity. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table C.5: Intent-to-Treat Estimates of the Effect of Follow-up on Vision Outcomes, by Free or Reduced-Price Lunch Status (SES Proxy) and by Race/Ethnicity

| | (1) Follow-up | (2) Eye exam | (3) New glasses | (4) Pass screening | (5) Vision improved |
|---|---|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trend | | | | | |
| Full-price lunch: | | | | | |
| Above cutoff | 0.758*** | 0.169*** | 0.208*** | -0.046 | 0.213*** |
| | (0.014) | (0.033) | (0.030) | (0.030) | (0.028) |
| Free/reduced-price lunch: | | | | | |
| Above cutoff | 0.834*** | 0.171*** | 0.157*** | -0.112*** | 0.210*** |
| | (0.007) | (0.013) | (0.013) | (0.012) | (0.013) |
| Hispanic: Above cutoff | 0.827*** | 0.192*** | 0.178*** | -0.091*** | 0.215*** |
| | (0.008) | (0.016) | (0.015) | (0.015) | (0.016) |
| Black: Above cutoff | 0.884*** | 0.154*** | 0.099*** | -0.132*** | 0.210*** |
| | (0.010) | (0.024) | (0.024) | (0.025) | (0.027) |
| White: Above cutoff | 0.750*** | 0.118** | 0.234*** | -0.081* | 0.205*** |
| | (0.020) | (0.048) | (0.048) | (0.047) | (0.043) |
| Asian: Above cutoff | 0.734*** | 0.109*** | 0.166*** | -0.094*** | 0.221*** |
| | (0.017) | (0.033) | (0.038) | (0.034) | (0.032) |
| Panel B. Regression using 20/50 to 20/80, single trend | | | | | |
| Full-price lunch: Above cutoff | 0.821*** | 0.252*** | 0.095 | 0.187*** | 0.181*** |
| | (0.041) | (0.071) | (0.066) | (0.064) | (0.064) |
| Full-price lunch: Vision Score | -0.030 | -0.039 | 0.053* | -0.110*** | 0.015 |
| | (0.018) | (0.029) | (0.027) | (0.028) | (0.028) |
| Free/reduced-price lunch: | | | | | |
| Above cutoff | 0.817*** | 0.226*** | 0.081*** | 0.078*** | 0.145*** |
| | (0.020) | (0.029) | (0.028) | (0.028) | (0.029) |
| Free/reduced-price lunch: | | | | | |
| Vision score | 0.008 | -0.026** | 0.036*** | -0.090*** | 0.031** |
| | (0.009) | (0.012) | (0.011) | (0.012) | (0.012) |
| Hispanic: Above cutoff | 0.801*** | 0.238*** | 0.103*** | 0.074** | 0.116*** |
| | (0.024) | (0.036) | (0.034) | (0.035) | (0.037) |
| Hispanic: Vision score | 0.013 | -0.022 | 0.036** | -0.079*** | 0.047*** |
| | (0.011) | (0.015) | (0.015) | (0.015) | (0.016) |
| Black: Above cutoff | 0.853*** | 0.098** | -0.059 | 0.109** | 0.193*** |
| | (0.030) | (0.046) | (0.049) | (0.050) | (0.055) |
| Black: Vision score | 0.014 | 0.025 | 0.071*** | -0.109*** | 0.008 |
| | (0.012) | (0.017) | (0.019) | (0.021) | (0.022) |
| White: Above cutoff | 0.806*** | 0.139 | 0.206** | 0.148 | 0.236*** |
| | (0.067) | (0.103) | (0.089) | (0.091) | (0.091) |
| White: Vision score | -0.026 | -0.010 | 0.013 | -0.106*** | -0.014 |
| | (0.030) | (0.043) | (0.036) | (0.039) | (0.038) |
| Asian: Above cutoff | 0.763*** | 0.215*** | 0.057 | 0.126 | 0.149* |
| | (0.057) | (0.075) | (0.084) | (0.077) | (0.086) |
| Asian: Vision score | -0.014 | -0.050 | 0.052 | -0.104*** | 0.034 |
| | (0.026) | (0.033) | (0.036) | (0.031) | (0.035) |

| | (1) Follow-up | (2) Eye exam | (3) New glasses | (4) Pass screening | (5) Vision improved |
|---|---|---|---|---|---|
| Panel C. Regression using 20/50 to 20/80, trends above and below the cutoff | | | | | |
| Full-price lunch: Above cutoff | 0.825*** | 0.254*** | 0.081 | 0.197*** | 0.168** |
| | (0.044) | (0.070) | (0.066) | (0.064) | (0.065) |
| Full-price lunch: Vision score X Above cutoff | 0.034 | 0.018 | -0.107 | 0.077 | -0.101 |
| | (0.035) | (0.071) | (0.071) | (0.068) | (0.069) |
| Full-price lunch: Vision score | -0.036 | -0.042 | 0.073** | -0.124*** | 0.034 |
| | (0.022) | (0.030) | (0.029) | (0.030) | (0.032) |
| Free/reduced-price lunch: Above cutoff | 0.817*** | 0.228*** | 0.075*** | 0.091*** | 0.147*** |
| | (0.021) | (0.028) | (0.028) | (0.028) | (0.030) |
| Free/reduced-price lunch: Vision score X Above cutoff | -0.002 | 0.018 | -0.040 | 0.103*** | 0.014 |
| | (0.013) | (0.031) | (0.032) | (0.030) | (0.031) |
| Free/reduced-price lunch: Vision score | 0.008 | -0.029** | 0.043*** | -0.108*** | 0.028** |
| | (0.010) | (0.012) | (0.012) | (0.013) | (0.014) |
| Hispanic: Above cutoff | 0.798*** | 0.228*** | 0.087** | 0.083** | 0.105*** |
| | (0.026) | (0.036) | (0.034) | (0.035) | (0.038) |
| Hispanic: Vision score X Above cutoff | -0.015 | -0.057 | -0.094** | 0.051 | -0.066* |
| | (0.018) | (0.039) | (0.040) | (0.038) | (0.039) |
| Hispanic: Vision score | 0.016 | -0.012 | 0.053*** | -0.089*** | 0.060*** |
| | (0.013) | (0.016) | (0.016) | (0.017) | (0.018) |
| Black: Above cutoff | 0.853*** | 0.095** | -0.060 | 0.104** | 0.191*** |
| | (0.030) | (0.045) | (0.049) | (0.050) | (0.055) |
| Black: Vision score X Above cutoff | 0.004 | 0.130** | 0.067 | 0.175*** | 0.058 |
| | (0.022) | (0.053) | (0.052) | (0.053) | (0.056) |
| Black: Vision score | 0.013 | 0.004 | 0.060*** | -0.137*** | -0.002 |
| | (0.014) | (0.018) | (0.021) | (0.022) | (0.024) |
| White: Above cutoff | 0.808*** | 0.160 | 0.199** | 0.161* | 0.247*** |
| | (0.070) | (0.102) | (0.087) | (0.090) | (0.090) |
| White: Vision score X Above cutoff | 0.026 | 0.233** | -0.077 | 0.144 | 0.124 |
| | (0.047) | (0.097) | (0.099) | (0.106) | (0.099) |
| White: Vision score | -0.030 | -0.051 | 0.027 | -0.132*** | -0.036 |
| | (0.035) | (0.046) | (0.036) | (0.041) | (0.042) |
| Asian: Above cutoff | 0.767*** | 0.230*** | 0.059 | 0.160** | 0.160* |
| | (0.061) | (0.076) | (0.088) | (0.080) | (0.091) |
| Asian: Vision score X Above cutoff | 0.022 | 0.088 | 0.015 | 0.186** | 0.061 |
| | (0.043) | (0.086) | (0.090) | (0.087) | (0.083) |
| Asian: Vision score | -0.018 | -0.067* | 0.049 | -0.140*** | 0.022 |
| | (0.031) | (0.036) | (0.041) | (0.037) | (0.042) |

| | (1) Follow-up | (2) Eye exam | (3) New glasses | (4) Pass screening | (5) Vision improved |
|---|---|---|---|---|---|
| Panel D. Regression using 20/40 to 20/100+, trends above and below the cutoff | | | | | |
| Full-price lunch: Above cutoff | 0.762*** | 0.217*** | 0.048 | 0.282*** | -0.466*** |
| | (0.027) | (0.046) | (0.041) | (0.042) | (0.039) |
| Full-price lunch: Vision score X Above cutoff | 0.003 | 0.011 | -0.087*** | 0.153*** | -0.378*** |
| | (0.012) | (0.023) | (0.022) | (0.023) | (0.020) |
| Full-price lunch: Vision score | -0.002 | -0.026** | 0.091*** | -0.176*** | 0.390*** |
| | (0.009) | (0.011) | (0.009) | (0.011) | (0.010) |
| Free/reduced-price lunch: Above cutoff | 0.809*** | 0.204*** | 0.014 | 0.181*** | -0.393*** |
| | (0.012) | (0.019) | (0.018) | (0.018) | (0.019) |
| Free/reduced-price lunch: Vision score X Above cutoff | -0.014*** | 0.006 | -0.064*** | 0.157*** | -0.325*** |
| | (0.005) | (0.011) | (0.010) | (0.010) | (0.010) |
| Free/reduced-price lunch: Vision score | 0.014*** | -0.016*** | 0.078*** | -0.162*** | 0.336*** |
| | (0.004) | (0.005) | (0.004) | (0.005) | (0.005) |
| Hispanic: Above cutoff | 0.793*** | 0.220*** | 0.033 | 0.209*** | -0.387*** |
| | (0.014) | (0.024) | (0.021) | (0.023) | (0.023) |
| Hispanic: Vision score X Above cutoff | -0.017*** | -0.009 | -0.075*** | 0.142*** | -0.323*** |
| | (0.006) | (0.013) | (0.013) | (0.012) | (0.012) |
| Hispanic: Vision score | 0.018*** | -0.010 | 0.082*** | -0.162*** | 0.336*** |
| | (0.004) | (0.006) | (0.005) | (0.007) | (0.006) |
| Black: Above cutoff | 0.862*** | 0.145*** | -0.063* | 0.148*** | -0.358*** |
| | (0.018) | (0.032) | (0.033) | (0.036) | (0.038) |
| Black: Vision score X Above cutoff | -0.007 | 0.015 | -0.010 | 0.167*** | -0.316*** |
| | (0.009) | (0.019) | (0.019) | (0.019) | (0.020) |
| Black: Vision score | 0.011** | -0.009 | 0.069*** | -0.164*** | 0.322*** |
| | (0.005) | (0.007) | (0.007) | (0.009) | (0.008) |
| White: Above cutoff | 0.729*** | 0.128* | 0.120** | 0.236*** | -0.487*** |
| | (0.039) | (0.068) | (0.060) | (0.060) | (0.062) |
| White: Vision score X Above cutoff | -0.012 | 0.037 | -0.146*** | 0.169*** | -0.412*** |
| | (0.015) | (0.032) | (0.031) | (0.031) | (0.030) |
| White: Vision score | 0.012 | -0.015 | 0.078*** | -0.167*** | 0.398*** |
| | (0.013) | (0.017) | (0.012) | (0.014) | (0.015) |
| Asian: Above cutoff | 0.734*** | 0.145*** | -0.020 | 0.195*** | -0.511*** |
| | (0.034) | (0.044) | (0.050) | (0.048) | (0.051) |
| Asian: Vision score X Above cutoff | -0.011 | 0.044* | -0.055** | 0.176*** | -0.375*** |
| | (0.016) | (0.027) | (0.025) | (0.029) | (0.024) |
| Asian: Vision score | 0.004 | -0.022* | 0.098*** | -0.161*** | 0.399*** |
| | (0.012) | (0.012) | (0.011) | (0.013) | (0.013) |

Notes: Regression results in each row are fully interacted with the student characteristic listed in the first column by limiting the sample to students with each FRPL-status or self-reported race/ethnicity. Results for students with "other" race/ethnicity are not reported. Each reported estimate is from a regression of the outcome indicated in the column header on the treatment variable "Above Cutoff," which is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. The specification in Panels A-C run these regressions only on students whose initial visual acuity scores were near to this cutoff, while Panel D includes students with better vision in order to better control for the "pre" trend by visual acuity. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table C.6: Intent-to-Treat Estimates of the Effect of Follow-up on BMI and Nurse Visits, by Free or Reduced-Price Lunch Status (SES Proxy) and by Race/Ethnicity

| | (1) BMI decreased | (2) Newly obese | (3) No longer obese | (4) Nurse visits decreased | (5) Vision nurse visits decreased |
|---|---|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trend | | | | | |
| Full-price lunch: | | | | | |
| Above cutoff | -0.054 | 0.011 | 0.010 | -0.029** | -0.006 |
| | (0.035) | (0.015) | (0.013) | (0.012) | (0.006) |
| Free/reduced-price lunch: | | | | | |
| Above cutoff | 0.020 | -0.002 | 0.002 | -0.003 | 0.001 |
| | (0.015) | (0.007) | (0.006) | (0.006) | (0.003) |
| Hispanic: Above cutoff | 0.013 | 0.003 | -0.003 | -0.013* | -0.001 |
| | (0.019) | (0.008) | (0.007) | (0.007) | (0.003) |
| Black: Above cutoff | 0.026 | -0.008 | -0.015 | -0.006 | 0.001 |
| | (0.034) | (0.015) | (0.012) | (0.010) | (0.005) |
| White: Above cutoff | -0.019 | -0.007 | 0.056** | 0.011 | 0.014 |
| | (0.048) | (0.021) | (0.029) | (0.027) | (0.016) |
| Asian: Above cutoff | -0.009 | 0.002 | 0.019 | 0.003 | -0.004 |
| | (0.035) | (0.016) | (0.016) | (0.015) | (0.007) |
| Panel B. Regression using 20/50 to 20/80, single trend | | | | | |
| Full-price lunch: Above cutoff | -0.010 | 0.018 | 0.016 | 0.014 | -0.004 |
| | (0.076) | (0.030) | (0.029) | (0.030) | (0.009) |
| Full-price lunch: Vision Score | -0.021 | -0.003 | -0.003 | -0.020 | -0.001 |
| | (0.034) | (0.013) | (0.012) | (0.012) | (0.005) |
| Free/reduced-price lunch: | | | | | |
| Above cutoff | 0.043 | -0.024 | 0.001 | 0.007 | -0.001 |
| | (0.034) | (0.016) | (0.015) | (0.014) | (0.006) |
| Free/reduced-price lunch: | | | | | |
| Vision score | -0.011 | 0.010 | 0.000 | -0.005 | 0.001 |
| | (0.015) | (0.007) | (0.007) | (0.006) | (0.003) |
| Hispanic: Above cutoff | 0.045 | -0.029 | 0.002 | 0.012 | 0.005 |
| | (0.039) | (0.020) | (0.017) | (0.016) | (0.007) |
| Hispanic: Vision score | -0.015 | 0.015* | -0.002 | -0.012* | -0.003 |
| | (0.017) | (0.009) | (0.008) | (0.007) | (0.003) |
| Black: Above cutoff | 0.009 | -0.012 | -0.065** | 0.005 | -0.003 |
| | (0.072) | (0.032) | (0.032) | (0.018) | (0.007) |
| Black: Vision score | 0.008 | 0.002 | 0.023 | -0.005 | 0.002 |
| | (0.029) | (0.013) | (0.014) | (0.008) | (0.004) |
| White: Above cutoff | 0.049 | -0.008 | 0.093** | 0.004 | -0.006 |
| | (0.107) | (0.039) | (0.047) | (0.064) | (0.033) |
| White: Vision score | -0.031 | 0.000 | -0.017 | 0.003 | 0.009 |
| | (0.044) | (0.018) | (0.016) | (0.024) | (0.013) |
| Asian: Above cutoff | -0.002 | 0.026 | 0.053* | -0.038 | -0.026 |
| | (0.079) | (0.033) | (0.029) | (0.045) | (0.023) |
| Asian: Vision score | -0.003 | -0.012 | -0.017 | 0.019 | 0.011 |
| | (0.036) | (0.015) | (0.012) | (0.018) | (0.010) |

| | (1) BMI decreased | (2) Newly obese | (3) No longer obese | (4) Nurse visits decreased | (5) Vision nurse visits decreased |
|---|---|---|---|---|---|
| Panel C. Regression using 20/50 to 20/80, trends above and below the cutoff | | | | | |
| Full-price lunch: Above cutoff | -0.011 | 0.018 | 0.014 | 0.015 | 0.001 |
| | (0.076) | (0.030) | (0.029) | (0.031) | (0.010) |
| Full-price lunch: Vision score X Above cutoff | -0.003 | -0.004 | -0.019 | 0.006 | 0.033* |
| | (0.079) | (0.036) | (0.031) | (0.026) | (0.019) |
| Full-price lunch: Vision score | -0.020 | -0.003 | 0.000 | -0.021 | -0.007 |
| | (0.036) | (0.014) | (0.013) | (0.014) | (0.005) |
| Free/reduced-price lunch: Above cutoff | 0.044 | -0.019 | -0.002 | 0.005 | -0.002 |
| | (0.034) | (0.017) | (0.015) | (0.014) | (0.007) |
| Free/reduced-price lunch: Vision score X Above cutoff | 0.009 | 0.032 | -0.017 | -0.009 | -0.002 |
| | (0.037) | (0.022) | (0.017) | (0.013) | (0.007) |
| Free/reduced-price lunch: Vision score | -0.013 | 0.005 | 0.003 | -0.003 | 0.001 |
| | (0.016) | (0.008) | (0.008) | (0.006) | (0.003) |
| Hispanic: Above cutoff | 0.046 | -0.022 | -0.002 | 0.012 | 0.005 |
| | (0.039) | (0.020) | (0.019) | (0.016) | (0.007) |
| Hispanic: Vision score X Above cutoff | 0.008 | 0.038 | -0.019 | -0.001 | 0.004 |
| | (0.046) | (0.028) | (0.019) | (0.016) | (0.007) |
| Hispanic: Vision score | -0.017 | 0.008 | 0.001 | -0.012 | -0.004 |
| | (0.018) | (0.010) | (0.009) | (0.008) | (0.003) |
| Black: Above cutoff | 0.008 | -0.012 | -0.066** | 0.004 | -0.004 |
| | (0.072) | (0.032) | (0.033) | (0.018) | (0.007) |
| Black: Vision score X Above cutoff | -0.011 | -0.004 | -0.016 | 0.030 | 0.023* |
| | (0.074) | (0.034) | (0.030) | (0.021) | (0.013) |
| Black: Vision score | 0.010 | 0.002 | 0.026 | -0.010 | -0.002 |
| | (0.033) | (0.015) | (0.016) | (0.008) | (0.004) |
| White: Above cutoff | 0.046 | -0.000 | 0.091** | -0.004 | -0.008 |
| | (0.108) | (0.040) | (0.045) | (0.064) | (0.033) |
| White: Vision score X Above cutoff | -0.035 | 0.085 | -0.025 | -0.094* | -0.019 |
| | (0.111) | (0.059) | (0.058) | (0.050) | (0.035) |
| White: Vision score | -0.025 | -0.014 | -0.013 | 0.020 | 0.013 |
| | (0.049) | (0.019) | (0.016) | (0.026) | (0.014) |
| Asian: Above cutoff | 0.012 | 0.027 | 0.054* | -0.048 | -0.031 |
| | (0.083) | (0.033) | (0.028) | (0.047) | (0.025) |
| Asian: Vision score X Above cutoff | 0.084 | 0.005 | 0.004 | -0.056 | -0.027* |
| | (0.083) | (0.039) | (0.038) | (0.040) | (0.014) |
| Asian: Vision score | -0.018 | -0.013 | -0.017 | 0.030 | 0.016 |
| | (0.042) | (0.016) | (0.012) | (0.022) | (0.012) |

| | (1) BMI decreased | (2) Newly obese | (3) No longer obese | (4) Nurse visits decreased | (5) Vision nurse visits decreased |
|---|---|---|---|---|---|
| Panel D. Regression using 20/40 to 20/100+, trends above and below the cutoff | | | | | |
| Full-price lunch: Above cutoff | -0.054 | 0.003 | 0.014 | -0.031 | -0.001 |
| | (0.046) | (0.020) | (0.018) | (0.019) | (0.008) |
| Full-price lunch: Vision score X Above cutoff | 0.020 | -0.012 | -0.009 | 0.021* | 0.011** |
| | (0.025) | (0.011) | (0.009) | (0.012) | (0.005) |
| Full-price lunch: Vision score | 0.006 | 0.004 | 0.001 | -0.001 | -0.005* |
| | (0.011) | (0.005) | (0.004) | (0.005) | (0.003) |
| Free/reduced-price lunch: Above cutoff | 0.029 | -0.016 | -0.003 | -0.001 | 0.001 |
| | (0.021) | (0.010) | (0.009) | (0.009) | (0.004) |
| Free/reduced-price lunch: Vision score X Above cutoff | 0.002 | 0.005 | -0.003 | -0.000 | -0.000 |
| | (0.011) | (0.005) | (0.005) | (0.005) | (0.002) |
| Free/reduced-price lunch: Vision score | -0.003 | 0.004 | 0.004 | -0.001 | -0.000 |
| | (0.006) | (0.003) | (0.003) | (0.002) | (0.001) |
| Hispanic: Above cutoff | 0.013 | -0.007 | -0.008 | -0.012 | 0.001 |
| | (0.026) | (0.012) | (0.011) | (0.010) | (0.005) |
| Hispanic: Vision score X Above cutoff | -0.004 | 0.001 | -0.002 | 0.000 | -0.000 |
| | (0.014) | (0.007) | (0.006) | (0.005) | (0.002) |
| Hispanic: Vision score | 0.004 | 0.002 | 0.004 | 0.000 | -0.001 |
| | (0.007) | (0.003) | (0.003) | (0.003) | (0.001) |
| Black: Above cutoff | 0.049 | -0.019 | -0.030* | -0.011 | 0.000 |
| | (0.046) | (0.022) | (0.018) | (0.013) | (0.005) |
| Black: Vision score X Above cutoff | 0.017 | 0.010 | -0.007 | 0.013* | 0.005 |
| | (0.025) | (0.013) | (0.009) | (0.008) | (0.003) |
| Black: Vision score | -0.009 | 0.002 | 0.009* | -0.001 | -0.002 |
| | (0.012) | (0.006) | (0.005) | (0.003) | (0.002) |
| White: Above cutoff | 0.014 | -0.032 | 0.074** | 0.017 | 0.017 |
| | (0.069) | (0.027) | (0.035) | (0.038) | (0.021) |
| White: Vision score X Above cutoff | 0.029 | 0.007 | -0.022 | 0.009 | -0.007 |
| | (0.034) | (0.015) | (0.017) | (0.021) | (0.010) |
| White: Vision score | -0.012 | 0.010 | -0.001 | -0.003 | 0.000 |
| | (0.017) | (0.007) | (0.006) | (0.008) | (0.005) |
| Asian: Above cutoff | -0.018 | -0.004 | 0.029 | -0.002 | -0.007 |
| | (0.047) | (0.021) | (0.020) | (0.025) | (0.011) |
| Asian: Vision score X Above cutoff | 0.009 | 0.004 | 0.008 | -0.014 | 0.005 |
| | (0.026) | (0.013) | (0.013) | (0.012) | (0.007) |
| Asian: Vision score | -0.001 | 0.003 | -0.005 | 0.002 | 0.001 |
| | (0.011) | (0.005) | (0.005) | (0.007) | (0.003) |

Notes: Regression results in each row are fully interacted with the student characteristic listed in the first column by limiting the sample to students with each FRPL-status or self-reported race/ethnicity. Results for students with "other" race/ethnicity are not reported. Each reported estimate is from a regression of the outcome indicated in the column header on the treatment variable "Above Cutoff," which is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. The specification in Panels A-C run these regressions only on students whose initial visual acuity scores were near to this cutoff, while Panel D includes students with better vision in order to better control for the "pre" trend by visual acuity. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table C.7: Intent-to-Treat Estimates of the Effect of Follow-up on Absence, by Free or Reduced-Price Lunch Status (SES Proxy) and by Race/Ethnicity

| | (1) Absence rate decreased | (2) Newly chronically absent | (3) No longer chronically absent |
|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trend | | | |
| Full-price lunch: Above cutoff | 0.011 | -0.020** | -0.004 |
| | (0.031) | (0.010) | (0.018) |
| Free/reduced-price lunch: Above cutoff | -0.026* | -0.009 | 0.001 |
| | (0.014) | (0.006) | (0.009) |
| Hispanic: Above cutoff | -0.013 | -0.014** | -0.002 |
| | (0.017) | (0.007) | (0.010) |
| Black: Above cutoff | -0.021 | -0.022* | -0.020 |
| | (0.030) | (0.013) | (0.017) |
| White: Above cutoff | 0.022 | -0.016 | 0.063* |
| | (0.049) | (0.019) | (0.032) |
| Asian: Above cutoff | -0.054 | 0.018 | 0.010 |
| | (0.034) | (0.014) | (0.022) |
| Panel B. Regression using 20/50 to 20/80, single trend | | | |
| Full-price lunch: Above cutoff | -0.118* | 0.030 | 0.008 |
| | (0.067) | (0.023) | (0.041) |
| Full-price lunch: Vision Score | 0.061** | -0.023** | -0.006 |
| | (0.028) | (0.010) | (0.016) |
| Free/reduced-price lunch: Above cutoff | -0.005 | 0.003 | -0.000 |
| | (0.032) | (0.013) | (0.020) |
| Free/reduced-price lunch: Vision score | -0.010 | -0.006 | 0.001 |
| | (0.014) | (0.006) | (0.009) |
| Hispanic: Above cutoff | -0.020 | -0.007 | 0.023 |
| | (0.038) | (0.017) | (0.022) |
| Hispanic: Vision score | 0.003 | -0.003 | -0.012 |
| | (0.016) | (0.008) | (0.010) |
| Black: Above cutoff | 0.002 | -0.007 | -0.071* |
| | (0.063) | (0.029) | (0.041) |
| Black: Vision score | -0.010 | -0.007 | 0.023 |
| | (0.025) | (0.011) | (0.018) |
| White: Above cutoff | -0.153* | 0.060** | 0.058 |
| | (0.092) | (0.031) | (0.068) |
| White: Vision score | 0.082** | -0.036** | 0.002 |
| | (0.036) | (0.014) | (0.026) |
| Asian: Above cutoff | 0.006 | 0.059** | 0.041 |
| | (0.085) | (0.025) | (0.050) |
| Asian: Vision score | -0.028 | -0.020* | -0.015 |
| | (0.035) | (0.010) | (0.021) |

| | (1) Absence rate decreased | (2) Newly chronically absent | (3) No longer chronically absent |
|---|---|---|---|
| Panel C. Regression using 20/50 to 20/80, trends above and below the cutoff | | | |
| Full-price lunch: Above cutoff | -0.134** | 0.032 | 0.000 |
| | (0.067) | (0.023) | (0.042) |
| Full-price lunch: Vision score X Above cutoff | -0.112 | 0.011 | -0.056 |
| | (0.077) | (0.021) | (0.037) |
| Full-price lunch: Vision score | 0.082*** | -0.026** | 0.005 |
| | (0.031) | (0.011) | (0.019) |
| Free/reduced-price lunch: Above cutoff | -0.003 | 0.003 | 0.002 |
| | (0.033) | (0.014) | (0.020) |
| Free/reduced-price lunch: Vision score X Above cutoff | 0.011 | 0.000 | 0.018 |
| | (0.033) | (0.014) | (0.021) |
| Free/reduced-price lunch: Vision score | -0.012 | -0.006 | -0.003 |
| | (0.015) | (0.007) | (0.009) |
| Hispanic: Above cutoff | -0.018 | -0.009 | 0.026 |
| | (0.039) | (0.018) | (0.023) |
| Hispanic: Vision score X Above cutoff | 0.011 | -0.010 | 0.018 |
| | (0.040) | (0.017) | (0.026) |
| Hispanic: Vision score | 0.001 | -0.002 | -0.015 |
| | (0.018) | (0.009) | (0.011) |
| Black: Above cutoff | 0.000 | -0.007 | -0.070* |
| | (0.063) | (0.029) | (0.041) |
| Black: Vision score X Above cutoff | -0.053 | -0.010 | 0.042 |
| | (0.065) | (0.025) | (0.041) |
| Black: Vision score | -0.001 | -0.005 | 0.016 |
| | (0.027) | (0.013) | (0.019) |
| White: Above cutoff | -0.157* | 0.067** | 0.042 |
| | (0.092) | (0.030) | (0.067) |
| White: Vision score X Above cutoff | -0.032 | 0.071 | -0.168** |
| | (0.114) | (0.045) | (0.065) |
| White: Vision score | 0.087** | -0.048*** | 0.032 |
| | (0.040) | (0.014) | (0.028) |
| Asian: Above cutoff | 0.010 | 0.066*** | 0.045 |
| | (0.087) | (0.023) | (0.048) |
| Asian: Vision score X Above cutoff | 0.024 | 0.037 | 0.022 |
| | (0.086) | (0.038) | (0.051) |
| Asian: Vision score | -0.033 | -0.027*** | -0.019 |
| | (0.038) | (0.009) | (0.022) |

| | (1) Absence rate decreased | (2) Newly chronically absent | (3) No longer chronically absent |
|---|---|---|---|
| Panel D. Regression using 20/40 to 20/100+, trends above and below the cutoff | | | |
| Full-price lunch: Above cutoff | -0.015 | -0.027* | -0.019 |
| | (0.045) | (0.015) | (0.026) |
| Full-price lunch: Vision score X Above cutoff | -0.043* | -0.005 | -0.023* |
| | (0.024) | (0.008) | (0.012) |
| Full-price lunch: Vision score | 0.016 | 0.008* | 0.011* |
| | (0.011) | (0.004) | (0.006) |
| Free/reduced-price lunch: Above cutoff | -0.031 | -0.004 | -0.005 |
| | (0.020) | (0.009) | (0.012) |
| Free/reduced-price lunch: Vision score X Above cutoff | 0.007 | 0.003 | 0.003 |
| | (0.010) | (0.005) | (0.007) |
| Free/reduced-price lunch: Vision score | 0.003 | -0.002 | 0.003 |
| | (0.005) | (0.002) | (0.003) |
| Hispanic: Above cutoff | -0.022 | -0.014 | 0.003 |
| | (0.025) | (0.011) | (0.014) |
| Hispanic: Vision score X Above cutoff | 0.010 | 0.001 | 0.009 |
| | (0.013) | (0.006) | (0.008) |
| Hispanic: Vision score | 0.003 | 0.001 | -0.002 |
| | (0.007) | (0.003) | (0.004) |
| Black: Above cutoff | -0.028 | -0.004 | -0.049** |
| | (0.042) | (0.019) | (0.024) |
| Black: Vision score X Above cutoff | -0.023 | 0.004 | 0.010 |
| | (0.021) | (0.009) | (0.014) |
| Black: Vision score | 0.010 | -0.007 | 0.007 |
| | (0.010) | (0.005) | (0.007) |
| White: Above cutoff | 0.002 | -0.036 | 0.075* |
| | (0.065) | (0.025) | (0.043) |
| White: Vision score X Above cutoff | -0.061* | 0.008 | -0.052** |
| | (0.035) | (0.015) | (0.021) |
| White: Vision score | 0.011 | 0.010 | 0.005 |
| | (0.016) | (0.006) | (0.010) |
| Asian: Above cutoff | -0.070 | 0.025 | -0.028 |
| | (0.049) | (0.017) | (0.030) |
| Asian: Vision score X Above cutoff | 0.015 | -0.001 | -0.029* |
| | (0.026) | (0.011) | (0.016) |
| Asian: Vision score | 0.008 | -0.003 | 0.020*** |
| | (0.013) | (0.004) | (0.007) |

Notes: Regression results in each row are fully interacted with the student characteristic listed in the first column by limiting the sample to students with each FRPL-status or self-reported race/ethnicity. Results for students with "other" race/ethnicity are not reported. Standard errors clustered at the school level are in parentheses. Each reported estimate is from a regression of the outcome indicated in the column header on the treatment variable "Above Cutoff," which is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. The specification in Panels A-C run these regressions only on students whose initial visual acuity scores were near to this cutoff, while Panel D includes students with better vision in order to better control for the "pre" trend by visual acuity. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table C.8: Intent-to-Treat Estimates of the Effect of Follow-up on Academic Outcomes, by Free or Reduced-Price Lunch Status (SES Proxy) and by Race/Ethnicity

| | (1) Repeated a grade | (2) ELA proficiency | (3) Math proficiency |
|---|---|---|---|
| Panel A. Regression using 20/50 to 2080, no trend | | | |
| Full-price lunch: Above cutoff | 0.009 | -0.020 | -0.047* |
| | (0.014) | (0.025) | (0.025) |
| Free/reduced-price lunch: Above cutoff | 0.006 | 0.001 | -0.004 |
| | (0.009) | (0.013) | (0.013) |
| Hispanic: Above cutoff | 0.005 | -0.005 | -0.010 |
| | (0.010) | (0.016) | (0.016) |
| Black: Above cutoff | 0.006 | 0.031 | 0.014 |
| | (0.020) | (0.027) | (0.027) |
| White: Above cutoff | 0.001 | -0.014 | -0.025 |
| | (0.018) | (0.042) | (0.039) |
| Asian: Above cutoff | 0.018 | 0.013 | 0.001 |
| | (0.015) | (0.023) | (0.026) |
| Panel B. Regression using 20/50 to 20/80, single trend | | | |
| Full-price lunch: Above cutoff | -0.003 | -0.099* | -0.152*** |
| | (0.031) | (0.055) | (0.054) |
| Full-price lunch: Vision Score | 0.006 | 0.037 | 0.050** |
| | (0.012) | (0.023) | (0.022) |
| Free/reduced-price lunch: Above cutoff | -0.022 | -0.021 | -0.026 |
| | (0.020) | (0.032) | (0.032) |
| Free/reduced-price lunch: Vision score | 0.013 | 0.011 | 0.011 |
| | (0.008) | (0.013) | (0.013) |
| Hispanic: Above cutoff | -0.028 | -0.019 | -0.061 |
| | (0.025) | (0.040) | (0.039) |
| Hispanic: Vision score | 0.016 | 0.007 | 0.025 |
| | (0.010) | (0.017) | (0.017) |
| Black: Above cutoff | 0.008 | -0.109* | -0.074 |
| | (0.036) | (0.056) | (0.058) |
| Black: Vision score | -0.001 | 0.063*** | 0.040* |
| | (0.014) | (0.021) | (0.022) |
| White: Above cutoff | 0.014 | 0.015 | -0.072 |
| | (0.047) | (0.097) | (0.090) |
| White: Vision score | -0.006 | -0.014 | 0.022 |
| | (0.018) | (0.037) | (0.035) |
| Asian: Above cutoff | 0.029 | 0.026 | 0.058 |
| | (0.029) | (0.060) | (0.058) |
| Asian: Vision score | -0.005 | -0.006 | -0.027 |
| | (0.012) | (0.027) | (0.024) |

| | (1)<br>Repeated<br>a grade | (2)<br>ELA<br>proficiency | (3)<br>Math<br>proficiency |
|---|---|---|---|
| Panel C. Regression using 20/50 to 20/80, trends above and below the cutoff | | | |
| Full-price lunch: Above cutoff | -0.010 | -0.091* | -0.147*** |
| | (0.031) | (0.055) | (0.053) |
| Full-price lunch: Vision score X Above cutoff | -0.049* | 0.059 | 0.041 |
| | (0.026) | (0.053) | (0.058) |
| Full-price lunch: Vision score | 0.015 | 0.026 | 0.042* |
| | (0.014) | (0.025) | (0.023) |
| Free/reduced-price lunch: Above cutoff | -0.023 | -0.011 | -0.021 |
| | (0.020) | (0.032) | (0.032) |
| Free/reduced-price lunch: Vision score X Above cutoff | -0.002 | 0.076** | 0.037 |
| | (0.021) | (0.029) | (0.032) |
| Free/reduced-price lunch: Vision score | 0.014 | -0.003 | 0.004 |
| | (0.009) | (0.014) | (0.015) |
| Hispanic: Above cutoff | -0.032 | -0.006 | -0.055 |
| | (0.025) | (0.041) | (0.040) |
| Hispanic: Vision score X Above cutoff | -0.022 | 0.074** | 0.035 |
| | (0.026) | (0.036) | (0.038) |
| Hispanic: Vision score | 0.020* | -0.007 | 0.018 |
| | (0.011) | (0.018) | (0.019) |
| Black: Above cutoff | 0.007 | -0.109* | -0.073 |
| | (0.036) | (0.056) | (0.058) |
| Black: Vision score X Above cutoff | 0.029 | 0.018 | -0.021 |
| | (0.040) | (0.057) | (0.057) |
| Black: Vision score | -0.006 | 0.060*** | 0.043* |
| | (0.015) | (0.023) | (0.024) |
| White: Above cutoff | 0.009 | 0.035 | -0.056 |
| | (0.047) | (0.098) | (0.090) |
| White: Vision score X Above cutoff | -0.060* | 0.221** | 0.184** |
| | (0.032) | (0.089) | (0.078) |
| White: Vision score | 0.004 | -0.053 | -0.011 |
| | (0.021) | (0.042) | (0.039) |
| Asian: Above cutoff | 0.031 | 0.033 | 0.054 |
| | (0.028) | (0.060) | (0.060) |
| Asian: Vision score X Above cutoff | 0.012 | 0.039 | -0.025 |
| | (0.041) | (0.061) | (0.071) |
| Asian: Vision score | -0.007 | -0.014 | -0.022 |
| | (0.013) | (0.029) | (0.028) |

| | (1) Repeated a grade | (2) ELA proficiency | (3) Math proficiency |
|---|---|---|---|
| Panel D. Regression using 20/40 to 20/100+, trends above and below the cutoff | | | |
| Full-price lunch: Above cutoff | -0.014 | 0.027 | -0.013 |
| | (0.020) | (0.035) | (0.036) |
| Full-price lunch: Vision score X Above cutoff | -0.012 | 0.037* | 0.039* |
| | (0.011) | (0.020) | (0.020) |
| Full-price lunch: Vision score | 0.014*** | -0.031*** | -0.026*** |
| | (0.005) | (0.009) | (0.009) |
| Free/reduced-price lunch: Above cutoff | -0.008 | 0.032 | 0.033* |
| | (0.012) | (0.020) | (0.019) |
| Free/reduced-price lunch: Vision score X Above cutoff | 0.004 | 0.014 | 0.007 |
| | (0.007) | (0.011) | (0.011) |
| Free/reduced-price lunch: Vision score | 0.006* | -0.021*** | -0.021*** |
| | (0.003) | (0.005) | (0.005) |
| Hispanic: Above cutoff | -0.007 | 0.040* | 0.036 |
| | (0.015) | (0.024) | (0.023) |
| Hispanic: Vision score X Above cutoff | 0.004 | 0.017 | 0.011 |
| | (0.008) | (0.013) | (0.013) |
| Hispanic: Vision score | 0.006 | -0.028*** | -0.026*** |
| | (0.004) | (0.007) | (0.007) |
| Black: Above cutoff | -0.009 | -0.000 | -0.016 |
| | (0.027) | (0.039) | (0.039) |
| Black: Vision score X Above cutoff | 0.008 | -0.002 | -0.012 |
| | (0.015) | (0.021) | (0.020) |
| Black: Vision score | 0.005 | 0.004 | 0.010 |
| | (0.006) | (0.009) | (0.009) |
| White: Above cutoff | -0.002 | 0.072 | -0.007 |
| | (0.027) | (0.059) | (0.057) |
| White: Vision score X Above cutoff | 0.008 | 0.018 | -0.012 |
| | (0.015) | (0.030) | (0.031) |
| White: Vision score | 0.005 | -0.040*** | -0.012 |
| | (0.006) | (0.013) | (0.014) |
| Asian: Above cutoff | 0.016 | 0.035 | 0.055 |
| | (0.019) | (0.034) | (0.035) |
| Asian: Vision score X Above cutoff | -0.007 | 0.009 | 0.019 |
| | (0.011) | (0.020) | (0.019) |
| Asian: Vision score | 0.002 | -0.015 | -0.029*** |
| | (0.005) | (0.010) | (0.009) |

Notes: Regression results in each row are fully interacted with the student characteristic listed in the first column by limiting the sample to students with each FRPL-status or self-reported race/ethnicity. Results for students with "other" race/ethnicity are not reported. Each reported estimate is from a regression of the outcome indicated in the column header on the treatment variable "Above Cutoff," which is equal to 1 if a student's initial worst visual acuity score is at or above the cutoff for increased follow-up (20/70) and 0 otherwise. The specification in Panels A-C run these regressions only on students whose initial visual acuity scores were near to this cutoff, while Panel D includes students with better vision in order to better control for the "pre" trend by visual acuity. Standard errors clustered at the school level are in parentheses. * $p < .10$, ** $p < .05$, *** $p < .01$.