

To the Graduate Council:

I am submitting herewith a dissertation written by Ge Wu entitled "Three Essays on the Economics of Education." I have examined the final paper copy of this dissertation for form and content and recommend that it be accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy, with a major in Economics.

Celeste K. Carruthers, Major Pro-

fessor

We have read this dissertation
and recommend its acceptance:

J. Scott Holladay

Maria Padilla-Romo

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Accepted for the Council:

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(Original signatures are on file with official student records.)

Three Essays on the Economics of Education

A Dissertation Presented for the
Doctor of Philosophy
Degree
The University of Tennessee, Knoxville

Ge Wu

August 2023

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Dedication

To my mother Haiying Zhang, father Xuesheng Wu, and sister Yunyan Wu, for their unwavering love and support that have made me who I am today. I am grateful for the numerous sacrifices you have made to provide me with the opportunity to pursue my dreams. Your encouragement and unconditional trust have been a source of strength, helping me overcome all the challenges along the way.

To my husband, Dr. Xueming Dong, for his patience and understanding. Thank you for being there for me and for putting up with my occasional bad mood. Moreover, I extend my heartfelt appreciation for your constant encouragement to pursue a doctoral degree and for your steadfast support during the past five years. Without your persistent nagging, I would not have been able to complete this dissertation on time.

To my cat, Xiaohei Dong, for his emotional support and all the joy he brings into my life. Meow! Meow!! Meow!!!

To my friends, who have been my constant companions. You have brightened my darkest days and shared my happiest moments.

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Abstract

This dissertation presents three essays that use empirical methods to study the economics of education.

The first chapter evaluates the effect of wildfire smoke on primary and middle school student achievement across the United States. The increasing number of wildfires and their destructive consequences has drawn much public attention in recent years. While a growing body of research explores the effect of wildfires on the environment and public health, the evidence of a causal relationship between wildfires and cognitive performance is limited, especially for young students. I merge satellite-based wildfire smoke boundaries, daily estimates of PM-2.5 concentrations at the 1km-grid level, and student achievement records for over 320,000 students across the U.S. to estimate the effect of wildfire-generated smoke pollution on student learning. Findings improve our understanding of the social cost of wildfires.

The second chapter studies whether high school students respond to labor market dynamics when choosing career & technical education electives. Research has shown that U.S. college and university students consider the broader economy and labor market when selecting a major, but it is unclear if high school students similarly respond to labor market signals, or if they have the opportunity to do so. I assess if students in Tennessee are more or less likely to enroll in advanced manufacturing classes, and if schools are more or less likely to offer advanced manufacturing classes, following announcements about layoffs or new jobs in local manufacturing firms.

Findings help to quantify the alignment between student skill development and workforce needs.

The third chapter evaluates the effect of the University of Tennessee-Knoxville (UTK) Flagship Scholarship on college enrollment and choice of college. In Fall 2007, UTK initiated the Flagship Scholarship, which offers a “free college” opportunity to students from designated high schools in Tennessee. I use statewide data on 2006-2009 graduating 12th graders to estimate the effect of attending a Flagship-eligible high school on college enrollment outcomes. Findings contribute to a large literature on financial aid, and in particular to the literature on scholarships that target disadvantaged students or their schools.

Table of Contents

1	Do Wildfires Harm Student Learning?	1
1.1	Introduction	3
1.2	Identification	8
1.3	Data	10
1.3.1	Education Data	11
1.3.2	Wildfire Smoke and Meteorological Data	13
1.3.3	Wildfire Exposure Measurement	14
1.3.4	Summary Statistics	16
1.4	Empirical Results	17
1.4.1	Main Results	17
1.4.2	School Day vs Non-School Day Exposure	20
1.4.3	Mobility Tests	22
1.4.4	Heterogeneous Effects	22
1.4.5	Placebo Tests	24
1.5	Discussion	25
2	Does Career & Technical Education Move with the Labor Market?	30
2.1	Introduction	32
2.2	Related Literature	35
2.3	Data	37
2.3.1	Education Data	37

2.3.2	Labor Market Signals	39
2.4	Methods	40
2.5	Results	43
2.5.1	Student Manufacturing CTE Course Enrollment Outcomes . .	43
2.5.2	School-level Manufacturing CTE Course Availability	46
2.5.3	Robustness and Falsification Tests	48
2.6	Discussion	50
3	Free College Opportunity for Disadvantaged Schools: Assessing Students' Responses to the Flagship Scholarship in Tennessee	52
3.1	Introduction	54
3.2	Literature Review	58
3.3	Identification	62
3.4	Data	64
3.5	Empirical Results	65
3.5.1	Main Results	65
3.5.2	Mobility Tests	66
3.5.3	Heterogeneous Effects	67
3.6	Conclusion	69
	Bibliography	72
	Appendix	88
	Appendix A	88
A.1	Tables	88
A.2	Figures	99
A.3	Supplementary Tables	105
	Appendix B	128
B.1	Tables	128

B.2	Figures	136
B.3	Supplementary Tables	138
Appendix C		140
C.1	Tables	140
C.2	Supplementary Tables	148
Vita		150

List of Tables

A.1	Descriptive Statistics for Students and GSDs	89
A.2	Descriptive Statistics for Meteorological and Wildfire Smoke Data . .	90
A.3	Estimated Effects of Wildfire Smoke Days (*10) on Academic Performance	91
A.4	Estimated Effects of Standardized Cumulative $PM_{2.5}$ on Academic Performance	92
A.5	Estimated Effects of Unhealthy Air Quality Days (*10) on Academic Performance	93
A.6	Estimated Effects of Wildfire Smoke Days (*10): School Day vs Non-School Day	94
A.7	Estimated Effects of Standardized Cumulative $PM_{2.5}$: School Day vs Non-School Day	95
A.8	Estimated Effects of Unhealthy Air Quality Days (*10): School Day vs Non-School Day	96
A.9	Estimated Effects of Wildfire on Total Enrollment (Standardized) . .	97
A.10	Placebo Test: Future Wildfire Exposure - 75% Exposure	98
A.11	States with Test Time Windows Started in October	105
A.12	Estimated Effects of the Wildfire Smoke Days (*10) on ELA Performance	106
A.13	Estimated Effects of Standardized Cumulative $PM_{2.5}$ on ELA Performance	107

A.14 Estimated Effects of the Unhealthy Air Quality Days (*10) on ELA Performance	108
A.15 Estimated Effects of the Wildfire Smoke Days (*10) on Math Performance	109
A.16 Estimated Effects of Standardized Cumulative $PM_{2.5}$ on Math Perfor- mance	110
A.17 Estimated Effects of the Unhealthy Air Quality Days (*10) on Math Performance	111
A.18 Heterogeneous Effects Analysis for Asian Students - 75% Exposure .	112
A.19 Heterogeneous Effects Analysis for Black Students - 75% Exposure . .	114
A.20 Heterogeneous Effects Analysis for Hispanic Students - 75% Exposure	116
A.21 Heterogeneous Effects Analysis for White Students - 75% Exposure .	118
A.22 Heterogeneous Effects Analysis for Female Students - 75% Exposure .	120
A.23 Heterogeneous Effects Analysis for Male Students - 75% Exposure . .	122
A.24 Heterogeneous Effects Analysis for Econ. Disadvantaged Students - 75% Exposure	124
A.25 Placebo Test: Future Wildfire Exposure - 25% Exposure	126
A.26 Placebo Test: Future Wildfire Exposure - 50% Exposure	127
B.1 Descriptive Statistics	129
B.2 Estimated Effects of County Labor Market Signals on Student-level Manufacturing Enrollment	130
B.3 Estimated Effects of Labor Market Signals on Manufacturing Enroll- ment: Grade 10-12 Student Subgroups	131
B.4 Estimated Effects of County and Metro Area Labor Market Signals on Student-Level Manufacturing Enrollment	132
B.5 Estimated Effects of County and Metro Area Labor Market Signals on School-Level Manufacturing Course Availability	133
B.6 Falsification Tests: Estimated Effects of Future Labor Market Signals on Student-Level Manufacturing Enrollment and School-Level Manu- facturing Availability	134

B.7	Estimated Effects of Other Industries' Labor Market Signals on Student-Level Manufacturing Enrollment and School-Level Manufacturing Availability	135
B.8	Descriptive Statistics for Manufacturing Courses Enrollees/Offering Schools	138
B.9	Nonlinear Models: Estimated Effects of Manufacturing Labor Market Signals on School-Level Manufacturing Enrollment and Availability . .	139
C.1	Descriptive Statistics	141
C.2	Estimated Effects of Flagship Scholarship	142
C.3	Effects of Flagship Scholarship: Event Study Estimations	143
C.4	Estimated Effects of Flagship Scholarship on Flagship HS Enrollment	144
C.5	Heterogeneity Analysis: All High Schools Sample	145
C.6	Heterogeneity Analysis: Low College-Going High Schools Sample . . .	146
C.7	Heterogeneity Analysis: Comparable HS Sample	147
C.8	Heterogeneity Analysis: Asian and Hispanic Students	149

List of Figures

A.1	Smoke Plumes across Colorado and New Mexico on July 15, 2011 . . .	100
A.2	Smoke Exposure by Census Tract Population in School District . . .	100
A.3	Average Wildfire Exposure across the United States	101
A.4	Heterogeneous Effects Analysis: Wildfire Smoke Days (*10)	102
A.5	Heterogeneous Effects Analysis: Standardized Cumulative $PM_{2.5}$. . .	103
A.6	Heterogeneous Effects Analysis: Unhealthy Air Quality Days (*10) . .	104
B.1	Statewide Manufacturing Layoffs and Subsidized Jobs Shares	136
B.2	Timing of Labor Market Signals in Relation to School Year	137

Chapter 1

Do Wildfires Harm Student Learning?

Abstract

I evaluate the effect of wildfire smoke on primary and middle school students' English Language Arts (ELA) and math achievement across the United States. To estimate students' exposure to wildfires at the school district level, I merge satellite-based wildfire smoke plume boundaries and 1km-grid daily $PM_{2.5}$ values with school district locations, and weight the exposure by census tract population. I find that recent drifting wildfire smoke plumes and cumulative $PM_{2.5}$ concentrations significantly lower ELA and math test scores. When I proxy the severe wildfires by smoke-generated unhealthy air quality, results suggest lasting effects on young students in primary school, and the effects are only transitory for students in middle school. Further analysis reveals that Black students in primary school and economically disadvantaged students are more negatively affected than others. Males are more affected by the unhealthy air quality in elementary ELA and middle school math than female students. Overall, findings suggest that more environmental and educational policy responses are needed to protect students with the increase in wildfire occurrence and intensity.

1.1 Introduction

The increasing number of wildfires and their destructive consequences have drawn much public attention in recent years. In 2017, over 71,000 wildfires burned 10 million acres of land ([U.S. Congressional Research Service, 2020](#)). Wildfires are known to generate direct effects on ecosystem and wildlife with the burning of biomass, the change of landcover, and the destruction of habitats ([Wardle et al., 2003](#); [Rollan and Real, 2011](#); [Pausas and Keeley, 2019](#); [Smith et al., 2021](#)). Wildfire-generated smoke can spread miles away and the smoke is shown to affect public health ([Liu et al., 2015](#); [Heft-Neal et al., 2022](#); [Johnston et al., 2012](#); [Reid et al., 2016](#); [Amjad et al., 2021](#); [Mccoy and Zhao, 2021](#)), migration ([McConnell et al., 2021](#); [Burke et al., 2022](#)), and labor market outcomes ([Borgschulte et al., 2022](#)).

While much research has been done to explore the effect of wildfires, the evidence of the causal relationship between wildfires and cognitive abilities is limited, especially for young students. The goal of this study is to investigate the effect of wildfires on student learning. The key challenge to identifying the causal effects is to assess geographical variations in wildfire exposure. Simply using wildfire burning coordinates to determine exposure is inadequate because smoke plumes can travel great distances, and are affected by wind, wildfire intensity, and other meteorological factors. A drifting smoke plume generates exogenous variation in wildfire exposure. Therefore, I employ satellite-based data on wildfire plume boundaries and spatially join these boundaries with school district maps to identify exposure areas.

However, smoke boundaries alone do not reveal details of the pollution intensity. I follow [Heft-Neal et al. \(2022\)](#) to merge smoke boundaries with ensemble-based model predictions of high-resolution 1km-grid daily Particulate Matter 2.5 ($PM_{2.5}$) concentrations, which is widely known as one of the most important indicators of wildfire smoke pollution ([United States Environmental Protection Agency, 2019](#)). In addition, I add the census tract level population to the dataset by geometric locations. This allows me to estimate district-level exposure to wildfires by both wildfire smoke

and the associated air pollution throughout the population of a given census tract. I link the exposure dataset to over 320 thousand student achievement records, including average English Language Arts (ELA) and math standardized test scores at the school district level across the United States, school years 2009-2010 through 2014-2015. This data offers a good representation of different ranges and variations of wildfires that affect primary and middle school students, and thus allows my work to systematically study the wildfires' aftermath.

Findings show that having wildfire smoke plumes pass over a school district within 1 year prior to tests significantly reduces students' ELA and math scores in both primary school and middle school. In order to understand the effects of wildfires by intensity, I evaluate the effect of cumulative smoke-generated $PM_{2.5}$ concentrations and find similar conclusions. I additionally estimate the effect of only severe wildfires. I follow U.S. Environmental Protection Agency (EPA) guidelines to construct a daily indicator of unhealthy air quality for sensitive groups using population-weighted $PM_{2.5}$ from wildfire smoke for each school district. I find that an increase in unhealthy air quality days 1 to 3 years prior to tests, with a $PM_{2.5}$ value greater or equal to $35.5\mu g/m^3$, significantly decreases students' ELA and math scores. The magnitudes of effects are larger for math than ELA. Younger students in primary school are more affected by the lagged unhealthy air quality exposure, which generates a lasting effect. Middle school students are only responsive to recent exposure and the effects are transitory.

I further compare the effect of wildfire smoke during school days and non-school days. Although the effects are mixed for ELA studies, there exists a consistent pattern for the effects on math. Students' math scores are solely affected by wildfire smoke on school days. Since school closures due to wildfire smoke exposure are uncommon, my findings imply that the mechanism underlying the adverse effects on math studies is likely associated with the impairment of brain development.

My empirical strategy relies on the assumption that wildfire breakouts are exogenous and are unrelated to omitted factors affecting student achievement. These

conclusions would suffer from endogeneity if students move in response to wildfires. To test for potential endogeneity associated with students or their families' mobility, I assess the effect of wildfires on the number of students in each demographic and socioeconomic group and conclude that the effect is limited.

This study contributes to the literature in the following ways: First, it provides critical implications for understanding the social costs of wildfires. A large number of studies address the effect of wildfire smoke on public health. [Heft-Neal et al. \(2022\)](#) show that additional exposure to wildfire smoke during pregnancy significantly increases the risk of preterm birth in California. [Mccoy and Zhao \(2021\)](#) identify a negative effect of wildfire smoke on infant birth weights. [Liu et al. \(2015\)](#), [Emmanuel \(2000\)](#), [Mirabelli et al. \(2009\)](#), and [Reid et al. \(2016\)](#) find an association between wildfires and respiratory disease. Exposure to wildfire smoke is also considered to be correlated with mortality ([Morgan et al., 2010](#); [Johnston et al., 2012](#); [Reid et al., 2016](#); [Miller et al., 2017](#); [Neller and Arenberg, 2022](#)). In addition, household migration is responsive to destructive wildfires ([McConnell et al., 2021](#); [Burke et al., 2022](#)). More recent studies evaluate the impact on the labor market. [Borgschulte et al. \(2022\)](#) show that a nearly 2% reduction in labor income is attributed to wildfire exposure, and such effects are larger for older workers. [Neller and Arenberg \(2022\)](#) find that wildfire exposure at an early age has long-lasting effects on lifetime earnings. If wildfire smoke affects health, productivity, and longevity, we might reasonably expect there also to be an effect of smoke on academic achievement. But only a few studies addressed this question in the literature, and this study seeks to fill the gap.

One study that evaluates the effects of wildfires from an environmental science viewpoint was recently published by [Wen and Burke \(2022\)](#). Similar to this paper, they utilize the satellite-based smoke plume and $PM_{2.5}$ to estimate school district wildfire exposure and find that smoke-attributable $PM_{2.5}$ is an important factor in education. My study contributes to [Wen and Burke \(2022\)](#) in a few important ways. First, I take a more granular approach to measuring wildfire exposure that offers more details on the variation of exposure within a school district. Specifically, I merge the

census tract population, which is a smaller unit than the school district, with smoke plume and school district boundaries, and provide an estimation of the percentage of the population that was exposed to smoke plume. Second, I contribute by introducing test time windows information in exposure measurement. This allows me to precisely measure the number of days that students were exposed to wildfires before tests. Third, my study evaluates the effect of smoke *per se* in addition to wildfire-generated $PM_{2.5}$ that was employed as the main treatment by [Wen and Burke \(2022\)](#). These estimates incorporate the potential psychological impact of wildfires nearby, even though the intensity is not heavy enough to generate negative health effects. I also evaluate the effect of severe wildfires only, which implies the nonlinear effects of wildfire intensity. Fourth, I estimate the effect of 1 to 3-year lagged wildfire exposure. This helps quantify the long-term adverse effects that were generated by short-term pollution exposure. Furthermore, my comparison between the school day and non-school day exposure provides an understanding of the mechanism. And finally, this study offers additional tests for endogenous student mobility.

In addition, I add to the literature by delivering additional evidence of the transitory effect of pollution on academic achievement. Although limited research has been done to evaluate wildfire-associated transitory pollution on achievement, a growing literature has connected transitory pollution more broadly to academic performance. [Ebenstein et al. \(2016\)](#) demonstrate the effect of pollution on high-stakes exams in Israel. They find that transitory $PM_{2.5}$ leads to a significant decline in test scores. [Heissel et al. \(2022\)](#) utilize wind direction variation to show that students who study in schools downwind of highways have worse academic performance due to the exposure to transitory traffic pollution in Florida. Pollution from agricultural fires is also shown to result in weaker performance in National College Entrance Examination in China ([Zivin et al., 2020](#)). In addition, [Gilraine and Zheng \(2022\)](#) leverage the instrumental variables and show that the reduction in $PM_{2.5}$ significantly increases test scores. My conclusions are consistent with previous work, where wildfire and wildfire-generated pollution reduce students' academic performance in ELA and

math, but for a less studied source of pollution and a wide range of grade levels across the United States.¹

Furthermore, this paper identifies an additional environmental factor that could drive educational outcome disparities. Previous work has shown that family income (Dahl and Lochner, 2012), teacher quality (Marioni et al., 2020), and parental involvement (Houtenville and Conway, 2008) are essential to education outcomes but access to these resources is not equitable across student race, ethnicity, gender, and location. In addition, external factors sometimes have different effects on students with different backgrounds. Recent research investigates environmental influences. Park et al. (2020) demonstrate that higher temperature significantly lowers students' test scores overall, with more negative effects for Black, Hispanic, and lower-income students, although the effects are not significantly different between genders. My results add to this literature by exploring how effects vary by student gender, race/ethnicity, and economic disadvantage. I find that Black students in primary schools are most affected among the four racial/ethnic groups. White students receive the least impact of wildfire smoke plumes but are heavily influenced by the lagged unhealthy air quality exposure with lasting effects. Asian students, although are not negatively affected in math, get the largest negative impact on ELA in middle school. A proportion of gender differences in educational achievement can be attributed to wildfires. The effect of 3-year lagged $PM_{2.5}$ concentrations and unhealthy air quality exposure applies more to male students, especially for ELA achievement. Wildfires also widen the gap between economically disadvantaged students and students with higher-income families. These findings help explain part of racial/ethnic and income-associated achievement gaps.

¹There is increasing evidence that children are differently affected by pollution across ages and early age exposure may generate a lasting effect on cognitive ability development (Ko et al., 2007; Calderón-Garcidueñas et al., 2015; Grandjean and Landrigan, 2014). Persico and Venator (2021) and Wen and Burke (2022) also show there exist heterogeneous effects of pollution across grades. A wide range of grade levels in my data allows me to identify the variation of effects by age and understand how different education level students may respond to environmental factors.

Finally, the frequency and severity of wildfires have been increasing along with climate change and global warming (Westerling et al., 2006; Shi et al., 2021; Gillett et al., 2004; Balch et al., 2017; Schoennagel et al., 2017; Flannigan et al., 2013). My results suggest that a 10-day increase in wildfire smoke within 1 year prior to tests results in an average of 0.003 standard deviations reduction in ELA and 0.004 standard deviations reduction in math test scores. Quantifying the effect of wildfires on young students' cognitive abilities helps the public understand the importance of environmental governance.

1.2 Identification

I use the following specification to analyze changes in aggregate standardized test scores for each school district in response to the various levels of wildfire exposure:

$$Y_{git} = \alpha + \beta_{\kappa} * \sum_{\kappa=3}^1 Exposure_{it-\kappa} + \theta X_{git} + \delta Z_{it} + \gamma C_{it} + \tau_i + \pi_t + \phi_g + \varepsilon_{it} \quad (1.1)$$

where Y_{git} represents standardized English Language Art (ELA) and math test scores for each grade g in Geographic School District (GSD) i during the school year t . $Exposure_{it-\kappa}$ denotes the typical number of days that 25%, 50%, or 75% of the population in school district i was exposed to wildfire smoke plumes over the κ^{th} year relative to district i tests in school year t , where I lag the exposure up to 3 years. The simple exposure to the drifting smoke plumes, however, does not reveal variation in the intensities of wildfires. Additionally, smoke plumes can travel up to thousands of miles from the actively burning spots. Students who live far from the wildfires may see smoke plumes overhead, while the pollution concentration was negligible. To evaluate if students who were close to severe wildfires were more affected than those who were not, $Exposure_{it-\kappa}$ alternatively measures the intensity in the following two ways: first, it represents the standardized cumulative wildfire smoke attributed $PM_{2.5}$

concentrations the school district i was exposed to within the κ^{th} year before testing in school year t . Second, it counts the number of days school district i had unhealthy $PM_{2.5}$ concentration produced by wildfire smoke plumes, which is greater or equal to $35.5\mu g/m^3$ as defined by EPA.²

X_{git} is a vector of GSD level demographic and socioeconomic characteristics for students in each grade g , including the percentage of students of each race and ethnicity, the percentage of students who are economically disadvantaged, and the percentage of students who have free and reduced-price lunches. I also use vector Z_{it} to control for district-specific characteristics, including the location of GSD (urban, suburban, town, or rural), the percentage of English language learners, the percentage of students who are in special education, the log of household median income, poverty rate, unemployment rate, SNAP receipt rate, single mother household rate, and the proportion of adults with a bachelor's degree or higher. C_{it} is a vector of meteorological control variables for each GSD: seasonal maximum, minimum, and average temperature, and seasonal average precipitation. I do not control wind direction or wind speed in this model, as the satellite readings of smoke plumes have already incorporated the effect of wind.

τ_i is GSD fixed effect that absorbs time-invariant GSD-specific factors which could affect test scores. School year fixed effect π_t flexibly controls for time-varying shocks that could influence outcomes across all GSD. Time-invariant grade-specific terms that could differentially change academic performance are captured by grade fixed effect ϕ_g . ε_{it} represents an idiosyncratic error term. I cluster standard errors at the treatment level, which is the GSD in this context, as advised by [Abadie et al. \(2017\)](#). I follow [Solon et al. \(2015\)](#) and use a modified Breusch-Pagan test to confirm heteroskedasticity due to variations in cohort size across school districts. Therefore, I weight all estimates by the number of students in each GSD-grade cell.

² $PM_{2.5}$ greater or equal to $35.5\mu g/m^3$ is defined by EPA as unhealthy air quality for sensitive groups, including children.

β_κ is the parameter of interest for the effect of wildfire smoke on student academic performance Y_{git} . To interpret β_κ as the causal effect of wildfires, I assume that students' exposure to wildfire smoke is conditionally exogenous. One might argue that people who live in states which are known to be susceptible to wildfires, for example, California, may expect the presence of wildfire smoke every year. However, the exact number of days with smoke plumes passing over each school district varies, and the severity of fires is not predictable. Yet, estimates could still be biased if students or their families tended to move away from places with high wildfire smoke concentrations. [McConnell et al. \(2021\)](#) show that destructive wildfires lead to out-tract migration. The effect is more pronounced with severe wildfire smoke exposure ([Burke et al., 2022](#)). Besides, wildfire-generated poor air quality could also change migration behavior. [Kim \(2019\)](#) finds that more frequent air quality alerts had a negative effect on the migration of households in California. To examine if wildfire smoke would result in students' migration, which would potentially bias [Equation 1.1](#) estimates, I conduct a mobility analysis. I test if enrollments in a school district change over time in response to wildfire exposure, in terms of smoke plumes, cumulative $PM_{2.5}$ concentrations, and smoke-produced unhealthy air quality. Specifically, I re-estimate [Equation 1.1](#) with the outcome variable replaced by the standardized student counts for student subgroups at the GSD level.

1.3 Data

To investigate the effect of wildfires on education outcomes, I construct a sample combining school district-level standardized test scores by grades, state test time windows, meteorological conditions, daily wildfire smoke plume locations, daily gridded $PM_{2.5}$ concentrations, and American Community Survey (ACS) census tract level population.

1.3.1 Education Data

Stanford Education Data Archive (SEDA) V30 dataset contains academic achievement at the GSD level across the United States. SEDA harmonizes achievement measures from the assessment data in the EDFacts data system.³ Specifically, I use Cohort Standardized (CS) English Language Arts (ELA) and math test scores from school years 2009-10 to 2014-15 in grades 3 to 8, which allows me to compare academic achievement across geographic locations, years, and education levels. For the consistency of the comparison, I only keep school districts and school years that report both ELA and math achievement for all grades.⁴ The year referred to in this paper indicates the spring semester of the school year, e.g., year 2009 represents the school year 2008-09.

In the SEDA dataset, the CS scale academic achievements are standardized relative to national reference cohorts. This baseline is created by taking the average of cohorts who were in 4th grade in 2009, 2011, and 2013. Standardized achievement is also reported separately by race/ethnicity, gender, and economically disadvantaged subgroups. This allows me to estimate heterogeneous effects of wildfires on different groups of students.

The covariates provided by the SEDA allow me to control for demographic and socioeconomic characteristics at the GSD level in the model. One group of variables is collected from the Common Core of Data (CCD) and EDFacts. These variables focus on the demographic characteristics of students and public schools: Urbanicity indicators are reported by GSD; the percentage of students of different race/ethnicity, the percentage of students who have free and reduced-price lunch, the percentage of students who are considered economically disadvantaged, and the number of students are reported for each grade-by-GSD; and the percentage of English language learners and the percentage of students in special education are aggregated to the

³See [Fahle et al. \(2019\)](#) for details on the SEDA dataset construction.

⁴24.7% of the observations are dropped in this step.

GSD level, regardless of grades. Another group of covariates is obtained from the American Community Survey (ACS). These covariates, including the log of median income, poverty rate, unemployment rate, SNAP receipt rate, the proportion of adults with a bachelor’s degree or higher, and single mother household rate, describe the demographic and socioeconomic characteristics of the total population of residents for each GSD.

In addition to academic achievement, I contact each state’s educational agency to collect the test time windows. The test time windows vary across states and years, but are mostly concentrated in the spring. Each school would choose an exact date to administer the test within the time window. I assess GSD level exposure by counting the number of days students are exposed to wildfire smoke within the year before they started the test. Although the actual test date can be any day during the time frame, I conservatively use the first day of the state-determined time window because any wildfires that break out after tests should not affect the current year’s test scores.⁵ Aberrant time windows, with a test window that starts in October and ends in spring (19 out of 296 state-by-year test windows), are dropped from the sample.⁶ For states with missing test time windows in certain years, I use the earliest test start record of the state. For states with no available test time window, I assume the test starts on the first day of spring, March 1st.⁷

⁵However, if severe wildfires are concentrated during the spring test time window, students may still be adversely affected when they take tests, which could introduce another causal pathway. In addition to health effects that persist for months, test-window wildfires could result in short-term problems (asthma, congestion, etc.) that affect performance on the test but not long-term learning. From March 1st to May 31st, which is when most tests are administered, the average number of smoke days for each school district is 6.4 per year with a maximum of 54 days and the average number of unhealthy air quality days is 0.004 with a maximum of 3 days. This is a relatively small amount of wildfire exposure compared to other parts of the year. Therefore, this should not alter the interpretation of my results.

⁶See Appendix [Table A.11](#) for details.

⁷25 out of 51 educational agencies from each state and Washington D.C. responded to me with test time windows. However, some states did not have records for all years between 2010 and 2015. Among 296 state-by-year time windows, 41% has a confirmed start date.

1.3.2 Wildfire Smoke and Meteorological Data

Wildfire dates and locations describe the incidence of wildfires in the United States, but they lack information on how the surrounding areas are affected by each fire. To assess the effect of wildfires, I use spatial data that describes wildfire smoke plumes. The wildfire smoke plume data is obtained from National Oceanic and Atmospheric Administration (NOAA) Hazard Mapping System (HMS) Fire and Smoke Product, which produces daily fire smoke plume boundaries through satellite imagery across the United States. Since smoke boundaries are generated through more than one satellite instrument's information, one location may report multiple smoke plumes on the same day. As shown in [Figure A.1a](#), for example, there is more than one smoke plume layer across the same region in Colorado and New Mexico on July 15, 2011. They can be generated from one satellite reading of multiple wildfires, multiple satellite readings of the same wildfire, or both. However, HMS data does not allow me to distinguish the source of these multiple smoke layers. Directly using this information would artificially increase students' smoke exposure measurement. To avoid the double-counting error, I dissolve the interacted smoke plume and only keep the outermost boundary for each location ([Figure A.1b](#)).

Smoke plume geometric locations allow me to overlap smoke with school districts. One limitation of HMS smoke data is that, when the ground is covered by snow or clouds, or when smoke only exists at night, smoke may not be detected from satellite imagery ([Heft-Neal et al., 2022](#); [Vargo, 2020](#); [Ruminski et al., 2006](#)). In my sample, there are 32 days (1.42% of the sample) have no smoke information. I assume there was no wildfire smoke on these days. Another limitation is that the smoke data does not include quantitative smoke density estimates. Even though it reports qualitative density descriptions, using thin, medium, and thick labels to distinguish the plumes, this measurement is not precise and 17.53% of the sample have missing density labels. The composition of wildfire smoke is complex, but $PM_{2.5}$ is considered as having high concentrations in the wildfire smoke and is harmful to public health

(United States Environmental Protection Agency, 2019). Therefore, to differentiate students' exposure by wildfire intensity, I follow Heft-Neal et al. (2022) to merge the HMS smoke data with 1km-grid $PM_{2.5}$ concentrations that are collected from NASA Socioeconomic Data and Applications Center (SEDAC). The $PM_{2.5}$ values are estimated by an ensemble-based model, which integrated three machine learning algorithms along with a large number of predictor variables from monitor-based data, satellite aerosol optical depth (AOD) measurements, NOAA meteorological values, chemical transport model (CTM) simulations, land-use variables, etc. (Di et al., 2019, 2021). Compared with the station-monitored $PM_{2.5}$, the grided level pollutant estimates allow me to precisely fit these values into the irregular smoke plume shapes and school districts. I assume that the region's unhealthy air quality, as measured by $PM_{2.5}$, is produced by wildfire smoke if the region has smoke plumes pass over on a daily basis.

The meteorological control variables are obtained from NOAA National Centers for Environmental Information (NCEI) Global Summary of the Month (GSOM) dataset at the monitor station level. I spatially overlay the stations on the school district map and aggregate the monthly maximum, minimum, and average temperature, and precipitation by season for each district.⁸ These control variables are merged to education data by school year, where I follow the test time window and smoke exposure measurement to define school year t for the meteorological variables as March 1st of year $t - 1$ to February 28th or 29th of year t .

1.3.3 Wildfire Exposure Measurement

To assess students' exposure to wildfires, I spatially join smoke plume regions with school district areas for each day. I begin with measuring exposure by counting the number of days the school district had wildfire smoke plumes overhead within 365 days prior to the first day of the state test time window. However, simply counting

⁸I follow the meteorological season definition to aggregate March to May variables for spring, June to August for summer, September to November for fall, and December to February for Winter.

the smoke days could misstate exposure if the smoke plume only covers a rural area where few people reside. As shown in [Figure A.2a](#), smoke plumes pass over part of the Mountain Valley School District RE-1 in Saguache County of Colorado on July 15th, 2011. To reduce potential exposure measurement errors, I merge the 2007-2011 American Community Survey (ACS) 5-year estimates of census tract level population with school district boundaries and identify the population that was directly affected by wildfire smoke. For example, as shown in [Figure A.2b](#), the Mountain Valley School District RE-1 region was divided by census tracts #9776 and #9777. The interacted areas account for 45.66% and 35.84% of the two tracts' areas, respectively. The population in tract #9776 is 2864, and in tract #9777 is 3301. Therefore, the population of this school district is calculated as $(45.66\% * 2864) + (35.84\% * 3301) = 2491$. My key assumption here is that the population is uniformly distributed across each census tract. [Figure A.2c](#) highlights the interacted area of the smoke plume with the school district and tract #9776, which is 28.11% of the tract #9776 area. This indicates that 805 $(= 28.11\% * 2864)$ people were exposed to wildfire smoke from this census tract. Likewise, [Figure A.2d](#) highlights the interacted area of the smoke plume with the school district and tract #9777. This interacted region accounts for 35.84% of the tract, i.e. a population of 1183 $(= 35.84\% * 3301)$ was exposed to the smoke. In total, 1988 out of 2491 people, which is 79.81%, in this school district were counted as exposed to wildfire smoke on July 15th, 2011. I repeat this calculation for each school district and each day, and then count the number of days a school district had more than 25%, 50%, and 75% of the population exposed to wildfire smoke plumes each year.

When measuring wildfire smoke intensity, I plot daily 1km-grid $PM_{2.5}$ values over school districts, tracts, and smoke plumes. I first calculate a simple average $PM_{2.5}$ for each interacted area, as [Figure A.2c](#) and [Figure A.2d](#) show, and then compute school district level $PM_{2.5}$ using population-weighted averages. To assess if severe wildfires have more of an effect on students, I also count the number of days 25%, 50%, and

75% of the population exposed to smoke-produced $35.5\mu\text{g}/\text{m}^3$ or higher $PM_{2.5}$, which is defined as unhealthy air quality or worse by EPA, over the year prior to testing.

1.3.4 Summary Statistics

[Table A.1](#) reports summary statistics for the education data. The sample includes 6740 unique districts in the United States, reporting over 160 thousand ELA and math score records from grades 3 to 8, school years 2009-2010 to 2014-2015. Since scores are standardized across national reference cohorts using the full SEDA dataset, after constructing the sample to answer the specific research question in this paper, the mean values are close but not equal to zero, as shown in Panel A. On average, there are 436 students in each grade of a school district.

[Table A.2](#) shows the summary statistics for seasonal meteorological conditions (Panel A) and wildfire exposure measurement (Panel B). Exposure to wildfire smoke is fairly common. On average, 75% of the population in a school district was exposed to 29 days of smoke per year. But in terms of the exposure to smoke produced unhealthy $PM_{2.5}$, the value is quite small, with a mean of near zero, and a maximum of 38 days. The “Smoke Exposure Indicator” in Panel B reports the mean of an indicator equal to one if a school district ever had wildfire smoke plume pass over. On average, almost all the school districts in this sample were exposed to at least one day of wildfire smoke each year, with mean values close to 1. The variations of smoke days, cumulative $PM_{2.5}$ concentrations, and unhealthy air quality days across the United States are also demonstrated in [Figure A.3](#).⁹

⁹The uncolored dotted area means the school district data is either unavailable or is excluded from the sample.

1.4 Empirical Results

1.4.1 Main Results

Table A.3 - Table A.5 present the main results from estimations of Equation 1.1 for ELA and math academic achievement. These tables include the preferred measurement with at least 75% of the school district population being exposed to wildfires.¹⁰ For presentation purposes, I scale the smoke day and unhealthy air quality day exposure by 10. Table A.3 reports the effects of a 10-day increase in students' exposure to wildfire smoke. Table A.4 shows the effect of standardized cumulative wildfire-attributed $PM_{2.5}$ in a given year. And Table A.5 presents the estimates of a 10-day change in students' exposure to wildfire smoke-produced unhealthy air quality, which is approximated by the concentration of $PM_{2.5}$. I separately report effects for all students in column 1, for primary schools only (grades 3-5) in column 2, and for middle schools only (grades 6-8) in column 3.

Smoke Plume Exposure In Table A.3, Panel A shows that a 10-day increase in smoke exposure within 1 year prior to tests significantly decreases students' ELA test scores by 0.003 standard deviations, and such effects apply to both primary and secondary school students. In Panel B, the results show that within 1 year before tests, an additional 10 days of smoke exposure decreases math scores by 0.004 standard deviations. However, such effects are disproportionate for students in primary and middle school. Wildfire smoke only marginally decreases younger students' math study with magnitudes of 0.003 at a 10% significance level and has an effect of 0.005 standard deviations on students' math study in middle school at a 1% significance level. The 2-year and 3-year lagged smoke exposure does not

¹⁰The estimates with at least 25%, 50%, and 75% of the exposure population are presented in Appendix Table A.12, Table A.13, and Table A.14 for ELA performance, and Table A.15, Table A.16, and Table A.17 for math performance. In general, as I tighten the restriction of the exposure population from 25% to 75%, the magnitudes of effects become larger, and the significance level increases.

negatively impact ELA or math studies. In [Table A.3](#), I also observe that the 2-year lagged smoke exposure has an unexpected positive estimated effect on middle school achievement. As shown in Appendix [Table A.12](#) and [Table A.15](#), both the positive coefficient and the significance level decline as the threshold for population exposure increases from 25% to 75%. Therefore, exposure measurement error may explain these counterintuitive positive coefficients. However, unobserved time-varying factors in middle grades may also play a role here. As discussed in section 4.5, middle-grade achievement has a positive relationship with *future* wildfire smoke, a pattern that may conceal negative effects in the [Equation 1.1](#) model.

Cumulative $PM_{2.5}$ Exposure The estimates of the effect of cumulative $PM_{2.5}$ concentrations, which reveal the variation in wildfire intensity, are presented in [Table A.4](#). In Panel A, one standard deviation increase in 1-year lagged $PM_{2.5}$ exposure decreases ELA test scores by 0.005 standard deviations for students in primary school, but these effects do not apply to students in middle school. When I evaluate the effect on math study, $PM_{2.5}$ exposure mostly harms middle school studies. The effects on primary school math achievements are only marginally significant.

Unhealthy Air Quality Exposure When I focus on the fierce wildfire only, which generated unhealthy air quality, both students' ELA and math academic performances are responsive to 1-year and 3-year lagged unhealthy air quality, as presented in [Table A.5](#). In addition, lagged unhealthy air exposure has larger effects than recent exposure, and the magnitudes of the coefficients are larger compared to the magnitudes of the other two measurements. In Panel A column 1, students' ELA test scores decrease by 0.026 standard deviations with an additional 10 unhealthy air quality days within 1 year prior to tests and decrease by 0.032 standard deviations with 3-year lagged exposure. Students' response varies by education level. Students in grades 3-5 are affected by 2-year and 3-year lagged unhealthy air quality, but students in grades 6-8 are only influenced by recent exposure. The effects of wildfire-generated unhealthy air quality on math test scores in Panel B are similar to the effects on ELA.

Math performance decreases by 0.03 and 0.04 the 10-day increases in unhealthy air quality days within 1-year and 3-year prior to tests, respectively. Primary school students are more affected by lagged exposure and middle school students are only responsive to the recent change in wildfire-associated air quality.

It should be mentioned that, although the coefficients in unhealthy air quality estimates are about 10 times larger than the coefficients in the smoke plume or $PM_{2.5}$ concentration exposure estimates, the average number of days a school district has unhealthy $PM_{2.5}$ is close to zero, and the maximum is only 38. In this regard, a 10-day increase in unhealthy air quality days is a notable change.

My estimates are consistent with findings by [Wen and Burke \(2022\)](#) that effects of wildfire smoke associated $PM_{2.5}$ are concentrated on primary school students. [Persico and Venator \(2021\)](#) also find the negative effect of pollution from Toxic Release Inventory sites on test scores is more pronounced for younger students. This can be driven by the variation in cognitive development by age as younger student's brain and central nervous system are shown to be more vulnerable to air pollution ([Calderón-Garcidueñas et al., 2008, 2011, 2015](#); [Block et al., 2012](#)).

Overall, wildfires are shown to significantly affect students' academic performance, but the extents vary across education levels, across years, and across measurement methods. Middle school students are negatively affected by recent exposure, while lower grade students are additionally affected by the earlier years' exposure when measuring exposure using unhealthy air quality. My findings also indicate that the effects of smoke plume and cumulative $PM_{2.5}$ exposure tend to be transitory, while more intense wildfire smoke could generate lasting adverse effects, especially for young students.

In the estimates for all students reported column 1 of [Table A.3](#), [Table A.4](#) and [Table A.5](#), the effects range from 0.003 to 0.004, from 0.004 to 0.005, and from 0.026 to 0.040, respectively. These effects are not trivial. For comparison, [Park et al. \(2020\)](#) find that 1°F increase in the average maximum temperature lower PSAT achievement by 0.002 standard deviations; [Persico and Venator \(2021\)](#) find

that pollution from Toxic Release Inventory (TRI) sites lower test scores by 0.024 standard deviations; [Wen and Burke \(2022\)](#) show that additional 10 $\mu\text{g}/\text{m}^3$ of wildfire smoke-associated $PM_{2.5}$ lower test scores by 0.029% of a standard deviation; and [Ebenstein et al. \(2016\)](#) conclude that 1 standard deviation increase in the $PM_{2.5}$ declines student performance by 0.039 standard deviations. A recent report by United Nations Environment Programme (UNEP) and GRID-Arendal shows that wildfires are projected to be more frequent and more destructive ([Sullivan et al., 2022](#)). Compared with 2010-2020, wildfire events are expected to increase by up to 33% in 2050 and 52% in 2100. Wildfire smoke exposure for a school district has a mean of 29 days and a maximum of 108 days from 2010 to 2015 in my sample. This means, as a back-of-the-envelope estimate, the exposure could increase up to 164 days in 2100, which translates into a 6.56 percent of a standard deviation decrease in academic achievement, and approximately \$3,532 reduction in net present value of future income per year for each vulnerable student, or \$3.5 million per year for a school with 1,000 students.¹¹

1.4.2 School Day vs Non-School Day Exposure

Although I am unable to directly determine the mechanism behind the estimated effects based on the current data, by reporting the effects of wildfires separately for school days and non-school days, it may be possible to gain a better understanding of whether the effects are caused by the disturbance of school activities. To do so, I estimate the following regression. Throughout the analysis, I follow [Wen and Burke \(2022\)](#) to define non-school days as federal holidays, weekends, and summer breaks

¹¹I follow [Chetty et al. \(2014\)](#), [Park et al. \(2020\)](#), and [Wen and Burke \(2022\)](#) to calculate the loss in the net present value of future income. Specifically, [Chetty et al. \(2014\)](#) estimate that a 1 standard deviation increase in teacher quality can be translated into a 0.13 standard deviation increase in test scores and \$7000 net present value of future earnings for a typical 12-year-old student in their analysis. Therefore, a 6.56% of a standard deviation decrease in academic achievement can result in \$3,532 ($= 6.56\% * 7000 / 0.13$) net present value loss in earnings. As discussed by [Park et al. \(2020\)](#), these estimates are based on the assumption that the magnitude of the effect of teacher quality on academic achievement is comparable to the effect of wildfire smoke on test scores.

from June 15th to August 15th.

$$Y_{git} = \alpha + \beta_{\kappa}^{school} * \sum_{\kappa=3}^1 Exposure_{it-\kappa}^{school} + \beta_{\kappa}^{non-school} * \sum_{\kappa=3}^1 Exposure_{it-\kappa}^{non-school} + \theta X_{git} + \delta Z_{it} + \gamma C_{it} + \tau_i + \pi_t + \phi_g + \varepsilon_{it} \quad (1.2)$$

Table A.6, Table A.7, and Table A.8 presents the estimated effects using smoke plume days, cumulative $PM_{2.5}$ concentrations, and unhealthy air quality days measurement, respectively. Comparing the school day with non-school day exposure, the effects on ELA performance are mixed. 1-year lagged exposure to smoke plumes on school days results in more negative effects than on non-school days. The effects of cumulative $PM_{2.5}$ are only significant during the non-school day exposure. Unhealthy air quality during school days, which approximates exposure to severe wildfires, produces lasting effects. Recent severe wildfires do not significantly alter ELA test scores when they outbreak during school days but decrease middle school students' ELA scores during non-school days.

In contrast, math test scores are only affected by wildfire exposure on school days in all three measurement methods. Non-school day exposure does not generate statistically significant effects on math studies. This indicates that the adverse effects of wildfire on math studies are potentially caused by interruptions to school activity, such as school closures and class absences. Although my data does not allow me to compute school closures for all schools in the U.S., the CalMatters Disaster Days, which reports the school closures associated with wildfires in California, shows that from 2003 to 2018, the average close days for each school ranges from 0 to 8 per year.¹² Therefore, in California, where wildfires are frequent and severe, school closures due to such incidents are uncommon. Another explanation for these effects is the harm to cognitive development during in-school studies. Clinical research shows that air

¹²The CalMatters Disaster Days data can be viewed here: <https://disasterdays.calmatters.org/california-school-closures/category/wildfires/>

pollution damages children’s central nervous system and in-class attention (Calderón-Garcidueñas et al., 2015, 2008; Sunyer et al., 2017; Block et al., 2012). Given the low school closure rates due to wildfires, the decreased math scores are likely driven by health factors.

1.4.3 Mobility Tests

One potential threat to Equation 1.1 identification is students’ migration in response to wildfires. If students and their families realize the potential effect of wildfire on education and other aspects of their lives, and if they tend to move out of the most affected school districts, students who remain may be inherently different than movers. In this case, students that stay in school districts with high wildfire smoke risks are less likely to be responsive to wildfires. As a result of positive attrition, the effect of wildfire smoke could be either not significant or significantly positive.

Table A.9 presents the results of the mobility tests with at least 75% of population exposure, where I replace the Equation 1.1 dependent variable with the standardized total number of students and the student counts in each demographic and socioeconomic group in the school district. The number of Asian, Black, Hispanic, white, and economically disadvantaged students does not significantly change with exposure to wildfire-generated smoke, cumulative $PM_{2.5}$ concentrations, or unhealthy air quality.

1.4.4 Heterogeneous Effects

To test if the effect of wildfires varies across different groups of students, I re-estimate Equation 1.1 by race, ethnicity, gender, and socioeconomics, and present the preferred estimation results with 75% of population exposure. The estimates are limited to school districts that separately report the test scores by these characteristics, therefore the number of observations is smaller than the full sample estimates.

Smoke Plume Exposure Figure A.4a presents the estimations for all students in the sample as a benchmark, and Figure A.4b to Figure A.4e show the heterogeneous effects for all grade students in each race/ethnicity group using smoke plume measurement.¹³ In Figure A.4b, Asian students' ELA scores decrease by 0.008 standard deviations as a result of 1-year lagged smoke exposure, which is the largest negative effect among all race/ethnicity groups, but their math studies are not affected. Black students, as presented in Figure A.4c, also receive sizable negative effects on both ELA and math studies. Among the four race/ethnicity groups, white students are the least affected by smoke plumes.

Figure A.4f and Figure A.4g compare the effects between female and male students. Female students' ELA scores are more adversely affected than male students with the 1-year lagged exposure, but the effects are temporary. Male students, on the other hand, are additionally affected by 3-year lagged smoke plume. Smoke exposure also lowers economically disadvantaged students' academic performance, but the magnitudes are comparable to the overall effect, as shown in Figure A.4h.

Besides, the estimated results also show that Hispanic, white, and female students' scores are positively correlated with lagged smoke exposure. Appendix Table A.20 - Table A.22 show that these positive trends are driven by middle school students, which is consistent with the conclusion from Table A.3. Therefore, the unobserved time-varying factors that lead to positive associations are potentially related to Hispanic, white, and female students in specific.

Cumulative $PM_{2.5}$ Exposure In Figure A.5, Black students' ELA studies experience the greatest magnitude of negative impact from cumulative wildfire-attributed $PM_{2.5}$. The negative effects on math are mainly concentrated on white students, but they have negligible negative effects on ELA scores. Both female and male students' scores are lowered by $PM_{2.5}$ in general, but the effects are disproportionately applied between education levels. As demonstrated in Appendix

¹³The detailed coefficients and standard errors for primary and middle school students are presented in Appendix Table A.18 - Table A.24

[Table A.23](#) Panel B, for male students, the effects on ELA are solely applied to students in primary school, while the effects on math are concentrated on middle school students.

Unhealthy Air Quality Exposure [Figure A.6](#) shows estimates using unhealthy air quality days to proxy severe wildfire exposure. Hispanic students' ELA scores are heavily influenced by recent exposure, but the effects are not long-lasting. And white students are significantly affected by 3-year lagged exposure, with magnitudes greater than 0.06 standard deviations for both subjects. Although the coefficients are not statistically significant for Asian students as a whole, middle school Asian students actually experience the largest impact. Appendix [Table A.18](#) Panel C shows that additional 10 days of unhealthy quality in 2-year prior to tests lower middle school Asian students' ELA scores by 0.208 standard deviations, which is about 8 times larger than the overall effects for all students in the sample.

When comparing the heterogeneous effects by gender, female students' ELA scores are responsive to recent exposure only, and males students' ELA scores are affected by the 3-year lagged exposure with larger magnitudes. The effects on their math scores are similar. Albeit the estimates for economically disadvantaged students are comparable to those for their counterparts in smoke plume and $PM_{2.5}$ measurement, economically disadvantaged students receive larger magnitudes of effects from unhealthy air quality days. This is especially true for students in primary school (Appendix [Table A.24](#)). These results indicate that younger economically disadvantaged students are more vulnerable to fierce wildfires, and the effect is long lasting. This could widen the achievement gap between economically disadvantaged and other students.

1.4.5 Placebo Tests

Finally, I utilize placebo tests to determine if students' academic achievement responds to wildfire exposure in the future. Since the exact test time is unknown, to

ensure students are matched to wildfires after testing in the spring, I measure future exposure starting in the summer, which ranges from July 1st of the school year t to June 30th of school year $t + 1$.

[Table A.10](#) presents the results with 75% of the population exposure specification.¹⁴ In all three specifications presented in Panel A, B and C, middle school students' academic performances are shown to be positively related to future wildfires. This means that there may be dynamic and unobserved factors that lead middle school students to have higher than expected achievement in areas with wildfire exposure in the near future. This would work against the hypothesized negative effects and bias [Equation 1.1](#) estimated effects upward. The negative effect of wildfire on students' achievement could have been larger if these factors for middle school were controlled. This also helps explain counterintuitive positive lagged coefficients for middle schools in [Table A.3](#). My heterogeneity analysis reveals that such positive effects are mainly driven by Hispanic, white, and female students, as shown in Appendix [Table A.20](#), [Table A.21](#), and [Table A.22](#), respectively. This will be further examined in detail in my future studies.

1.5 Discussion

In this study, I estimate the effect of wildfire smoke and the associated pollution on primary and middle school students' ELA and math test scores across the United States. Results show that an increase of 10 wildfire smoke days within 1 year prior to tests significantly lowers students' ELA and math test scores by 0.003 and 0.004 standard deviations, respectively. Such effects are transitory as students are not negatively impacted by lagged exposure. One standard deviation increase in cumulative $PM_{2.5}$ concentrations decreases test scores by similar magnitudes. However, more severe wildfires, which produce unhealthy air quality, tend to have

¹⁴The estimations with 25% and 50% of population exposure are in Appendix [Table A.25](#) – [Table A.26](#).

lasting effects, and the lagged effects mainly apply to students in primary school. My results also suggest that the effects of wildfire-produced unhealthy air quality on primary school students have larger magnitudes than that for students in middle school. These findings indicate that primary school students are more vulnerable to less frequent but more severe wildfires and the negative impacts could last for three years. Middle school students are only impacted by wildfires in the short term.

In addition to the overall negative impact of wildfires on students' academic performance, this study also highlights how natural disasters contribute to educational outcome disparities across race/ethnicity, gender, and income. Asian students' math achievements are not significantly affected by wildfires, but their ELA study in middle school receives the largest impact using all three measurement methods among the four racial/ethnic groups. Black students in primary school are most vulnerable as their academic achievement in both subjects falls by a remarkable size with wildfire smoke, $PM_{2.5}$, and unhealthy air quality exposure. And white students receive lasting negative effects on both subjects by the lagged unhealthy air quality exposure. When I compare the effects by gender, female students' academic achievements are reduced by recent exposure, while the effects last longer for male students. When I examine the effect by income, primary school students who are economically disadvantaged are disproportionately harmed when exposed to additional unhealthy pollution by wildfires.

Wildfires are shown to unevenly affect students' academic performance across race, ethnicity, gender, and income, but patterns of results vary by exposure measurement methods, years of exposure, and education level. However, this is not a surprising finding as the heterogeneity of environmental factor effects is also shown to be mixed in related literature. For example, [Ebenstein et al. \(2016\)](#) find a pattern that men and students from lower-income families are more likely to be affected by $PM_{2.5}$. The results by [Park et al. \(2020\)](#) are more mixed: lower-income students receive larger effects than higher-income students by prior year heat, but this is reversed when evaluating the effect of four-year cumulative heat. And [Persico and Venator \(2021\)](#)

show that pollution from Toxic Release Inventory does not differently affect students by race, gender, and socioeconomic status.

My results are consistent with findings in the medical and health literature. [Mohai et al. \(2011\)](#) show that high pollution concentration is associated with more test failures in higher grades than in lower grades. Environmental pollution is shown to harm children’s central nervous system and cognitive ability ([Suglia et al., 2008](#); [Calderón-Garcidueñas et al., 2015, 2008](#)). [Wang et al. \(2009\)](#) find that higher air pollution levels correlate with worse neurobehavioral performance for children. And [Seifi et al. \(2022\)](#) find that higher $PM_{2.5}$ and PM_{10} exposure is associated with lower IQ for children between age 6 and 8.

Overall, my results provide evidence from an educational aspect to call for more investment in environmental protection and wildfire prevention. In addition, more attention should be directed to Black students and economically disadvantaged students in primary school, who are more negatively impacted. Asian students in middle schools also are in need of additional help in the ELA study as they receive the largest size of effect among the racial/ethnic groups in this analysis. This study also reveals the details of gender differences in ELA and math studies with exposure to wildfires. Schools should provide more support to help eliminate gender gaps in the event of wildfires. Thanks to the large sample size of district-level education performance, and the precise estimation of the daily wildfire smoke plume and grided $PM_{2.5}$ values, this study offers systematic evaluations and insights on the causal relationship between wildfire smoke and education across the United States. Due to the increasing number of wildfires worldwide induced by climate change, the knowledge offered in this study will be increasingly important.

However, my study is still limited in the following ways: first, my estimation only focuses on wildfire smoke exposure from one to three years before the test windows. Accumulative long-term effects are not identified. Second, the exact test time in each school district is unknown and thus I can only collect test windows that are determined by the states’ educational agencies. As a result, I conservatively use the

first day of the test window as the test start time. In this regard, the effect of wildfire exposure could be underestimated if it breaks out right before the actual exam starts but after the first day of the test time window.

Third, due to the limitation of the dataset, I am not able to clearly identify all the underlying mechanisms. Although in general, the three measures of wildfire exposure indicate negative effects on education, estimates of cumulative $PM_{2.5}$ concentrations and wildfire-produced unhealthy air quality do not align across subjects and education levels. Additional exposure to $PM_{2.5}$ only decreases ELA scores of primary school students and decreases students' math scores in both primary and middle schools. Middle school students are only responsive to the unhealthy air quality within 1 year prior to tests while primary school students are more influenced by the lagged exposure. It is likely that some unidentified factors would lead to such differences. The comparison between school day and non-school day measurements provides some implications that the impairment of the central nervous system and brain development could affect students' math studies, but further investigations are still in need.

Fourth, the precision of smoke plume boundaries that are employed in this study suffers from inherent satellite data limitations. Smoke days may be underestimated if the ground is covered by snow or clouds (Heft-Neal et al., 2022; Vargo, 2020; Di et al., 2019). Night smoke is also unidentified from satellite imagery (Heft-Neal et al., 2022; Di et al., 2019). The height of the smoke plume to the ground is not observed from satellite imagery, nevertheless, this should not raise concerns in this study. A smoke plume that is closer to the ground is believed to generate higher pollution levels, and this is captured by the gridded $PM_{2.5}$ concentrations.

Additionally, when matching the census tract level population to school districts and smoke plume boundaries, I assume the population is uniformly distributed across the tract. The within-tract population distribution information could possibly lead to a much more accurate estimation of students' wildfire exposure.

Future research that addresses the limits discussed above would substantially increase the precision of wildfire effects estimates. Furthermore, non-linear model

estimates would help explain if students with more exposure are disproportionately more or less affected. Data on ventilation systems in each school would allow me to examine if ventilation could help alleviate the adverse effects of wildfires.

Chapter 2

Does Career & Technical Education Move with the Labor Market?

Abstract

A large and growing volume of research has shown that U.S. college and university students consider the broader economy and labor market when selecting a major. It is unclear, however, if labor markets affect high school students' choice of coursework, or if high schools make industry-relevant programs more or less available in response to labor market dynamics. We study whether positive and negative labor market signals in the manufacturing industry affect high school course enrollments and course offerings in manufacturing. Focusing on public high schools in Tennessee, we find evidence that grade 10-12 students are more likely to take a manufacturing class after a manufacturing jobs announcement in their county. Lower achieving students are particularly responsive to jobs announcements, as well as males, Hispanic students, and students with an identified disability. Students are less responsive to local layoffs in manufacturing, to the number of jobs included in layoff or jobs announcements, and to signals from the broader metro or non-metro area. Estimated effects are small and persist 1-2 years. We also find that 9th grade students are less likely to enroll in manufacturing shortly after local jobs announcements, which would be consistent with crowd out in capacity constrained courses. Crowd out and small effects may be due to differences in how students and schools respond to local labor market signals. The likelihood that the schools offer manufacturing at all is more affected by the volume of metro-area layoffs than by jobs announcements or county-level signals.

2.1 Introduction

Education is essential for the long-term benefits of both individuals and society. More access to education significantly increases wages and reduces the incidence of adult poverty ([Jackson et al., 2016](#)). An individual’s investment in human capital is influenced by perceptions about future wages, financial security, job quality, amenities, and location, among many other factors. In the United States, students exercise a lot of discretion in how long they persist in school and what they choose to study. As they are making such decisions, they consider tuition costs, aptitude, preferences, as well as employment opportunities and potential earnings that lead from different educational paths ([Dynarski et al., 2021](#); [Wiswall and Zafar, 2015](#)). Likewise, new signals about the growth or contraction of particular industries could alter student specializations before they complete their education ([Han and Winters, 2020](#); [Blom et al., 2021](#); [Liu et al., 2019](#)). Policymakers often call for alignment between schools and labor market needs ([Gonzales and Gang, 2019](#); [Education Commission of the States, 2019](#); [Scott and Thompson, 2019](#); [Tennessee Department of Education, 2020](#); [State Council of Higher Education for Virginia, 2020](#)), reflecting public interest in coursework that develops skills and knowledge used in the workforce later on.

Research suggests that college and university students update their view of a field once they learn what people earn after they graduate with a degree in that field ([Wiswall and Zafar, 2015](#)), they move away from fields experiencing layoffs ([Acton, 2021](#)), and during recessions, they tend to move toward majors in higher-earning fields ([Blom et al., 2021](#)). Most of college graduates report that they work in jobs that are related to their degree field, more so if they are younger or have an advanced degree ([Robst, 2007](#)). About one in three U.S. high school graduates do not enroll directly in college, however, and more do not complete a college certificate or degree. This number has even been increasing since 2018 ([Bureau of Labor Statistics, U.S. Department of Labor, 2022](#)). For them, high school coursework may be the last free

opportunity to explore career interests and develop skills that offer a return in the labor market. But it is not clear if high school students have the same opportunity or inclination to select or change their coursework in response to labor market dynamics.

Career and technical education (CTE) programs offer one way for high school students to sample or invest in particular industries. Throughout much of the United States (including Tennessee, our setting for this study), CTE is offered as one or more elective sequences within comprehensive high schools. The modern organization of CTE is around “career clusters” that cover a broad, nearly comprehensive set of industries as well as occupations requiring every level of education. CTE, formerly “vocational” education, is no longer exclusively targeted toward students going to work immediately after high school.¹ So it is possible for labor market signals to affect CTE enrollment across a broad range of students. But we know very little about why or how students select into particular CTE programs, or more generally, if the labor market affects students’ choice of coursework in high school.

We test the hypothesis that labor market signals matter for high school students’ human capital investments in manufacturing. Specifically, we study whether manufacturing course enrollments in Tennessee high schools are affected by area layoffs or publicly subsidized jobs in manufacturing firms. Students’ course investments are restricted by what is available in their schools. Whether or not enrollments change in response to changes in the labor market might be credited to changes in course offerings. To investigate school supply as a potential mechanism, we also test whether manufacturing becomes more or less available in response to local layoffs and new jobs.

Manufacturing is an ideal setting to find evidence of student alignment with local labor markets. First, manufacturing coursework is directly aligned with the skills and knowledge applied in a well-defined industry. Other CTE clusters, such as Business Administration, Information Technology, or Finance, offer diffuse pathways to many different industries. Second, although a considerable number of students

¹The nationwide semantic shift from “vocational” education to “career and technical” education in the 1990s was in response to a negative stigma attached to vocational programs.

who pursue higher education eventually opt for CTE courses at their high schools, those specifically enrolling in manufacturing coursework might be disproportionately less likely to go on to college than students in other clusters, and, therefore more receptive to short-term changes in the local labor market. A majority of jobs in manufacturing do not require a college degree (Watson, 2017). Other clusters—such as Architecture and Construction, and Health Science—also align with well-paying jobs that do not require college, but in parallel with coursework focused on college-level occupations. And finally, the manufacturing industry issues a large number of signals of local growth or contraction. Like much of the U.S., Tennessee’s manufacturing industry rebounded unevenly after the Great Recession. Most of the state’s public incentives for jobs went to manufacturing firms, while at the same time, most of the state’s large layoffs were also among manufacturing firms.²

Although our setting may be rich with opportunities to identify evidence of student alignment with local industry, we nonetheless find small, inconsistent, and short-lived responses to signals of manufacturing growth and contraction. Results indicate that students are more likely to take a class in manufacturing 1-2 years following the announcement of new jobs in manufacturing firms in their county. A typical jobs announcement increases course-taking by a small amount, roughly 5% of the mean, driven by higher course-taking among grade 10-12 students. Some portion of that increase may be attributed to general optimism about local economic development, since manufacturing enrollment also increases by 2-3% following job announcements from industries other than manufacturing. Grade 9 students, by contrast, are somewhat less likely to take manufacturing after a jobs announcement, incompletely offsetting gains by upperclassmen. Grade 10-12 students’ responsiveness to local jobs is driven by lower achieving students, Hispanic students, males, and students with an identified disability. Students are largely unresponsive to local layoffs in manufacturing and layoffs or jobs from the broader metro or non-metro area

²In 2013, there was a notable surge in both manufacturing subsidized jobs and subsidized jobs in general, followed by a subsequent return to normal levels. Following the extraordinary rise in jobs was a decline in layoffs in 2014, but the change was less pronounced in the manufacturing industry.

surrounding their county. They also tend to be more responsive to job announcements than to the number of affected jobs. Schools, on the other hand, appear to be more responsive to the volume of manufacturing layoffs in the metro area, and not to the more localized positive signals that register with students.

Misalignment between student and school responses to the local labor market could help to explain crowd out of 9th graders from manufacturing courses as well as small and short-lived gains in course-taking among 10th-12th graders. This is not to say, however, that students and schools are unaware of labor market conditions or that students will be worse off for having a muted response to new jobs and layoffs. Manufacturing employment was somewhat volatile during our study period, and it is possible that students and their schools viewed growth and contraction signals from job and layoff announcements as temporary. More generally, the question of whether there are even short-term returns to alignment between workforce fluctuations and CTE is left to future research.

2.2 Related Literature

There is a small but growing literature on the effects of CTE coursework on later outcomes such as high school graduation, college enrollment, and early-career earnings (Mane, 1999; Dougherty et al., 2019; Hemelt et al., 2019; Kreisman and Stange, 2020). The literature is comparatively quiet on why students choose particular CTE pathways in high school, why they choose CTE at all, or why a school has some CTE pathways but not others. The labor market is a likely candidate for all of these choices. CTE purports to introduce high school students to in-demand workforce skills, so it is reasonable to expect that labor market dynamics will factor into student and school investments in CTE. Changes to the *local* labor market may be particularly relevant to student decisions, since most young adults live within a short distance of where they were at age 16 (Sprung-Keyser et al., 2022)

Despite abundant evidence that labor markets affect postsecondary knowledge and skill development (Long et al., 2015; Goulas and Megalokonomou, 2019; Han and Winters, 2020; Liu et al., 2019; Acton, 2021; Blom et al., 2021; Weinstein, 2020), we do not have a clear hypothesis that high school students behave similarly when selecting CTE courses or pathways. The assumption that schools can accommodate student demand for particular fields is less viable for high schools than it is for colleges and universities. High school students have less discretion in choosing their courses. Schools typically offer a limited set of CTE pathways, and the labor market may be more of a remote factor in younger students’ decisions about what to study.

Descriptive work on this question includes a report by Sublett and Griffith (2019), who find that “students take more CTE courses in fields that support more local jobs.” Carruthers et al. (2023) study alignment between CTE course-taking and local labor markets across five states, likewise, finding that the distribution of CTE concentrators in an area resembles the distribution of local employment across industries. Specifically, they report that a 10-percentage-point increase in the share of local jobs most related to a given CTE career cluster is associated with a 3-point increase in CTE concentration in that cluster.

Carruthers et al. (2023) find less evidence of dynamic alignment. As local employment shifts across industries, CTE course-taking changes very little. There are exceptions to that pattern, however, as Carruthers and Wu (2020) show for manufacturing in Tennessee. Looking across four cohorts, they find that a school’s course-taking in manufacturing changes roughly in proportion to changes in area manufacturing employment. Carruthers and Wu (2020) probe the causal relationship between manufacturing employment and related CTE course-taking using a shift-share identification strategy. Despite dynamic alignment between manufacturing employment and CTE, they find no chain-reaction effect of national manufacturing shifts on local course-taking.

We add to this literature by studying how positive and negative labor market signals about local manufacturing firms affect course enrollment in, and school supply

of, manufacturing CTE in high schools. Our analysis is similar to that of [Acton \(2021\)](#), who shows that in Michigan, local layoffs decrease student enrollment in related community college programs. We take this question to the high school context in Tennessee and study negative signals from layoffs as well as more optimistic signals from announcements of subsidized jobs.

2.3 Data

2.3.1 Education Data

We start with state administrative data describing the universe of public high school students in Tennessee, grades 9-12, school years 2013-14 through 2017-18. In addition to comprehensive course enrollment data for each student, we observe school assignment, gender, race, Hispanic ethnicity, and achievement on standardized tests.

Tennessee has very few schools dedicated exclusively to technical education, and almost all high schools offer CTE programs as elective sequences. The state has about 60 CTE programs organized into 16 career clusters that cover almost any occupation one could have, including those requiring a college degree.³ Courses in a CTE program can be very specific (such as Welding I and II in the Welding program, Advanced Manufacturing cluster), or relatively broad (such as Statistics in the Accounting program, Finance cluster). CTE has a large footprint in Tennessee high schools. Close to half of the state’s high school graduates concentrate in CTE

³Tennessee’s organization of career clusters follows the National Career Clusters Framework (<https://careertech.org/career-clusters>). The clusters are (1) Agriculture, Food, and Natural Resources; (2) Architecture and Construction; (3) Arts, Audio/Visual Technology and Communications; (4) Business Management and Administration; (5) Education and Training; (6) Finance; (7) Government and Public Administration; (8) Health Science; (9) Hospitality and Tourism; (10) Human Services; (11) Information Technology; (12) Law, Public Safety, Corrections, and Security; (13) Manufacturing; (14) Marketing; (15) Science, Technology, Engineering, and Mathematics; and (16) Transportation, Distribution, and Logistics.

(Goldring et al., 2021), meaning that they complete three or more courses in a program of study.⁴

We identify the CTE cluster associated with each high school course in the course enrollment data. We start with a crosswalk developed by the Tennessee Department of Education that links each course number with a CTE program of study (if applicable), and in turn, one of the 16 career clusters. The crosswalk was not populated for all school years, so we supplement this with hand-matching based on course names and CTE Program of Study matrices published by the Department of Education.

Table B.1 presents summary statistics for our sample, which includes 531 unique public high schools in Tennessee. Our first outcome of interest is student engagement in manufacturing coursework. We find that 3.4% of students enrolled in at least one manufacturing class in a given school year. It was uncommon for students to take more than one manufacturing class in a year: there were just 4.2 manufacturing sittings per 100 students, on average.⁵ With this in mind, we focus our student-level analysis on the extensive margin of manufacturing course-taking.

The rest of Table B.1 Panel A summarizes student demographics, gifted, special education, and English language learner designations, and economic conditions in students' surrounding counties. Just under half of students in the sample were female, 23.2% were Black, 7.5% were Hispanic, non-white, and 3.7% were Asian, Native American, or of multiple races or ethnicities, and the remaining 65.6% were white, non-Hispanic. Just under 5% of students were current or former English Language Learners, 1.3% were identified as academically gifted, and 14.0% had an identified disability. Our 2013-2018 sample window covers a long economic expansion. On average, students were in counties where 5.8% of the labor force was unemployed.

⁴The college enrollment rate is similar between CTE concentrators and non-concentrators. However, CTE concentrators are more likely to attend 2-year colleges and non-concentrators are more likely to attend 4-year institutions (Goldring et al., 2021).

⁵We observe each students' courses as well as the schools where they enrolled, but for students who attended multiple schools in a year, we do not observe where they took each course. So, we scale each student's total observed course load in manufacturing by the number of schools they attended.

Our second outcome of interest is whether or not a school offered any manufacturing classes. We infer this from student course-taking records. We designate schools as offering manufacturing in a given year if they had at least 3 students who took manufacturing classes during that year. As shown in Panel B of [Table B.1](#), this describes 41.9% of high schools in the sample. Manufacturing was not a large part of the curriculum, however, with 4 seats typically filled per 100 students. Schools enrolled 768 students, on average. Panel B also reports that 11.9% of school-year observations were from non-traditional school settings (adult, alternative, or virtual), and 5.3% were from charter schools.⁶

2.3.2 Labor Market Signals

We wish to study how signals from the labor market affect student engagement with manufacturing courses as well as school-level availability of those manufacturing CTE courses. To do so, we collect public notices of subsidized job creation, and of layoffs and closures, and transform these into measures of positive and negative labor market news about the local manufacturing industry.

The federal Worker Adjustment and Retraining Notification (WARN) Act requires that firms with 100 or more full-time employees publicize advance notice of mass layoffs or plant closings. Tennessee’s Plant Closings and Reduction in Operations Act extends this requirement to firms with 50-99 employees. We collect 511 layoff notices from Tennessee firms covering 2012-2017, totaling 47,630 lost jobs.⁷ Notices include firm name, dates (of the notice, and of the layoffs), city and county, and the number of affected employees. Using publicly available information about each firm and, where possible, each layoff, we flag those whose primary or secondary business in Tennessee was in manufacturing.

⁶[Table B.8](#) shows summary statistics for students who enroll in at least one manufacturing course and schools that offer manufacturing courses. As presented in Panel B, these schools are usually in a larger size with 945 students on average. Only 0.5% of these schools are charter schools and none of them is a non-traditional school.

⁷Although the WARN act only requires firms to report *mass* layoffs or plant closings, the number of layoffs in notices can be as small as 2.

To represent positive signals about CTE clusters, we collect 6,908 records of 2012-2017 public subsidies to Tennessee firms recorded by Good Jobs First. Good Jobs First is a nonprofit advocacy organization that tracks property tax abatements, training grants, loans, and other public subsidies from a variety of local, state, and federal sources. Subsidy records include firm name, location, subsidy program, and for 13% of subsidies, a number of associated jobs or training slots. We focus on subsidies attached to a specific number of jobs and identify firms whose primary or secondary business in Tennessee was manufacturing. Both layoffs and subsidized jobs are often accompanied by related articles in the media, and jobs announcements tend to be promoted by state agencies as well.

Panel C of [Table B.1](#) summarizes 2012-2017 layoff and job signals at the county level. We find that, on average, there were 0.305 manufacturing layoff notices per county each year describing 0.072 jobs per 100 people in the age 18-64 working-age population. There were 0.853 subsidized job notices describing 0.299 jobs per 100 people. Both represent large shares of the total number of layoffs and subsidized jobs. As shown in [Figure B.1](#), manufacturing accounted for 20-40% of all large layoffs in the state over this time period, and about half of all subsidized jobs. This underscores the importance of studying positive and negative labor market signals at the same time. The manufacturing industry experienced widespread layoffs in Tennessee over this time frame, but state and local governments were simultaneously recruiting and subsidizing manufacturing firms. In results to follow, we control separately for layoff and job notices, as well as the number of jobs affected by layoffs and firm entry or expansion.

2.4 Methods

We study two levels of decision-making about manufacturing courses in Tennessee high schools: student choices to enroll in manufacturing courses, and school choices to offer manufacturing courses. For both, our research design is informed by Acton's

(2021) analysis of layoffs and related enrollment in Michigan community college programs.

For the student-level analysis, we start with the following specification:

$$Y_{isct} = \beta LO_{ck} + \delta GJ_{ck} + \theta X_{isct} + \tau_s + \mu_t + \varepsilon_{isct}, \quad (2.1)$$

where Y_{isct} represents student i 's engagement with manufacturing coursework in school year t , while they were enrolled in school s located in county c . We measure engagement by the binary incidence of having taken any manufacturing courses during the year.

Variables of interest are LO_{ck} and GJ_{ck} , which represent the number of layoff notices, the number of layoffs, the number of new job notices, and the number of new subsidized jobs from manufacturing firms in county c , year $k \leq t$, per 100 working-age population in that county. We interpret the coefficients β and δ as causal effects of negative and positive labor market signals, respectively, on aligned course enrollment.

It is reasonable to believe that it may take time for students to adjust after observing these signals, so we include partially or fully lagged labor market signals in LO_{ck} and GJ_{ck} vectors. We observe the month of each WARN notice, but source data for subsidized jobs only report the calendar year of each deal between firms and governments. For comparability, we define the year of layoff and subsidized job notices as the calendar year k associated with each notice. School year t represents the year of the fall semester for a traditional fall-spring academic year. When $k = t$ in Equation 2.1, estimates represent the effect of layoff and job creation notices issued January - December, 201Y on aligned enrollment in Fall 201Y, Spring 201Y+1, and (less common) Summer 201Y+1. That is to say, the market signals could lag the beginning of the fall or spring semester by up to 8 or 12 months, respectively, although some signals will come after the beginning of the fall semester. Main results also control for $k = t - 1$ signals. We cannot account for any anticipatory effects of layoffs

or new jobs, although estimated effects are robust to additional controls for notices from $k = t + 2$.

Figure B.2 depicts the timeline of labor market signals and how they precede, overlap, and follow a given school year. Taking the 2014-15 school year as an example (top bar), the figure shows how labor market signals for year $t = 2014$ and year $t + 1 = 2015$ partially overlap the school year (middle and bottom bars). Year $t - 1$ signals would occur 1-2 years before the school year.

It is also reasonable to expect students to react to labor market signals outside of their home county. In some specifications, we aggregate LO_{ck} and GJ_{ck} to Tennessee's set of 14 multi-county Metropolitan Statistical Areas (MSAs) and non-metropolitan areas, as defined by the Bureau of Labor Statistics.

X_{isct} is a vector of time-varying student, school, and county covariates: the percentage of students who were Asian, Black, Native American, Hispanic, or of multiple/other races in each school; the percent who were female; the share of English language learners (ELL), former ELL, and gifted students; the percent of students with disabilities; an indicator for non-traditional schools; an indicator for charter schools; and the unemployment rate for each county and year. Lastly, τ_s is a school fixed effects that absorbs unobserved, time-invariant, school-specific heterogeneity, and μ_t is a school-year fixed effect that accounts for unobserved factors affecting course-taking choices over time, such as statewide efforts that may be correlated with subsidized jobs. We cluster standard errors by school.

We adopt a similar specification for our school-level analysis of the likelihood that manufacturing is offered at all:

$$Y_{sct} = \beta LO_{ck} + \delta GJ_{ck} + \theta X_{sct} + \tau_s + \mu_t + \varepsilon_{sct}, \quad (2.2)$$

where Y_{sct} is a binary variable equal to one if school s had at least three course enrollments in manufacturing, and X_{sct} controls are aggregations of student and school controls in Equation 2.1. Labor market signals, school fixed effects, and year fixed

effects are as defined in [Equation 2.1](#). We estimate Y_{sct} by linear probability and allow for correlated errors within schools. Findings are similar in nonlinear models, including logit for binary Y_{sct} and Poisson for the count of manufacturing course sittings.

2.5 Results

2.5.1 Student Manufacturing CTE Course Enrollment Outcomes

[Table B.2](#) presents estimates on the effect of county-level manufacturing labor market signals, both negative and positive, on the probability of students' aligned CTE course enrollment in high school. Odd-numbered columns report results for specifications where we control for the number of announced layoffs and subsidized jobs, which implicitly combines the standalone effect of notices themselves with the effect of the volume of affected jobs. Even-numbered columns present results from specifications that control separately for the number of layoff and job notices, and for the total number of affected positions. Columns 1-2 report findings for grades 9-12 together, and columns 3-10 focus on individual grade levels.

As noted in [section 2.4](#), concurrent results for year t reflect the effect of signals that partially lag the school year, and the lagged labor market signals in year $t - 1$ happened at least one year prior to course-taking. Our column 1 estimates for grades 9-12 indicate that students are not responsive to either layoffs or subsidized jobs. Column 2 shows that students are responsive to notices *per se* when we control separately for the number of notices and the number of jobs. The probability of taking a manufacturing class increases by 0.164 and 0.155 percentage points, respectively, following each additional concurrent and lagged subsidized job notice. This represents a 5% increase in the average course enrollment likelihood. The effect of negative labor

market signals is less remarkable; manufacturing enrollment falls by 0.098 percentage points per lagged layoff notice, but this is only significant at 10% level.

The effect of subsidized job notices is driven by students in grades 10, 11, and 12. For each additional notice that firms are entering or expanding in the county, grade 10-12 students are 0.163 - 0.238 percentage points more likely to enroll in manufacturing. This amounts to 4-6% of average enrollment, or 500 - 800 additional students in each grade. Grade 10 and 11 students are somewhat more responsive to job signals than grade 12 students. In addition, the probability of manufacturing enrollment for students in grade 10 is negatively responsive to each additional layoff notice from one year prior, by 0.251 percentage points.

Grade 9 students, however, are shown to be adversely responsive to the number of subsidized jobs in both specifications. Our results in column 3 and column 4 show that students in grade 9 are less likely to take manufacturing CTE courses when they see an increase in the number of aligned subsidized jobs. Following a jobs announcement sized at 1% of the local working-age population, the likelihood that a 9th-grade student enrolls in manufacturing falls by 0.346 to 0.539 percentage points. This amounts to an 18-28% decline relative to the 1.9% of 9th graders who typically enroll in manufacturing. A typical jobs announcement is sized at about 0.35% of the county workforce, which would translate into about 400 - 650 fewer 9th graders enrolled in manufacturing. If manufacturing classes are capacity constrained, it is possible that upperclassmen crowd out 9th graders from these courses in response to new manufacturing jobs in their county.

In [Table B.3](#), we focus on 10th-12th graders and report effects of labor market signals for several student subgroups described by ACT performance, race, Hispanic ethnicity, gender, current and former status as an English Language Learner (ELL), and disability identification. Looking first to dependent variable means across these subgroups, we find that manufacturing enrollment is most common for students with no ACT sittings or ACT scores less than 21 (5.0%), males (6.0%), and students with physical and/or learning disabilities (5.5%).

We divide the grade 10-12 sample into two groups according to their highest composite ACT score: students with a “college-ready” ACT score of at least 21 (which describes 40% of the sample), and students with either no ACT on record or whose best score was less than 21. When we compare high-ACT and low/missing-ACT students in columns 1-2, we find that lower-achieving students are more responsive to labor market signals. They are more likely to enroll in manufacturing following concurrent or lagged job notices (by 4-5% of the mean), and they are less likely to take manufacturing coursework following lagged layoff notices (by 3% of the mean, but weakly significant). Students who score higher on the ACT are not unresponsive to these signals, however, increasing their course-taking by 6% following lagged job notices. Counter-intuitively, higher-ACT students are also somewhat more likely to take a manufacturing class in proportion to recent layoffs. One additional layoff per 100 working-age population 1-2 years prior increases their course-taking likelihood by 0.218 percentage points (10% of the high-ACT mean). Potential explanations for high-achieving students’ positive response to layoffs might include countercyclical optimism for the sector given frequent fluctuation in the manufacturing industry, effects of area layoffs on ACT scores and college plans, or more open seats in manufacturing classes as lower-achieving students opt into other coursework.

Looking across [Table B.3](#) results by race, gender, and ethnicity, we find that the effect of subsidized job notices is relatively larger for Hispanic students than for Black or white students, larger for men than for women, and larger for students with an identified disability.

[Table B.4](#) reports results from two modifications of [Equation 2.1](#): a model that controls for layoff and job signals from year t , $t - 1$, and $t - 2$ (columns 2 and 4) and a model that controls for layoff and job signals from the broad metro or non-metro area around a student’s school (columns 3-4). For column 3-4 models, we assign each student and their school to one of 14 multi-county areas which include 10 metro statistical areas and 4 non-metro areas as defined by the Bureau of Labor Statistics. The sample in each model covers school years 2014-15 through 2017-18, one less than

in [Table B.2-Table B.3](#) to include one additional year of lagged labor market signals. Despite somewhat different time periods and specifications, column 1-2 results for county-level job notices are of a similar sign and significance as in our main analysis. These models also suggest that students take less manufacturing following year $t - 1$ layoff notices, but offsetting coefficients for layoffs render the net effect inconclusive.

Our first takeaway finding from [Table B.4](#) is that students are more responsive to local job announcements in their county than to job or layoff announcements in the broader metro/non-metro area. Manufacturing course-taking rises by a significant 5-6% of the mean for each additional job notice in a student’s county 1-2 years prior, but by no more than an insignificant 1.7% for each notice in the surrounding area. Our second takeaway is that year $t - 2$ signals do not appear to register a significant effect on course-taking. This suggests that students’ alignment with new job notices fades after two years.

2.5.2 School-level Manufacturing CTE Course Availability

Do schools respond to labor market signals with more or less aligned course availability? We estimate [Equation 2.2](#) where the dependent variable is the binary incidence of manufacturing course availability at a given school and year. This gives us a sense of high schools’ responses to labor market signals from the course supply perspective. We limit our sample to school years 2014-15 to 2017-18 to include labor market signals from years t , $t - 1$, and $t - 2$. [Table B.5](#) reports estimated effects of labor market signals from the county (columns 1-3) and from the metro/non-metro area (columns 4-6).

At the county level, our results in column 1 indicate that course availability in schools is not affected by the number of layoffs or subsidized jobs. Inferences are similar in column 2, where we control for the number of layoff and job notices separate from the number of affected jobs. Within 1-2 years of a manufacturing layoff notice, there is a 3-6% decrease in the likelihood of manufacturing course availability,

and within 1-2 years of a subsidized job notice, there is a 4-5% increase in course availability. But the number of notices and affected jobs are positively correlated with each other, and we find an offsetting relationship between course offerings and the number of jobs that were affected by layoff or entry/expansion notices. For example, we estimate that course availability increases by 2.4 percentage points with each additional jobs notice in year t . But it is also shown to decrease by 6.4 percentage points due to additional population-scale subsidized job. At the average number of new subsidized jobs per 100 in the workforce (0.299), we would expect that increase to be only 0.5 percentage points (attenuating 2.4 percentage points by -6.4×0.299). School course availability is not responsive to the 3-year lagged labor market signals, i.e. year $t - 2$, as shown in column 3.

At the metro/non-metro level, in contrast to students' course enrollment, schools appear to adjust their course availability in response to layoff signals. The probability of manufacturing course availability significantly decreases by 12-16% of the mean in response to each population-scaled layoff within 2 years prior (column 4). These estimated effects are large, but weakly precise or sensitive to specification. When we disaggregate the effect of notices from layoff/job volumes (column 5), coefficients for metro-level signals are statistically insignificant. When we also control for $t - 2$ signals in the column 6 specification, schools appear to have a counter-intuitive, negative relationship with subsidized job notices from 2-3 years prior. But the 0.68 percentage-point coefficient is equivalent to only a 1% change.⁸

⁸We also test the effect of labor market signals on course availability using non-linear models. Specifically, we use the Poisson fixed effect model to examine the effect on school-level course-taking counts, and use the logistic fixed effect model to examine the effect on the probability of school course availability. In the Poisson model, estimating samples necessarily omit schools without any manufacturing course enrollments during the sample window, and logit results are limited to schools that switched between offering and not offering a manufacturing course. About half of the observations are dropped in the Poisson model, and 75% of the observations are dropped in the logistic model. Results, shown in [Table B.9](#), indicate that schools in these limited samples are largely unresponsive to labor market signals.

2.5.3 Robustness and Falsification Tests

One threat to interpreting results for layoffs and subsidized jobs as causal is the possibility that these episodic changes in local jobs were correlated with unobserved trends in student interest in particular fields, or in school capacity to offer particular fields. For example, perhaps students perceive that the manufacturing industry is faltering and adjust their course-taking well before it comes to the point of layoffs. Or, perhaps growth in manufacturing interest in area schools is a selling point as governments recruit related businesses. With these possibilities in mind, [Table B.6](#) reports [Equation 2.1](#) and [Equation 2.2](#) results for *future* layoff and subsidized job-related announcements, testing if there is an observed change in the likelihood of course enrollment and course availability before the signal is released. Since the labor market information is recorded by calendar year, layoff or job announcement in year $t + 1$ potentially overlap school year t by up to 8 months, as discussed in [section 2.4](#) and depicted in [Figure B.2](#). To avoid the concurrent effect during the overlapped timeline, we define future labor market signals as those in year $t + 2$. We additionally control for current and once-lagged signals, limiting the sample in columns 1-4 to school years 2013-14 - 2015-16.

If students and schools did not anticipate layoffs or subsidized jobs, there should be no effect of signals yet to come on current course-taking or course availability. Indeed, point estimates for future labor market signals in [Table B.6](#) are statistically insignificant, with the exception of the effect of layoffs on course availability in column 3, which presents a significant coefficient at 10% level. This effect is diminished when we separately control notices and the number of layoffs/jobs.

It is possible for us to mistake student reactions to economic development in general as their reactions to manufacturing announcements. That is, if manufacturing employment is procyclical or attuned to area growth more generally, students may be more likely to take manufacturing courses following *any* new job notice. Manufacturing employment tends to fall during recessions ([BLS, 2023](#)), so it is

reasonable that manufacturing enrollment could behave similarly. This kind of reaction to local labor markets would be related to the broader question of CTE-workforce alignment but distinct from the question of whether students sync their coursework to signals from specific industries. To test this possibility, we use [Equation 2.1](#) and [Equation 2.2](#) to test whether students are more or less likely to take manufacturing in response to layoff and job signals from industries *other than* manufacturing. [Table B.7](#) reports findings.

In Panel A, we find that students' manufacturing course enrollment is not significantly affected by layoffs in other industries, either the number of layoffs or layoff announcements *per se*, for both the county and metro/non-metro level. However, manufacturing enrollment significantly increases in response to concurrent subsidized job notices, by 2.5 - 3.4% of the mean. Students are additionally affected by new jobs in other industries 1-2 years prior. These estimates are smaller in magnitude than the estimated effect of an additional manufacturing jobs notice. Nonetheless, [Table B.7](#) results indicate that manufacturing course enrollment is responsive to general perceptions of economic and labor market development due to more subsidized job notices and job creations, regardless of industry.

In Panel B, we show how layoffs and subsidized jobs from other industries affect manufacturing course availability. Similar to our findings for manufacturing signals, schools appear to respond to a different set of signals than students, if any. Column 1 indicates that school-level manufacturing is not responsive to signals from industries other than manufacturing in the county, unlike student responses to local non-manufacturing jobs. In the broader metro/non-metro area, however, there is a large negative association between non-manufacturing layoffs in year $t-1$ and the likelihood that a school offers manufacturing. We would expect the mean number of non-manufacturing layoffs (0.601 per 100 in the workforce) to decrease the likelihood that a school offers manufacturing by 6 percentage points, a 14% reduction. Also different from student reactions, school-level availability does not appear to adjust in response to new metro-area jobs in non-manufacturing industries.

2.6 Discussion

Does CTE move with the labor market? Results point to small and short-lived changes in manufacturing enrollment in response to manufacturing job creation notifications in a student’s county. Some part of those changes may be related to local economic development in general, since students were more likely to take manufacturing in response to signals from other industries as well. Students were unresponsive or less responsive to local layoffs, the number of affected jobs described by notifications, as well as job or layoff notifications from throughout their metro or non-metro area. This is consistent with related work finding that area employment plays a limited role in shaping aligned CTE course-taking (Carruthers et al., 2023; Carruthers and Wu, 2020). Our results stand in contrast with much of the related literature on college and university students, whose program or major choice appears to be more substantially responsive to layoffs and industry perceptions.

What might explain the difference? Constraints on the school supply of aligned coursework could be one factor. Our Table B.5 results for the availability of manufacturing coursework supports this channel. Schools may have scaled manufacturing back in response to metro area layoffs, but this represents a different set of signals than the more local jobs notifications that affected grade 10-12 student enrollment. Another indicator of capacity constraints is found in Table B.2, where results indicate that those upperclassmen may have crowded 9th graders out of manufacturing courses. More qualitatively, a large majority of state CTE directors report “a moderate or severe CTE teacher shortage in at least one Career Cluster” (Advance CTE, 2020). Advanced Manufacturing is an “in-demand” industry in Tennessee (Scott and Thompson, 2019) and the recipient of more subsidized jobs than any other cluster (Figure B.1), but delivering courses such as Welding or Mechatronics involves equipment, expertise, and funding beyond that required for a regular classroom.

Students' time constraints could be another factor. Tennessee students need 22 credits to graduate high school, 19 of which usually come from core subjects. CTE coursework is organized as a sequence or sampling of electives that can make up the other 3 credits, competing with advanced math and science, humanities, Advanced Placement, fine arts, and other electives. College and university students also invest in core subjects, but the necessity of choosing a major does not generally have an analog in high school.

A final possibility—and a motivation for future research—is that the labor market signals we study here may have been too small to elicit significant movements toward more manufacturing enrollment or availability. Large economic shifts such as natural resource booms in Texas ([Kovalenko, 2023](#)), widespread plant closing in Michigan ([Acton, 2021](#)), or Ford's \$11 billion investment in West Tennessee after our sample window may have resulted in larger changes in related coursework. Another open question is whether, and for how long, students benefit from CTE coursework that is well aligned with workforce needs. Addressing this question could potentially support the cost and benefit analysis for both students and high schools, while also furnishing policymakers with substantiated evidence regarding CTE programs.

Chapter 3

Free College Opportunity for Disadvantaged Schools: Assessing Students' Responses to the Flagship Scholarship in Tennessee

Abstract

In Fall 2007, the University of Tennessee, Knoxville (UTK), which is the flagship university of the state, initiated the Flagship Scholarship program. This program offers a "free college" opportunity to students from designated high schools if they are admitted to UTK. We employ difference-in-differences estimations to examine the effect of this program, which reduces many of the obstacles of application complexity, cost uncertainty, and information mismatch. Our results show that the Flagship Scholarship program substantially boosts targeted high school students' enrollment in UTK by up to 81%. Additionally, among all college enrollees, we find that students eligible for the Flagship program are more likely to attend higher-quality colleges. However, this program does not alter the choice of college sectors or increase overall college attendance.

3.1 Introduction

Postsecondary education yields higher income for students in the long run ([Card, 1999](#); [Hoekstra, 2009](#); [Oreopoulos and Petronijevic, 2013](#); [Baum et al., 2013](#); [Zimmerman, 2014](#)). However, high tuition and fees generate financial burden, especially for low-income students. Such financial hardship may dissuade academically qualified students from applying to selective colleges ([Hoxby and Avery, 2012](#); [Dynarski, 2003](#)). Furthermore, the high price of postsecondary education may lead students to put less effort into high school studies and to give up further education opportunities. These postsecondary educational differences reinforce and perpetuate lower incomes as students move into adulthood ([Machin and Vignoles, 2004](#)).

There are numerous scholarship opportunities available from both federal and state governments to alleviate the financial burden of higher education. In addition, colleges and universities provide institutional aid to students. Despite these efforts, low-income students remain underrepresented in higher education. Studies have shown a persistent gap in college enrollment rate between students from low-income and high-income families ([Carneiro and Heckman, 2002](#); [Belley and Lochner, 2007](#); [Bailey and Dynarski, 2011](#); [Chetty et al., 2017](#)). For individuals born between 1961 and 1964, 58% of students from the top 25% household income bracket pursued higher education, whereas only 19% of those in the lowest quartile enrolled in college ([Bailey and Dynarski, 2011](#)). In the 1980s, the college enrollment rate increased from 62% to over 70% among students in the top half of family incomes, but the participation rate remained below 45% for students from the bottom quartile income family ([Carneiro and Heckman, 2002](#)).

One reason for the low enrollment rate of low-income students is the complexity and uncertainty of financial aid applications ([Long and Riley, 2007](#); [Bettinger et al., 2012](#); [Burland et al., 2022](#)). Many financial assistance programs require a separate application in addition to the college application package and Free Application for

Federal Student Aid (FAFSA), and the award is not guaranteed¹. Furthermore, information asymmetry can potentially reduce financial aid applications if low-income students lack full information about financial aid availability and are unable to accurately estimate college costs (Dynarski et al., 2021; Marx and Turner, 2019; Long and Riley, 2007). These together can play an important role in students' college decisions (Bettinger et al., 2012; Dynarski et al., 2021; Dynarski and Scott-Clayton, 2006, 2008, 2013; Page and Scott-Clayton, 2016).

In this study, we examine the effect of a program that reduces the obstacles of application complexity, cost uncertainty, and information mismatch simultaneously. The Flagship Scholarship program, originally named Tennessee Promise, was implemented by the University of Tennessee, Knoxville (UTK) starting in fall 2007 (Collins, 2006). It was proposed followed by the end of the *Geier* lawsuit case regarding racial desegregation in Tennessee higher education (Collins, 2006; González, 2017) and the termination of legitimacy to award scholarship based on race. The Flagship Scholarship's goal was to enhance the university's outreach and diversity and help students from disadvantaged schools get high-quality postsecondary education. Students who attend Flagship high schools in Tennessee are awarded this scholarship if they are admitted to UTK, the state's flagship college. No additional effort is required to receive and retain this scholarship besides filing a FAFSA. The Flagship Scholarship, accompanied by the HOPE and other university-provided scholarships, would be sufficient to cover four years of tuition and mandatory fees (Collins, 2006). This is essentially a full tuition guarantee for students.

The initial Flagship Scholarship program included 35 high schools in Tennessee. These schools were selected because of the economically and academically disadvantaged circumstances their students faced, and are serving mostly minority students. Students from these high schools usually encountered financial obstacles

¹Federal financial grants usually only require FAFSA, for example, the Pell Grant. Some state-level grants require additional applications, for example, Tennessee Promise Scholarship and New York's Excelsior Scholarship (Bell, 2021; Poutré and Voight, 2018; Scott-Clayton et al., 2022). College-funded scholarships and private scholarships usually have their own requirements.

when applying to college, historically had lower chances to enroll in UTK, and were potential first-generation college students (Collins, 2006). Five of these high schools closed or consolidated since then, and the program expanded to add 8 new high schools in school year 2021-22 (Stephens and Payton, 2020).² As of 2023, 38 public high schools in Tennessee are eligible for this scholarship.

We use state administrative data from school years 2005-06 through 2008-09 to evaluate the effect of the Flagship Scholarship on students' postsecondary enrollment. Specifically, we answer two research questions. First, does the Flagship eligibility increase the likelihood of enrolling in UTK or any other college, or alter students' college sector choice? And second, conditional on enrolling in college immediately after high school, does the program impact students' decisions regarding college quality?³ We employ difference-in-differences estimation models to compare students who enrolled in designated Flagship high schools, before and after the implementation of the Flagship Scholarship, to similar students who enrolled in ineligible high schools. In our model, we construct the control group in three ways to provide comparisons in more than one dimension: students who graduated from any non-Flagship public high schools in Tennessee, students from non-Flagship high schools that are similar to Flagship schools with low college enrollment, and students from non-Flagship high schools that are similar to Flagship schools in terms of graduation rates, attendance rates, and students' Algebra I and English II end-of-course exams proficient rates.

Our findings show that Flagship Scholarship eligibility significantly increases enrollment in UTK immediately after high school graduation by 1.3 - 1.8 percentage points, equivalent to 59% - 81% of the pre-treatment enrollment rate. Event study estimations suggest that this effect is consistent and tends to increase over time. However, eligibility does not alter students' decisions on college attendance in general

²Five closed or consolidated Flagship high schools were all in Memphis: Frayser Middle/High School, Northside High School, South Side High School, Treadwell High School, and Westside High School.

³In this study, we assume that higher quality colleges are those with higher per-student college expenditure, lower admission rates, and higher graduation rates.

and does not significantly change their college sector choices. Among college enrollees, we observe that the Flagship program makes students more likely to attend more selective colleges with 2.163 - 3.360 percentage points lower admission rates. We find no statistically significant evidence on per-student college expenditure and graduation rates. Collectively, these findings suggest that Flagship eligibility shifted students to UTK from somewhat less selective 4-year colleges and universities.

One potential threat to the causal interpretation of our results is that high-achieving students might be sorted into Flagship high schools in order to take advantage of this scholarship program. To test this hypothesis, we examine if Flagship eligibility has an impact on the shift in student composition in Flagship high schools and find little evidence.

We divide our sample into subgroups to investigate heterogeneous effects across race and gender. We find that effects on increased UTK enrollment are more pronounced for female and Black students, and effects on college quality dimensions are similarly applied to female and male students.

Understanding how the Flagship Scholarship program helps disadvantaged students is essential, especially when the program was expanded to include more high schools in Tennessee recently. Our study shows that this program successfully increased enrollment in the targeted university. Although we cannot uncover the mechanism details with certainty, previous studies suggest that information symmetry, financial aid, and application simplification factors may have been contributing factors ([Marx and Turner, 2019](#); [Carruthers and Fox, 2016](#); [Bettinger et al., 2012](#); [Page and Scott-Clayton, 2016](#); [Page et al., 2019](#); [Andrews et al., 2020](#); [Dynarski et al., 2021](#)). Our results emphasize the importance of offering assistance to address multiple barriers at the same time.

3.2 Literature Review

Previous literature shows that the implementation and effectiveness of financial assistance policies for students depend on many aspects. One of them is the information asymmetry. Accessing sufficient information about financial aid opportunities is shown to be essential, especially for economically disadvantaged students. Survey results show that more than 70% of high school graduates do not know what the FAFSA is used for, and how FAFSA could help them, not to mention receiving benefits from it ([Johnson et al., 2011](#)). [Marx and Turner \(2019\)](#) use experimental evidence to show that more information on student loans significantly affects students' borrowing behavior, and hence students' academic performance in college.

Additionally, low-income students and their parents often perceive college expenses to be higher than they actually are due to the uncertainty surrounding scholarship availability and the lack of information regarding college costs ([Grodsky and Jones, 2007](#); [Horn et al., 2003](#)). This misconception can discourage them from applying to selective colleges. The survey of the College Opportunity and Career Help (COACH) program in Boston shows that the actual college costs are about two to three times less than students' estimations ([Avery and Kane, 2004](#)). [Hoxby et al. \(2013\)](#) conduct a randomized controlled trial and find that early information intervention about the selective college application process and college net costs drive higher enrollment rates among low-income students.

The complexity of financial aid applications also generates serious barriers for low-income, high-achieving students, which is the group of students the financial assistance aims to help ([Reindl, 2015](#); [Page and Scott-Clayton, 2016](#)). [Bettinger et al. \(2012\)](#) use a randomized field experiment to simplify the financial aid application process by assisting randomly selected low-income families with FAFSA. They show that the assistance significantly increases the likelihood of financial aid application and college attendance. Such application complexity is shown to do little help but comes with high costs, not only in terms of compliance and administrative expenses

but also in social costs ([Dynarski and Scott-Clayton, 2006, 2008, 2013](#)). This can discourage students from applying for financial aid. Knox Achieves is a financial aid program that eliminated complexity obstacles by promising eligibility for all high school senior students in Knox County of Tennessee. [Carruthers and Fox \(2016\)](#) show that Knox Achieves significantly increases participants' college enrollment.⁴

One recent study conducted by [Dynarski et al. \(2021\)](#) used a randomized controlled trial to test a program that addresses concerns around complexity and uncertainty. In this study, the HAIL (High Achieving Involved Leader) Scholarship was offered to randomly selected low-income, high-achieving students, promising free tuition and fees if they applied and were admitted to the University of Michigan. HAIL simultaneously removes time and effort costs from filing aid forms and resolves uncertainty about college costs at the same time. The university already had a policy offering full tuition discounts to these students, but the authors hypothesize that this was not well known. By clearly communicating costs of attending the University of Michigan, the likelihood of targeted students applying to and enrolling in college (any college — not just the University of Michigan) significantly increased.

This study contributes to [Dynarski et al. \(2021\)](#) by using a quasi-experimental method to identify the effect of a program that also removes imperfect information, college cost uncertainty, and application complexity constraints simultaneously. Similar to HAIL, the Flagship simplifies the eligibility process by requiring no additional effort to learn about or qualify for the scholarship, and students would receive monetary assistance if they get admitted to the targeted institution - the University of Tennessee.⁵ However, the Flagship program was implemented earlier

⁴Both Knox Achieves and Flagship Scholarship remove application complexity, however, there are differences in eligibility requirements. Knox Achieves is available for all high school students in Knox County ([Carruthers and Fox, 2016](#)). This indicates that it is not exclusively reserved for disadvantaged students, as those from higher-income backgrounds or with greater awareness of college costs are also eligible for it. In contrast, the Flagship Scholarship specifically aims to help students from disadvantaged high schools.

⁵Students are still required to submit FAFSA each year, maintain a 2.0 GPA, and have continuous full-time enrollment to retain the scholarship. However, these are usually the baseline requirements for other scholarships. There are no specific retention requirements unique to the Flagship

than HAIL, which was before the steep increase in tuition and fees after the Great Recession. So effects of a similar program like HAIL might be more potent. Besides, Flagship eligibility was not targeted at individual students, but rather, at whole schools. This could make it easier to administer and reduce the administrative costs for both university and high schools.

Two other programs that are similar to the Flagship Scholarship were launched in Texas: the Longhorn Opportunity Scholarship (LOS) program at the University of Texas at Austin and the Century Scholars (CS) program at Texas A&M University – College Station. Both provide financial aid to students in targeted public high schools in Texas, which usually have more economically disadvantaged students and lower rates of enrollment in selective colleges. [Andrews et al. \(2020\)](#) use a quasi-experimental method to show that the LOS program has a large and positive effect on both enrollments and earnings, while the effect of the CS program is limited.

Both LOS and CS programs are only available to targeted high schools and aim to help disadvantaged students, which are the same features and goals as the Flagship Scholarship in Tennessee. But there are a few key differences from Flagship that motivate our study of the UTK program, in addition to socioeconomic differences between Texas and Tennessee. Both the LOS and CS programs require additional scholarship applications besides FAFSA, and the award is not guaranteed ([The University of Texas at Austin, 2012](#); [Texas A&M University LAUNCH: Learning Communities, 2023](#)). For the LOS, all students who enroll in LOS-targeted high schools are eligible for academic support, but not necessarily financial aid ([Andrews et al., 2020](#)). For the CS, only selected students from CS high schools can receive the scholarship, and additional academic requirements in college must be met each year to continue receiving payments, including but not limited to maintaining a 2.75 GPA, attending mandatory events, and completing seminar courses.⁶ As a comparison, students in Flagship high schools are provided with a guarantee of free Scholarship. HAIL scholarship does not require FAFSA, although they encouraged students to complete it [Dynarski et al. \(2021\)](#).

⁶See [Texas A&M University LAUNCH: Learning Communities \(2023\)](#) for detailed requirements

college study, as long as they get admitted to UTK, file a FAFSA, and maintain a 2.0 GPA. Furthermore, benefits differ between Flagship and LOS/CS. Academic assistance services are provided by LOS and CS in addition to financial aid, whereas the Flagship Scholarship concentrates solely on lessening the financial burden for eligible students. In this respect, Flagship is more similar to HAIL, but targeted to whole schools rather than individual students.

A scholarship that induces students to attend a specific group of universities may generate unintended outcomes. The John and Abigail Adams Scholarship in Massachusetts provides a tuition waiver for students with high test scores if they attend state's public colleges. [Goodman \(2008\)](#) shows that the Adams Scholarship successfully encourages scholarship recipients to attend four-year public college in Massachusetts. However, [Cohodes and Goodman \(2014\)](#) show that many of these students could have gone to higher-quality colleges with higher graduation rates in the absence of this scholarship. The Adams Scholarship which aims to help students generates unintended adverse effects. The Flagship Scholarship shares a similar feature with the Adams Scholarship in providing monetary assistance to eligible students to attend a specific university, UTK. However, Flagship essentially provides a "free college" guarantee, which significantly alleviates students' financial burden, while the assistance provided by the Adams Scholarship only accounts for 16%-24% of costs ([Goodman, 2008](#)). Nevertheless, if some students would have otherwise attended higher quality or better resourced schools, Flagship may inadvertently lower their likelihood of graduating. Therefore, in addition to estimating effects of Flagship on college enrollment outcomes, we also examine its influence on the quality characteristics of colleges students attended.

3.3 Identification

To identify effects of the Flagship Scholarship, we estimate the following equation:

$$Y_{ist} = \alpha + \beta \text{Flagship}_{is} * \text{Post}_t + \gamma X_{ist} + \delta_s + \pi_t + \epsilon_{ist} \quad (3.1)$$

where Y_{ist} denotes college outcome variables of interests for student i of high school s and 12th grade cohort t . For college enrollment outcomes (two-year, four-year, any college, or UTK), we estimate Equation 3.1 for public high school graduates in the state. For college quality outcomes (per-student college spending, admission rates, and graduation rates), we limit the sample to high school graduates who immediately enroll in college.

Flagship_{is} is a dummy variable that equals 1 if student i in high school s is eligible for the Flagship Scholarship. We limit our sample and define the control group in three ways: first, we compare Flagship high school senior cohorts with those from any non-Flagship high schools in Tennessee. Therefore, Flagship_{is} equals 0 if student i graduates from a non-Flagship high school. Second, since Flagship high school students tended to have lower college enrollment rates, we define Flagship_{is} equals 0 for students from non-Flagship high schools with below median college-going, as recorded in 2005. Third, we define Flagship_{is} equals 0 for non-flagship schools that are comparable to Flagship schools in terms of student achievement and graduation rates. Specifically, we use a logistic regression model to fit the probability of being a Flagship high school as a function of graduation rates, attendance rates, and the percent of students who scores “proficient” on Algebra I and English II end-of-course exams. We construct the control group as 25% non-flagship schools that are most similar to Flagship schools in our analysis.

Post_t is an indicator variable that equals 1 if high school graduation year t is greater than or equal to school year 2006-07, which is when Flagship Scholarship was first introduced. X_{ist} represents a vector of individual-level characteristics, including

gender, race/ethnicity, and type of high school diploma. δ_s is school fixed effect controlling for school-specific factors that could differently affect students' academic performance. Time fixed effect π_t absorbs time-varying shocks to students. ϵ_{ist} represents an idiosyncratic error term. Standard errors are clustered at high school level.

β is the difference-in-differences estimator that represents the effect of Flagship Scholarship eligibility on postsecondary enrollment. To interpret β as the causal effect, we assume that college enrollment decisions of students who are eligible for the Flagship Scholarship would have followed a similar pattern to the control group in the absence of the program. One way to test the validation of this assumption is to examine if there exists a parallel trend before the introduction of the Flagship program. However, as a result of constraints in our data, we are only able to obtain information on a single cohort prior to the implementation of the policy, making it difficult for us to examine parallel trends. We also assume that the compositions of students in Flagship high schools are not affected by Flagship eligibility. We use mobility tests to examine the existence of these changes and present the results in [subsection 3.5.2](#)

Instead, we estimate time-varying treatment effects of the Flagship Scholarship by event study specification:

$$Y_{ist} = \alpha + \sum_{\tau=-q}^{-1} \beta_{\tau} Flagship_{is\tau} + \sum_{\tau=0}^m \beta_{\tau} Flagship_{is\tau} + \gamma X_{ist} + \delta_s + \pi_t + \epsilon_{ist} \quad (3.2)$$

where τ represents the number of years until or since Flagship implementation in $\tau = 0 = 2016$, and q is the maximum of time period before the implementation of program. However, we only have one cohort before treatment, so $q = 1$ in our application, and β_{-1} is omitted for collinearity. We only detect post-treatment effects β_{τ} that are interpreted as conditional differences from the single pre-treatment period.

3.4 Data

The Tennessee administrative education data that is employed in this study is drawn from Tennessee Higher Education Commission (THEC), which contains high school senior cohorts from 372 public high schools in Tennessee, school years 2005-06 to 2008-09, and is matched to college enrollment records using the National Student Clearinghouse. This student-level data includes students' high school of attendance, which enables us to determine students' Flagship Scholarship eligibility. Additionally, we observe students' postsecondary education enrollment and the sector of colleges (two-year or four-year) they attend. This information is crucial in assessing effects of the Flagship Scholarship on the likelihood of college enrollment, UTK enrollment, and college sector choice. We also observe students' gender, race/ethnicity, and high school diploma type.⁷

In addition, we incorporate institutional characteristics obtained from the Integrated Postsecondary Education Data System (IPEDS) into our analysis. This includes per-student spending, college admission rates, and college graduation rates, which are used as a measure of college quality. The high school graduation rates, attendance rates, and Algebra I and English II end-of-course exams proficiency levels as recorded in 2010, are obtained from the Tennessee Department of Education, which allows us to construct a control group that shares similar features as Flagship high schools.

Table C.1 presents summary statistics for high school graduates from 371 high schools in Tennessee, including 35 Flagship high schools. Panel A includes all high school students in our sample. As shown in the table, regardless of whether it is for UTK, a 2-year/4-year institution, or any college in general, students who qualify for the Flagship Scholarship exhibited a lower enrollment rate. Only 48% of students

⁷ According to Tennessee Department of Education's Diploma Decision Guide, Tennessee has four diploma options, including regular diploma, alternative academic diploma, occupational diploma, and special education diploma. Among the four types of diplomas, only a regular diploma is considered for traditional acceptance to postsecondary education and/or training.

who graduated from Flagship high schools pursued postsecondary education, which is lower than those from non-Flagship high schools (59%). In terms of demographic characteristics, the majority of Flagship high school graduates were Black students (85%). These schools also had a slightly higher proportion of female and Hispanic students compared to non-eligible high schools. Panel B presents descriptive statistics for high school senior cohorts who enrolled immediately in college. Flagship high school students tended to enroll in colleges with lower college expenditures, admission rates, and graduation rates.

3.5 Empirical Results

3.5.1 Main Results

[Table C.2](#) presents difference-in-differences estimations of [Equation 3.1](#). In column (1), our analysis includes students from both Flagship and non-Flagship high schools. Column (2) presents estimates comparing students from Flagship high schools with those from low college-going non-Flagship schools. In column (3), we limit our control group to students from non-Flagship high schools that are 25% most comparable to Flagship schools.

Results are generally consistent across comparison methods. As presented in Panel A, Flagship eligibility significantly increases the probability of UTK enrollment by 1.3 to 1.8 percentage points, with slightly higher effects observed in the comparable high school sample. We find no statistically significant effect on 2-year, 4-year college enrollments, or overall college-going decisions in both specifications.

In Panel B, we limit our sample to high school graduates who enroll immediately in college and present estimates of college quality characteristics. Our findings suggest that Flagship eligibility encourages students to enroll in more selective colleges with 2.163 to 3.360 percentage points lower admission rates, although the coefficient for the comparable high school sample is smallest and only significant at 10% level. This

indicates that students from Flagship high schools are more likely to attend higher-quality colleges, and these effects are attributed to the increased enrollment in UTK. Nonetheless, we observe no significant effect on per-student college expenditure and graduation rates.

In addition to average treatment effects, we employ an event study design to demonstrate how Flagship program effects vary over time. Our results in [Table C.3](#) show that positive effects on the UTK enrollment are statistically significant over the time window we observe, and magnitudes are slightly higher in the last event period (school year 2008-09) than the initial period (school year 2006-07). These patterns are observed in all three specifications, as shown in Panel A to C: the magnitude of the effect increases from 1.4 to 2.0 percentage points when we define all non-Flagship high schools as the control group; from 1.3 to 1.6 percentage points when we define low college enrollment high schools as the control group; and from 1.6 to 2.2 percentage points when we define comparable high schools as the control group, which is the largest size of the effect among the three methods.

We also find negative effects on 4-year colleges and any college attendance when we limit the sample to high schools with low college enrollment rates, as shown in Panel B columns (3) and (4), respectively. However, effects on 4-year college enrollment are only marginally significant at 10% level in the first two periods, and effects on any college enrollment are only significant in the initial treatment period. Magnitudes of these effects demonstrate a tendency to approach zero over time. This suggests the Flagship program would not generate adverse effects on college enrollment in the long run.

3.5.2 Mobility Tests

We use mobility tests to investigate if demographic characteristics change in response to the Flagship program implementation, which is potentially a threat to our identification and causal interpretations. It is possible that the scholarship convinced

students who wanted to enroll in UTK to switch high schools to gain eligibility. We estimate the difference-in-differences model at the high school level and replace dependent variables with the number of students, the percentage of students in each demographic group, and the percentage of students who graduated with regular diplomas in each school.

Table C.4 shows that the Flagship Scholarship significantly increases the number of students who graduated from designated Flagship high schools by 13 to 15 students, which accounts for roughly a 10% increase from the pre-treatment average. Flagship also lowered the regular diploma rates by 2.4 and 2.9 percentage points in our full sample and comparable high school sample analysis, respectively. But these only account for a less than 3% decrease. There are no significant changes in demographic characteristics. This implies, although Flagship Scholarship coincided with more students attending designated high schools, we do not find strong evidence that student composition changed to favor more *ex ante* college going.

3.5.3 Heterogeneous Effects

We divide our sample into subgroups by gender and race, and re-estimate Equation 3.1 for each group of students. As listed in Table C.5, compared to students who are not eligible for the Flagship Scholarship, female, male, and Black students exhibit a significant increase in UTK enrollment, with more pronounced effects for females. Positive effects are observed for enrollment in any college, but only for white students. When evaluating college quality, we show that effects on female and male students are consistent with our main estimations, where they are sorted into colleges with lower admission rates. Female students also tend to enroll in colleges with marginally higher graduation rates. We do not find similar effects on Black or white students. The Flagship program's effects on per-student college expenditure are not statistically significant.⁸

⁸We also estimate heterogeneous effects for Asian and Hispanic students. Results show that Hispanic students' enrollments in UTK and 4-year colleges significantly increase, and they tend to

Table C.6 lists the difference-in-differences estimations for gender and racial subgroups of students enrolled in high schools with low rates of pretreatment college enrollment. Similar to findings in Table C.5, the likelihood of UTK enrollment increases for female, male, and Black students. We observe a negative effect on any college enrollment for male students, but the coefficient is only significant at a 10% level. This is possibly caused by the coincidental enrollment growth or an effect of the scholarship on high school graduation, as shown in Table C.4. Although our data do not allow further investigation into these hypotheses, this imprecise negative effect does not alter our conclusion, as similar negative effects do not apply to other students.

In terms of college quality, we find that all students are more likely to attend colleges with lower admission rates except white students. It is not surprising to see this pattern since white students only account for 11% in Flagship high schools in our sample.⁹

In Table C.7, we present the heterogeneity analysis for the comparable high school sample. The estimates for college enrollment are consistent with the full sample analysis besides white students. In column (4), white students also exhibit an increase in UTK and 4-year college enrollment with larger but marginally significant coefficients. However, in contrast with the other two specifications, we do not find any significant effect on college quality.¹⁰

enroll in colleges with 10.924 percentage points higher admission rates. Flagship Scholarship does not generate significant effects on Asian students. However, the sample sizes for Hispanic and Asian students are very small, ranging from 3340 to 5487 statewide. So we report these estimates in Appendix Table C.8 column (1) and (2).

⁹The Flagship Scholarship policy makes Hispanic students more likely to enroll in 4-year colleges and higher admission rates institutions, while no significant effects are observed for Asian students, as presented in Table C.8 column (3) - (4).

¹⁰The estimates for Asian and Hispanic students are consistent with the other two specifications, as shown in Table C.8 column (5) - (6)

3.6 Conclusion

We evaluate the effectiveness of the University of Tennessee Flagship Scholarship program. Our difference-in-differences estimates show that the program significantly increases the likelihood of targeted students' enrollment in the University of Tennessee, Knoxville (UTK) immediately after high school graduation by 1.3 to 1.8 percentage points. Magnitudes of these effects are shown to increase over time. This conclusion is drawn from three samples of administrative data: all Tennessee public high school graduates, graduates from schools with low rates of college enrollment, and graduates from schools that share similar characteristics with Flagship high schools. Our findings suggest that the Flagship Scholarship program initiated by UTK successfully achieved its goal of helping students from economically and academically disadvantaged high schools pursue higher education at a selective college. By eliminating scholarship application complexity, and award uncertainty, and by clarifying information about the cost of college, eligible students in Flagship high schools were more likely to enroll in UTK.

Besides the direct effect on seamless enrollment, we also examine the quality of college that students attended. Flagship eligibility leads to a greater tendency to select higher-quality colleges, but this does not apply to white students. These shifts that we observe were primarily driven by the significant rise in enrollment at UTK.

These treatment effects are not trivial. The UTK enrollment rate for Flagship high schools is only 2.52% on average, and only 2.21% before the implementation of the program. A 1.8 percentage point effect accounts for an 81% increase relative to the pre-treatment enrollment rate. As a comparison, Longhorn Opportunity Scholars (LOS) generated a similar effect, which increases the University of Texas at Austin enrollment by 71% ([Andrews et al., 2020](#)). Century Scholars (CS) program did not significantly affect Texas A&M enrollment ([Andrews et al., 2020](#)). And [Dynarski et al. \(2021\)](#) find that HAIL Scholarship increase the University of Michigan enrollment by 15 percentage point from a control mean of 12 percent.

The Flagship Scholarship program aims to overcome various barriers that students may encounter during their college preparation, resulting in positive impacts on students and the education system in general. Nevertheless, due to intricacies of the policy design, which addresses multiple obstacles simultaneously, identifying the underlying mechanisms is a challenging task. Despite the difficulty in identifying mechanisms directly, our research suggests that the Flagship Scholarship program potentially benefits students in two ways: First, it encourages students who are qualified for selective colleges but are deterred from applying due to financial difficulty, information asymmetry, or financial aid application complexity to enroll in UTK, the 4-year public flagship university in Tennessee. Further investigation into the recruiting process at designated Flagship high schools may reveal more details. Second, monetary benefits incentivize students to make better college preparation during high school years. Although admission to UTK is not guaranteed, students may still benefit from their hard work during high school in the long run. We would be able to test this hypothesis by estimating the effect of the Flagship program on students' ACT test scores, or on the likelihood that they graduate from high school.

Further studies need to be done in the following areas with more data available. First, we would like to quantify the impact of the Flagship Scholarship on high school students' college preparation, specifically in terms of their ACT test scores. This would help us to investigate mechanisms of the program as discussed above. Second, we would like to test if the Flagship Scholarship affects college completion rate and future income. This policy would generate larger impacts and more social values for students from disadvantaged high schools if it ultimately increases education attainment and labor market outcomes. And finally, we would like to detect the existence of substitution effects where students forgo a better college choice due to the "free college" incentive. Although [Bruce and Carruthers \(2014\)](#) conclude that scholarship switches students' college choices from 2-year community colleges to 4-year colleges with higher quality, [Cohodes and Goodman \(2014\)](#) show that the tuition waiver induces students to make a worse-off choice and are less likely to obtain the

degree. If such effects also exist in the Flagship program, it would work against the original purpose.

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Appendix

Appendix A

A.1 Tables

Table A.1: Descriptive Statistics for Students and GSDs

	Observations	Mean
<i>Panel A: Academic Achievement by GSD-Grade-School Year</i>		
English/Language Arts	160050	0.04
Math	160050	0.04
<i>Panel B: Demographic Characteristic by GSD-Grade-School Year</i>		
Asian	160050	0.02
Black	160050	0.09
Hispanic	160050	0.11
Native American	160050	0.02
White	160050	0.76
Economically Disadvantaged	160050	0.49
Free and Reduced-Priced Lunch	160050	0.49
Number of Students in Grade	160050	436
<i>Panel C: Demographic Characteristic by GSD-School Year</i>		
City/Urban Location	26675	0.08
Suburban Location	26675	0.20
Town Location	26675	0.27
Rural Location	26675	0.45
Log of Median Income	26675	10.80
Poverty Rate	26675	0.16
Unemployment Rate	26675	0.08
SNAP Receipt Rate	26675	0.11
Rate of Bachelor's Degree or Higher	26675	0.22
Single Mother Household Rate	26675	0.16
English language learners	26675	0.04
Special Education	26675	0.14
Number of Unique School Districts	6740	

Notes: The sample includes academic achievement, demographic and socioeconomic variables for students in Geographic School District (GSD) from grade 3 to 8 between school year 2009-10 and 2014-15. Academic achievement, race/ethnicity, economically disadvantaged, free and reduced-price lunch, and the number of students in each grade are measured by each grade in GSD for each school year. The location of GSD, log of median income, poverty rate, unemployment rate, SNAP receipt rate, rate of Bachelor's degree or higher, single mother household rate, English language learners rate, and special education rate are measured at the GSD by school year level.

Table A.2: Descriptive Statistics for Meteorological and Wildfire Smoke Data

	Observations	Mean	Min	Max
<i>Panel A: Meteorology</i>				
Avg. Temp Fall	26675	54.9	16.2	83.3
Avg. Temp Spring	26675	53.0	2.6	84.2
Avg. Temp Summer	26675	73.4	44.0	96.2
Avg. Temp Winter	26675	31.7	-15.7	73.2
Max Temp Fall	26675	66.1	21.1	97.8
Max Temp Spring	26675	64.4	9.0	96.8
Max Temp Summer	26675	84.5	50.2	111.1
Max Temp Winter	26675	41.1	-10.2	80.2
Min Temp Fall	26675	43.8	7.5	75.5
Min Temp Spring	26675	41.5	-3.8	74.3
Min Temp Summer	26675	62.2	37.9	84.0
Min Temp Winter	26675	22.3	-22.3	68.0
Precipitation Fall	26675	81.5	0.0	565.2
Precipitation Spring	26675	92.2	0.0	454.5
Precipitation Summer	26675	98.8	0.0	536.3
Precipitation Winter	26675	69.3	0.0	557.1
<i>Panel B: Wildfire Exposure</i>				
Days with Smoke, 25% Pop	26675	30.90	0	112
Days with Smoke, 50% Pop	26675	29.94	0	109
Days with Smoke, 75% Pop	26675	29.09	0	108
Cummulative PM2.5, 25% Pop	26675	346.58	0	3283
Cummulative PM2.5, 50% Pop	26675	337.36	0	3278
Cummulative PM2.5, 75% Pop	26675	328.82	0	3278
Days with Unhealthy PM2.5, 25% Pop	26675	0.06	0	38
Days with Unhealthy PM2.5, 50% Pop	26675	0.06	0	38
Days with Unhealthy PM2.5, 75% Pop	26675	0.06	0	38
Smoke Exposure Indicator	26675	1.00	0	1

Notes: The sample includes meteorological and wildfire smoke variables at Geographic School District (GSD) between school year 2009-10 and 2014-15. Temperatures are reported in Fahrenheit, and precipitation is reported in millimeters. The "Days with Smoke" measures the typical number of days at least 25%, 50%, and 75% of people in a GSD are exposed to wildfire smoke over the year before the test start. The "Days with unhealthy Air Quality" measures the typical number of days at least 25%, 50%, and 75% of people in each GSD have a $PM_{2.5}$ level greater or equal to $35.5 \mu g/m^3$ due to wildfire smoke, over the year before tests start. The "Cumulative $PM_{2.5}$ " measures the total amount of smoke-associated $PM_{2.5}$ the district was exposed to over the year before tests.

Table A.3: Estimated Effects of Wildfire Smoke Days (*10) on Academic Performance

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: ELA Performance</i>			
1 Year Prior	-0.003 (0.001)***	-0.003 (0.001)**	-0.003 (0.001)***
2 Year Prior	0.001 (0.001)	-0.001 (0.001)	0.002 (0.001)**
3 Year Prior	0.000 (0.001)	-0.002 (0.001)	0.001 (0.001)
<i>Panel B: Math Performance</i>			
1 Year Prior	-0.004 (0.001)***	-0.003 (0.001)*	-0.005 (0.001)***
2 Year Prior	0.001 (0.001)	-0.002 (0.002)	0.003 (0.001)**
3 Year Prior	-0.001 (0.001)	-0.002 (0.002)	0.000 (0.001)
Observations	160050	80025	80025

Notes: The dependent variables are standardized ELA test scores in Panel A and standardized math test scores in Panel B. Smoke days are measured with at least 75% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.4: Estimated Effects of Standardized Cumulative $PM_{2.5}$ on Academic Performance

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: ELA Performance</i>			
1 Year Prior	-0.004 (0.002)**	-0.005 (0.002)***	-0.003 (0.002)
2 Year Prior	0.001 (0.002)	-0.002 (0.002)	0.003 (0.002)
3 Year Prior	-0.000 (0.001)	-0.002 (0.002)	0.001 (0.002)
<i>Panel B: Math Performance</i>			
1 Year Prior	-0.005 (0.002)***	-0.004 (0.002)*	-0.005 (0.002)***
2 Year Prior	0.002 (0.002)	-0.001 (0.003)	0.004 (0.002)
3 Year Prior	-0.002 (0.002)	-0.004 (0.002)*	-0.001 (0.002)
Observations	160050	80025	80025

Notes: The dependent variables are standardized ELA test scores in Panel A and standardized math test scores in Panel B. Standardized Cumulative $PM_{2.5}$ are measured with at least 75% of the population being exposed to smoke-associated $PM_{2.5}$ aggregated values in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.5: Estimated Effects of Unhealthy Air Quality Days (*10) on Academic Performance

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: ELA Performance</i>			
1 Year Prior	-0.026 (0.011)**	-0.021 (0.013)	-0.032 (0.012)***
2 Year Prior	-0.014 (0.033)	-0.052 (0.024)**	0.016 (0.051)
3 Year Prior	-0.032 (0.013)**	-0.047 (0.014)***	-0.020 (0.015)
<i>Panel B: Math Performance</i>			
1 Year Prior	-0.030 (0.013)**	-0.034 (0.017)**	-0.030 (0.014)**
2 Year Prior	-0.005 (0.028)	-0.020 (0.038)	-0.002 (0.038)
3 Year Prior	-0.040 (0.017)**	-0.057 (0.020)***	-0.023 (0.016)
Observations	160050	80025	80025

Notes: The dependent variables are standardized ELA test scores in Panel A and standardized math test scores in Panel B. Unhealthy Air Quality days are measured with at least 75% of the population being exposed to smoke-associated $PM_{2.5}$ greater or equal to $35.5\mu g/m^3$. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.6: Estimated Effects of Wildfire Smoke Days (*10): School Day vs Non-School Day

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: ELA Performance</i>			
<i>School Days Only</i>			
1 Year Prior	-0.004 (0.002)*	-0.002 (0.003)	-0.006 (0.003)**
2 Year Prior	0.005 (0.003)	0.003 (0.004)	0.005 (0.004)
3 Year Prior	0.005 (0.003)**	0.005 (0.002)**	0.005 (0.003)
<i>Non-School Days Only</i>			
1 Year Prior	-0.003 (0.002)*	-0.004 (0.002)*	-0.002 (0.002)
2 Year Prior	-0.001 (0.002)	-0.004 (0.002)*	0.001 (0.002)
3 Year Prior	-0.004 (0.002)*	-0.006 (0.002)***	-0.002 (0.002)
<i>Panel B: Math Performance</i>			
<i>School Days Only</i>			
1 Year Prior	-0.009 (0.003)***	-0.011 (0.003)***	-0.006 (0.003)*
2 Year Prior	-0.001 (0.004)	-0.004 (0.005)	0.002 (0.004)
3 Year Prior	0.001 (0.003)	0.001 (0.004)	0.002 (0.003)
<i>Non-School Days Only</i>			
1 Year Prior	-0.000 (0.002)	0.003 (0.003)	-0.004 (0.002)
2 Year Prior	0.001 (0.002)	-0.002 (0.003)	0.003 (0.002)
3 Year Prior	-0.003 (0.002)	-0.005 (0.003)	-0.001 (0.002)
Observations	160050	80025	80025

Notes: Non-school days are defined as federal holidays, weekends, and summer (between June 15 to August 15). All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.7: Estimated Effects of Standardized Cumulative $PM_{2.5}$: School Day vs Non-School Day

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: ELA Performance</i>			
<i>School Days Only</i>			
1 Year Prior	-0.001 (0.001)	-0.001 (0.002)	-0.002 (0.002)
2 Year Prior	0.002 (0.002)	0.000 (0.003)	0.003 (0.003)
3 Year Prior	0.003 (0.002)	0.003 (0.002)	0.003 (0.003)
<i>Non-School Days Only</i>			
1 Year Prior	-0.003 (0.002)*	-0.005 (0.002)**	-0.002 (0.002)
2 Year Prior	-0.001 (0.002)	-0.002 (0.003)	0.000 (0.003)
3 Year Prior	-0.003 (0.002)	-0.005 (0.002)**	-0.001 (0.002)
<i>Panel B: Math Performance</i>			
<i>School Days Only</i>			
1 Year Prior	-0.005 (0.002)***	-0.006 (0.002)***	-0.004 (0.002)*
2 Year Prior	0.001 (0.002)	-0.001 (0.003)	0.002 (0.003)
3 Year Prior	-0.001 (0.002)	-0.001 (0.003)	-0.000 (0.002)
<i>Non-School Days Only</i>			
1 Year Prior	0.000 (0.002)	0.002 (0.003)	-0.002 (0.003)
2 Year Prior	0.001 (0.002)	-0.000 (0.003)	0.001 (0.003)
3 Year Prior	-0.002 (0.002)	-0.004 (0.003)	-0.001 (0.002)
Observations	160050	80025	80025

Notes: Non-school days are defined as federal holidays, weekends, and summer (between June 15 to August 15). All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.8: Estimated Effects of Unhealthy Air Quality Days (*10): School Day vs Non-School Day

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: ELA Performance</i>			
<i>School Days Only</i>			
1 Year Prior	-0.015 (0.020)	-0.020 (0.025)	-0.009 (0.022)
2 Year Prior	-0.001 (0.036)	-0.057 (0.043)	0.039 (0.038)
3 Year Prior	-0.034 (0.015)**	-0.037 (0.017)**	-0.031 (0.017)*
<i>Non-School Days Only</i>			
1 Year Prior	-0.048 (0.033)	-0.025 (0.042)	-0.078 (0.038)**
2 Year Prior	-0.021 (0.048)	-0.054 (0.031)*	0.007 (0.078)
3 Year Prior	-0.028 (0.024)	-0.069 (0.027)**	0.004 (0.033)
<i>Panel B: Math Performance</i>			
<i>School Days Only</i>			
1 Year Prior	-0.041 (0.025)*	-0.009 (0.031)	-0.073 (0.027)***
2 Year Prior	-0.051 (0.052)	-0.127 (0.062)**	0.004 (0.051)
3 Year Prior	-0.062 (0.019)***	-0.080 (0.024)***	-0.043 (0.019)**
<i>Non-School Days Only</i>			
1 Year Prior	-0.007 (0.048)	-0.079 (0.059)	0.057 (0.056)
2 Year Prior	0.030 (0.035)	0.046 (0.048)	0.003 (0.057)
3 Year Prior	0.013 (0.033)	-0.006 (0.047)	0.024 (0.035)
Observations	160050	80025	80025

Notes: Non-school days are defined as federal holidays, weekends, and summer (between June 15 to August 15). All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.9: Estimated Effects of Wildfire on Total Enrollment (Standardized)

	All Students (1)	Asian (2)	Black (3)	Hispanic (4)	White (5)	Econ. Disadvantaged (6)
<i>Panel A: Smoke Days (*10)</i>						
1 Year Prior	-0.05 (0.04)	-0.04 (0.04)	-0.00 (0.02)	-0.08 (0.05)	-0.03 (0.03)	-0.07 (0.06)
2 Year Prior	-0.03 (0.02)	-0.04 (0.02)*	0.00 (0.01)	-0.04 (0.02)	-0.02 (0.02)	-0.03 (0.03)
3 Year Prior	0.02 (0.03)	0.03 (0.02)	0.02 (0.02)	0.00 (0.03)	0.03 (0.02)*	0.01 (0.03)
<i>Panel B: Standardized Cumulative PM_{2.5}</i>						
1 Year Prior	-0.07 (0.06)	-0.05 (0.05)	-0.01 (0.03)	-0.11 (0.07)*	-0.03 (0.04)	-0.10 (0.07)
2 Year Prior	-0.04 (0.03)	-0.07 (0.04)*	0.01 (0.01)	-0.06 (0.04)*	-0.04 (0.03)	-0.06 (0.04)
3 Year Prior	0.07 (0.07)	0.07 (0.06)	0.06 (0.06)	0.06 (0.08)	0.05 (0.04)	0.08 (0.09)
<i>Panel C: Unhealthy Air Quality Days (*10)</i>						
1 Year Prior	-0.15 (0.16)	-0.08 (0.17)	0.00 (0.08)	-0.32 (0.19)*	-0.02 (0.11)	-0.30 (0.21)
2 Year Prior	-0.07 (0.15)	-0.28 (0.20)	0.03 (0.19)	-0.01 (0.16)	-0.12 (0.20)	-0.05 (0.22)
3 Year Prior	0.47 (0.50)	0.48 (0.42)	0.29 (0.38)	0.45 (0.58)	0.37 (0.27)	0.52 (0.63)
Observations	25927	25927	25927	25927	25927	25927
Avg. std of Students in School District	0.00	-0.00	-0.00	0.00	-0.00	0.00

Notes: The dependent variables are the within-district standard deviation of the number of students in each group at the school district level. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.10: Placebo Test: Future Wildfire Exposure - 75% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Future	0.001 (0.001)	-0.000 (0.001)	0.002 (0.002)
<i>Math</i>			
1 Year Future	0.001 (0.001)	-0.002 (0.002)	0.003 (0.001)**
<i>Panel B: Cumulative PM2.5</i>			
<i>ELA</i>			
1 Year Future	0.002 (0.002)	-0.000 (0.002)	0.004 (0.002)*
<i>Math</i>			
1 Year Future	0.001 (0.002)	-0.004 (0.003)	0.005 (0.002)**
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Future	0.023 (0.009)**	0.007 (0.010)	0.037 (0.012)***
<i>Math</i>			
1 Year Future	0.006 (0.013)	-0.020 (0.016)	0.031 (0.013)**
Observations	160050	80025	80025

Notes: The dependent variables are standardized ELA and math test scores. Wildfire exposure is measured with at least 75% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

A.2 Figures

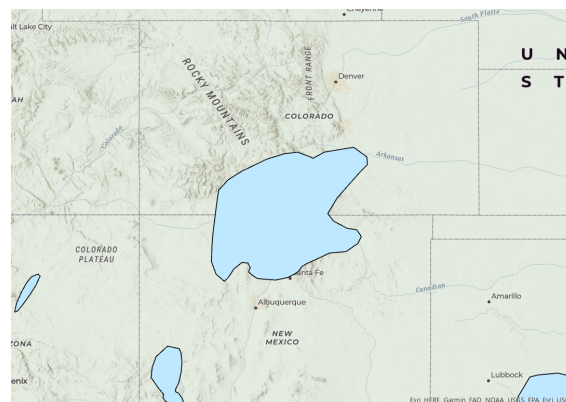
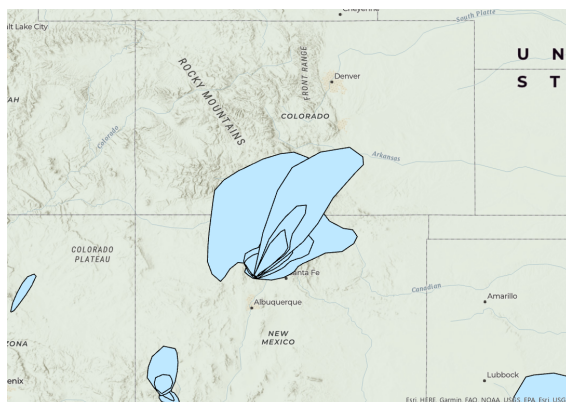


Figure A.1: Smoke Plumes across Colorado and New Mexico on July 15, 2011

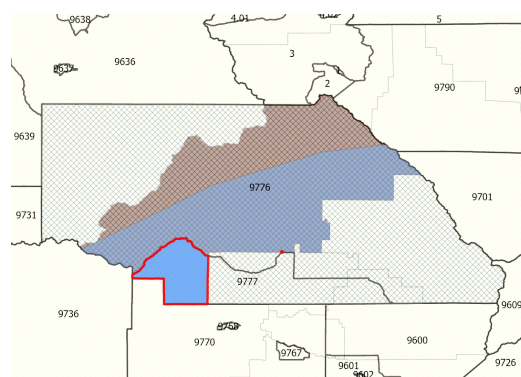
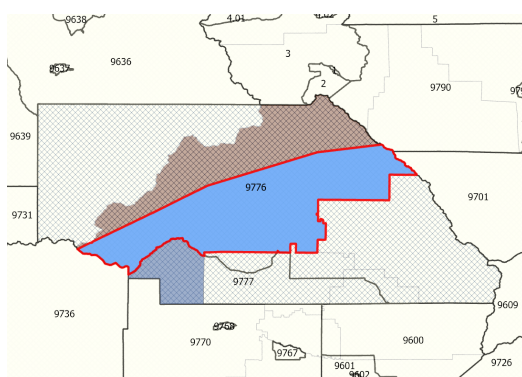
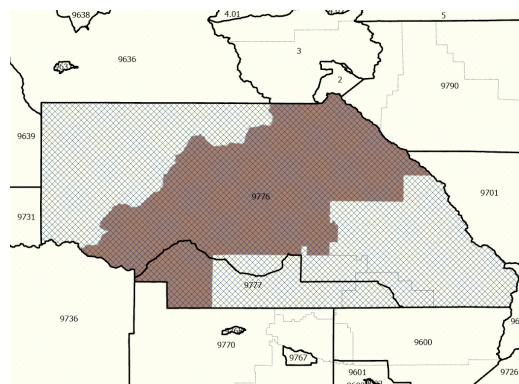
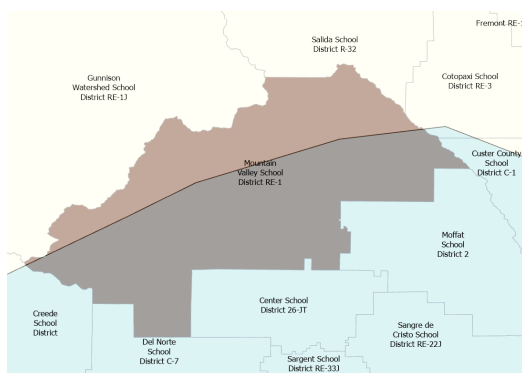
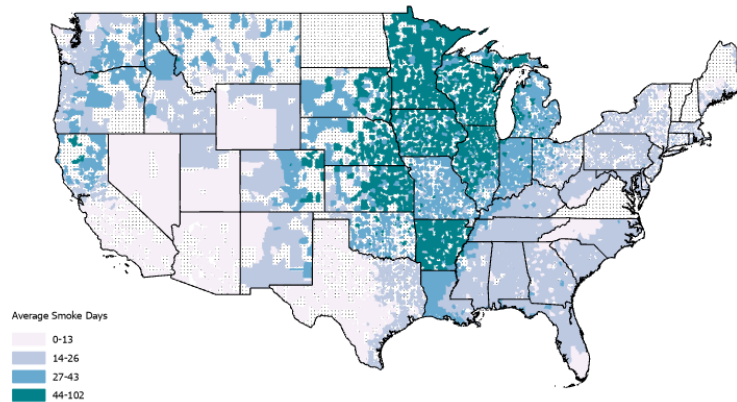
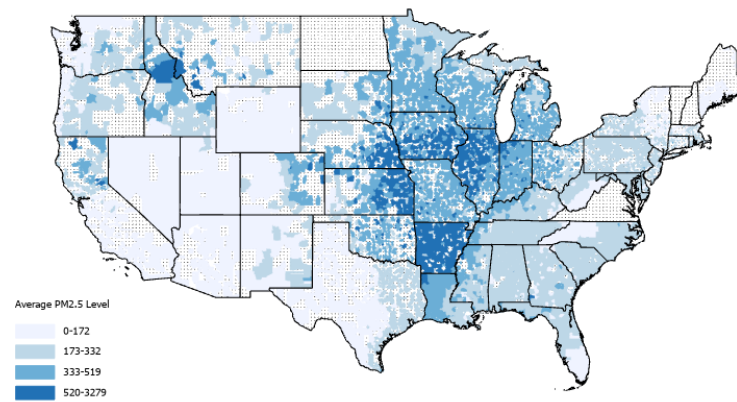


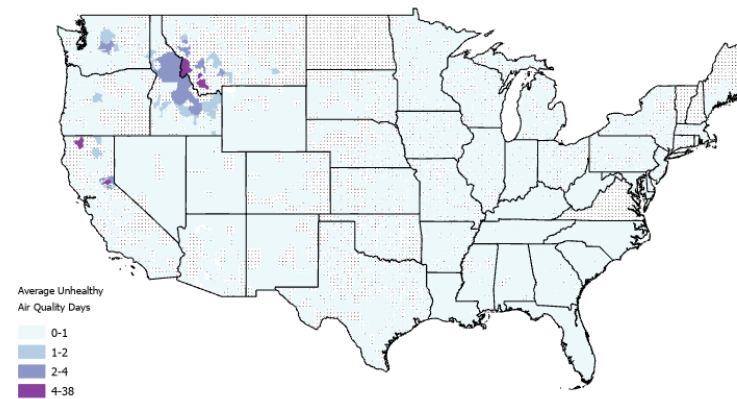
Figure A.2: Smoke Exposure by Census Tract Population in School District



(a) Average Smoke Days

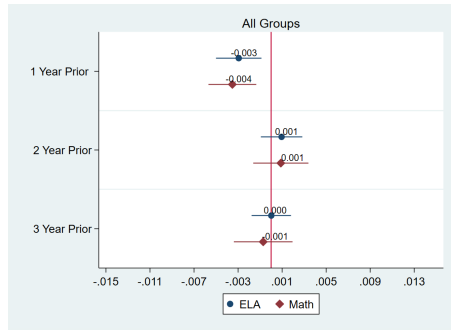


(b) Average $PM_{2.5}$ concentrations

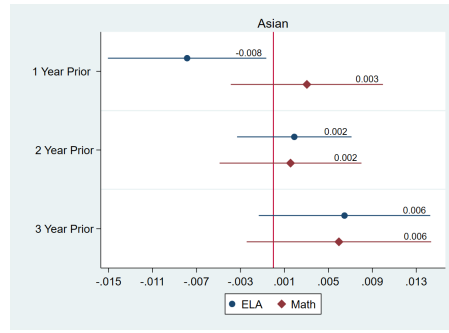


(c) Average Unhealthy Air Quality Days

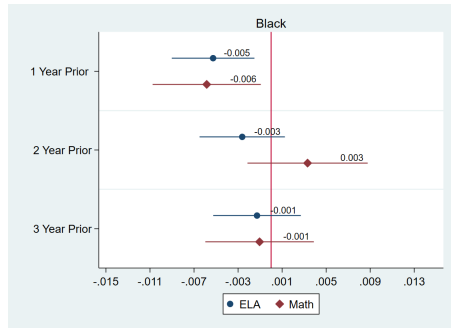
Figure A.3: Average Wildfire Exposure across the United States



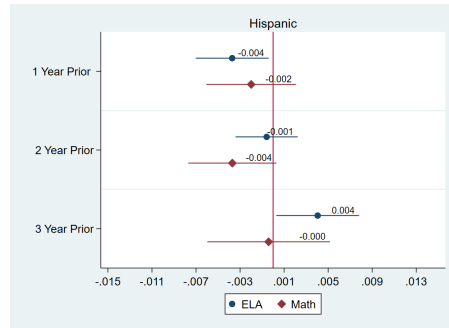
(a) All Students



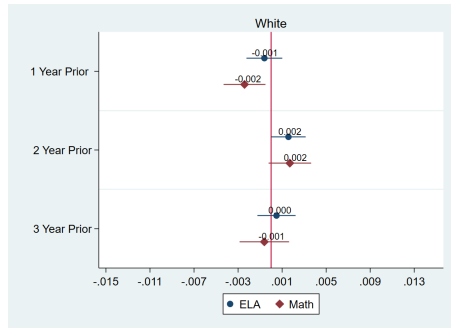
(b) Asian Students



(c) Black Students



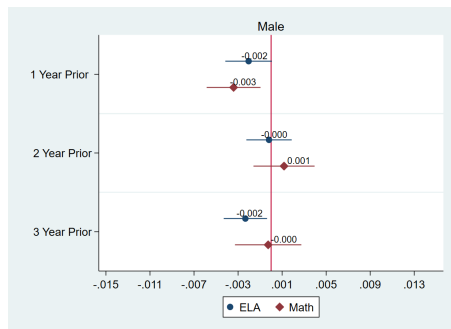
(d) Hispanic Students



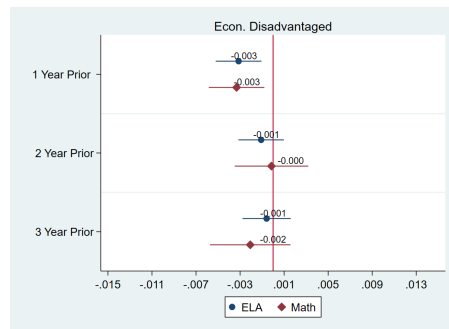
(e) White Students



(f) Female Students

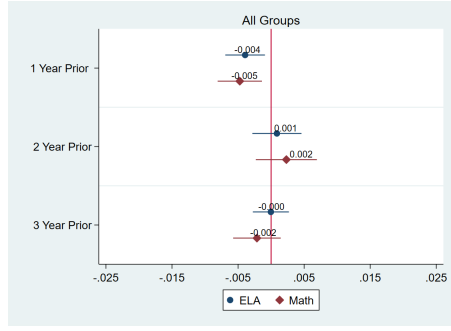


(g) Male Students

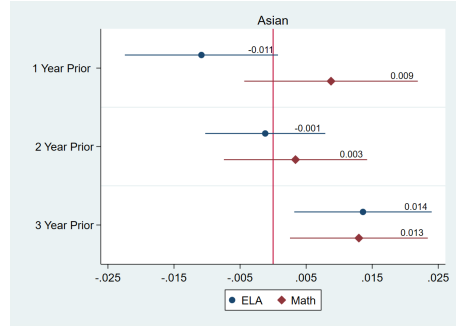


(h) Econ. Disadvantaged Students

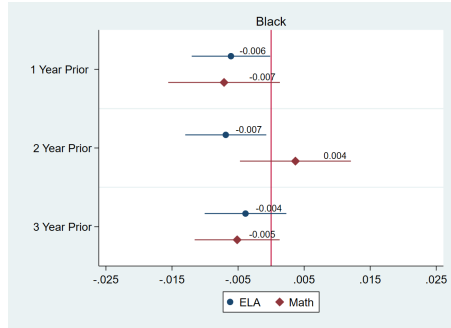
Figure A.4: Heterogeneous Effects Analysis: Wildfire Smoke Days (*10)



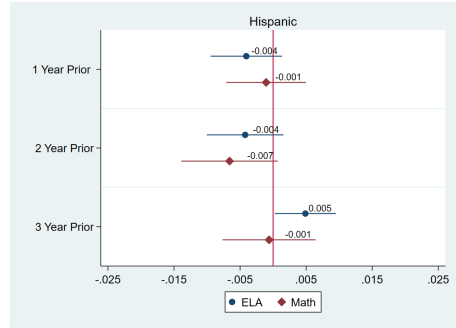
(a) All Students



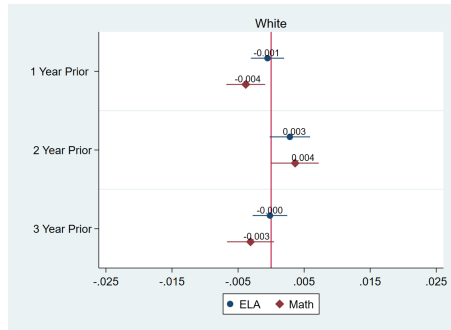
(b) Asian Students



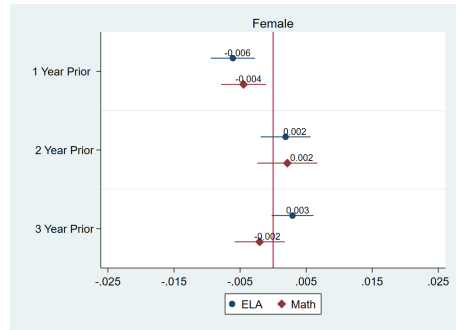
(c) Black Students



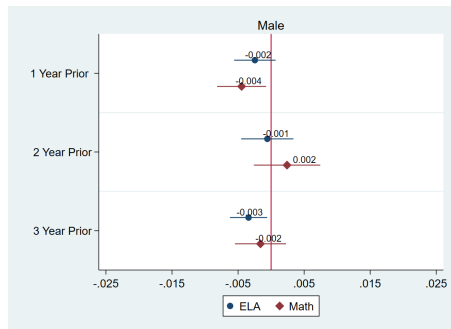
(d) Hispanic Students



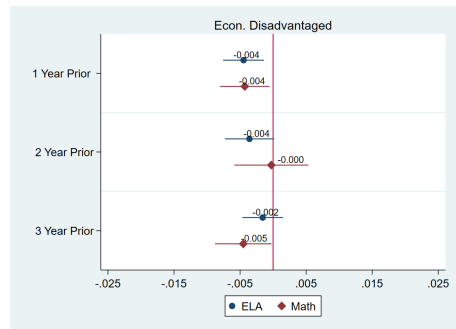
(e) White Students



(f) Female Students

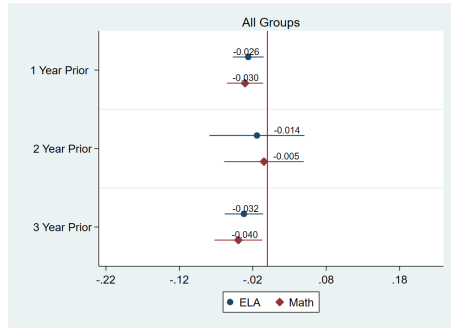


(g) Male Students

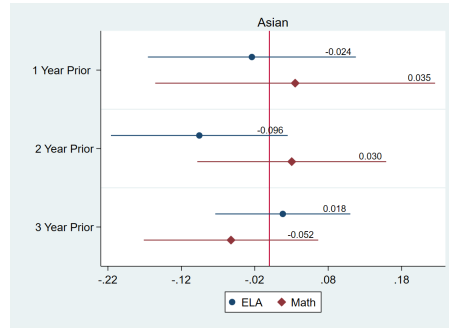


(h) Econ. Disadvantaged Students

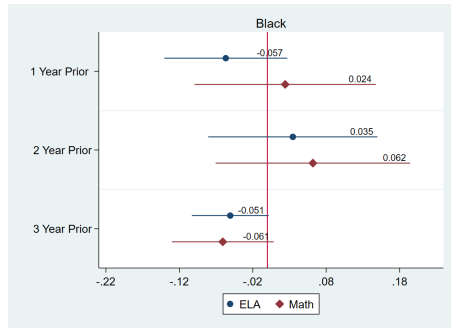
Figure A.5: Heterogeneous Effects Analysis: Standardized Cumulative $PM_{2.5}$



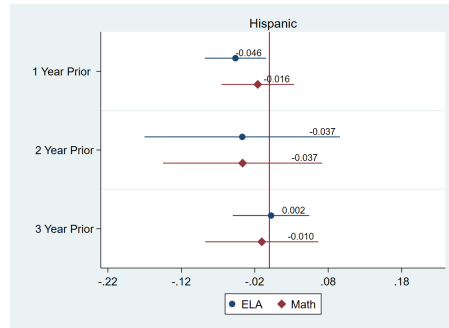
(a) All Students



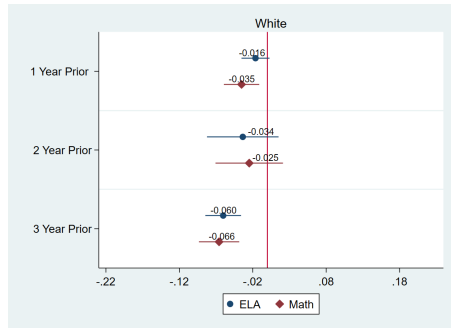
(b) Asian Students



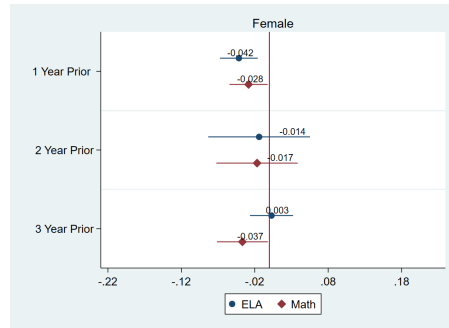
(c) Black Students



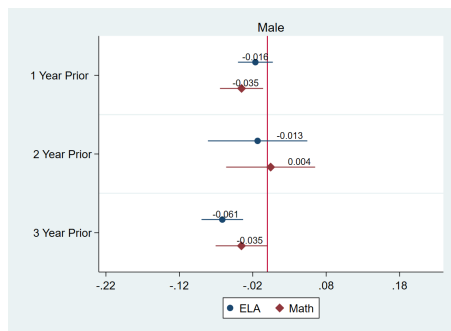
(d) Hispanic Students



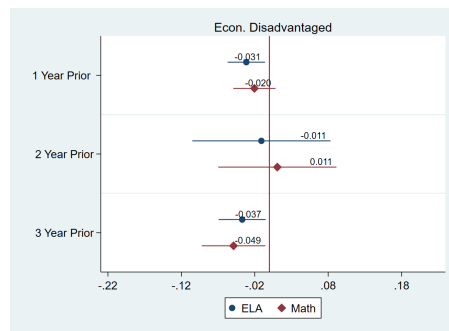
(e) White Students



(f) Female Students



(g) Male Students



(h) Econ. Disadvantaged Students

Figure A.6: Heterogeneous Effects Analysis: Unhealthy Air Quality Days (*10)

A.3 Supplementary Tables

Table A.11: States with Test Time Windows Started in October

State	Spring of the School Year
Hawaii	2011 - 2014
New Hampshire	2010 - 2014
North Dakota	2010 - 2014
Vermont	2010 - 2014

Notes: This table presents states and school years with the test time window that started in October and are dropped in the analysis. All the estimates only include states that provided the exact test time window by the state educational agency, or states without a test time window, which are assumed to start the test in the Spring.

Table A.12: Estimated Effects of the Wildfire Smoke Days (*10) on ELA Performance

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: 25% Exposure</i>			
1 Year Prior	-0.001 (0.001)**	-0.001 (0.001)*	-0.002 (0.001)*
2 Year Prior	0.002 (0.001)**	-0.000 (0.001)	0.004 (0.001)***
3 Year Prior	0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)
<i>Panel B: 50% Exposure</i>			
1 Year Prior	-0.003 (0.001)**	-0.003 (0.001)**	-0.003 (0.001)**
2 Year Prior	0.001 (0.001)	-0.001 (0.001)	0.003 (0.001)***
3 Year Prior	-0.000 (0.001)	-0.002 (0.001)*	0.001 (0.001)
<i>Panel C: 75% Exposure</i>			
1 Year Prior	-0.003 (0.001)***	-0.003 (0.001)**	-0.003 (0.001)***
2 Year Prior	0.001 (0.001)	-0.001 (0.001)	0.002 (0.001)**
3 Year Prior	0.000 (0.001)	-0.002 (0.001)	0.001 (0.001)
Observations	160050	80025	80025

Notes: The dependent variables are standardized ELA test scores. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.13: Estimated Effects of Standardized Cumulative $PM_{2.5}$ on ELA Performance

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: 25% Exposure</i>			
1 Year Prior	-0.002 (0.001)	-0.003 (0.001)**	-0.000 (0.002)
2 Year Prior	0.003 (0.002)*	-0.001 (0.002)	0.005 (0.002)***
3 Year Prior	-0.000 (0.001)	-0.002 (0.002)	0.000 (0.002)
<i>Panel B: 50% Exposure</i>			
1 Year Prior	-0.004 (0.002)**	-0.005 (0.002)***	-0.003 (0.002)
2 Year Prior	0.002 (0.002)	-0.002 (0.002)	0.004 (0.002)*
3 Year Prior	0.000 (0.001)	-0.002 (0.002)	0.001 (0.002)
<i>Panel C: 75% Exposure</i>			
1 Year Prior	-0.004 (0.002)**	-0.005 (0.002)***	-0.003 (0.002)
2 Year Prior	0.001 (0.002)	-0.002 (0.002)	0.003 (0.002)
3 Year Prior	-0.000 (0.001)	-0.002 (0.002)	0.001 (0.002)
Observations	160050	80025	80025

Notes: The dependent variables are standardized ELA test scores. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.14: Estimated Effects of the Unhealthy Air Quality Days (*10) on ELA Performance

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: 25% Exposure</i>			
1 Year Prior	-0.020 (0.010)**	-0.022 (0.013)	-0.021 (0.012)*
2 Year Prior	0.056 (0.058)	0.024 (0.055)	0.081 (0.069)
3 Year Prior	-0.026 (0.014)*	-0.039 (0.015)***	-0.018 (0.015)
<i>Panel B: 50% Exposure</i>			
1 Year Prior	-0.025 (0.011)**	-0.022 (0.014)	-0.030 (0.012)**
2 Year Prior	0.002 (0.036)	-0.039 (0.025)	0.035 (0.055)
3 Year Prior	-0.030 (0.013)**	-0.042 (0.014)***	-0.022 (0.014)
<i>Panel C: 75% Exposure</i>			
1 Year Prior	-0.026 (0.011)**	-0.021 (0.013)	-0.032 (0.012)***
2 Year Prior	-0.014 (0.033)	-0.052 (0.024)**	0.016 (0.051)
3 Year Prior	-0.032 (0.013)**	-0.047 (0.014)***	-0.020 (0.015)
Observations	160050	80025	80025

Notes: The dependent variables are standardized ELA test scores. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.15: Estimated Effects of the Wildfire Smoke Days (*10) on Math Performance

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: 25% Exposure</i>			
1 Year Prior	-0.002 (0.001)**	-0.002 (0.001)*	-0.002 (0.001)**
2 Year Prior	0.002 (0.001)	-0.001 (0.001)	0.004 (0.001)***
3 Year Prior	-0.000 (0.001)	-0.002 (0.002)	0.000 (0.001)
<i>Panel B: 50% Exposure</i>			
1 Year Prior	-0.003 (0.001)**	-0.002 (0.001)*	-0.003 (0.001)**
2 Year Prior	0.001 (0.001)	-0.001 (0.001)	0.004 (0.001)***
3 Year Prior	-0.001 (0.001)	-0.002 (0.002)	-0.000 (0.001)
<i>Panel C: 75% Exposure</i>			
1 Year Prior	-0.004 (0.001)***	-0.003 (0.001)*	-0.005 (0.001)***
2 Year Prior	0.001 (0.001)	-0.002 (0.002)	0.003 (0.001)**
3 Year Prior	-0.001 (0.001)	-0.002 (0.002)	0.000 (0.001)
Observations	160050	80025	80025

Notes: The dependent variables are standardized math test scores. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.16: Estimated Effects of Standardized Cumulative $PM_{2.5}$ on Math Performance

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: 25% Exposure</i>			
1 Year Prior	-0.003 (0.002)*	-0.003 (0.002)*	-0.003 (0.002)
2 Year Prior	0.004 (0.002)**	0.001 (0.002)	0.006 (0.002)***
3 Year Prior	-0.002 (0.002)	-0.003 (0.002)	-0.001 (0.002)
<i>Panel B: 50% Exposure</i>			
1 Year Prior	-0.004 (0.002)**	-0.004 (0.002)**	-0.004 (0.002)*
2 Year Prior	0.003 (0.002)	0.000 (0.003)	0.006 (0.002)***
3 Year Prior	-0.002 (0.002)	-0.004 (0.002)*	-0.002 (0.002)
<i>Panel C: 75% Exposure</i>			
1 Year Prior	-0.005 (0.002)***	-0.004 (0.002)*	-0.005 (0.002)***
2 Year Prior	0.002 (0.002)	-0.001 (0.003)	0.004 (0.002)
3 Year Prior	-0.002 (0.002)	-0.004 (0.002)*	-0.001 (0.002)
Observations	160050	80025	80025

Notes: The dependent variables are standardized Math test scores. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.17: Estimated Effects of the Unhealthy Air Quality Days (*10) on Math Performance

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: 25% Exposure</i>			
1 Year Prior	-0.026 (0.013)**	-0.031 (0.018)*	-0.024 (0.014)*
2 Year Prior	0.047 (0.038)	0.040 (0.046)	0.041 (0.046)
3 Year Prior	-0.030 (0.016)*	-0.044 (0.019)**	-0.019 (0.015)
<i>Panel B: 50% Exposure</i>			
1 Year Prior	-0.031 (0.013)**	-0.037 (0.018)**	-0.028 (0.014)*
2 Year Prior	0.019 (0.033)	0.009 (0.044)	0.016 (0.041)
3 Year Prior	-0.034 (0.015)**	-0.047 (0.019)**	-0.023 (0.015)
<i>Panel C: 75% Exposure</i>			
1 Year Prior	-0.030 (0.013)**	-0.034 (0.017)**	-0.030 (0.014)**
2 Year Prior	-0.005 (0.028)	-0.020 (0.038)	-0.002 (0.038)
3 Year Prior	-0.040 (0.017)**	-0.057 (0.020)***	-0.023 (0.016)
Observations	160050	80025	80025

Notes: The dependent variables are standardized math test scores. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.18: Heterogeneous Effects Analysis for Asian Students - 75% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.008 (0.004)**	-0.007 (0.005)	-0.012 (0.005)**
2 Year Prior	0.002 (0.003)	-0.004 (0.004)	0.003 (0.004)
3 Year Prior	0.006 (0.004)	0.001 (0.005)	0.005 (0.004)
<i>Math</i>			
1 Year Prior	0.003 (0.004)	0.005 (0.004)	0.001 (0.004)
2 Year Prior	0.002 (0.003)	-0.000 (0.004)	0.001 (0.004)
3 Year Prior	0.006 (0.004)	0.007 (0.005)	0.002 (0.005)
<i>Panel B: Standardized Cumulative PM_{2.5}</i>			
<i>ELA</i>			
1 Year Prior	-0.011 (0.006)*	-0.007 (0.008)	-0.018 (0.008)**
2 Year Prior	-0.001 (0.005)	-0.008 (0.007)	-0.002 (0.007)
3 Year Prior	0.014 (0.005)**	0.010 (0.007)	0.010 (0.005)**
<i>Math</i>			
1 Year Prior	0.009 (0.007)	0.013 (0.008)*	0.006 (0.009)
2 Year Prior	0.003 (0.006)	0.004 (0.009)	-0.002 (0.006)
3 Year Prior	0.013 (0.005)**	0.017 (0.006)***	0.008 (0.006)

Table A.18: Heterogeneous Effects Analysis for Asian Students - 75% Exposure
Cont.

	All (1)	Primary School (2)	Middle School (3)
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.024 (0.072)	-0.023 (0.094)	-0.046 (0.084)
2 Year Prior	-0.096 (0.061)	0.004 (0.135)	-0.208 (0.069)***
3 Year Prior	0.018 (0.047)	-0.003 (0.050)	0.050 (0.056)
<i>Math</i>			
1 Year Prior	0.035 (0.097)	0.059 (0.116)	-0.021 (0.118)
2 Year Prior	0.030 (0.066)	0.046 (0.149)	-0.009 (0.109)
3 Year Prior	-0.052 (0.061)	-0.057 (0.069)	-0.038 (0.082)
Observations	14052	7026	7026

Notes: The dependent variables are standardized ELA and math test scores of Asian students. Wildfire exposure is measured with at least 75% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.19: Heterogeneous Effects Analysis for Black Students - 75% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.005 (0.002)***	-0.005 (0.002)**	-0.005 (0.002)**
2 Year Prior	-0.003 (0.002)	-0.004 (0.003)	-0.001 (0.002)
3 Year Prior	-0.001 (0.002)	-0.005 (0.003)*	0.001 (0.002)
<i>Math</i>			
1 Year Prior	-0.006 (0.003)**	-0.006 (0.003)**	-0.005 (0.003)*
2 Year Prior	0.003 (0.003)	0.004 (0.004)	0.002 (0.002)
3 Year Prior	-0.001 (0.003)	-0.004 (0.003)	0.001 (0.003)
<i>Panel B: Standardized Cumulative PM_{2.5}</i>			
<i>ELA</i>			
1 Year Prior	-0.006 (0.003)**	-0.006 (0.003)*	-0.005 (0.004)
2 Year Prior	-0.007 (0.003)**	-0.008 (0.005)*	-0.006 (0.003)**
3 Year Prior	-0.004 (0.003)	-0.007 (0.004)**	-0.002 (0.004)
<i>Math</i>			
1 Year Prior	-0.007 (0.004)*	-0.007 (0.005)	-0.006 (0.005)
2 Year Prior	0.004 (0.004)	0.005 (0.006)	0.001 (0.003)
3 Year Prior	-0.005 (0.003)	-0.010 (0.004)**	-0.002 (0.004)

Table A.19: Heterogeneous Effects Analysis for Black Students - 75% Exposure
Cont.

	All (1)	Primary School (2)	Middle School (3)
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.057 (0.043)	-0.003 (0.046)	-0.098 (0.050)*
2 Year Prior	0.035 (0.059)	0.031 (0.072)	0.049 (0.059)
3 Year Prior	-0.051 (0.027)*	-0.075 (0.029)***	-0.031 (0.029)
<i>Math</i>			
1 Year Prior	0.024 (0.063)	-0.012 (0.088)	0.061 (0.063)
2 Year Prior	0.062 (0.068)	0.071 (0.097)	0.045 (0.064)
3 Year Prior	-0.061 (0.035)*	-0.107 (0.041)***	-0.021 (0.033)
Observations	39108	19554	19554

Notes: The dependent variables are standardized ELA and math test scores of Black students. Wildfire exposure is measured with at least 75% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.20: Heterogeneous Effects Analysis for Hispanic Students - 75% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.004 (0.002)**	-0.002 (0.002)	-0.006 (0.002)***
2 Year Prior	-0.001 (0.001)	-0.004 (0.002)**	0.003 (0.002)
3 Year Prior	0.004 (0.002)**	0.001 (0.003)	0.006 (0.002)***
<i>Math</i>			
1 Year Prior	-0.002 (0.002)	-0.001 (0.003)	-0.004 (0.002)**
2 Year Prior	-0.004 (0.002)*	-0.007 (0.002)***	-0.001 (0.002)
3 Year Prior	-0.000 (0.003)	-0.005 (0.004)	0.004 (0.003)
<i>Panel B: Standardized Cumulative PM_{2.5}</i>			
<i>ELA</i>			
1 Year Prior	-0.004 (0.003)	-0.002 (0.003)	-0.007 (0.004)*
2 Year Prior	-0.004 (0.003)	-0.009 (0.003)***	0.001 (0.004)
3 Year Prior	0.005 (0.002)**	-0.000 (0.003)	0.008 (0.003)***
<i>Math</i>			
1 Year Prior	-0.001 (0.003)	0.001 (0.004)	-0.004 (0.003)
2 Year Prior	-0.007 (0.004)*	-0.010 (0.005)**	-0.004 (0.004)
3 Year Prior	-0.001 (0.004)	-0.007 (0.005)	0.005 (0.004)

Table A.20: Heterogeneous Effects Analysis for Hispanic Students - 75% Exposure
Cont.

	All (1)	Primary School (2)	Middle School (3)
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.046 (0.021)**	-0.041 (0.025)*	-0.053 (0.031)*
2 Year Prior	-0.037 (0.068)	-0.060 (0.044)	-0.009 (0.101)
3 Year Prior	0.002 (0.027)	-0.032 (0.033)	0.026 (0.027)
<i>Math</i>			
1 Year Prior	-0.016 (0.025)	-0.048 (0.036)	0.013 (0.027)
2 Year Prior	-0.037 (0.055)	-0.021 (0.068)	-0.059 (0.071)
3 Year Prior	-0.010 (0.039)	-0.044 (0.047)	0.020 (0.043)
Observations	39480	19740	19740

Notes: The dependent variables are standardized ELA and math test scores of Hispanic students. Wildfire exposure is measured with at least 75% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.21: Heterogeneous Effects Analysis for White Students - 75% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.001 (0.001)	0.000 (0.001)	-0.002 (0.001)*
2 Year Prior	0.002 (0.001)**	-0.001 (0.001)	0.003 (0.001)***
3 Year Prior	0.000 (0.001)	-0.001 (0.001)	0.001 (0.001)
<i>Math</i>			
1 Year Prior	-0.002 (0.001)**	-0.001 (0.001)	-0.004 (0.001)***
2 Year Prior	0.002 (0.001)*	-0.000 (0.001)	0.003 (0.001)***
3 Year Prior	-0.001 (0.001)	-0.000 (0.001)	-0.001 (0.001)
<i>Panel B: Standardized Cumulative PM_{2.5}</i>			
<i>ELA</i>			
1 Year Prior	-0.001 (0.001)	-0.000 (0.002)	-0.001 (0.001)
2 Year Prior	0.003 (0.002)*	-0.000 (0.002)	0.005 (0.002)***
3 Year Prior	-0.000 (0.001)	-0.001 (0.001)	0.000 (0.002)
<i>Math</i>			
1 Year Prior	-0.004 (0.002)**	-0.002 (0.002)	-0.006 (0.002)***
2 Year Prior	0.004 (0.002)**	0.002 (0.002)	0.004 (0.002)*
3 Year Prior	-0.003 (0.002)*	-0.002 (0.002)	-0.004 (0.002)**

Table A.21: Heterogeneous Effects Analysis for White Students - 75% Exposure
Cont.

	All (1)	Primary School (2)	Middle School (3)
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.016 (0.010)*	-0.017 (0.013)	-0.016 (0.012)
2 Year Prior	-0.034 (0.025)	-0.062 (0.026)**	-0.019 (0.032)
3 Year Prior	-0.060 (0.012)***	-0.062 (0.014)***	-0.057 (0.013)***
<i>Math</i>			
1 Year Prior	-0.035 (0.012)***	-0.033 (0.017)*	-0.040 (0.013)***
2 Year Prior	-0.025 (0.023)	-0.049 (0.038)	-0.018 (0.029)
3 Year Prior	-0.066 (0.014)***	-0.070 (0.017)***	-0.060 (0.014)***
Observations	146298	73149	73149

Notes: The dependent variables are standardized ELA and math test scores of white students. Wildfire exposure is measured with at least 75% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.22: Heterogeneous Effects Analysis for Female Students - 75% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.004 (0.001)***	-0.003 (0.001)**	-0.006 (0.001)***
2 Year Prior	0.002 (0.001)*	-0.001 (0.001)	0.004 (0.001)***
3 Year Prior	0.002 (0.001)**	0.002 (0.001)	0.002 (0.001)
<i>Math</i>			
1 Year Prior	-0.003 (0.001)***	-0.003 (0.001)**	-0.004 (0.001)***
2 Year Prior	0.001 (0.001)	-0.002 (0.002)	0.003 (0.001)**
3 Year Prior	-0.001 (0.001)	-0.003 (0.002)*	0.001 (0.002)
<i>Panel B: Standardized Cumulative PM_{2.5}</i>			
<i>ELA</i>			
1 Year Prior	-0.006 (0.002)***	-0.005 (0.002)***	-0.007 (0.002)***
2 Year Prior	0.002 (0.002)	-0.002 (0.002)	0.005 (0.002)**
3 Year Prior	0.003 (0.002)*	0.003 (0.002)*	0.002 (0.002)
<i>Math</i>			
1 Year Prior	-0.004 (0.002)***	-0.005 (0.002)**	-0.004 (0.002)**
2 Year Prior	0.002 (0.002)	-0.002 (0.003)	0.004 (0.002)*
3 Year Prior	-0.002 (0.002)	-0.005 (0.002)**	-0.000 (0.002)

Table A.22: Heterogeneous Effects Analysis for Female Students - 75% Exposure
Cont.

	All (1)	Primary School (2)	Middle School (3)
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.042 (0.013)***	-0.029 (0.016)*	-0.055 (0.016)***
2 Year Prior	-0.014 (0.035)	-0.043 (0.025)*	0.005 (0.054)
3 Year Prior	0.003 (0.015)	-0.014 (0.015)	0.015 (0.017)
<i>Math</i>			
1 Year Prior	-0.028 (0.013)**	-0.040 (0.018)**	-0.019 (0.016)
2 Year Prior	-0.017 (0.028)	-0.036 (0.043)	-0.012 (0.036)
3 Year Prior	-0.037 (0.018)**	-0.058 (0.021)***	-0.019 (0.018)
Observations	132894	66447	66447

Notes: The dependent variables are standardized ELA and math test scores of female students. Wildfire exposure is measured with at least 75% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.23: Heterogeneous Effects Analysis for Male Students - 75% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.002 (0.001)*	-0.003 (0.001)***	-0.001 (0.001)
2 Year Prior	-0.000 (0.001)	-0.001 (0.001)	0.000 (0.001)
3 Year Prior	-0.002 (0.001)**	-0.005 (0.001)***	0.000 (0.001)
<i>Math</i>			
1 Year Prior	-0.003 (0.001)***	-0.001 (0.001)	-0.006 (0.001)***
2 Year Prior	0.001 (0.001)	-0.001 (0.002)	0.003 (0.001)*
3 Year Prior	-0.000 (0.002)	0.001 (0.002)	-0.002 (0.002)
<i>Panel B: Standardized Cumulative PM_{2.5}</i>			
<i>ELA</i>			
1 Year Prior	-0.002 (0.002)	-0.006 (0.002)***	0.001 (0.002)
2 Year Prior	-0.001 (0.002)	-0.002 (0.002)	-0.000 (0.002)
3 Year Prior	-0.003 (0.001)**	-0.007 (0.002)***	-0.000 (0.002)
<i>Math</i>			
1 Year Prior	-0.004 (0.002)**	-0.003 (0.002)	-0.007 (0.002)***
2 Year Prior	0.002 (0.003)	0.001 (0.003)	0.003 (0.003)
3 Year Prior	-0.002 (0.002)	-0.001 (0.002)	-0.003 (0.002)

Table A.23: Heterogeneous Effects Analysis for Male Students - 75% Exposure Cont.

	All (1)	Primary School (2)	Middle School (3)
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.016 (0.012)	-0.020 (0.015)	-0.015 (0.015)
2 Year Prior	-0.013 (0.035)	-0.053 (0.029)*	0.018 (0.055)
3 Year Prior	-0.061 (0.014)***	-0.079 (0.015)***	-0.047 (0.017)***
<i>Math</i>			
1 Year Prior	-0.035 (0.015)**	-0.034 (0.018)*	-0.041 (0.018)**
2 Year Prior	0.004 (0.031)	-0.003 (0.040)	-0.002 (0.043)
3 Year Prior	-0.035 (0.018)**	-0.054 (0.022)**	-0.019 (0.017)
Observations	134274	67137	67137

Notes: The dependent variables are standardized ELA and math test scores of male students. Wildfire exposure is measured with at least 75% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.24: Heterogeneous Effects Analysis for Econ. Disadvantaged Students -
75% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.003 (0.001)***	-0.003 (0.001)***	-0.003 (0.001)**
2 Year Prior	-0.001 (0.001)	-0.003 (0.001)**	0.000 (0.001)
3 Year Prior	-0.001 (0.001)	-0.003 (0.001)**	0.001 (0.001)
<i>Math</i>			
1 Year Prior	-0.003 (0.001)**	-0.002 (0.001)	-0.005 (0.001)***
2 Year Prior	-0.000 (0.002)	-0.002 (0.002)	0.002 (0.002)
3 Year Prior	-0.002 (0.002)	-0.003 (0.002)	-0.001 (0.002)
<i>Panel B: Standardized Cumulative PM_{2.5}</i>			
<i>ELA</i>			
1 Year Prior	-0.004 (0.002)***	-0.006 (0.002)***	-0.003 (0.002)
2 Year Prior	-0.004 (0.002)*	-0.006 (0.003)**	-0.002 (0.002)
3 Year Prior	-0.002 (0.002)	-0.005 (0.002)***	0.000 (0.002)
<i>Math</i>			
1 Year Prior	-0.004 (0.002)**	-0.003 (0.002)	-0.006 (0.002)**
2 Year Prior	-0.000 (0.003)	-0.002 (0.004)	0.001 (0.003)
3 Year Prior	-0.005 (0.002)**	-0.007 (0.003)**	-0.003 (0.002)

Table A.24: Heterogeneous Effects Analysis for Econ. Disadvantaged Students -
75% Exposure Cont.

	All (1)	Primary School (2)	Middle School (3)
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Prior	-0.031 (0.013)**	-0.022 (0.015)	-0.042 (0.017)**
2 Year Prior	-0.011 (0.048)	-0.050 (0.036)	0.027 (0.074)
3 Year Prior	-0.037 (0.016)**	-0.062 (0.017)***	-0.019 (0.018)
<i>Math</i>			
1 Year Prior	-0.020 (0.015)	-0.016 (0.019)	-0.027 (0.017)
2 Year Prior	0.011 (0.041)	0.008 (0.048)	0.006 (0.054)
3 Year Prior	-0.049 (0.022)**	-0.077 (0.026)***	-0.025 (0.021)
Observations	123750	61875	61875

Notes: The dependent variables are standardized ELA and math test scores of Economically Disadvantaged students. Wildfire exposure is measured with at least 75% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.25: Placebo Test: Future Wildfire Exposure - 25% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Future	0.001 (0.001)	-0.001 (0.001)	0.002 (0.001)*
<i>Math</i>			
1 Year Future	0.001 (0.001)	-0.001 (0.002)	0.003 (0.001)**
<i>Panel B: Cumulative PM2.5</i>			
<i>ELA</i>			
1 Year Future	0.002 (0.002)	-0.001 (0.002)	0.004 (0.002)*
<i>Math</i>			
1 Year Future	0.001 (0.002)	-0.003 (0.003)	0.004 (0.002)**
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Future	0.011 (0.012)	-0.003 (0.013)	0.023 (0.016)
<i>Math</i>			
1 Year Future	0.007 (0.012)	-0.015 (0.017)	0.029 (0.013)**
Observations	160050	80025	80025

Notes: The dependent variables are standardized ELA and math test scores. Wildfire exposure is measured with at least 25% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Table A.26: Placebo Test: Future Wildfire Exposure - 50% Exposure

	All (1)	Primary School (2)	Middle School (3)
<i>Panel A: Smoke Days (*10)</i>			
<i>ELA</i>			
1 Year Future	0.002 (0.001)	0.001 (0.001)	0.003 (0.001)*
<i>Math</i>			
1 Year Future	0.001 (0.001)	-0.001 (0.002)	0.004 (0.002)**
<i>Panel B: Cumulative PM2.5</i>			
<i>ELA</i>			
1 Year Future	0.003 (0.002)	0.001 (0.002)	0.005 (0.002)**
<i>Math</i>			
1 Year Future	0.001 (0.002)	-0.002 (0.003)	0.005 (0.002)**
<i>Panel C: Unhealthy Air Quality Days (*10)</i>			
<i>ELA</i>			
1 Year Future	0.023 (0.009)**	0.007 (0.010)	0.037 (0.012)***
<i>Math</i>			
1 Year Future	0.007 (0.013)	-0.019 (0.016)	0.032 (0.013)**
Observations	160050	80025	80025

Notes: The dependent variables are standardized ELA and math test scores. Wildfire exposure is measured with at least 50% of the population being exposed to wildfire smoke in the school district. All estimates include the full panel of control variables. Standard errors are reported in parentheses and are clustered at the GSD level. Estimates are weighted by the grade size of each GSD. */**/** denotes significance at the 10/5/1 percent level.

Appendix B

B.1 Tables

Table B.1: Descriptive Statistics

	Mean	Min	Max
<i>Panel A: Student Characteristics School Year 2013-17</i>			
Number of Manufacturing CTE Courses Taken	0.042	0	6
Any Manufacturing CTE Course-taking	0.034	0	1
Female	0.488	0	1
Asian	0.018	0	1
Black	0.232	0	1
Native American	0.002	0	1
Hispanic	0.075	0	1
White	0.656	0	1
Multi/Other Race	0.017	0	1
English Language Learners	0.022	0	1
Former English Language Learners	0.025	0	1
Gifted	0.013	0	1
With Disabilities	0.140	0	1
Unemployment Rate	5.809	2.700	14.400
Observations	1,469,574		
<i>Panel B: School Characteristics School Year 2013-17</i>			
Manufacturing CTE Course-taking per 100 Students	3.882	0	81
Any Manufacturing CTE Course-taking	0.419	0	1
Nontraditional School	0.119	0	1
Charter School	0.053	0	1
Number of Students, Grades 9-12	768	1	2800
Observations	2069		
<i>Panel C: County Level Labor Market Signals Year 2012-2017</i>			
Manufacturing WARN Layoff Notices	0.305	0	8
Manufacturing Subsidized Job Notices	0.853	0	10
Other WARN Layoff Notices	0.601	0	16
Other Subsidized Job Notices	0.612	0	25
Manufacturing WARN Layoffs per 100 Population	0.072	0	9.105
Manufacturing Subsidized Jobs per 100 Population	0.299	0	7.524
Other WARN Layoffs per 100 Working-Age Population	0.110	0	5.843
Other Subsidized Jobs per 100 Working-Age Population	0.103	0	6.050
Observations	564		

Notes: Panel A includes students-level manufacturing CTE course enrollment, and demographic and socioeconomic variables from school year 2013-14 to 2017-18. Panel B includes school-level manufacturing CTE course enrollment, course offering, and school characteristics from school year 2013-14 to 2017-18. Panel C includes county-level labor market signals from the calendar year 2012 to 2017. The number of layoffs and subsidized jobs are measured per 100 persons age 18-64 in the county.

Table B.2: Estimated Effects of County Labor Market Signals on Student-level Manufacturing Enrollment

	All Grades		Grade 9		Grade 10		Grade 11		Grade 12	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Layoff Notices Year t		-0.000731 (0.000671)		0.000093 (0.000654)		-0.000830 (0.000939)		-0.000986 (0.001182)		-0.001615 (0.000890) ⁺
Layoffs Year t	-0.000191 (0.001151)	0.000377 (0.001220)	-0.002227 (0.002274)	-0.002420 (0.002257)	-0.000512 (0.001670)	0.000434 (0.001856)	-0.000726 (0.001368)	-0.000109 (0.001477)	0.001026 (0.001419)	0.001926 (0.001581)
Subsidized job Notices Year t		0.001635 (0.000618) ^{***}		-0.000089 (0.000768)		0.002382 (0.000953) ^{**}		0.002227 (0.000946) ^{**}		0.001654 (0.000721) ^{**}
Subsidized jobs Year t	0.001305 (0.001659)	-0.001518 (0.001920)	-0.005388 (0.001974) ^{***}	-0.005234 (0.002397) ^{**}	0.003643 (0.002386)	-0.000629 (0.002446)	0.003257 (0.002177)	-0.000548 (0.002747)	0.003979 (0.002324) ⁺	0.001014 (0.002796)
Layoff Notices Year t-1		-0.000988 (0.000574) ⁺		0.000061 (0.000717)		-0.002509 (0.000887) ^{***}		-0.000946 (0.000838)		-0.000775 (0.000649)
Layoffs Year t-1	0.001088 (0.001342)	0.001514 (0.001304)	-0.000822 (0.003022)	-0.001085 (0.003033)	-0.000084 (0.001472)	0.001230 (0.001600)	0.003303 (0.002496)	0.003625 (0.002414)	0.000136 (0.001448)	0.000510 (0.001403)
Subsidized job Notices Year t-1		0.001552 (0.000559) ^{***}		0.000496 (0.000516)		0.001821 (0.000916) ^{**}		0.002228 (0.000807) ^{***}		0.001627 (0.000676) ^{**}
Subsidized jobs Year t-1	0.001757 (0.001519)	-0.000748 (0.001477)	-0.003464 (0.001314) ^{***}	-0.004188 (0.001450) ^{***}	0.002602 (0.002952)	-0.000350 (0.002675)	0.004422 (0.002116) ^{**}	0.000806 (0.002207)	0.003484 (0.001759) ^{**}	0.000667 (0.001975)
Observations	1,275,992	1,275,992	337,015	337,015	329,944	329,944	308,824	308,824	300,202	300,202
Dep. Var Mean	0.034	0.034	0.019	0.019	0.037	0.037	0.042	0.042	0.038	0.038

Notes: The dependent variable in each specification is an indicator equal to one if a student enrolled in any manufacturing courses during the school year. The sample includes public Tennessee high school students in grades 9-12, school years 2013-14 to 2017-18. Standard errors, in parentheses, are clustered by school. ⁺ $p < 0.10$, ^{**} $p < 0.05$, ^{***} $p < 0.01$.

Table B.3: Estimated Effects of Labor Market Signals on Manufacturing Enrollment: Grade 10-12 Student Subgroups

	High ACT (1)	Low or No ACT (2)	Black (3)	Hispanic (4)	White (5)	Female (6)	Male (7)	ELL (8)	Disability (9)
Layoff Notices Year t	-0.000280 (0.000743)	-0.001607 (0.001004)	-0.001150 (0.001075)	-0.002488 (0.001606)	-0.001741 (0.000967) ⁺	-0.000823 (0.000340)**	-0.001402 (0.001329)	-0.002934 (0.002009)	-0.001626 (0.001063)
Layoffs Year t	-0.001183 (0.001106)	0.002369 (0.001767)	0.004334 (0.003631)	0.008241 (0.003829)**	0.000517 (0.001180)	0.001335 (0.000695) ⁺	0.000083 (0.002381)	0.022596 (0.007924)***	0.000807 (0.002817)
Subsidized Job Notices Year t	0.000729 (0.000624)	0.002695 (0.000973)***	0.001129 (0.000784)	0.004499 (0.001182)***	0.002686 (0.000845)***	0.001037 (0.000445)**	0.003081 (0.001031)***	0.002287 (0.001568)	0.003901 (0.001249)***
Subsidized Jobs Year t	-0.000373 (0.002723)	-0.000235 (0.002562)	0.004498 (0.002739)	0.001189 (0.004056)	-0.002078 (0.002490)	-0.000156 (0.001009)	0.000219 (0.003659)	0.011195 (0.006177) ⁺	-0.001443 (0.003503)
Layoff Notices Year t-1	-0.000383 (0.000491)	-0.001695 (0.000919) ⁺	-0.002208 (0.001010)**	-0.002254 (0.001541)	-0.001311 (0.000773) ⁺	-0.000475 (0.000341)	-0.002124 (0.001031)**	-0.001801 (0.002430)	-0.002977 (0.001099)***
Layoffs Year t-1	0.002184 (0.000899)**	0.002119 (0.002056)	0.003046 (0.002670)	0.006262 (0.002608)**	0.001404 (0.001496)	0.001250 (0.000855)	0.000960 (0.002345)	0.001879 (0.004010)	-0.001767 (0.001637)
Subsidized Job Notices Year t-1	0.001290 (0.000585)**	0.001956 (0.000912)**	0.000702 (0.000769)	0.002632 (0.001178)**	0.002771 (0.000756)***	0.000893 (0.000388)**	0.002894 (0.001065)***	0.000431 (0.001127)	0.003210 (0.000946)***
Subsidized Jobs Year t-1	-0.000805 (0.002083)	0.001001 (0.001993)	0.004490 (0.002417) ⁺	0.004164 (0.003079)	-0.001248 (0.001885)	0.000792 (0.000718)	0.000160 (0.002970)	0.008746 (0.004078)**	-0.000045 (0.002659)
Observations	320,587	487,096	210,825	71,468	621,434	459,933	479,039	39,404	126,948
Dep. Var Mean	0.021	0.050	0.017	0.032	0.048	0.008	0.069	0.030	0.055

Notes: The dependent variable in each specification is an indicator equal to one if a student enrolled in any manufacturing courses during the school year. The sample includes public Tennessee high school students in grades 10-12, school years 2013-14 to 2017-18. Standard errors, in parentheses, are clustered by school. ⁺ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: Estimated Effects of County and Metro Area Labor Market Signals on Student-Level Manufacturing Enrollment

	County-Level		Metro-Level	
	(1)	(2)	(3)	(4)
Layoff Notices Year t	0.000345 (0.000695)	0.000566 (0.000733)	-0.000045 (0.000779)	0.000118 (0.000812)
Layoffs Year t	0.001482 (0.001090)	0.001428 (0.000990)	-0.002382 (0.006273)	-0.009419 (0.009466)
Subsidized job Notices Year t	0.002079 (0.000815)**	0.001945 (0.000785)**	0.000469 (0.000411)	0.000629 (0.000464)
Subsidized jobs Year t	-0.004773 (0.003121)	-0.004519 (0.003052)	-0.005829 (0.004809)	-0.009436 (0.007109)
Layoff Notices Year t-1	-0.001435 (0.000704)**	-0.001476 (0.000757)+	-0.000005 (0.000679)	-0.000096 (0.000682)
Layoffs Year t-1	0.001822 (0.001006)+	0.001911 (0.001009)+	0.005137 (0.009688)	0.000472 (0.010424)
Subsidized job Notices Year t-1	0.001944 (0.000611)***	0.002162 (0.000715)***	-0.000216 (0.000268)	-0.000098 (0.000287)
Subsidized jobs Year t-1	-0.000855 (0.001532)	-0.000720 (0.002061)	0.000315 (0.004143)	-0.004999 (0.006727)
Layoff Notices Year t-2		-0.000476 (0.000767)		-0.000660 (0.000719)
Layoffs Year t-2		-0.003317 (0.005522)		0.008927 (0.025691)
Subsidized job Notices Year t-2		0.000589 (0.000502)		-0.000248 (0.000304)
Subsidized jobs Year t-2		0.000208 (0.002124)		-0.002735 (0.004418)
Observations	995,065	995,065	995,065	995,065
Dep. Var. Mean	0.036	0.036	0.036	0.036

Notes: The dependent variable in each specification is an indicator equal to one if a student enrolled in any manufacturing courses during the school year. The sample includes public Tennessee high school students in grades 9-12, school years 2014-15 to 2017-18. Standard errors, in parentheses, are clustered by school. + $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5: Estimated Effects of County and Metro Area Labor Market Signals on School-Level Manufacturing Course Availability

	County-Level			MSA-Level		
	(1)	(2)	(3)	(4)	(5)	(6)
Layoff Notices Year t		-0.016001 (0.009103) ⁺	-0.016091 (0.009085) ⁺		-0.002813 (0.008226)	-0.005843 (0.009231)
Layoffs Year t	0.007442 (0.016888)	0.021069 (0.019306)	0.018614 (0.017896)	-0.054154 (0.032265) ⁺	-0.034184 (0.046626)	-0.234205 (0.088458) ^{***}
Subsidized Job Notices Year t		0.023845 (0.007861) ^{***}	0.025485 (0.007667) ^{***}		0.005976 (0.003938)	0.001072 (0.005267)
Subsidized Jobs Year t	-0.033866 (0.024588)	-0.064256 (0.026635) ^{**}	-0.067191 (0.026951) ^{**}	0.027386 (0.050825)	-0.002117 (0.054375)	-0.058468 (0.074701)
Layoff Notices Year t-1		-0.029962 (0.007719) ^{***}	-0.032306 (0.008064) ^{***}		-0.004908 (0.007574)	-0.014121 (0.007830) ⁺
Layoffs Year t-1	-0.011307 (0.009241)	0.005857 (0.011223)	0.002034 (0.009648)	-0.075993 (0.037555) ^{**}	-0.054421 (0.044468)	-0.157980 (0.072563) ^{**}
Subsidized Job Notices Year t-1		0.017320 (0.006615) ^{***}	0.020179 (0.007378) ^{***}		-0.004790 (0.003357)	-0.002396 (0.003710)
Subsidized Jobs Year t-1	-0.020963 (0.016180)	-0.039847 (0.018245) ^{**}	-0.045747 (0.021688) ^{**}	0.022122 (0.030489)	0.058242 (0.046710)	-0.106983 (0.081277)
Layoff Notices Year t-2			-0.001715 (0.008248)			0.002332 (0.008319)
Layoffs Year t-2			-0.082363 (0.075396)			-0.247119 (0.246376)
Subsidized Job Notices Year t-2			0.007856 (0.005524)			-0.006828 (0.003048) ^{**}
Subsidized Jobs Year t-2			-0.016659 (0.016953)			-0.078884 (0.050173)
Observations	1501	1501	1501	1501	1501	1501
Dep. Var. Mean	0.466	0.466	0.466	0.466	0.466	0.466

Notes: The dependent variable in each specification is an indicator equal to one if a school offered any manufacturing courses during the year. The sample includes public Tennessee high schools, school years 2014-15 to 2016-17. Standard errors, in parentheses, are clustered by school. ⁺ $p < 0.10$, ^{**} $p < 0.05$, ^{***} $p < 0.01$.

Table B.6: Falsification Tests: Estimated Effects of Future Labor Market Signals on Student-Level Manufacturing Enrollment and School-Level Manufacturing Availability

	Student Any Course-Taking		School Any Course-offering	
	(1)	(2)	(3)	(4)
Layoffs Notices Year t+2		-0.000988 (0.001146)		-0.008488 (0.016887)
Layoffs Year t+2	-0.000858 (0.000655)	-0.001058 (0.000787)	0.022978 (0.013053) ⁺	0.014883 (0.010243)
Subsidized Job Notices Year t+2		0.001587 (0.001297)		0.000480 (0.017060)
Subsidized Jobs Year t+2	0.000110 (0.002460)	-0.000396 (0.003171)	-0.036528 (0.030156)	-0.025302 (0.038256)
Layoffs Notices Year t		-0.001685 (0.000994) ⁺		-0.018732 (0.010681) ⁺
Layoffs Year t	0.002081 (0.011579)	0.009723 (0.014019)	0.016053 (0.084087)	0.081180 (0.089884)
Subsidized Job Notices Year t		0.000898 (0.000679)		0.024279 (0.007869) ^{***}
Subsidized Jobs Year t	0.000456 (0.002229)	-0.002116 (0.002765)	0.007537 (0.018188)	-0.030032 (0.023998)
Layoff Notices Year t-1		-0.001119 (0.000794)		-0.006293 (0.009377)
Layoffs Year t-1	0.008249 (0.005308)	0.010637 (0.005328) ^{**}	0.085047 (0.064637)	0.093574 (0.066605)
Subsidized Job Notices Year t-1		0.001593 (0.000760) ^{**}		0.019111 (0.009514) ^{**}
Subsidized Jobs Year t-1	0.002429 (0.002149)	0.000104 (0.002103)	-0.002067 (0.017557)	-0.027839 (0.021295)
Observations	713420	713420	1081	1081
Dep. Var Mean	0.030	0.030	0.414	0.414

Notes: The dependent variable in Columns 1-2 is an indicator equal to one if a student enrolled in any manufacturing courses during the school year. The dependent variable in Columns 3-4 is an indicator equal to one if a school offered any manufacturing courses during the year. The sample includes public Tennessee high school students (Columns 1-2) or schools (Columns 3-4), school years 2013-14 to 2015-16. Standard errors, in parentheses, are clustered by school. ⁺ $p < 0.10$, ^{**} $p < 0.05$, ^{***} $p < 0.01$.

Table B.7: Estimated Effects of Other Industries' Labor Market Signals on Student-Level Manufacturing Enrollment and School-Level Manufacturing Availability

	County-Level (1)	MSA-Level (2)
<i>Panel A: Any Mfr. Courses-taking by Students</i>		
Layoff Notices Year t	-0.000603 (0.000477)	0.000097 (0.000500)
Layoffs Year t	-0.000419 (0.001931)	0.001644 (0.006035)
Subsidized Job Notices Year t	0.001138 (0.000428)***	0.000855 (0.000319)***
Subsidized Jobs Year t	0.000424 (0.002108)	-0.003027 (0.003845)
Layoff Notices Year t-1	0.000016 (0.000415)	-0.000028 (0.000451)
Layoffs Year t-1	-0.002313 (0.001731)	0.000817 (0.006794)
Subsidized Job Notices Year t-1	-0.000619 (0.000404)	-0.000091 (0.000238)
Subsidized Jobs Year t-1	0.007077 (0.002885)**	0.004868 (0.004487)
Observations	1275992	1275992
Dep. Var. Mean	0.034	0.034
<i>Panel B: Any Mfr. Courses Availability by School</i>		
Layoff Notices Year t	0.000795 (0.005690)	-0.001411 (0.005519)
Layoffs Year t	0.012302 (0.015137)	-0.038396 (0.040019)
Subsidized Job Notices Year t	0.000993 (0.005050)	0.004232 (0.003003)
Subsidized Jobs Year t	0.019363 (0.024591)	0.001853 (0.057532)
Layoff Notices Year t-1	0.001875 (0.004640)	0.000305 (0.003441)
Layoffs Year t-1	-0.014765 (0.012089)	-0.102823 (0.030401)***
Subsidized Job Notices Year t-1	0.000696 (0.004553)	0.000269 (0.002437)
Subsidized Jobs Year t-1	0.028618 (0.020498)	0.024791 (0.050536)
Observations	1945	1945
Dep. Var. Mean	0.439	0.439

Notes: The dependent variable in Columns 1-2 is an indicator equal to one if a student enrolled in any manufacturing courses during the school year. The dependent variable in Columns 3-4 is an indicator equal to one if a school offered any manufacturing courses during the year. The sample includes public Tennessee high school students (Columns 1-2) or schools (Columns 3-4), school years 2013-14 to 2017-18. Standard errors, in parentheses, are clustered by school. ⁺ $p < 0.10$, ^{**} $p < 0.05$, ^{***} $p < 0.01$.

B.2 Figures

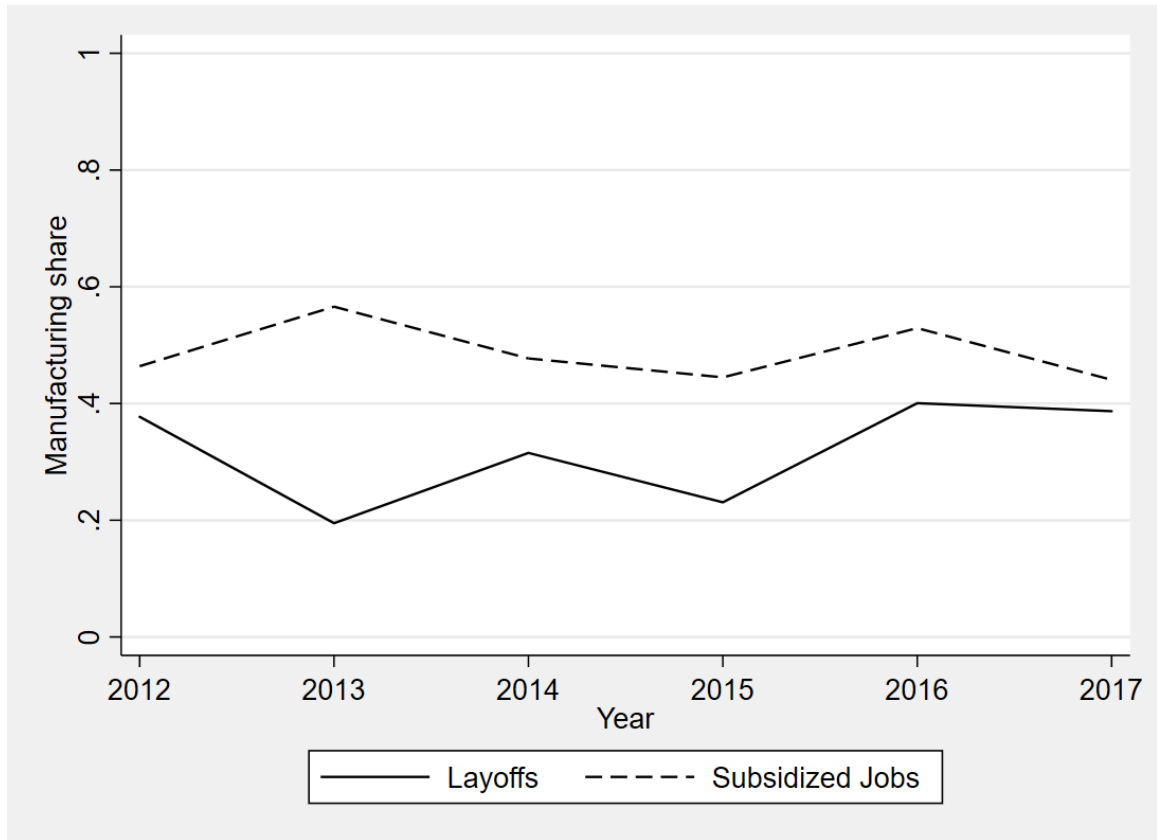


Figure B.1: Statewide Manufacturing Layoffs and Subsidized Jobs Shares

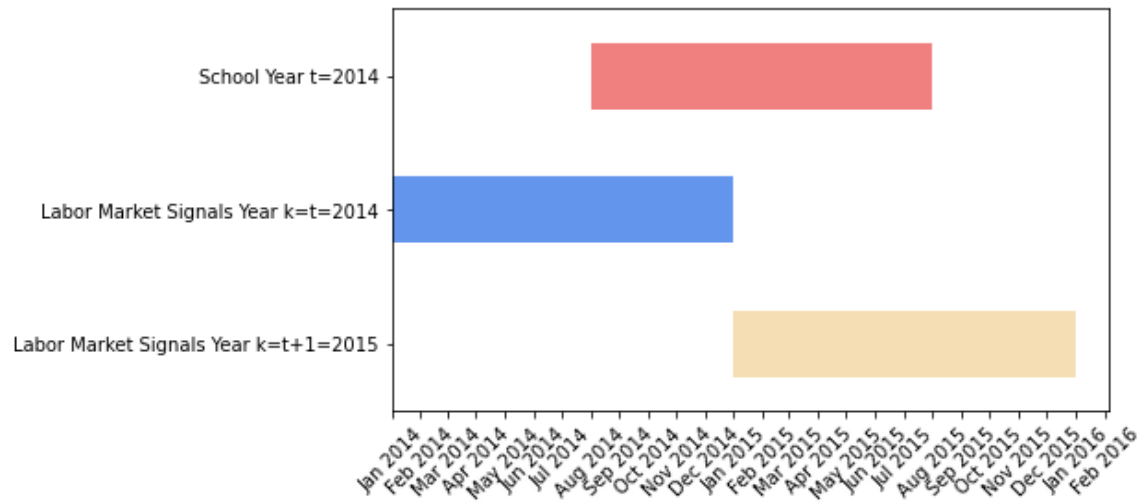


Figure B.2: Timing of Labor Market Signals in Relation to School Year

B.3 Supplementary Tables

Table B.8: Descriptive Statistics for Manufacturing Courses Enrollees/Offering Schools

	Mean	Min	Max
<i>Panel A: Student Characteristics School Year 2013-17</i>			
Number of Manufacturing CTE Courses Taken	1.207	0	6
Any Manufacturing CTE Course-taking	1.000	1	1
Female	0.100	0	1
Asian	0.007	0	1
Black	0.099	0	1
Native American	0.002	0	1
Hispanic	0.058	0	1
White	0.822	0	1
Multi/Other Race	0.012	0	1
English Language Learners	0.015	0	1
Former English Language Learners	0.019	0	1
Gifted	0.005	0	1
With Disabilities	0.186	0	1
Observations	50,626		
<i>Panel B: School Characteristics School Year 2013-17</i>			
Manufacturing CTE Course-taking per 100 Students	9.194	0	81
Any Manufacturing CTE Course-taking	1.000	1	1
Nontraditional School	0.000	0	0
Charter School	0.005	0	1
Number of Students, Grades 9-12	945	5	2800
Observations	867		

Notes: Panel A includes students' demographic and socioeconomic variables from the school year 2013-14 to 2017-18 for schools that enroll in at least one manufacturing CTE courses. Panel B includes school-level characteristics from the school year 2013-14 to 2017-18 for schools that offer manufacturing CTE courses.

Table B.9: Nonlinear Models: Estimated Effects of Manufacturing Labor Market Signals on School-Level Manufacturing Enrollment and Availability

	Manufacturing Course-Taking				Manufacturing Course Availability			
	Lin. Prob.	Poisson	Lin. Prob.	Poisson	Lin. Prob.	Logit	Lin. Prob.	Logit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Layoff Notices Year t			-2.888603 (1.444909)**	-0.084679 (0.025591)***			0.416575 (1.651664)	-0.533007 (0.228944)**
Layoffs Year t	-0.648325 (1.889365)	0.002337 (0.024799)	1.068339 (2.383939)	0.042412 (0.025509) ⁺	-2.069946 (5.933360)	0.491055 (0.786187)	-3.983465 (6.786425)	1.786715 (0.981913) ⁺
Subsidized Job Notices Year t			2.893690 (0.917216)***	0.084721 (0.013272)***			2.804848 (1.242341)**	0.449646 (0.145387)***
Subsidized Jobs Year t	0.029211 (1.571581)	-0.001454 (0.021522)	-3.493907 (1.816499) ⁺	-0.117859 (0.027727)***	-0.995279 (2.012988)	-0.355969 (0.220009)	-2.948128 (2.049372)	-0.948926 (0.326313)***
Layoff Notices Year t-1			-1.381263 (0.982935)	-0.057597 (0.018878)***			-0.945520 (1.113180)	-0.554988 (0.204022)***
Layoffs Year t-1	-1.289540 (2.085202)	-0.023481 (0.024984)	-0.301252 (2.120684)	0.006213 (0.026290)	6.190038 (6.547245)	0.784746 (0.655668)	7.309649 (6.714587)	1.666699 (0.806974)**
Subsidized Job Notices Year t-1			3.149578 (1.036539)***	0.059440 (0.012192)***			2.226620 (1.108860)**	0.370867 (0.134481)***
Subsidized Jobs Year t-1	2.639468 (1.585156) ⁺	0.054268 (0.021414)**	-1.309872 (1.601531)	-0.022568 (0.026567)	1.163199 (2.347424)	-0.131907 (0.242985)	-0.332784 (2.170918)	-0.697012 (0.354932)**
Observations	1088	1088	1088	1088	510	510	510	510
Dep. Var. Mean	0.775	0.775	0.775	0.775	0.529	0.529	0.529	0.529

Notes: The dependent variable in Columns 1-4 is the total number of manufacturing course enrollments at a school during a given year. The dependent variable in Columns 5-8 is the binary incidence of any manufacturing course-taking in a school during a given year. The sample public Tennessee high schools, school years 2013-14 to 2017-18. ⁺ $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C

C.1 Tables

Table C.1: Descriptive Statistics

	All Students (1)	Flagship Eligible (2)	Flagship Non-Eligible (3)
<i>Panel A: High School Graduates</i>			
Enrolled in any public college ^a	0.58	0.48	0.59
Enrolled in UTK ^b	0.05	0.03	0.06
Enrolled in 2-year public college ^c	0.26	0.21	0.27
Enrolled in 4-year public college ^d	0.35	0.29	0.36
Female	0.51	0.55	0.51
Asian	0.01	0.01	0.02
Black	0.23	0.85	0.16
Hispanic	0.02	0.03	0.02
White	0.73	0.11	0.80
Other race/ethnicity	0.00	0.00	0.00
Regular high school diploma	0.98	0.96	0.98
Observations	226104	23886	202218
<i>Panel B: College Enrollees</i>			
Female	0.55	0.61	0.54
Asian	0.02	0.02	0.02
Black	0.21	0.86	0.15
Hispanic	0.02	0.01	0.02
White	0.75	0.11	0.81
Other race/ethnicity	0.00	0.00	0.00
Regular high school diploma	1.00	1.00	1.00
Per-student expenditure (thousands) ^e	15.09	13.96	15.20
Admission rates ^f	80.08	67.31	81.04
Graduation rates ^g	37.76	30.04	38.50
Observations	130258	11381	118877
Percentage of all high school graduates	57.61	47.65	58.79

Notes: Panel A includes the sample of all high school graduates for the school year 2005-2006 to 2008-2009. Panel B presents the sample of students who enrolled in college immediately after high school.

^{a b c d} refer to the enrollment immediately after high school graduation.

^{e f g} Measured by the school year 2007-08 records.

Table C.2: Estimated Effects of Flagship Scholarship

	All High Schools Sample (1)	Low College-Going HS Sample (2)	Comparable HS Sample (3)
<i>Panel A: High School Graduates</i>			
Enroll in UTK	0.016 (0.004)***	0.013 (0.004)**	0.018 (0.004)***
Enroll in 2-year College	0.000 (0.010)	-0.006 (0.010)	-0.003 (0.011)
Enroll in 4-year College	0.007 (0.011)	-0.020 (0.011)	0.008 (0.013)
Enroll in Any College	0.012 (0.013)	-0.019 (0.013)	0.008 (0.014)
Observations	226092	97241	61856
<i>Panel B: College Enrollees</i>			
Per-student expenditure (thousands)	0.283 (0.331)	0.308 (0.355)	0.455 (0.367)
Admission rates	-3.360 (0.682)***	-3.310 (0.755)***	-2.163 (0.875)*
Graduation rates	1.097 (0.613)	0.670 (0.681)	1.086 (0.806)
Observations	129891	47606	31297

Notes: */**/** denotes significance at the 10/5/1 percent level. Standard errors reported in parentheses are clustered at the high school level. Column (1) Presents the difference-in-differences estimations which compare the flagship high school students with all non-flagship high school students, before and after the implementation of policy. Column (2) presents the difference-in-differences estimations that compare the flagship high school students with below median college-going rate high school students, before and after the implementation of policy. Column (3) presents the difference-in-differences estimations that compare the flagship high school students with students from comparable high schools, which are 25% most similar to Flagship schools in terms of graduation rates, attendance rates, and the percent of students who scores “proficient” on Algebra I and English II end-of-course exams.

Table C.3: Effects of Flagship Scholarship: Event Study Estimations

	UTK (1)	2-Year College (2)	4-Year College (3)	Any College (4)
<i>Panel A: All HS Sample</i>				
Event Period 1: 2006	0.014 (0.006)**	-0.004 (0.011)	0.012 (0.017)	0.008 (0.016)
Event Period 1: 2007	0.013 (0.004)***	0.000 (0.009)	0.001 (0.010)	0.008 (0.013)
Event Period 1: 2008	0.020 (0.004)***	0.004 (0.013)	0.009 (0.011)	0.020 (0.014)
Observations	226092	226092	226092	226092
<i>Panel B: Low College-Going HS Sample</i>				
Event Period 1: 2006	0.012 (0.006)**	-0.008 (0.012)	-0.028 (0.016)*	-0.033 (0.017)*
Event Period 1: 2007	0.011 (0.004)**	-0.007 (0.010)	-0.019 (0.011)*	-0.017 (0.014)
Event Period 1: 2008	0.015 (0.005)***	-0.004 (0.013)	-0.015 (0.012)	-0.009 (0.015)
Observations	97241	97241	97241	97241
<i>Panel C: Comparable HS Sample</i>				
Event Period 1: 2006	0.016 (0.006)**	-0.009 (0.012)	0.008 (0.018)	-0.001 (0.018)
Event Period 1: 2007	0.015 (0.005)***	0.000 (0.011)	-0.001 (0.012)	0.006 (0.015)
Event Period 1: 2008	0.022 (0.005)***	-0.002 (0.014)	0.016 (0.014)	0.018 (0.016)
Observations	61856	61856	61856	61856

Notes: */**/** denotes significance at the 10/5/1 percent level. Standard errors reported in parentheses are clustered at the high school level. Panel A presents the event study estimations which compare the flagship high school students with all non-flagship high school students. Panel B presents the estimations that compare with below median college-going rate high school students. Panel C presents the estimations compared to students from comparable high schools, which are 25% the most similar to Flagship schools in terms of graduation rates, attendance rates, and the percent of students who scores “proficient” on Algebra I and English II end-of-course exams.

Table C.4: Estimated Effects of Flagship Scholarship on Flagship HS Enrollment

	All Student (1)	Female (2)	Male (3)	Asian (4)	Black (5)	Hispanic (6)	White (7)	Regular Diploma (8)
<i>Panel A: All HS Sample</i>								
	15.147 (5.328)***	-0.003 (0.011)	0.003 (0.011)	-0.002 (0.002)	0.003 (0.008)	-0.003 (0.002)	0.002 (0.007)	-0.024 (0.007)***
Observations	1293	1293	1293	1293	1293	1293	1293	1293
<i>Panel B: Low College-Going HS Sample</i>								
	13.064 (5.619)**	-0.005 (0.013)	0.005 (0.013)	-0.002 (0.002)	0.008 (0.009)	-0.003 (0.003)	-0.002 (0.008)	-0.014 (0.011)
Observations	630	630	630	630	630	630	630	630
<i>Panel C: Comparable HS Sample</i>								
	15.111 (6.438)**	0.002 (0.012)	-0.002 (0.012)	-0.001 (0.002)	-0.006 (0.009)	-0.004 (0.003)	0.011 (0.008)	-0.029 (0.006)***
Observations	373	373	373	373	373	373	373	373
Pre-treatment Mean	146	0.56	0.44	0.01	0.86	0.02	0.11	1.00

Notes: The dependent variable in column (1) is the total number of students in each high school. The dependent variables in columns (2) to (8) represent the percentage of students in each group at the school level. Pre-treatment mean represents the average for Flagship high schools before the implementation of the scholarship program. Estimations control for school and school-year fixed effects. Standard errors are reported in parentheses and clustered at the high school level. */**/** denotes significance at the 10/5/1 percent level.

Table C.5: Heterogeneity Analysis: All High Schools Sample

	Female (1)	Male (2)	Black (3)	White (4)
<i>Panel A: High School Graduates</i>				
Enroll in UTK	0.019 (0.005)***	0.012 (0.004)**	0.017 (0.005)***	0.021 (0.011)
Enroll in 2-year College	0.005 (0.013)	-0.007 (0.012)	-0.006 (0.013)	0.025 (0.017)
Enroll in 4-year College	0.013 (0.014)	0.003 (0.013)	-0.000 (0.015)	0.052 (0.027)
Enroll in Any College	0.023 (0.015)	-0.002 (0.015)	-0.006 (0.015)	0.075 (0.028)**
Observations	115216	110861	52028	164528
<i>Panel B: College Enrollees</i>				
Per-student expenditure (thousands)	0.544 (0.351)	-0.122 (0.448)	0.411 (0.378)	0.381 (0.711)
Admission rates	-3.337 (0.935)***	-3.549 (1.010)***	-1.325 (0.708)	-2.438 (1.492)
Graduation rates	1.519 (0.763)*	0.486 (0.777)	0.628 (0.769)	2.792 (1.447)
Observations	71115	58764	27556	97767

Notes: The control group is defined as students from non-Flagship High Schools. */**/** denotes significance at the 10/5/1 percent level. Standard errors reported in parentheses are clustered at the high school level.

Table C.6: Heterogeneity Analysis: Low College-Going High Schools Sample

	Female (1)	Male (2)	Black (3)	White (4)
<i>Panel A: High School Graduates</i>				
Enroll in UTK	0.015 (0.005)**	0.010 (0.005)*	0.016 (0.005)**	0.020 (0.011)
Enroll in 2-year College	-0.000 (0.013)	-0.016 (0.013)	-0.011 (0.015)	0.017 (0.017)
Enroll in 4-year College	-0.017 (0.015)	-0.024 (0.013)	-0.020 (0.017)	0.025 (0.027)
Enroll in Any College	-0.008 (0.016)	-0.033 (0.017)*	-0.026 (0.019)	0.042 (0.028)
Observations	49810	47428	31164	62348
<i>Panel B: College Enrollees</i>				
Per-student expenditure (thousands)	0.413 (0.380)	0.114 (0.489)	0.755 (0.478)	0.421 (0.727)
Admission rates	-3.452 (0.988)***	-3.188 (1.094)**	-2.151 (0.846)*	-2.341 (1.538)
Graduation rates	0.965 (0.853)	0.179 (0.890)	0.939 (1.002)	2.287 (1.440)
Observations	27042	20561	15040	31194

Notes: The control group is defined as students from non-Flagship high schools with below-median college enrollment. */**/** denotes significance at the 10/5/1 percent level. Standard errors that reported in parentheses are clustered at the high school level.

Table C.7: Heterogeneity Analysis: Comparable HS Sample

	Female (1)	Male (2)	Black (3)	White (4)
<i>Panel A: High School Graduates</i>				
Enroll in UTK	0.020 (0.006)***	0.015 (0.005)**	0.015 (0.006)*	0.025 (0.012)*
Enroll in 2-year College	0.009 (0.014)	-0.019 (0.014)	-0.007 (0.015)	0.022 (0.018)
Enroll in 4-year College	0.000 (0.016)	0.017 (0.015)	-0.015 (0.018)	0.059 (0.028)*
Enroll in Any College	0.019 (0.016)	-0.005 (0.018)	-0.017 (0.018)	0.077 (0.029)**
Observations	32477	29378	31702	27445
<i>Panel B: College Enrollees</i>				
Per-student expenditure (thousands)	0.487 (0.393)	0.360 (0.524)	0.667 (0.488)	0.435 (0.743)
Admission rates	-1.990 (1.050)	-2.496 (1.281)	-1.579 (0.815)	-1.372 (1.696)
Graduation rates	1.031 (0.994)	1.109 (1.007)	0.508 (0.910)	2.740 (1.567)
Observations	18039	13256	15988	14281

Notes: The control group is defined as students from non-Flagship high schools that are 25% most similar to flagship high schools in terms of graduation rates, attendance rates, and the percent of students who scores “proficient” on Algebra I and English II end-of-course exams. */**/** denotes significance at the 10/5/1 percent level. Standard errors that reported in parentheses are clustered at the high school level.

C.2 Supplementary Tables

Table C.8: Heterogeneity Analysis: Asian and Hispanic Students

	All High Schools Sample		Low College-Going HS Sample		Comparable HS Sample	
	Asian (1)	Hispanic (2)	Asian (3)	Hispanic (4)	Asian (5)	Hispanic (6)
Panel A: High School Graduates						
Enroll in UTK	0.043 (0.023)	0.033 (0.015)*	-0.001 (0.028)	0.018 (0.013)	0.030 (0.025)	0.020 (0.014)
Enroll in 2-year College	-0.061 (0.078)	0.029 (0.038)	-0.070 (0.084)	0.026 (0.039)	-0.095 (0.094)	0.006 (0.042)
Enroll in 4-year College	0.086 (0.083)	0.078 (0.025)**	0.099 (0.091)	0.067 (0.028)*	0.008 (0.090)	0.068 (0.030)*
Enroll in Any College	-0.014 (0.086)	0.078 (0.047)	-0.008 (0.093)	0.076 (0.051)	-0.139 (0.103)	0.064 (0.055)
Observations	3340	5487	910	2527	792	1764
Panel B: College Enrollees						
Per-student expenditure (thousands)	2.060 (1.374)	4.023 (2.187)	1.628 (1.946)	2.495 (2.213)	2.097 (1.784)	3.183 (2.539)
Admission rates	-2.494 (3.225)	10.924 (3.573)**	-1.223 (4.203)	11.486 (3.622)**	1.254 (3.544)	12.018 (4.071)**
Graduation rates	1.487 (3.175)	9.080 (6.933)	2.524 (3.930)	9.615 (7.112)	2.356 (4.372)	8.242 (7.729)
Observations	2194	1903	537	654	503	429

Notes: The control groups in columns (1) and (2) are defined as students from non-Flagship High Schools. The control groups in columns (3) and (4) are defined as students from non-Flagship high schools with below-median college enrollment. The control group in columns (5) and (6) are defined as students from non-Flagship high schools that are 25% most similar to flagship high schools in terms of graduation rates, attendance rates, and the percent of students who scores “proficient” on Algebra I and English II end-of-course exams.

*/**/*** denotes significance at the 10/5/1 percent level. Standard errors reported in parentheses are clustered at the high school level.

Vita

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