# College and Career Readiness: Essays on Economics of Education and Employment

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# Abstract

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This dissertation consists of three quantitative studies analyzing educational policies and programs that help to improve college readiness, college success, as well as the transition from college to the labor market. The first chapter studies the impact of raising the bar for high school math on college readiness and success. The chapter examines the causal effects of recent state-level reforms increasing the high school graduation math requirement on high school graduation and college outcomes. Using nationally-representative survey data, the study exploits variation in the reform timing across states in a staggered difference-in-difference framework. Findings indicate that raising the high school graduation requirement in math coursework is a potentially effective policy tool to prepare students better for college, but highlight racial disparities in the effectiveness of the math reforms.

The second chapter discusses experiential learning through work-based courses at 2-year and 4-year colleges. Experiential learning is a critical component for a smooth transition from higher education to the workforce. In this chapter, I apply an innovative text mining technique to identify and analyze work-based courses from transcript data. The study examines patterns and post-degree labor market outcomes of taking work-based courses at college, and has important

implications for colleges and policymakers to better support students on gaining from the courses.

The third chapter estimates heterogeneity in labor market returns to Master's degrees. Using an individual fixed effects model with rich administrative data, the study provides up-to-date causal evidence on labor market returns to Master's degrees, and examines heterogeneity in the returns by field area, student characteristics, and the macroeconomic condition in which students graduate and enter the labor market. Findings show that obtaining a Master's degree increases quarterly earnings, but the return varies widely by field of study. And economic downturns appear to reduce but not eliminate the positive return to a Master's degree.

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# Dedication

This dissertation is dedicated to my parents and my husband, who shared their endless love and support through the hills and valleys of my Ph.D. journey.

# Preface

Higher education yields substantial gains to labor market success, especially as today's economy increasingly demands for a skilled workforce. However, socioeconomic and racial gaps persist in college access and post-college employment opportunities in the United States. Different levels of college readiness at college entry and career readiness at college graduation are important contributing factors to the gaps. In face of this reality, this dissertation studies three issues in education, aiming to inform policymakers, colleges and students to make better decisions towards improved college and career readiness: (1) statewide high school graduation requirement in math; (2) experiential learning through work-based courses at college; (3) heterogeneity in labor market returns to Master's degrees.

The first chapter examines the causal impact of recent statewide reforms increasing the high school graduation requirement in math on college readiness and success. While quantitative skills play an important role in higher education and the workforce under today's increasingly technical economy, a large proportion of college freshmen are not prepared for college-level math. These students are placed into remedial math courses upon college entry, and the remediation rate is especially high among disadvantaged students. This chapter intends to understand if and how statewide policies on high school math coursework would prepare students better for college math. In particular, states have been increasing the number of math courses required for students to graduate from high school. The chapter builds a conceptual framework based on economic theories to hypothesize the impact of a policy change in the high school math graduation requirement, and empirically estimates how recent reforms increasing the graduation requirement to four years of math affect students' high school graduation, college

enrollment and college degree completion. To estimate the reform impact causally, the study exploits variation in the reform timing across states in a staggered difference-in-difference framework, and implements a newly-developed estimator that is robust to treatment effect heterogeneities. Findings show that the math reforms had positive impacts on college degree completion, in particular on-time Bachelor's degree completion in general and also in STEM fields. However, there exists significant racial disparities in the reforms' effectiveness: college outcomes of Black students did not improve as a result of the reforms, and the reforms brought about unintended negative impacts on high school graduation outcomes among minoritized students. Suggestive evidence indicates that course unavailability and the lack of school resources may contribute to the unequal effectiveness of the reforms.

The second chapter analyzes experiential learning through work-based courses at college. Experiential learning has become a critical component in higher education and the transition from college to the workforce. Through experiential learning, students could deepen the understanding of theoretical knowledge learned from classroom, become more reflective about their career goals, and demonstrate work readiness to prospective employers. Nevertheless, quantitative research on experiential learning is extremely limited, mostly due to the challenge to quantifying such experience. This chapter intends to fill the knowledge gap by examining experiential learning through work-based courses, such as internship, practicum, and fieldwork courses, that students take at college to gain practical skills for the labor market. I apply an innovative text mining technique to identify work-based courses in transcript data, and analyzed student characteristics, course patterns and labor market outcomes associated with taking work-based courses at college. Findings have important implications for colleges and policymakers to

understand the current landscape of work-based course-taking, and to further improve students' career readiness through these courses.

The third chapter provides up-to-date causal evidence on labor market returns to Master's degrees, and studies heterogeneity in the returns by field area, student characteristics and the macroeconomic condition in which students graduate and enter the labor market. Graduate education is among the fastest growing segments of the U.S. higher education system. While the increase in graduate student debt has attracted policy concern, less attention has been given to understanding what returns yield from graduate degrees. To estimate the returns rigorously, this chapter uses rich administrative data from Ohio and an individual fixed effects model that compares students' earnings trajectories before and after obtaining a Master's degree. Findings show that obtaining a Master's degree increased quarterly earnings by about 12% on average, but the returns vary largely across graduate fields. We also find gender and racial disparities in the returns, with higher average returns for women than for men, and for White graduates than for their Black counterparts. In addition, by comparing returns among students who graduated before and under the Great Recession, we show that economic downturns appear to reduce but not eliminate the positive returns to Master's degrees.

To summarize, these three chapters jointly provide empirical evidence for policymakers, colleges and students to make better informed decisions for improved college and career readiness, and hopefully for a more efficient and equal educational system.

# Chapter 1: Raising the Bar for High School Math – Impacts on High School Graduation and College Success

## 1.1 Introduction

Strong quantitative and analytical skills play an increasingly important role in both higher education and the workforce in today's increasingly technical economy. In particular, mathematical skills have been shown to yield substantial economic returns (Acemoglu & Autor, 2011; Banks & Oldfield, 2007). Despite these high returns, research indicates that almost one-third of college freshmen in the United States are not prepared for college-level math (Long, Iatarola, & Conger, 2009), and are placed into remedial courses upon college entry (Bailey, Jeong, & Cho, 2010). Also, the rate of remediation is particularly high among students from minoritized and low-income groups (Long, Iatarola, & Conger, 2009; Scott-Clayton & Rodriguez, 2015). Unfortunately, the effectiveness of college remedial education appears questionable (Boatman & Long, 2017; Martorell & McFarlin, 2011; Scott-Clayton & Rodriguez, 2015; Xu & Dadgar, 2017). The unpreparedness for college math can have harmful implications for both college outcomes and subsequent labor market outcomes, especially among disadvantaged student groups.

Increasing the number and level of math courses students take during high school is one possible policy remedy to prepare students better for college. A range of quasi-experimental studies have found that the completion of rigorous and advanced high school math courses has positive impacts on college outcomes (Cortes, Goodman, & Nomi, 2015; Gaertner et al., 2014; Long, Conger, & Iatarola, 2012; Speroni, 2011). However, prior literature is focused on examining the effect of taking specific high school math courses under certain programs/contexts. It is less clear how a statewide policy raising the high school math

requirement, especially on the number of math courses, will affect college outcomes or bring about any unintended consequences.

For the past several decades, states in the U.S. have been gradually raising the high school graduation requirement (HSGR) in math. While the majority of U.S. states required none or only one year of math[1] to graduate from high school in the 1980's, 43 states and the District of Columbia (DC) have required at least three years of math by the high school graduation class of 2019. Among these states, 16 states and DC have required four years of math to graduate from high school. And 14 states and DC had the reform increasing the requirement to four years of math in recent years, ranging from the class of 2008 to 2014.

Despite states' increasing effort of raising the HSGR in math, causal evidence on the impact of such reforms remains limited. Goodman (2019) examined an early wave of reforms increasing the HSGR from zero or one to one or two years of math. The study showed that the reforms had positive impacts on completed high school math coursework, concentrating in basic math courses, as well as labor market outcomes, among Black students. However, the study found scant evidence of improved college outcomes as a result of the reforms. While it is important to understand the past, results based on the earlier era of the reforms may not generalize to recent policy changes in the math HSGR. As the majority of states have already required three years of math to graduate from high school nowadays, reforms further increasing the math requirement would introduce a higher standard and may affect high school course-taking patterns and college outcomes in different ways from the earlier reforms. Also, even if

[1] Throughout the paper, one year of math in the HSGR stands for one Carnegie unit of math. The Carnegie unit is a standard measure of the amount of time a student has studied a subject; and one unit represents the completion of a course that meets one period per day for one year.

some positive effects have been found from the early reforms, the policy change may not necessarily to be always effective when pushing to the higher margin.

It remains unclear how recent reforms further increasing the math HSGR would affect students' educational attainment. On one hand, students' college outcomes would improve if the reforms were effective in preparing high school graduates better for college-level math. Students equipped with a stronger math foundation would be more likely to enroll and persist in college. The reforms may also narrow the racial and socioeconomic gaps in college degree completion and representation in science, technology, engineering and mathematics (STEM) fields. On the other hand, students and school districts may be constrained in skills or resources to benefit from the reforms. Students' academic preparation, the status-quo of high school math course-taking, as well as peer and cohort effects following the reforms, would serve as important factors determining the impact of the reforms on students' educational attainment. Furthermore, the policy change could discourage high school students and bring about unintended consequences on high school graduation outcomes, especially among students who were already at an academically-disadvantaged position.

This paper intends to fill the knowledge gap by examining the causal effects of recent reforms increasing the HSGR in math on high school graduation and college outcomes. Specifically, the paper is mainly focused on states that reformed the HSGR to four years of math in recent years, and exploits the differential timing of these reforms in a staggered difference-in-difference framework using nationally-representative survey data. To account for concerns about the standard two-way fixed effects (TWFE) model raised in recent econometric literature, the paper implements a newly-developed estimator that is robust to treatment effect heterogeneities.

Empirical results are discussed under a conceptual framework built on economic theories with hypothesized mechanisms supported by descriptive analyses.

Findings show that raising the bar for high school math is potentially an effective policy tool to improve college readiness and success under the current policy landscape. In particular, recent reforms increasing the HSGR to four years of math had positive impacts on students' college degree completion outcomes. However, results also indicate significant racial disparities in the reforms' effectiveness: The study only finds positive impacts on college outcomes among White and Hispanic students, but not among Black students; Also, the reforms had unintended negative impacts on high school graduation outcomes among minoritized students.

This paper contributes to the literature in three ways. First, the paper provides causal evidence on the impacts of recent policy changes increasing the statewide math HSGR on college outcomes, which is an understudied area and has important policy implications for a smooth and efficient transition from high school to college. Second, the paper brings new insights into mechanisms through which statewide math reforms in the HSGR would affect educational attainment in different student groups. Finally, the paper demonstrates the implementation of a newly-developed estimator under the staggered difference-in-difference framework for a setting to measure the causal effect of a policy change in education.

The remainder of the paper proceeds as follows: Section 1.2 provides the policy context of the statewide HSGR in math, and reviews prior literature on the relationship between high school math and college success. Section 1.3 builds a conceptual framework with three alternative hypotheses to guide the empirical analysis on the impact of the math reforms. Section 1.4 describes the empirical strategy of the paper. Section 1.5 presents results along with robustness and validity checks. Section 1.6 explores mechanisms of the reforms' impact with

suggestive evidence. Section 1.7 concludes the paper with a discussion of policy implications and future research.

## 1.2 Policy Background and Literature Review

### 1.2.1 Statewide High School Graduation Requirement in Mathematics

As states set different math requirements for high school students, the trend of increasingly requiring students to complete more years of math and to engage in more rigorous math content at high school is apparent. In the 1980s, students in the vast majority of states could graduate from high school with zero or one completed math course, and none of the states required three math courses. From 1982 to 1994, 31 states increased the graduation requirement from zero or one to two math courses, and 9 states reformed the requirement to three math courses (Goodman, 2019). Between 2000 and 2008, 37 states increased the requirement to three or more years of math courses (Stillman & Blank, 2009). By the high school graduation class of 2019, 43 states and DC have required at least three years of math to graduate from high school. Among them, 16 states and DC have set the statewide graduation requirement to four years of math, equivalent to a continuity of math instruction spanning through each year of high school (Education Commission of the States, 2019).

Appendix Table A1 lists the states with a four-math minimum in the HSGR by the high school graduation class of 2019, along with the timing of the reforms that increased the requirement to four years of math. I collected the policy data on the math reforms from the Digest of Education Statistics (2000, 2001), as well as the policy tracking database and historical documentations of the Education Commission of the States (Education Commission of the States, 2006, 2007, 2012, 2019). This allows me to track down the first effective high school graduation cohort subject to the four-math requirement in each of the reforming states. As shown

in Appendix Table A1, Alabama and South Carolina increased the HSGR to four years of math in 2000 and 2001 respectively, and the other 14 states and DC reformed the requirement to four years of math in more recent years, ranging from the high school graduation class of 2008 to 2014. These 14 states and DC that recently reformed the HSGR to four years of math courses are highlighted in Figure 1.1, grouped by the reform timing. The reforms were generally announced several years before the first effective cohort to allow time for preparation among students and schools.

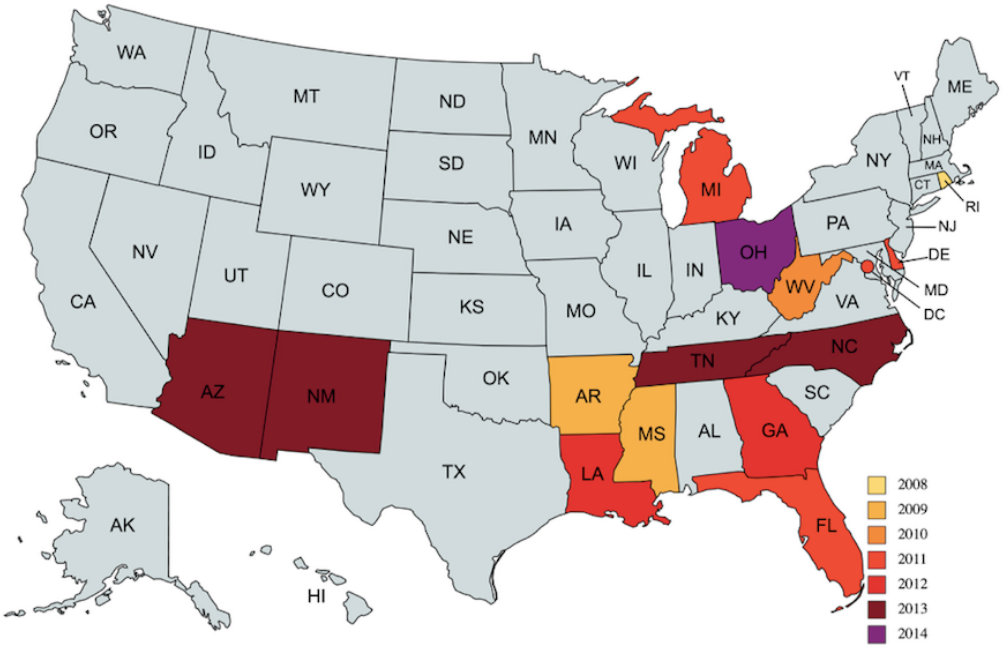



**Figure 1.1: States Recently Reformed the HSGR to Four Math by Reform Timing**

Note: The figure highlights states that had a recent reform increasing the HSGR to four years of math, grouped by reform timing.

While most of the reforming states increased the HSGR statewide from three to four years of math, there are several exceptions. First, Michigan allowed students to graduate from high school with no math requirement before the reform, and increased the requirement directly

to four years of math courses from the graduation class of 2011. Arizona first reformed the HSGR from two to three years of math in 2012, and further increased the requirement to four years of math in the subsequent year. Also, Georgia and North Carolina used to require only students in the College Prep track to graduate from high school with four years of math, and expanded the requirement to all the students regardless of their tracks in 2012 and 2013 respectively. Similarly, West Virginia first raised the requirement to four years of math among students enrolled in the professional & skilled pathway in 2008, and then included all the students in the state under the four-math requirement in 2010.

Moreover, the reforms increasing the HSGR to four years of math are closely aligned with more rigorous standards in the math pipeline set by states. Among states with a HSGR lower than four years of math, only three states have required the completion of Algebra II. In contrast, more than half of the states that have required students to graduate from high school with four years of math have also required the completion of Algebra II or a course with equivalent content. And many of the states have specified the completion of the course sequence recommended by colleges and employers - Algebra I, Geometry and Algebra II. Generally, states do not specify any course requirements beyond Algebra II. That is, students have the flexibility to pick math courses that best fit their needs after fulfilling the requirement of Algebra II. However, we should be cautious about interpreting the math pipeline requirement as previous research has shown that course titles do not always reflect course content or rigor (Lee & Ready, 2009; Warner & Ready, 2019).

Nationwide, there exists a gap between the number of math courses students have been taking at high school and the four-math requirement that an increasing number of states have been adopting in the HSGR, especially among students from a lower socioeconomic background.

Dalton et al. (2016) studied high school transcript data of Fall 2009 ninth-graders in a nationally-representative sample, and found that students on average completed fewer than 4 high school math courses (3.6 Carnegie units) by 2013. While students from the highest fifth socioeconomic quintile on average completed 3.9 math courses, students from the lowest fifth quintile completed only 3.4 math courses. Also, students whose parents' highest education is a Bachelor's degree completed 3.8 math courses. In contrast, students with the highest parent education level of high school completed 3.5 math courses and those with the level below high school completed 3.2 math courses only. In terms of racial disparities, minoritized students completed fewer high school math courses than their counterparts: On average, White students completed 3.7 math courses, Asian students completed 3.9 math courses, Black students completed 3.6 math courses and Hispanic students completed 3.5 math courses. The four-math requirement could be a more challenging standard for students from a lower socioeconomic background, probably due to the lack of school and family resources in high poverty neighborhoods and lower academic preparation at the entrance of high school.

### 1.2.2 Prior Literature on High School Math and College Success

A range of quasi-experimental studies suggest that the completion of high school math courses, especially rigorous and advanced ones, improves subsequent outcomes such as college enrollment and college degree completion. For example, Long, Conger and Iatarola (2012) applied a propensity score matching method among high school students in Florida, and found that taking a rigorous math course in high school increased the likelihood of attending a four-year college, postsecondary performance, and the likelihood of earning a Bachelor's degree. Based on a regression discontinuity design, Cortes, Goodman and Nomi (2015) found that doubling instructional time in Algebra among underprepared ninth graders in Chicago had

substantial positive impacts on high school graduation and college enrollment rates. Also, Gaertner et al. (2014) used an instrumental variable approach to study the impact of completing Algebra II among high school students in Florida, and concluded with a positive impact on the probability of attending college, particularly two-year colleges, but found no significant impact on college degree attainment. Using a regression discontinuity design, Speroni (2011) showed that taking College Algebra in high school through a dual enrollment program in Florida had positive impacts on high school graduation and college degree completion.

In general, prior literature provides causal evidence supporting positive impacts of taking high school math courses on college outcomes. However, prior research is mostly focused on examining the effect of taking specific math courses under certain programs/contexts. The impact of statewide policies raising the high school math requirement, especially on the number of math courses students take in high school, is less straightforward. Such policies would affect a wider student body, including students from diverse backgrounds, and the policy impact depends on how students respond to the policy in their course-taking behaviors.

Causal evidence on the impact of statewide policies increasing the number of math courses required to take in high school is limited. Clotfelter, Hemelt and Ladd (2019) studied a statewide college admission policy that increased the number of high school math courses required for admission to any of the four-year public institutions in North Carolina with an instrumental variable approach. The findings suggested that the policy shift led to higher high school graduation as well as enrollment rates at universities in the University of North Carolina system. Beyond college enrollment, however, the study found limited and noisy evidence that the policy had positive impacts on college performance measures.

The closest prior research to this study is Goodman (2019) - the paper examined the effect of state-level reforms in the minimum high school graduation math requirement for the high school graduation class of 1982 to 1994. The study leveraged the differential timing of all the state policy changes during the period, most of which increased the requirement from zero or one to two math courses. Thus, the focus of the study was an early wave of statewide reforms in the math HSGR, targeting basic math coursework at high school. The findings showed that the math reforms substantially increased completed high school math coursework among Black students, concentrating in basic math courses. The study also found positive impacts of the reforms on labor market outcomes among Black students. However, the reforms did not alter White students' course-taking patterns because they had already fulfilled the requirement before the policy change. Also, the reforms did not show significant impacts on subsequent college outcomes, possibly due to the focus on basic high school math courses of the reforms that had a less direct connection to college-level math. While it is important to understand the early wave of the math reforms, the results may have limited policy implications under the current policy landscape that most states have already required three years of math to graduate from high school. A policy change that further increases the math requirement would target on more advanced high school math coursework, and may affect all the student groups in ways different from the early reforms.

Another line of prior literature has examined the role of high school math in explaining demographic and socioeconomic disparities in college readiness. Minoritized and low-income students have a higher need for college remediation than their peers. Using data on students in Florida public postsecondary institutions, Long, Iatarola and Conger (2009) found that more than one fourth of Black-White, Hispanic-White and poverty gaps in college readiness could be

explained by differences in the highest math course taken among college-going students. Minoritized and low-income students are less likely to enroll in high-track math courses or persist in taking math courses at high school compared to their White, higher-income peers (Crosnoe & Schneider, 2010; Kelly, 2009; Long, Iatarola, & Conger, 2009; Minaya, 2021). In addition, literature indicates that returns to taking high school math courses are relatively lower for minoritized and low-income students. For example, Long, Iatarola and Conger (2009) concluded that Black and low-income students benefited less from taking Algebra II and other math courses in high school than their White and higher-income counterparts. Also, minoritized and low-income students who earned credits for advanced placement (AP) courses in Texas public schools were more likely to fail state exit and AP exams than their peers (Dougherty, Mellow, & Jian, 2006). The difference in the return to high school math is likely to result from differences in the quality of the math instruction and/or learning environment.

In addition, previous studies have documented that minoritized students are underrepresented in STEM fields at college, which yield higher returns in the labor market (e.g., Anderson & Kim, 2006; Huang, Taddese, & Wlater, 2000). High school STEM learning experiences, including high school math exposure and achievement, have been found essential towards students' intent to pursue a STEM major in college as well as the major persistence and degree completion in STEM fields (Blickenstaff, 2005; Bottia et al., 2015; Wang, 2013). For example, Wang (2013) indicated that the intent to pursue STEM fields in college was positively influenced by math achievement at the 12th grade among both White students and underrepresented minorities. Therefore, policies raising the bar for high school math have the potential to narrow racial and socioeconomic gaps in college readiness, college major choice, college success, as well as subsequent labor market outcomes.

## 1.3 Conceptual Framework and Hypotheses

To understand why and how a policy change in the math HSGR would affect students' high school course-taking behaviors and college outcomes, I build a conceptual framework based on economic theories. First, the economic theory of human capital investment states that students make educational decisions based on a comparison of expected benefits with expected costs associated (Becker, 1964). That is, the theory suggests that students would invest in an additional math course at high school if the marginal benefit of taking the course equal to or exceed its marginal cost.

However, students may not have perfect information about benefits and costs associated with taking high school math courses, especially in regard to long-run benefits and indirect costs, to make the optimal investment in high school math coursework. For example, a student would underinvest in high school math coursework if he/she underpredicts the long-run benefits of taking a high school math course, such as on college success and subsequent labor market returns, or overestimates the time and effort required to complete the course. Even if students are fully informed about benefits and costs associated with high school math courses, they may not always rationally evaluate the payoffs when making their course plans. Behavioral economics has acknowledges that psychological and emotional factors can influence people's decision making. In particular, adolescents could act myopic in trading off present and future consequences (O'Donoghue & Rabin, 2000). That is, students may not invest in high school math coursework to the optimal level if they pursue immediate gratification at the expense of long-term payoffs. Under these circumstances, a well-designed HSGR could guide students to make better course-taking decisions in high school math that consistently match their current and future preferences.

A reform in the math HSGR could change students' cost-benefit calculations on high school course-taking decisions as well as college-related decisions. For example, a policy change raising the required number of math courses for high school graduation would increase the costs of not taking additional high school math courses because students would risk losing the high school diploma as a result. Also, as students complete more math courses in high school in response to the policy change, they may be waived the math remediation requirement upon college entry, or even earn college credits from AP/IB/dual enrollment math courses taken at high school, both of which would decrease costs associated with college enrollment and persistence. As a result, a math reform in the HSGR may affect students' high school math course-taking patterns and college outcomes.

In addition, neoclassical economic theory holds that economic agents allocate resources to maximize profits in the face of resource scarcity according to the interaction of supply and demand. That is, schools allocate limited resources to meet the needs of students and parents (i.e. supply responds to demand). A policy change in the math requirement for high school graduation could drive students to demand for longer math instructional time and higher-track math courses, and thus lead schools to allocate more resources towards meeting the demand. The cost of offering an additional math course would be reduced as the number of students enrolled in the course increases because of economies of scale. In turn, students' course-taking behaviors may be affected by changes in schools' course offerings.

Guided by the conceptual framework above, potential impacts of a policy change in the math HSGR on subsequent educational outcomes are categorized in three alternative hypotheses below.

First, the math reforms may improve students' college outcomes if the reforms could lead to increased high school math course-taking that would prepare students better for college. For example, the reforms may expand students' exposure to developing and practicing math abilities for more years in high school, which could strengthen their math understanding before college. Students may also be motivated by the reforms to take more advanced math courses that would connect to college-level math more closely. In particular, well-prepared students may take college-level math courses in high school that could be transferred to college credits later. High school graduates equipped with more rigorous preparation in math skills would be more likely to enroll in college or be admitted into selective ones. Beyond college enrollment, the likelihood of persisting through college would rise as students become more likely to be placed into college-level math courses directly and tackle those courses successfully. Not only would students be more likely to complete a college degree, they may also earn the degree in a shorter period of time, which would allow them to accumulate less college debt and enter the workforce earlier. In addition, students may be influenced to pursue a college degree in majors requiring strong math skills, such as those in STEM fields, which have been documented with high labor market payoffs and are also a key contributor to macroeconomic growth and national competitiveness.

Nevertheless, a policy change increasing the number of math courses required for high school graduation may not always have an impact on students' college outcomes. Students who were already taking math courses as stated in the new requirement may not be affected by the policy change much. Students who did not satisfy the new requirement before the policy change may fulfill the requirement with additional math courses that they could gain little from. Reasons that students may not always benefit from the additional math courses following the reforms could be threefold. First, schools may not have the resources to offer additional math courses

with good quality and rigor. Offering additional math courses, especially advanced ones, would require funding for teacher recruitment, teacher training and instructional materials. Schools lacking the resources may have to enroll students in math courses that they have already mastered in response to the policy change in the math HSGR. Second, even in schools where math courses with good quality and rigor are available, students may not have the ability to evaluate and select themselves into courses at the right level, especially without good advice from school counselors and families. Furthermore, the extent to which students could gain from the additional math courses depends on their academic preparation and learning environment. Students with low preparation and under an ineffective environment would encounter more learning barriers to gain from the courses.

Last, the math reforms may bring about unintended and unfavorable consequences on high school graduation and subsequent college outcomes. The new requirement may discourage students and burden schools that have already struggled with hiring and retaining enough math teachers. As a result, students may drop out from high school early, leave high school without a regular diploma, or switch to alternative high school credentials that yield lower returns. Moreover, academically-disadvantaged students and under-resourced schools may disproportionately suffer from harmful consequences on college outcomes, in particular as their peers become more competitive in college math preparation.

## 1.4 Empirical Strategy

### 1.4.1 Data & Analysis Sample

To test the hypotheses and examine the causal effects of recent reforms increasing the HSGR in math on high school graduation and college outcomes empirically, I use data from the 2010-2019 American Community Survey (ACS). The ACS data provide an individual-level

repeated cross-sectional sample on a yearly basis representing the nation and its people. The data are well-suited to track educational attainment of the population in recent years. In particular, the 2009-onward data contain information on the major fields of Bachelor's degrees, which allows to examine students' major choices among Bachelor's degree completers.

One limitation of the data is that there is no retrospective information on the year and the state of high school graduation. In order to identify the HSGR that individuals were subject to, I use the year when an individual turned 18 years old as the proxy for high school graduation class, and the individual's birth state as the proxy for high school state. The proxies would introduce bias to the identification of the HSGR as well as the estimation of the treatment effects of the math reforms under three cases. First, an individual graduated from high school at an age younger than 18, and the high school state reformed the math HSGR after the graduation but before or during the year the individual turned 18. That is, the individual graduated from high school before the math reforms, but the proxies would misassign the individual to the treatment group who graduated under the new HSGR. Second, an individual graduated from high school at an age older than 18, and the high school state reformed the math HSGR before the graduation but after the year the individual turned 18. That is, the individual graduated from high school after the math reforms, but would be misassigned to the control group who graduated under the old HSGR. Third, an individual moved and graduated from high school in another state with a different math HSGR from the birth state. For example, if the individual graduated from high school in a state that had reformed the math HSGR, during which time the birth state had not reformed yet, the proxies would misassign the individual from the treatment group to the control group. And if the math reforms indeed had a positive impact on educational attainment, a misassignment between the treatment group and the control group would introduce downward

bias to the estimation of the reform effect. However, there were unlikely any systematic associations between the timing of the math reforms and the age an individual graduated from high school or the cross-state relocation decision before high school graduation. Among individuals who turned 18 years old between 2006 and 2015 in the reforming states, which approximate high school graduation cohorts across the years of the recent reforms, 73% stayed in their birth state at the age of 22. The other 27% include individuals who moved out of state for college or for work after high school graduation. It is reasonable to assume a low cross-state mobility rate before high school graduation, and thus an insignificant bias introduced by the proxies.

I restrict the analysis sample to individuals who were 22 years old at the time of the survey to ensure an equivalent comparison of educational attainment across high school graduation cohorts. The sample corresponds to the high school graduation class of 2006 to 2015, which spans from a cohort before any of the recent math reforms to a cohort after all the reforms. High school graduation and college outcomes are constructed into six dummy variables from the highest educational attainment variable in the data: earning any high school credentials, earning a regular high school diploma, college enrollment, college degree completion, Bachelor's degree completion and Bachelor's degree completion in STEM fields, all measured at the age of 22.[2] The outcomes capture the least educational level and are not mutually exclusive. The age cutoff restricts to on-time Bachelor's degree completion for students who follow a conventional

[2] Specifically: Using the highest educational attainment variable in the data, I define the outcomes as follows: (1) Earning any high school credentials: The highest educational attainment is at or above "Regular high school diploma", including "GED or alternative credential"; (2) Earning the regular high school diploma: The highest educational attainment is at or above "Regular high school diploma", excluding "GED or alternative credential"; (3) College enrollment: The highest educational attainment is at or above "Some college"; (4) College degree completion: The highest educational attainment is at or above "Associate's degree"; (5) Bachelor's degree completion: The highest educational attainment is at or above "Bachelor's degree"; (6) Bachelor's degree completion in STEM fields: The highest educational attainment is at or above "Bachelor's degree", and the field of the degree is in STEM.

educational path - graduating from high school at an age of 18, enrolling in college right after high school graduation and completing a Bachelor's degree in four years after enrollment. Therefore, the outcomes evaluate both the probability of college degree completion and how fast students progress through college. A faster progression in college is meaningful to students because they would accumulate less college debt and start gaining working experience at an earlier age.

The main analysis sample is focused on states that recently had a statewide reform increasing the minimum HSGR to four years of math, as highlighted in Figure 1.1 and discussed in more details in the policy background section. Table 1.1 presents summary statistics of the main analysis sample and a national comparison sample including all the individuals who are 22 years old in the 2010-2019 ACS data. The gender distribution is similar between the reforming states and the national sample. The reforming states have a slightly higher proportion of Black students and a lower proportion of Hispanic students than the national sample. Overall, the main analysis sample is representative of the nation in terms of gender and racial distributions.

Table 1.1 also shows average rates of the six educational attainment measures in the reforming states and the national sample for the high school graduation class of 2006 and 2015. The high school graduation class of 2006 is the baseline cohort before any of the reforming states increased the HSGR to four years of math. And the high school graduation class of 2015 is a post-reform cohort when all the students in the reforming states have been required to graduate from high school with four years of math. The educational attainment rates were slightly lower in the reforming states than the national sample for the baseline cohort. A racial comparison of White, Black and Hispanic students[3] in the reforming states shows that White students had the

[3] Other racial groups only account for a small proportion of the sample, and are therefore neglected from the analysis due to sample size concerns.

highest baseline educational attainment while Black students had the lowest. Also, the table reveals that all the educational attainment outcomes improved in the reforming states from the baseline cohort to the post-reform cohort, and the paper intends to estimate if and to what extent of the improvement could be attributed to the math reforms in the HSGR.

**Table 1.1: Summary Statistics**

| | Analysis Sample | | | | National Sample | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) All Races | (2) White | (3) Black | (4) Hispanic | (5) All Races | (6) White | (7) Black | (8) Hispanic |
| ***Individual Characteristics*** | | | | | | | | |
| Male | 51% | 51% | 50% | 50% | 51% | 51% | 50% | 51% |
| Female | 49% | 49% | 50% | 50% | 49% | 49% | 50% | 49% |
| White | 64% | 100% | 0% | 0% | 62% | 100% | 0% | 0% |
| Black | 24% | 0% | 100% | 0% | 16% | 0% | 100% | 0% |
| Hispanic | 9% | 0% | 0% | 100% | 17% | 0% | 0% | 100% |
| Other Races | 3% | 0% | 0% | 0% | 4% | 0% | 0% | 0% |
| ***Educational Attainment*** | | | | | | | | |
| *High School Credential* | | | | | | | | |
| Class of 2006 | 89% | 91% | 83% | 86% | 90% | 93% | 84% | 84% |
| Class of 2015 | 93% | 94% | 91% | 92% | 94% | 95% | 92% | 92% |
| *Regular High School Diploma* | | | | | | | | |
| Class of 2006 | 84% | 86% | 77% | 81% | 86% | 89% | 79% | 79% |
| Class of 2015 | 90% | 91% | 89% | 89% | 91% | 92% | 90% | 89% |
| *College Enrollment* | | | | | | | | |
| Class of 2006 | 59% | 62% | 51% | 54% | 63% | 67% | 52% | 52% |
| Class of 2015 | 62% | 64% | 55% | 59% | 64% | 67% | 56% | 58% |
| *College Degree* | | | | | | | | |
| Class of 2006 | 18% | 20% | 9% | 16% | 22% | 25% | 11% | 14% |
| Class of 2015 | 25% | 28% | 15% | 25% | 28% | 32% | 17% | 20% |
| *Bachelor's Degree* | | | | | | | | |
| Class of 2006 | 11% | 13% | 4% | 7% | 15% | 17% | 6% | 7% |
| Class of 2015 | 16% | 19% | 9% | 12% | 19% | 23% | 10% | 11% |
| *Bachelor's Degree in STEM* | | | | | | | | |
| Class of 2006 | 2% | 3% | 0% | 1% | 3% | 3% | 1% | 1% |
| Class of 2015 | 4% | 5% | 2% | 3% | 5% | 6% | 2% | 2% |
| *N* | 87,366 | 60,068 | 17,979 | 7,471 | 324,556 | 216,352 | 44,739 | 51,202 |

Note: The table shows summary statistics using the 2010-2019 ACS data restricted to individuals who were 22 years old at the survey time. Analysis sample includes states that recently reformed the HSGR to four years of math. National sample includes all the states in the data. All the educational attainment measures are measured at age 22. All the observations are weighted by the person weight.

### 1.4.2 Identification Strategy

To causally estimate the effects of statewide reforms increasing the HSGR to four years of math on subsequent educational attainment outcomes, I exploit variation in the reform timing

across states in a staggered difference-in-difference framework. For an analysis with staggered treatment timing, researchers used to apply a linear regression model with group and time fixed effects, also known as the two-way fixed effects (TWFE) model. However, recent econometric literature demonstrates that the TWFE estimator is biased under treatment effect heterogeneities (Athey & Imbens, 2021; Borusyak et al., 2017; Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfceuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021). Specifically, the TWFE estimator equals a weighted average of treatment effects in each group and period. That is, the TWFE estimator can be decomposed into all possible two-by-two difference-in-difference (2x2 DD) estimators that compare groups treated at two different times with each other. All the groups serve as controls for some 2x2 DD estimators: some estimators use later-treated groups before treatment as controls, while other estimators use earlier-treated groups after treatment as controls. And the TWFE estimator takes an average of all the 2x2 DD estimators with weights proportional to group sizes and treatment variance in each pair. The variance is larger when the two group sizes are similar or when the treatment happens closer to the midpoint of the relevant time window.

Under the parallel trends assumption and in the case that treatment effects are constant across groups and over time, the TWFE estimator would measure the average treatment effect on the treated (ATT) as researchers would expect. However, if treatment effects vary across groups, the TWFE estimator is different from the ATT because the effects are weighted not only by sample shares but also by treatment variance in each pair. As the weights do not have any policy justification, the TWFE estimator would be difficult to interpret. Even worse, if treatment effects are not constant over time, the TWFE estimator could be misleading and may have the opposite sign of the ATT in an extreme scenario. The bias arises from the case when the treatment effect

gets differenced out by the dynamic treatment effects using already-treated groups as the control. To sum up, the TWFE estimator could be biased under treatment effect heterogeneities. And the heterogeneities both across states and over time are relevant in the study context of the math reforms.

In response to the concerns of the TWFE estimator, I implement a new estimator developed by Callway & Sant'Anna (2020, hereafter the C&S estimator) that is robust to treatment effect heterogeneities. The building block of the C&S estimator is the group-time average treatment effect of a treatment group $g$ and a time period $t$. Under my study context, $g$ is defined as a state group including states with the same reform timing, and $t$ is defined as a high school graduation cohort. Following the notation used by the authors, $G_i$ indicates the state group that individual $i$ belongs to. $Y_{it}(0)$ denotes the untreated potential outcome for individual $i$ in high school graduation cohort $t$, and $Y_{it}(g)$ denotes the treated potential outcome for individual $i$ in high school graduation cohort $t$ if the treatment timing corresponds to that of the state group $g$. And the group-time average treatment effect can be denoted by:

$$ATT(g,t) = E_i[Y_{it}(g) - Y_{it}(0)|G_i = g] \qquad (1.1)$$

The authors show that the group-time average treatment effects can be nonparametrically point-identified. The approach uses the most recent time period when untreated outcomes are observed as the reference time period, and "never-treated" or "not-yet-treated" groups as the comparison group. That is, the approach does not include "already-treated" groups in the control to avoid bias due to dynamic treatment effects. To identify the treatment effects of the math reforms, I follow Goodman (2019) and exclude "never-treated" states from the control group. Goodman (2019) argued that including the never-reformed states could introduce bias through endogeneity of the decision to adopt statewide math reforms at all. For example, Gao, Lopes and

Lee (2017) reported that California hasn't revised its statewide HSGR in math since 2003; however, the state encourages local districts to set their own requirements, and 63% of high school districts in the state have required an additional year of math by the 2015-16 school year. If students' math course-taking behaviors at high school in the "never-treated" states were driven more by district-level policy changes in the HSGR, or there exist other endogenous factors of the non-reforming decision, these "never-treated" states would not serve as good counterfactual controls in the estimation. Therefore, I only include "not-yet-treated" states as the control group in my estimation.

The group-time average treatment effect, $ATT(g,t)$, imposes no restrictions on treatment effect heterogeneities across groups or over time. Once the group-time average treatment effects are identified, they could be aggregated into summary measures of the causal effects of the math reforms with carefully-chosen weights in the following form:

$$\theta = \sum_g \sum_t w(g,t) ATT(g,t) \qquad (1.2)$$

For example, the authors show that the $ATT(g,t)$ could be combined into aggregations for dynamic effects as in an event study. However, an event-study analysis may not be well suited under the study context due to the small number of the reforming states. Instead, the study is focused on the aggregation that results in a single summary parameter with the same interpretation as the ATT in a 2x2 DD setup.

Not only does the C&S estimator allow for the flexibility of the aggregation weights and the examination of the overall treatment effect, the approach also imposes a weaker parallel trends assumption than other recently-developed estimators for a staggered difference-in-difference analysis and no other assumptions beyond (Marcus & Sant'Anna, 2021). Specifically, the C&S estimator based on "not-yet-treated" units assumes that treated units would have

followed parallel trends after the first unit is treated as the not-yet-treated units in the absence of the treatment. Furthermore, the authors developed an open-source package in R named "did", and the estimation could be easily implemented with the package.

## 1.5 Results

### 1.5.1 Main Results

Table 1.2 provides results from the C&S estimations. Each column represents a separate estimation for one of the six outcome measures. And results for the full analysis sample including all the racial groups are presented in Panel A. Subgroup estimates for White students, Black students, and Hispanic students in the sample are presented in Panel B, C, and D respectively.[4]

According to results based on the full analysis sample, recent reforms increasing the HSGR to four years of math did not have a significant impact on high school graduation, and had a positive impact on college success overall. Specifically, estimation results on the measures of high school graduation outcomes have small magnitudes and are not statistically significant. For college outcomes measured at age 22, results show that the treated states that have reformed the HSGR to four years of math on average had a 1.6 percentage point higher college degree completion rate and a 1.5 percentage point higher on-time Bachelor's degree completion rate than the control states. That is, the math reforms increased the college degree completion rate by about 9 percent from the baseline rate of 17.7%, and the on-time Bachelor's degree completion rate by about 14 percent from the baseline rate of 11%. Also, the on-time Bachelor's degree

[4] Other racial groups only account for a small proportion of the sample, and are therefore neglected from subgroup analyses due to sample size concerns. All the racial groups are included in the full analysis sample.

completion rate in STEM fields of the full sample was 0.5 percentage points higher in the treated states than the control states, indicating a 21 percent increase from the baseline rate of 2.4%.

**Table 1.2: C&S Estimates of the Math Reforms' Impacts on High School Graduation and College Outcomes**

| OUTCOMES | Outcomes by Age 22 | | | | | |
|---|---|---|---|---|---|---|
| | High School Credential | Regular High School Diploma | College Enrollment | College Degree | Bachelor's Degree | Bachelor's Degree in STEM |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***Panel A: All Races*** | -0.003 | -0.005 | 0.012 | 0.016 | 0.015 | 0.005 |
| | (0.014) | (0.013) | (0.013) | (0.009)* | (0.007)** | (0.002)*** |
| Baseline rates: Class of 2006 | 0.888 | 0.838 | 0.587 | 0.177 | 0.110 | 0.024 |
| N | 87,366 | | | | | |
| ***Panel B: White*** | 0.010 | 0.015 | 0.018 | 0.010 | 0.013 | 0.006 |
| | (0.010) | (0.010) | (0.013) | (0.008) | (0.005)*** | (0.002)*** |
| Baseline rates: Class of 2006 | 0.913 | 0.864 | 0.619 | 0.204 | 0.133 | 0.029 |
| N | 60,068 | | | | | |
| ***Panel C: Black*** | -0.034 | -0.041 | -0.050 | -0.013 | -0.009 | -0.011 |
| | (0.017)** | (0.014)*** | (0.022)** | (0.006)** | (0.008) | (0.007) |
| Baseline rates: Class of 2006 | 0.827 | 0.772 | 0.508 | 0.090 | 0.045 | 0.005 |
| N | 17,979 | | | | | |
| ***Panel D: Hispanic*** | -0.031 | -0.152 | 0.049 | 0.102 | 0.069 | 0.013 |
| | (0.031) | (0.004)*** | (0.037) | (0.010)*** | (0.010)*** | (0.005)*** |
| Baseline rates: Class of 2006 | 0.857 | 0.807 | 0.539 | 0.165 | 0.071 | 0.009 |
| N | 7,471 | | | | | |

Note: The table shows estimated impacts of the math reforms increasing the statewide minimum HSGR to four years of math on high school graduation and college outcomes using the C&S estimator aggregated to a single summary parameter. The full analysis sample includes individual aged 22 in the 2010-2019 ACS data among states recently reformed the minimum HSGR to four years of math. Each column represents a separate estimation, and each panel represents a separate sample. All the educational attainment outcomes are measured at age 22. Baseline rates are measured among the high school graduation class of 2006. All the observations are weighted by the person weight. Standard errors are calculated using the cluster-robust multiplier bootstrap procedure clustered at state level. *** p<0.01, ** p<0.05, * p<0.1.

Although the overall impact of the recent math reforms on educational attainment was positive, results from subgroup analyses by racial groups indicate large racial disparities in the effectiveness of the reforms. Among White students, the math reforms increased the on-time Bachelor's degree completion rate by 1.3 percentage points, equivalent to a 10 percent increase from the baseline rate of 13.3%. Also, the reforms increased the on-time Bachelor's degree completion rate in STEM fields by 0.6 percentage points, equivalent to a 21 percent increase

from the baseline rate of 2.9%. Results for White students do not show significant impacts of the reforms on high school graduation or college attainment lower than Bachelor's degree completion.

In contrast, the math reforms had negative effects on high school graduation as well as subsequent college outcomes among Black students. Specifically, I find a 3.4 percentage point decrease in the likelihood of earning any high school credentials by age 22 (a 4 percent decrease from the baseline of 82.7%), and a 4.1 percentage point decrease in the likelihood of earning a regular high school diploma by age 22 (a 5 percent decrease from the baseline of 77.2%). Black students were also less likely to enroll in college or complete a college degree by age 22 following the reforms: results show that the reforms led to a 5 percentage point decrease in the college enrollment rate (a 10 percent decrease from the baseline rate of 50.8%), and a 1.3 percentage point decrease in the college degree completion rate (a 14 percent decrease from the baseline rate of 9%). The reforms did not have any significant impacts on Black students' Bachelor's degree completion outcomes.

Among Hispanic students, the math reforms were a double-edged sword. Results show that the reforms discouraged a group of Hispanic students from earning a regular high school diploma: the likelihood of earning a regular high school diploma by age 22 was 15.2 percentage points lower, equivalent to a 18 percent decrease from the baseline of 80.7%. Unlike Black students, the estimated effect on the probability of earning any high school credentials by age 22 for Hispanic students is not statistically significant. On the other hand, the reforms strongly improved the college degree completion outcomes of Hispanic students. In particular, I find that the reforms resulted in a 10.2 percentage point increase in their college degree completion rate (a 62 percent increase from the baseline rate of 16.5%), a 6.9 percentage point increase in the on-

time Bachelor's degree completion rate (a 97 percent increase from the baseline rate of 7.1%), and a 1.3 percentage point increase in the on-time Bachelor's degree completion rate in STEM fields (a 144 percent increase from the baseline rate of 0.9%).

To sum up, estimation results show that the math reforms increasing the HSGR to four years of math had positive impacts on college degree completion among White and Hispanic students, targeting Bachelor's degree seekers among White students and including 2-year degree seekers among Hispanic students. The positive impacts were particularly strong among Hispanic students, who had relatively lower baseline educational attainment than their White counterparts. However, the reforms also had negative effects on high school graduation among Black and Hispanic students. In particular, the reforms led to lower likelihoods of both earning a regular high school diploma and any high school credential by age 22 among Black students, suggesting that Black students were likely to be discouraged by the reforms and drop out from high school early. Also, Black students suffered from subsequent negative impacts of the reforms on college enrollment and college degree completion. Among Hispanic students, the reforms decreased the likelihood of earning a regular high school diploma by age 22, but the impact of the reforms on the likelihood of earning any high school credentials was not statistically significant. The results suggest that Hispanic students were likely to switch from a regular high school diploma to an alternative high school credential in response to the reforms. It may partially explain why Hispanic students did not experience strong negative impacts on subsequent college outcomes as Black students did. Nevertheless, a switch from the regular high school diploma to an alternative credential could have adverse implications on labor market outcomes among those who directly entered the workforce after high school. Overall, the results highlight treatment heterogeneity of the math reforms across racial groups as well as within the group of Hispanic students.

### 1.5.2 Robustness Checks

Results above present the estimated impacts of recent math reforms increasing the statewide HSGR to four years of math on educational attainment. The paper also examines two alternative samples to test the robustness of the results. While the main analysis sample includes all the states that recently increased the minimum graduation requirement for all high school students in the state to four years of math, the first alternative sample is focused on states that introduced a graduation requirement of four years of high school math for the first time to some student groups in the state in recent years. As discussed in the policy background, the main analysis sample includes Georgia and North Carolina that had already required students in the College Prep track to graduate from high school with four years of math before the recent reforms expanded it to a statewide requirement. College-going students in Georgia and North Carolina may not be affected by the policy expansion much, and the two states are excluded from the first alternative sample. Similarly, West Virginia first raised the requirement to four years of math among students enrolled in the professional & skilled pathway in 2008, and then expanded the requirement to all the students in the state in 2010. While the main analysis uses 2010 as the statewide policy change year for West Virginia, the first robustness check uses 2008 as its policy change year to examine the effects of first introducing the four-math requirement to the state. The other states in the main analysis sample reformed the HSGR to four years of math once and for all, and are therefore kept as they were for the first robustness check.

Table 1.3 presents estimation results based on the first alternative sample. Overall, the results are consistent with findings from the main analysis: the math reforms improved college outcomes among White and Hispanic students, but had negative impacts on high school graduation outcomes among Black and Hispanic students.

**Table 1.3: C&S Estimates of the Math Reforms' Impacts on High School Graduation and College Outcomes – First Alternative Sample: States First Introducting a 4-Math HSGR**

| OUTCOMES | Outcomes by Age 22 | | | | | |
|---|---|---|---|---|---|---|
| | High School Credential | Regular High School Diploma | College Enrollment | College Degree | Bachelor's Degree | Bachelor's Degree in STEM |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***All Races*** | -0.013 | -0.019 | 0.010 | 0.025 | 0.019 | 0.005 |
| | (0.007)* | (0.010)* | (0.015) | (0.009)*** | (0.009)** | (0.002)** |
| Baseline rates: Class of 2006 | 0.891 | 0.841 | 0.586 | 0.181 | 0.110 | 0.024 |
| N | 69,927 | | | | | |
| ***White*** | 0.000 | -0.003 | 0.014 | 0.022 | 0.015 | 0.004 |
| | (0.004) | (0.009) | (0.013) | (0.008)*** | (0.007)** | (0.002)*** |
| Baseline rates: Class of 2006 | 0.917 | 0.869 | 0.617 | 0.206 | 0.131 | 0.028 |
| N | 48,568 | | | | | |
| ***Black*** | -0.036 | -0.042 | -0.042 | -0.014 | -0.004 | -0.014 |
| | (0.013)*** | (0.017)** | (0.029) | (0.007)* | (0.011) | (0.006)** |
| Baseline rates: Class of 2006 | 0.826 | 0.761 | 0.500 | 0.090 | 0.041 | 0.005 |
| N | 12,956 | | | | | |
| ***Hispanic*** | -0.045 | -0.175 | 0.023 | 0.093 | 0.064 | 0.013 |
| | (0.020)** | (0.024)*** | (0.009)** | (0.010)*** | (0.010)*** | (0.005)** |
| Baseline rates: Class of 2006 | 0.860 | 0.807 | 0.541 | 0.172 | 0.073 | 0.010 |
| N | 6,810 | | | | | |

Note: The table shows estimated impacts of the math reforms first introducing a four-math HSGR to some student groups on high school graduation and college outcomes using the C&S estimator aggregated to a single summary parameter. The full analysis sample includes individual aged 22 in the 2010-2019 ACS data among states recently introduced a four-math HSGR to some student groups in the state. Each column represents a separate estimation, and each panel represents a separate sample. All the educational attainment outcomes are measured at age 22. Baseline rates are measured among the high school graduation class of 2006. All the observations are weighted by the person weight. Standard errors are calculated using the cluster-robust multiplier bootstrap procedure clustered at state level. *** p<0.01, ** p<0.05, * p<0.1.

On the positive side, the math reforms that first introduced a four-math graduation requirement had a positive effect on the probability of completing any college degrees by age 22 among White students. The result is statistically significant with a magnitude larger than the estimation on on-time Bachelor's degree completion, suggesting that the reforms in the first robustness check improved college degree completion among both 2-year and 4-year White students. Among Hispanic students, results from the first robustness check show that the reforms improved both their college enrollment and college degree completion outcomes. Compared to the main analysis, the first robustness check examines policy changes that would have a larger

implication among potential college-going students, and therefore the results show stronger positive impacts on college outcomes. On the negative side, results from the first robustness check show slightly larger negative impacts on high school graduation outcomes among both Black and Hispanic students than the main results. One reason for the larger negative effects could be that schools in states that have never set a four-math HSGR were more likely to be underprepared for such a requirement than schools that have already been supporting some student groups to fulfill the requirement.

The second robustness check examines a broader set of math reforms. Specifically, the check estimates the impacts of increasing the statewide HSGR to three or more years of math on high school graduation and college success. That is, the second alternative sample includes all the states that recently had a reform increasing the statewide HSGR either from lower than three to three or from lower than four to four years of math.[5] It covers math reforms with a lower standard than the main analysis on average, but allows to understand the general effect of recent reforms increasing the HSGR with a target on relatively more advanced math coursework using a larger sample of states.

Table 1.4 shows estimation results of the second robustness check. The results are not as significant as the main results, but the patterns are still consistent. White and Hispanic students in the reforming states were more likely to achieve college success than those in the control states. In particular, the estimation result on college enrollment for the White subgroup is positive and statistically significant, showing that the math reforms in the second alternative

[5] There are in total 25 states in the second alternative sample (reform years in parentheses): (1) States reformed from 2 to 3 years of math recently include South Dakota (2008), Idaho (2009), Illinois (2009), Missouri (2010), Oregon (2010), Utah (2011), Arizona (2012), Idaho (2013), and Washington (2013); (2) States reformed from 3 to 4 years of math recently include Rhode Island (2008), Arkansas (2009), Mississippi (2009), West Virginia (2010), Delaware (2011), District of Columbia (2011), Florida (2011), Georgia (2012), Louisiana (2012), New Mexico (2013), North Carolina (2013), Tennessee (2013), and Ohio (2014); (3) States reformed from none to 3 or 4 recently include: Minnesota (2008), Michigan (2011), and Iowa (2011).

sample had a positive impact on enrolling marginal students to college. Similar to the main results, the math reforms in the second robustness check had positive impacts on Bachelor's degree completion among White students and college degree completion among Hispanic students, but a negative impact on the probability of earning a regular high school diploma among minoritized students.

**Table 1.4: C&S Estimates of the Math Reforms' Impacts on High School Graduation and College Outcomes – Second Alternative Sample: States Increasing the HSGR to 3 or More Math**

| OUTCOMES | Outcomes by Age 22 | | | | | |
|---|---|---|---|---|---|---|
| | High School Credential | Regular High School Diploma | College Enrollment | College Degree | Bachelor's Degree | Bachelor's Degree in STEM |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***All Races*** | 0.007 | 0.005 | 0.023 | 0.002 | 0.009 | 0.002 |
| | (0.005) | (0.006) | (0.008)*** | (0.007) | (0.004)** | (0.002) |
| Baseline rates: Class of 2006 | 0.895 | 0.850 | 0.609 | 0.200 | 0.125 | 0.024 |
| N | 130,099 | | | | | |
| ***White*** | 0.009 | 0.011 | 0.026 | -0.004 | 0.008 | 0.003 |
| | (0.006) | (0.007) | (0.008)*** | (0.008) | (0.004)* | (0.003) |
| Baseline rates: Class of 2006 | 0.920 | 0.876 | 0.644 | 0.232 | 0.148 | 0.029 |
| N | 93,726 | | | | | |
| ***Black*** | -0.021 | -0.023 | -0.002 | -0.003 | 0.000 | -0.009 |
| | (0.014) | (0.010)** | (0.021) | (0.006) | (0.009) | (0.006) |
| Baseline rates: Class of 2006 | 0.826 | 0.776 | 0.509 | 0.091 | 0.050 | 0.006 |
| N | 22,111 | | | | | |
| ***Hispanic*** | 0.020 | -0.042 | 0.038 | 0.059 | 0.041 | -0.001 |
| | (0.019) | (0.018)*** | (0.033) | (0.017)*** | (0.015)*** | (0.009) |
| Baseline rates: Class of 2006 | 0.855 | 0.804 | 0.533 | 0.151 | 0.069 | 0.007 |
| N | 11,307 | | | | | |

Note: The table shows estimated impacts of the math reforms increasing the statewide HSGR to three or more years of math on high school graduation and college outcomes using the C&S estimator aggregated to a single summary parameter. The full analysis sample includes individual aged 22 in the 2010-2019 ACS data among states recently reformed the HSGR to three or more years of math. Each column represents a separate estimation, and each panel represents a separate sample. All the educational attainment outcomes are measured at age 22. Baseline rates are measured among the high school graduation class of 2006. All the observations are weighted by the person weight. Standard errors are calculated using the cluster-robust multiplier bootstrap procedure clustered at state level. *** p<0.01, ** p<0.05, * p<0.1.

### 1.5.3 Validity Checks

The parallel trends assumption is critical to ensure the validity of a difference-in-difference estimation. As discussed in the empirical strategy section, the C&S estimator relies on

the assumption that treated units would have followed a parallel trend after the first unit is treated as control units in the absence of the treatment. In order to check for the parallel trends assumption, I examine average treatment effects by length of exposure to the math reforms. If estimations on leads are non-zero, the parallel trends assumption may be violated or the previous analysis results may be obscured from a reverse causality interpretation. The check would also illustrate dynamic effects of the reforms. However, the dynamic effects to the far end of the length of exposure should be interpreted with caution as they are represented by only a small number of reforming states.

Appendix Figure A1 presents estimation results by length of exposure to the math reforms for each of the six educational attainment measures using the main analysis sample. The majority of the estimation results on the leads, especially those closer to the reform timing and are therefore represented by more states in the sample, are not significantly different from zero. The check shows no evidence for violation of the parallel trends assumption or reverse causality in the analysis. Also, estimation results on the lags for college outcomes show an increasing trend of positive impacts over time.

In addition, I run a falsification test by running the same main analysis on outcomes for racial composition. Racial composition in the reforming states should not be affected by the math reforms. As presented in Appendix Table A2, the results did not show any significant impacts of the math reforms on the proportion of White or minoritized students in the reforming states. The results reassure the internal validity of the empirical design.

Furthermore, Appendix Table A3 presents estimation results to the main analysis using a TWFE approach for the comparison purpose. The signs of the TWFE coefficients and the C&S estimations are consistent, confirming the overall effect of the math reforms. However,

compared with the C&S estimation results, the TWFE estimation results are less significant with smaller magnitudes. As discussed in the empirical strategy section, the TWFE estimator can be biased under treatment effect heterogeneities. For example, under treatment effect heterogeneities over time, the TWFE estimator may difference out some of the actual treatment effect because it uses "already-treated" units in the control. Alternatively, under treatment effect heterogeneities across states, the TWFE estimation would be smaller than the actual ATT if the TWFE estimator assigns more weights to states with a smaller treatment effect.

## 1.6 Mechanisms

The C&S estimation results presented above show the causal impacts of the math reforms on subsequent high school graduation and college outcomes. To examine mechanisms behind the impacts, I would like to understand how the reforms affected students' course-taking patterns in high school math. Unfortunately, the ACS data do not provide information on students' high school course-taking behaviors, and therefore do not allow for a causal analysis on the mechanisms of the reforms. I explore several other datasets and provide suggestive evidence for a better interpretation of the causal findings from the main analysis.

First, the statewide math reforms would not have much impact on students' course-taking patterns if students were already required to graduate from high school with four years of math at the district level or the school level. Using the Education Longitudinal Study of 2002 (ELS:2002), I examine district-level and school-level requirements in the reforming states for a graduation class before the math reforms. ELS:2002 surveyed a nationally representative sample of 10th graders in 2002, as well as their parents, teachers and school administrators, throughout their secondary years. Students in the sample were in the 12th grade in the year of 2004, which was before any of the reforming states in my analysis sample increased the statewide HSGR to

four years of math. In particular, school administrators were surveyed in 2004 on math coursework required to meet the high school graduation requirement in their school districts or schools, and responses were matched on the student file for students attending the sampled schools as of Spring 2004 in the data.

Table 1.5 presents percentages of students in the ELS:2002 sample whose school districts or schools required four years of math to meet the high school graduation requirement for the graduation class of 2004 in the reforming states of my main analysis. Results show that only around 20% of the sample were facing the four-math requirement at the district or school level before the statewide math reforms. The percentages are similar across racial groups. Therefore, the results suggest that the majority of students in the reforming states were not already required to graduate from high school with four years of math in their school districts or schools at the time of the statewide reforms.

**Table 1.5: Percentage of Students with a 4-Math HSGR by Districts or Schools in Reforming States, High School Graduation Class of 2004**

| SAMPLE | % | N |
|---|---|---|
| Full Sample | 20% | 3,660 |
| White Sample | 21% | 2,280 |
| Black Sample | 21% | 600 |
| Hispanic Sample | 19% | 310 |

Note: The sample for the table includes all responding students to ELS:2002 First Follow-up attending the sampled schools as of Spring 2004 in the reforming states that recently increased the HSGR to four years of math. The table shows percentage of students in the sample with non-missing value whose school districts or schools required four years of math to meet the high school graduation requirement for the class of 2004. All the observations are weighted by the first follow-up questionnaire cross-sectional weight. States are based on state code for school locale, and races are based on student's race/ethnicity-composite variable. The table shows unweighted sample sizes rounded to the nearest ten, and suppresses cells in which there are fewer than three (3) respondents.

In addition, high school transcripts are available in the ELS:2002 data to examine students' baseline course-taking patterns before the math reforms. Among Spring 2004 senior cohort members with transcript data available for four academic years in the ELS:2002 sampled

students, I calculate the percentage of students who took four or more years of high school math, the percentage of students who completed an advanced high school math course, as well as the percentage of students who completed an AP/IB math course at high school. That is, I examine math course-taking patterns among students who progressed to the last year of high school. Students who dropped out from high school early were not included in the sample because I could not differentiate them from students who transferred out of the sampled schools or those with missing transcript data.

Table 1.6 presents results across racial groups for both the reforming states in my main analysis and other non-reforming states. Before the math reforms, around half of the students were already taking four or more years of high school math in the reforming states. The percentage was slightly higher than the non-reforming states, and White students were more likely to take four or more years of math than minoritized students in both the reforming and the non-reforming samples. Also, the percentage of students who completed an advanced high school math course was similar to the percentage of students who took four or more years of high school math, but the percentage of students who completed an AP/IB math course at high school was much lower. The results suggest that students who progressed to the fourth math before the reforms were most likely to complete an advanced math course at high school, but less likely to complete a course that could be transferred to college credits later. The racial difference between White and minoritized students in the completion of an advanced or AP/IB math course at high school was larger than the difference in the probability of taking four or more years of high school math in the reforming states, suggesting that minoritized students were more likely to take a fourth math at a relatively lower level. Overall, the math reforms increasing the statewide HSGR to four years of math had the potential to affect a large proportion of students

who were not already fulfilling the requirement, across all the racial groups and especially among underrepresented minoritized students.

**Table 1.6: High School Math Course-Taking Patterns, High School Graduation Class of 2004**

| SAMPLE | STATE | % Taking 4 or More High School Math | % Completing Advanced High School Math | % Completing AP/IB Math | N |
|---|---|---|---|---|---|
| Full Sample | Reforming | 54% | 53% | 12% | 3,710 |
| | Non-reforming | 45% | 46% | 12% | 7,660 |
| White Sample | Reforming | 56% | 57% | 14% | 2,350 |
| | Non-reforming | 48% | 51% | 13% | 4,180 |
| Black Sample | Reforming | 50% | 45% | 4% | 590 |
| | Non-reforming | 42% | 33% | 5% | 590 |
| Hispanic Sample | Reforming | 50% | 44% | 6% | 310 |
| | Non-reforming | 35% | 31% | 7% | 1,130 |

Note: The sample for the table includes Spring 2004 senior cohort members with transcript data available for four academic years in the ELS:2002 sampled members. The table is based on the following variables in the student file: units in mathematics, math course taking pipeline, and total AP/IB math courses. Advanced math courses are defined as courses higher than Algebra II. All the observations are weighted by cross-sectional high school transcript weight. States are based on last transcript school state, and races are based on students' race/ethnicity-composite variable. The table shows unweighted sample sizes rounded to the nearest ten, and suppresses cells in which there are fewer than three (3) respondents. Reforming states are states that recently increased the HSGR to four years of math, and non-reforming states are the other states with a HSGR lower than four years of math.

In order to examine changes in students' high school math course-taking patterns from before to after the math reforms, I use high school transcripts data in the High School Longitudinal Study of 2009 (HSLS:09) and compare with results based on the ELS:2002. Similar to the ELS:2002, the HSLS:09 is a nationally representative, longitudinal study of 9th graders in 2009. Students in the sample were in the 12th grade in the year of 2012, which was after most of the reforming states have increased the HSGR to four years of math in my main analysis.[6] And I examine the percentage of students who took four or more years of high school math, the percentage of students who completed an advanced high school math course, as well as the

[6] Five of the reforming states increased the HSGR to four years of math after the graduation class of 2012. However, the HSLS:09 is the latest high school transcript data for a nationally representative sample that I have access to.

percentage of students who completed an AP/IB math course at high school, to understand how the patterns changed from a cohort before the math reforms to a cohort after the reforms.

**Table 1.7: High School Math Course-Taking Patterns, High School Graduation Class of 2012**

| SAMPLE | STATE | % Taking 4 or More High School Math | % Completing Advanced High School Math | % Completing AP/IB Math | N |
|---|---|---|---|---|---|
| Full Sample | Reforming | 75% | 65% | 13% | 7,070 |
| | Non-reforming | 58% | 59% | 17% | 10,740 |
| White Sample | Reforming | 75% | 66% | 15% | 3,920 |
| | Non-reforming | 59% | 64% | 18% | 5,420 |
| Black Sample | Reforming | 76% | 64% | 7% | 800 |
| | Non-reforming | 65% | 53% | 8% | 870 |
| Hispanic Sample | Reforming | 76% | 62% | 12% | 810 |
| | Non-reforming | 52% | 49% | 12% | 1,940 |

Note: The sample for the table includes students with transcript data available for four academic years in the HSLS:09 sampled members. The table is based on the following variables in the student file: credits earned in mathematics, highest level mathematics course taken/pipeline, and credits earned in AP/IB mathematics courses. Advanced math courses are defined as courses higher than Algebra II. All the observations are weighted by student high school transcript weight. States are based on state code for school, and races are based on students' race/ethnicity-composite variable. The table shows unweighted sample sizes rounded to the nearest ten, and suppresses cells in which there are fewer than three (3) respondents. Reforming states are states that recently increased the HSGR to four years of math, and non-reforming states are the other states with a HSGR lower than four years of math.

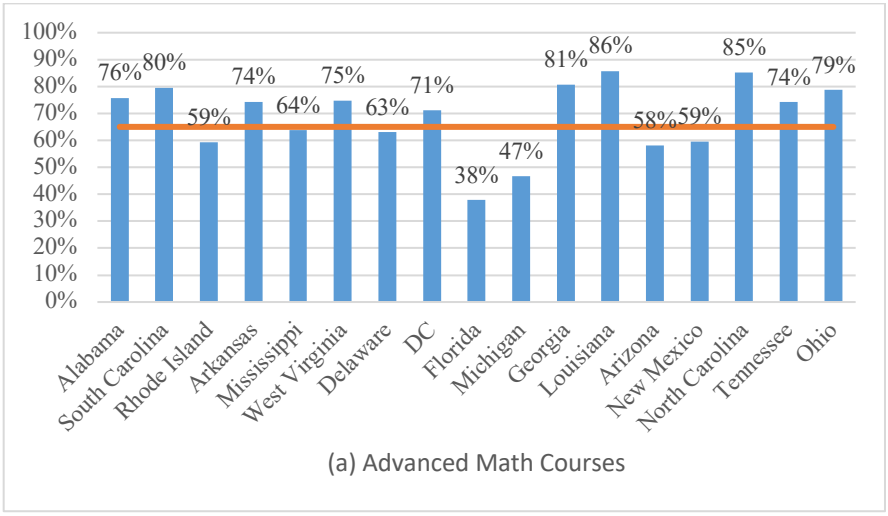
Table 1.7 presents results among students with transcript data available for four academic years in the HSLS:09 sampled members. Compared with results in Table 1.6, students in a post-reform cohort were more likely to take four or more years of high school math. The increase could be found in all the racial groups, with a larger magnitude for minoritized students. In fact, minoritized students who progressed to the 12th grade were equally likely to take four or more years of high school math as White students after the reforms in the reforming states. The percentage of taking four or more years of high school math was higher among the reforming states than other states that have not set a four-math requirement. Regarding course-taking in advanced math, the percentage of students who completed an advanced high school math course and the percentage of students who completed an AP/IB math course at high school were both

higher in the post-reform cohort across racial groups. Nevertheless, the increases were not as large as the increase in the percentage of taking four or more years of math. Among the post-reform cohort in the reforming states, the completion rate of AP/IB math courses at high school was still low, and minoritized students were still less likely than White students to complete advanced or AP/IB math courses at high school. However, there was a relatively larger increase in the AP/IB math course completion following the reforms among Hispanic students.

To sum up, course-taking in high school math was higher after the math reforms, in terms of both the number and the level of math courses completed at high school. However, the results suggest that not all the students completed the fourth math with an advanced course, and an even lower proportion of students took an AP/IB math course that could be transferred to college credits later. While minoritized students were less likely to take advanced math courses than their White counterparts, the increase in the AP/IB math completion was relatively higher among Hispanic students than other racial groups.

The reason that some students did not take advanced math courses to fulfill the four-math requirement could be student-based or school-based. Students may self-select into lower-track math courses according to their previous academic preparation or for other personal concerns. However, students may not be able to take advanced math courses even if they would like to because of course unavailability at their schools. Using Civil Rights Data Collection (CRDC), I examine course offerings in advanced math at public high schools in the school year of 2015-16. Figure 1.2 shows percentages of public high schools offering advanced math courses or Calculus in each of the states that have reformed the HSGR to four years of math in the post-reform school year of 2015-16. While states with a four-math requirement on average had a higher percentage of schools offering advanced math courses or Calculus than the national average

(shown as the horizontal line), there were still a significant number of schools in which such courses were not offered, especially for Calculus. That is, students in these schools would not be able to fulfill the four-math requirement with advanced math courses because the courses were not available in their schools.



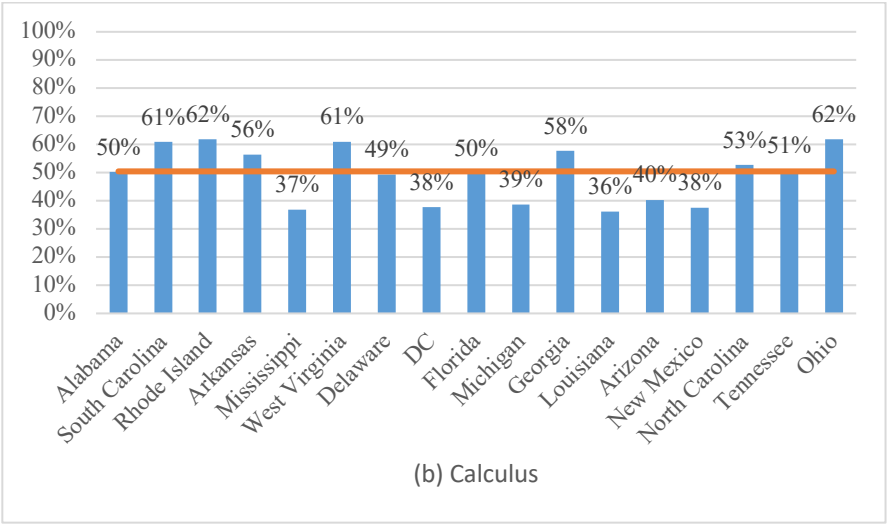




**Figure 1.2: Percentage of Public High Schools Offering Advanced Math Courses/Calculus in States with a 4-Math HSGR**

Note: The figures show percentage of public high schools offering advanced math courses or Calculus in states with a graduation requirement of four years of math using the CRDC data for the school year of 2015-16. The horizontal line represents the national average level. Advanced math courses include trigonometry, trigonometry/algebra, trigonometry/analytic geometry, trigonometry/math analysis, analytic geometry, math analysis, math analysis/analytic geometry, probability and statistics, and precalculus.

Schools would need financial support to offer additional math course sessions and support students to fulfill the higher graduation standard. Without funding support along with the math reforms in the HSGR, the reforms would be a particularly challenging policy change among schools that were already lacking resources. I examine trends in school revenues using the Common Core of Data (CCD) from the fiscal year of 2006 to 2015. The data collected total revenues per pupil as well as state revenues per pupil for all public schools by state and year with the National Public Education Financial Survey (State Fiscal). Figure 1.3 & 1.4 present trends in state revenue per pupil and total revenue per pupil in each of the reforming states respectively, grouping states by the reform year shown as the vertical line. Although there was a small increase in the revenue level before the reform in several states, we could not observe a sharp and universal funding increase around the reform year among the reforming states. That is, the figures suggest that there was no specific funding support corresponding to the math reforms in the HSGR from the state or other sources.

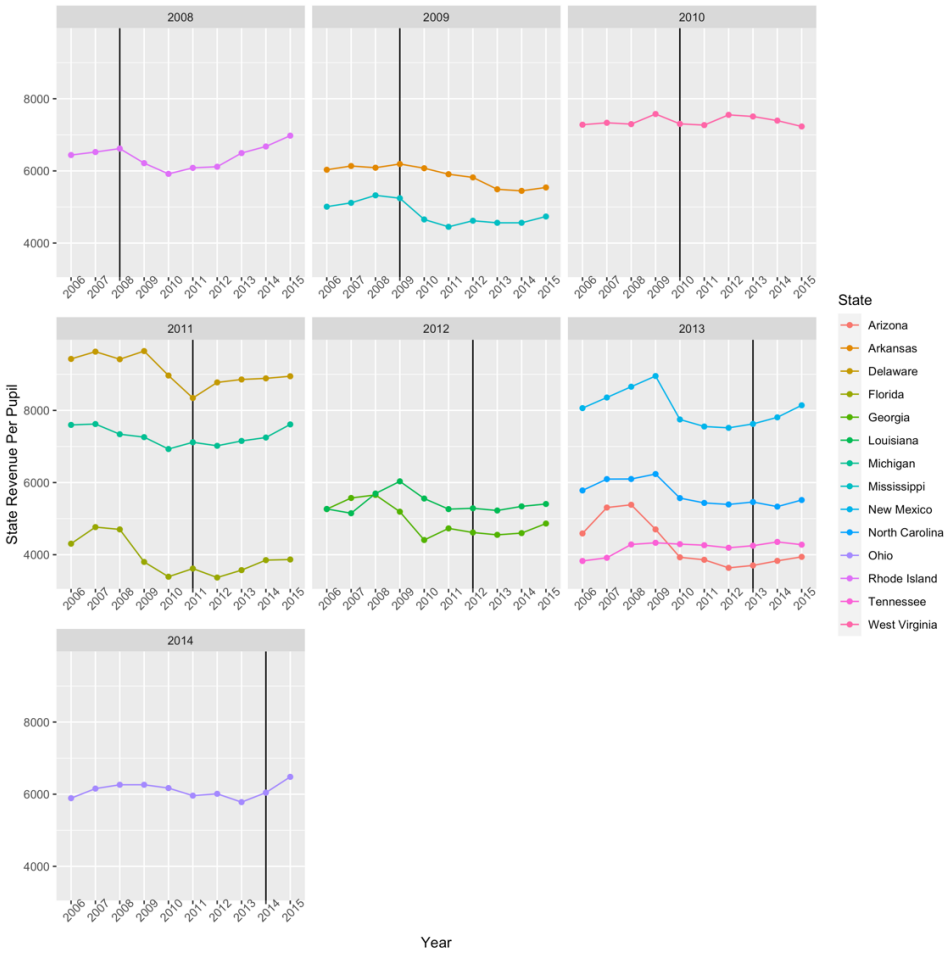



**Figure 1.3: Trends in State Revenue Per Pupil in Reforming States**

Note: The figure shows trends in revenue per pupil from state sources among public schools in the reforming states using the CCD data for fiscal years of 2006-2015. States are grouped by reform timing, shown as the vertical line. All the revenues are adjusted by Consumer Price Index to 2006 dollars.

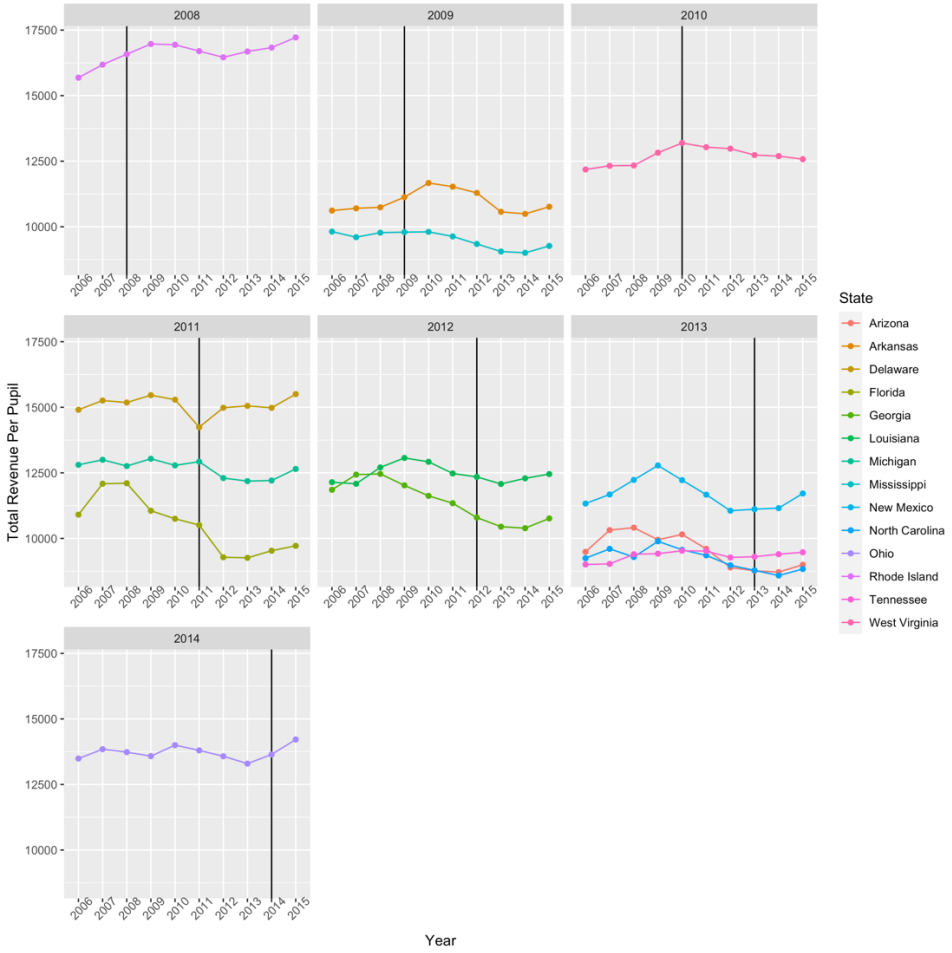



**Figure 1.4: Trends in Total Revenue Per Pupil in Reforming States**

Note: The figure shows trends in revenue per pupil from any sources among public schools in the reforming states using the CCD data for fiscal years of 2006-2015. States are grouped by reform timing, shown as the vertical line. All the revenues are adjusted by Consumer Price Index to 2006 dollars.

Last, I examine if students responded to the higher graduation standard for math at the cost of taking fewer English courses at high school, which are also critical for students' college readiness. Using senior cohort members with transcript data available for four academic years in the ELS:2002 and the HSLS:09 data, Appendix Table A4 calculates average years of English courses students took across racial groups among the high school graduation class of 2004 and 2012 respectively. Results show no evidence that students' English course-taking dropped after

the math reforms. And the average years of English courses taken at high school increased among minoritized students from the pre-reform cohort to the post-reform cohort.

To sum up, suggestive evidence shows that students were indeed more likely to take four math courses after the reforms increasing the HSGR to four years of math. Among students who progressed into the last year of high school, the increase in the probability of taking four high school math courses was shown in all the racial groups. However, not all the students fulfilled the four-math requirement with advanced math courses, and the rate of completing AP/IB math courses that could be transferred to college credits and thus lower college costs was even lower. While white students were more likely to complete advanced math courses at high school than minoritized students, there was a relatively larger increase in the AP/IB math completion among Hispanic students. Course unavailability could be one possible reason for students not taking advanced math courses in the reforming states. And there was no evidence of a funding increase along with the math reforms for schools to offer additional math courses and support students in response to the higher graduation requirement.

## 1.7 Discussion

The paper examines the impacts of recent statewide reforms increasing the HSGR in math on high school graduation and college success. I first build a conceptual framework based on economic theories and discuss the potential impacts of a policy change in the math HSGR on students' educational attainment. I then empirically estimate the causal impacts of the math reforms on high school graduation and college outcomes using a newly-developed estimator that is robust to treatment effect heterogeneities. Mechanisms behind the causal results are also examined with suggestive evidence on changes in students' math course-taking patterns, schools' course availability and funding revenues following the reforms.

Findings show that raising the bar for high school math under the current policy landscape could be a potentially effective policy tool to improve college readiness and college success. The statewide reforms increasing the HSGR to four years of math had positive impacts on college degree completion outcomes, in particular on-time Bachelor's degree completion in any fields as well as in STEM fields. That is, students were more likely to enter the workforce at an early age with a Bachelor's degree that could yield a high labor market return following the math reforms. Suggestive evidence indicates that students in a post-reform cohort were more likely to take four or more math courses at high school than students in a pre-reform cohort. They on average were also more likely to take advanced math courses after the math reforms. Therefore, the evidence suggests that students were driven to engage in longer math instruction time and/or a higher level of math content following the reforms. A stronger math foundation built from it would lower college-related costs, leading to improved college outcomes as the causal results show. For example, students may enroll in college-level math courses directly without remediation requirements. Some students may transfer credits from AP/IB math courses to college, but it may not be a major contributing factor to the improved college outcomes given the low completion rate in AP/IB math courses in a post-reform cohort shown in the suggestive evidence.

Although the overall impact of the math reforms was positive, I also find significant racial disparities in the reforms' effectiveness, and unintended consequences among minoritized students. Specifically, the positive impacts of the reforms were only experienced by White and Hispanic students. The reforms led to increases in on-time Bachelor's degree completion rates in any fields and in STEM fields among 4-year degree seeking White and Hispanic students, and also increased the probability of completing 2-year college degrees by age 22 among Hispanic

students. And the positive impacts were larger among Hispanic students. Hispanic students had lower baseline college degree completion rates than White students. Also, suggestive evidence shows that Hispanic students were less likely to take four or more math courses or advanced math courses at high school than their White counterparts before the reforms. After the reforms, Hispanic students were equally likely to take four or more high school math courses as White students, and there was a relatively larger increase in the AP/IB math completion among Hispanic students. Even if Hispanic students were still less likely to complete advanced math courses than White students, a longer exposure to math instruction throughout high school years may also prepare them better for college with a more solid understanding of basic math concepts. The results therefore suggest that the math reforms had a large positive effect on marginal Hispanic students.

On the negative side, the math reforms had a discouraging effect on minoritized students. Black students were more likely to drop out from high school early and Hispanic students were more likely to switch from the regular high school diploma to alternative high school credentials as a result. Black students also suffered from negative impacts on subsequent college outcomes, which could be driven by both the worse high school graduation outcomes and cohort effects.

Suggestive evidence indicates that minoritized students were still less likely to take advanced math courses at high school than White students after the reforms. Also, not all the schools in the reforming states offered advanced math courses. That is, students may not be able to take math courses with the same content and quality following the reforms. The difference may be due to previous academic preparation or course availability at school. A higher graduation requirement in math could be more challenging for academically-disadvantaged students and under-resourced schools. I also find no suggestive evidence of a funding increase

along with the math reforms. Therefore, racial disparities in the reforms' effectiveness may be partially due to differences in course availability and school resources. I would like to also note that courses categorized as advanced math could differ in content and quality across schools. And students taking the same course may gain differently because of differences in academic preparation and learning environment.

States may want to consider standardizing the content of the additional math course on top of requiring more years of math instruction. Also, the math reforms may be more effective if states ensure that districts, schools and students have the resources to fulfill and gain from the new graduation requirement. Nevertheless, the unequal effectiveness of the reforms across student groups could be due to differences in both resources and academic preparation. Therefore, it would call for a broader reform of math curriculum in earlier years of high school or even before high school to prepare students equally sufficient for the new graduation requirement.

As a result of this paper, a few critical questions are identified for future research to explore. First, although the paper provides suggestive evidence on mechanisms behind the impact of the math reforms on college success, it would be great for future research with access to better course data to tease out the causal relationship between the reforms and students' high school course-taking patterns. Such practices would help us understand which high school math courses contributed to college success the most following the reforms. For example, how much of the improved college outcomes could be attributed to AP/IB/dual enrollment courses that could be transferred to college credits? Also, it would be necessary to understand how students trade between math courses and other courses in response to the math reforms and its implications. Besides, as students took more math courses at high school following the reforms,

the positive impacts of the reforms on students' college outcomes may be due to improved math skills or simply a signaling effect to college. It would be interesting for future research with access to data on students' math skill measures to explore the extent that the math reforms improved students' math skills at high school graduation. In addition, none of the racial subgroup is monolithic. In particular, the Hispanic population may be distinctive across regions. It is also possible that college outcomes among some Black students improved as a result of the reforms but the positive impacts are buried within the overall effect. Future research with access to data including more refined subgroup categorizations or variables such as the high school family income level would help understand the racial disparities in the reform effectiveness better. Last, future qualitative research may contribute to the understanding of constraints in schools, teachers, families and students themselves in response to a higher HSGR in math, which would lead to critical policy recommendation for a more effective policy implementation.

# Chapter 2: Understanding Experiential Learning Through Work-Based Courses at College - Evidence from Applying a Text Mining Technique with Transcript Data[7]

## 2.1 Introduction

Decades of empirical research have documented high returns of a college degree on students' labor market outcomes: On average, college-educated students earn more and suffer a lower incidence of unemployment than their less-educated counterparts (see review by Oreopoulos & Petronijevic, 2013). However, less is known about heterogeneity in labor market outcomes among college graduates, or about how students' experiences at college contribute to their labor market success. In particular, experiential learning has become a meaningful component of students' college experience for a smooth transition to the labor market.

As stated in the 2015 Opportunity Agenda of New York State, simply a degree or a certificate in a specific field without relevant experience to demonstrate work readiness to prospective employers is no longer sufficient for students to gain post-graduation employment. According to a 2014 national survey of employers (Hart Research Associates, 2015), large majorities emphasize the value of applied learning experiences at college to improve both the quality of learning and the quality of students' preparation for careers. When hiring recent college graduates, employers would be more likely to consider a job candidate who has participated in an internship/apprenticeship with company/organization, a senior project, a

[7] The author gratefully acknowledges the City University of New York (CUNY) for its support to facilitate the use of the restricted data in this study. The opinions expressed are those of the author and do not represent views of CUNY.

collaborative research project, a field-based project in diverse community with people from different background/culture, or a service-learning project with community organization.

While experiential learning can take different forms at college, one important type of experiential learning that colleges can provide to students is work-based courses. Work-based courses are courses students enroll in college that the core component is to prepare them with practical skills for the workforce, such as internship, practicum or fieldwork courses. Such courses can improve not only students' understanding of more theoretical knowledge gained from classroom but also the ability to apply the knowledge in a real-world setting. Furthermore, students can polish their communication and teamwork skills, and be more thoughtful about their lifelong career passions and their roles in the society through the courses.

Despite the potential of work-based courses to improve college learning and better prepare students for career success, rigorous and systematic research on work-based courses at college is very limited. Prior literature has analyzed the impact of working while enrolled on academic and post-school labor market outcomes more broadly. Results show mixed-to-negative impacts of student employment on academic outcomes (Scott-Clayton, 2011; Scott-Clayton & Minaya, 2016; Stinebrickner & Stinebrickner, 2003; Tyler, 2003), probably because working while enrolled could distract students from academic activities. On the other hand, research has documented positive relationships between student employment and post-school employment and earnings (Light, 2001; Scott-Clayton & Minaya, 2016). Compared to student employment in general, experiential learning through work-based courses would be more connected to and compatible with students' schoolwork, and be more accessible especially for students with no prior working experience.

This paper intends to examine patterns and post-graduation labor market outcomes of taking work-based courses at 2-year and 4-year colleges using rich administrative data from the City University of New York (CUNY). The paper applies a text mining technique to identify work-based courses from college transcript records, including internship, practicum, fieldwork, co-operative and service learning courses. The course-taking data are linked with students' college data and post-college labor market data. First, the paper studies patterns of taking work-based courses across time, degree areas and student characteristics. Also, the paper matches college graduates with similar majors and characteristics in order to compare and understand labor market outcomes associated with taking work-based courses at college. Three primary questions are addressed in this study:

1. What is the current landscape of work-based course-taking at 2-year and 4-year colleges?

2. Who takes work-based courses at college? Are there disparities in taking work-based courses by student characteristics or college-related factors?

3. How do post-graduation labor market outcomes of students taking work-based courses at college compare to those without?

Results show that 11% of enrollees and 31% of graduates for 2-year credentials, and 17% of enrollees and 29% of graduates for 4-year credentials take a work-based course in six years after college entry. Students typically earn credits from work-based courses, and take the courses in later years of their programs. Work-based course-taking is generally steady over time, but varies largely across fields of study. Apart from the field of study, college is another major predictor of taking work-based courses. While there is no significant racial disparity in work-based course-taking, female students are more likely than male students to take the courses.

Also, students who are younger at enrollment and US-born are generally more likely to take work-based courses than their counterparts. For labor market outcomes, results show a positive association between work-based course-taking at college and the probability of being employed after degree completion among both 2-year and 4-year degree completers. While the association between taking work-based courses at college and post-degree earnings is also positive for 4-year degree completers, results suggest a negative association between the two for 2-year degree completers.

The paper has major contributions to research and practice. First, as the value of experiential learning has been widely endorsed in higher education, the study provides concrete analyses of one important type of experiential learning at college – work-based courses. Evidence for a thorough understanding of work-based course-taking at college is essential for colleges to better design and offer these courses, and for policymakers to better support students on gaining from the courses. In addition, the study sheds light on how taking work-based courses at college prepares students for the transition to the labor market. Since the selection into taking work-based courses at college depends on a variety of non-random factors, especially for community college students, the study lacks a rigorous causal framework to examine the impact of taking work-based courses on post-graduation labor market outcomes. Nevertheless, the study controls for the effect of a wide range of observables, and the findings offer important insights to college practitioners in workforce development as well as future research to identify quasi-experimental opportunities for work-based courses. Last, the study introduces the usage of a text mining technique in analyzing messy transcript records, and contributes to the general education literature examining students' course-taking patterns from transcripts.

The remainder of the paper proceeds as follows: Section 2.2 provides background on experiential learning at college. Section 2.3 introduces data of the study and empirical strategy used to clean and analyze the data. Section 2.4 presents findings on patterns of work-based course-taking at college. And Section 2.5 discusses results on labor market outcomes associated with taking work-based courses at college. Section 2.6 concludes with implications for research and policy.

## 2.2 Experiential Learning at College

Experiential learning is rooted from long-established theories in human learning and development. In the philosophical ideology of Dewey (1938), education is conducted upon the basis of experience. The experiential learning theory by Kolb (1984) believes that learning comes from the transformation of experience through a cyclic process of experience, concept, reflection and action. That is, learning occurs when knowledge responds to personal and environmental demands arising from the cyclic process (Kayes, 2002). And when experience interacts with concepts that have been addressed in class, students comprehend the experience within a theoretical framework, leading to effective learning and personal development (Jarvis, 1987; Sweitzer & King, 2004). In addition, experiential learning empowers students with increased motivation as it brings about easily identifiable and trackable results (Perrin, 2014).

Ben-Porath (1967) included experiential training/work experience in his model of human capital investment. Both schoolwork and working while enrolled are factors in the production function of human capital. Given the concavity of human capital production, it would be optimal for students to combine school and work at a level that the marginal benefits of working while enrolled equal the marginal costs to academic performance and progress (Scott-Clayton, 2012). Empirical research has documented positive impacts of student employment on post-school labor

market outcomes. For example, Light (2001) estimated earnings function using data from the National Longitudinal Survey of Youth, and concluded that conventional models that do not control for in-school work experience overstate the wage effects of schooling, indicating the value of in-school work experience on post-school wages. Scott-Clayton and Minaya (2016) compared college students who participated in the Federal Work-Study program and their non-working counterparts, and found a positive effect of program participation on post-college employment. Several studies examined the Integrated Basic Education and Skills Training (I-BEST) program in Washington State, in which basic skills instructors and career-technical faculty jointly design and offer occupational courses for basic skills students at community and technical colleges. Result suggested that I-BEST students achieved better educational outcomes, including increased participation in college level courses, number of credits earned and certificates attainment (Jenkins, Zeidenberg, & Kienzl, 2009; Martinson et al., 2018; Wachen, Jenkins, & Van Noy, 2011). Furthermore, Le Barbanchon et al. (2020) leveraged controlled random variation in job offers through a Uruguayan program and found a 9% increase in earnings over the four post-program years for youth completing a program job. On the other hand, prior literature has also documented mixed-to-negative impacts of student employment on academic outcomes. For example, Stinebrickner and Stinebrickner (2003) used data from a college with a mandatory work-study program, and found a negative impact of working during the first semester at college on GPA. Scott-Clayton (2011) exploited institutional variation in access to Federal Work-Study funds, and found negative impacts of program participation on grade and graduation outcomes for women, but may not for men. Le Barbanchon et al. (2020) found that the Uruguayan program offering part-time employment while enrolled had positive effects on school enrollment and no significant reductions in studying time.

Moore (2010) discussed various forms of experiential learning programs in higher education. The most general term used to denote experiential learning at college is internship. Internship may refer to an independent activity outside a classroom where students work in a business, a social agency, or a cultural or governmental institution. Alternatively, internship may be an add-on to a classroom course, such as a practicum or field component conceptually related to the class. The missions of internship programs typically lie at linking theory and practice, personal and career development, as well as enhancing professional skills such as critical thinking, ethical behavior, and the ability to work with diverse people (Fedorko 2006; Inkster & Ross 1995; Moore, 2010; Sweitzer & King 2004). While internship may not always bear college credits, most credit-bearing programs incorporate a guided reflection component, such as written reports and journals. Other terms are also used to denote these programs, such as externship for short internship, or practicum/fieldwork for activities integrating classroom learning with real-world practice under the supervision of a faculty member or professionals in the field.

Another major form of experiential learning in higher education is service-learning. As discussed in Moore (2010), service learning combines out-of-class community service activities with the study of academic concepts and theories. It is often attached to a course in social sciences or related professions, and carries out the missions of both enhancing student learning and driving social changes (Butin, 2005). The third widespread form of experiential learning for college students discussed in Moore (2010) is cooperative education (commonly known as "co-op"). The co-op program is a structured job experience combining classroom-based education with practical work experience to build students' career skills and knowledge. Most co-op programs serve preprofessional students in engineering, business, and healthcare.

Experiential learning is valuable for career development of college students. First, students gain real-world work experience through experiential learning programs, and develop job skills that cannot be easily taught in a classroom. For example, employers find that students with experiential learning experience have stronger self-awareness and time management skills (Ghannadian, 2013). Also, experiential learning programs allow students to observe and interact with professionals in the workforce (Fussell Policastro, 2006). Students would be more reflective about their career goals, and may obtain networking opportunities or professional references that would facilitate the job search. On the other hand, students may complete experiential learning programs at the expense of academic learning. While students would have a short-term advantage in the job search with the experiential learning experience, Kijinski (2018) argued that they would be worse off in the long term because of the loss of important academic knowledge that cannot be always learned on the job.

This paper examines work-based courses that students enroll in college to gain experiential learning experience. Generally, such experience is closely related to the student's field of study, and is supervised by a faculty member at college. While it is interesting to explore the interaction of work-based courses and academic learning, the paper is focused on the career development aspect of taking work-based courses. The paper looks into labor market outcomes, including the probability of being employed and post-college earnings, associated with taking work-based courses at college.

## 2.3 Data & Empirical Strategy

The study utilizes rich administrative data from the City University of New York (CUNY) system. As the largest urban university system and one of the most diverse public postsecondary systems in the United States, CUNY provides an ideal setting for the study. The

data include demographic information and term-level academic records for CUNY enrollees, linked to quarterly earnings records from the New York State Department of Labor.

### 2.3.1 Identifying Work-Based Courses

Based on the categorical definition of experiential learning opportunities at CUNY (CUNY, 2016), I examine transcript records and identify work-based courses at CUNY under five broad categories: (1) Internship, externship, apprenticeship, or clerkship courses; (2) Practicum, placement, professional practice, or professional experience courses (including clinical practicum and student teaching courses); (3) Fieldwork, field experience, or field practice courses; (4) Co-operative (co-op) courses; (5) Service-learning courses.

Identifying work-based courses in transcript data is challenging. Course titles are the only identifier for courses in the data, but contain abbreviations and misspellings that are not consistent across years or colleges. Therefore, we would not be able to identify all the internship courses in the data if we just search for the word "internship" in course titles. Also, it would be extremely time-consuming to go through course titles one by one and manually identify all the variations of "internship" in the titles.

To overcome the challenge, the paper uses a text mining algorithm based on edit distance. Edit distance quantifies the similarity between two words by counting the minimum number of operations required to transform one word into the other. And the study relies on the Levenshtein distance, which allows for single-character edits (insertions, deletions or substitutions) to transform between the two words. For example, the edit distance between "internship" and "intnshp" is three because it takes deletions of three characters to transform from "internship" to "intnshp". Using the algorithm, I calculate the edit distance of every word in course titles with "internship", and record the minimum distance for each title. Starting from the lowest minimum

distance, I review corresponding words and flag those that are variations of "internship" until the words become irrelevant. This approach allows me to flag all the variations of "internship" in course titles and thus identify internship courses in the transcript data systematically. Apart from internship courses, I include all the words listed under the five categories of work-based courses at CUNY as keywords[8], and repeat the procedure for each keyword to identify other work-based courses in the data. Last, I manually review all the course titles flagged as work-based courses in the data to ensure the quality of the data cleaning procedure.

One limitation of the approach is that the identification of work-based courses is solely based on course titles. That is, I would not be able to identify courses with a work-based component that is not reflected in the course title. Most of such courses would be classroom-based but incorporate a work-based session related to the course during the semester. And the study is focused on courses that the main component is work-based so that it is captured in the course title.

### 2.3.2 Analytical Methods

To understand patterns of work-based course-taking at college, the analysis sample includes first-time college entry cohorts of 2004/05 to 2013/14 at CUNY's 2-year and 4-year institutions. The data track enrollment, course-taking and degree outcomes of each enrollee in the sample for six years after college entry. And the study examines patterns of work-based course-taking at college based on descriptive analyses.

To understand labor market outcomes associated with taking work-based courses at college, descriptive comparisons between work-based course takers and non-takers would be

[8] Specifically, I include the following work-based keywords: internship, externship, apprenticeship, clerkship, practicum, placement, professional practice/experience, clinical practice/practicum/experience, supervised/student/clinical/practicum/practice teaching, fieldwork, field work/experience/practice/practicum/study, cooperative, co-op, service learning, community service.

problematic due to factors that affect both work-based course-taking and labor market outcomes of interest. First, work-based course-taking may be correlated with degree completion or further education decisions (for example, transferring to a four-year institution among community college students, or pursuing a graduate school degree among senior college students). To control for the effect of a college degree or further education on labor market outcomes, the study restricts the sample to degree completers who were not enrolled in another program in the CUNY system after degree completion. However, I could not observe further enrollment outside the CUNY system from the data. For example, community college students who transferred to a four-year university outside the CUNY system would still be included in the sample. Specifically, the analysis sample to examine labor market outcomes includes first-time college entry cohorts of 2004/05-2008/09 who completed a college degree in six years after college entry and were not further enrolled after degree completion in the CUNY system. And the data track labor market outcomes of each graduate in the sample for five years after degree completion.

Even within the restricted sample, other factors can still introduce selection bias to simple descriptive comparisons of labor market outcomes between students taking work-based courses and those without. For example, if students in fields with better labor market chances are more likely to take work-based courses, the labor market outcomes of work-based course-takers compared to non-takers would also include the effect of differences in the field of study. In order to mitigate the selection bias through controlling for observable differences, the study employs the propensity score matching (PSM) method. I first match students by the field of study and other observable characteristics such as demographics, academic performance and employment status at college enrollment. To isolate the effect of work-based course-taking, I only compare labor market outcomes of matched students whose work-based course-taking status varies but

who have a similar propensity of taking these courses. Although the observables used for the matching may not control for all the factors that affect both the selection and the outcomes of taking work-based courses, the method would largely reduce the selection bias arising from these intrinsic differences in students because of the richness of the data.

The propensity of a student taking work-based courses at college is calculated using a logit model:

$$logit(WB_i) = \beta_0 + \beta_1 X_i + \mu_i \qquad (2.1)$$

where $WB_i$ indicates if student i takes work-based courses at college; $X_i$ is a vector of student variables, including gender, race, age at enrollment, instate-residency status, foreign-born status, zip-code level average household income level, degree intent at enrollment, full-time enrollment status in the first term, credits attempted/earned in the first term, GPA in the first term, employment status (employed, employed full-time, and quarterly earnings) in the first term after college entry, degree program area based on the 2-digit CIP code, college and cohort; and $\mu_i$ is the error term.

The matching uses a caliper of 0.05, with replacement and excluding observations that violate the overlap assumption. To compare labor market outcomes of work-based course-takers and non-takers in the matched students, I run the following ordinary least squares regression:

$$Y_i = \alpha_0 + \alpha_1 WB_i + \alpha_2 X_i + \omega_i \qquad (2.2)$$

where $Y_i$ is the labor market outcome for student i; $WB_i$ is a dummy variable that equals to 1 if student i takes work-based courses at college; $X_i$ is the vector of student variables as specified in Eq. 2.1; and $\omega_i$ is the error term. The analysis is executed using "teffects psmatch" command in STATA.

Under the assumption that the variables used for the matching can control for all the factors that affect the selection and the outcomes of work-based course-taking, the coefficient $\alpha_1$ estimates the average treatment on the treated (ATT) effect of taking work-based courses at college on labor market outcomes of interest. Under the current study context, however, it is challenging to fully satisfy the assumption. First, as noted, the analysis could not control for further enrollment outside the CUNY system. If the transfer rate from a 2-year college within the CUNY system to a 4-year university outside the CUNY system is inherently different between work-based course-takers and non-takers, the assumption would be violated because the short-term post-graduation labor market outcomes of transfer students would be naturally worse than students who fully enter the labor market after the 2-year completion. Also, as the selection into taking work-based courses can be based on a large range of non-random factors, it is challenging to control for all. For example, students are matched by the 2-digit CIP code of the degree program area, which is not the most detailed program categorization in order to allow for adequate samples for the comparison. That is, the assumption may be violated if there are program-specific differences in both the requirement for work-based courses and the labor market return to the degree within a 2-digit CIP area. Therefore, while the PSM model would control for observable differences, a rigorous causal inference could not be guaranteed and results should be interpreted with caution.

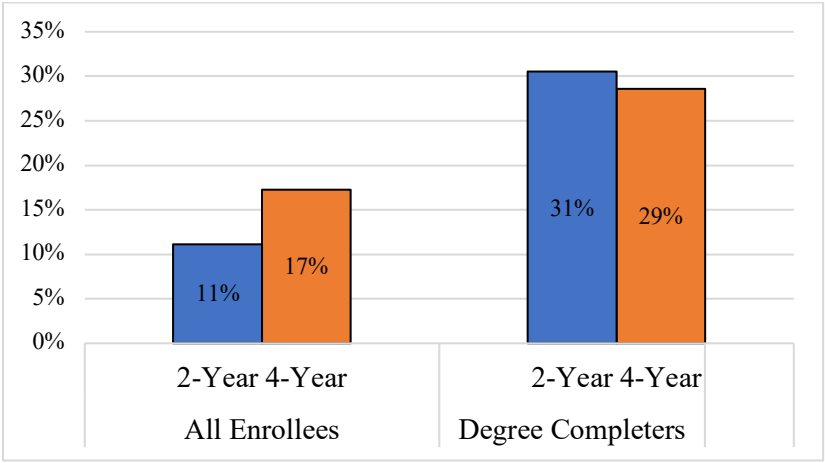
## 2.4 Patterns of Work-Based Course-Taking at College

In Panel A of Figure 2.1, bars on the left show that 11% of 2-year enrollees and 17% of 4-year enrollees in the analysis sample took a work-based course in six years after college entry. An analysis of course credits indicates that the majority of students earn credits from taking work-based courses. Among work-based course-takers in the sample, 94% of 2-year students and

98% of 4-year students only took credit-bearing work-based courses. On average, the 2-year students earned 3.6 credits and the 4-year students earned 4.4 credits from work-based courses in six years after enrollment.

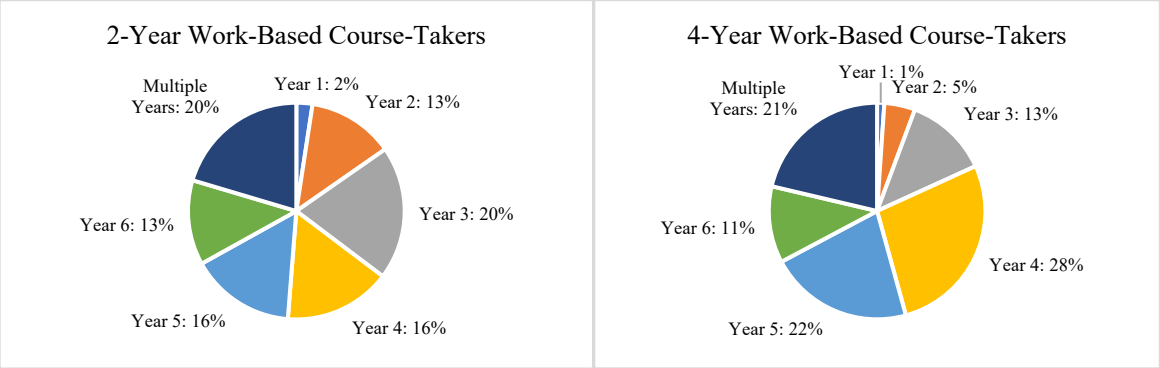
Panel B of Figure 2.1 shows the composition of students taking work-based courses in the analysis sample by the year relative to college entry when the course was taken. Students are generally far along in their programs when they take work-based courses. For example, among 2-year enrollees in the sample who took a work-based course in six years after college entry, only 2% took the course in the first year. Similarly, among 4-year work-based course-takers in the sample, only 1% took the course in the first year and 5% took the course in the second year after enrollment.

(A) Percentage of students taking work-based courses at 2-year and 4-year colleges




Note: Left bars of the figure show percentages of 2-year and 4-year enrollees in the analysis sample who took a work-based course in six years after college entry. Right bars of the figure show percentages of 2-year and 4-year degree completers in the analysis sample who took a work-based course in six years after college entry.

(B) Composition of work-based course takers by the year relative to college entry when taking the work-based course




Note: The figures show compositions of work-based course takers among 2-year and 4-year enrollees in the analysis sample by the year relative to college entry when the work-based course was taken.

**Figure 2.1: Percentage and Composition of Students Taking Work-Based Courses at College**

As students take work-based courses in later years of their programs, the proportion of students who take work-based courses would be higher if the sample is limited to those who progress to later years. In Panel A of Figure 2.1, bars on the right limit the analysis sample to students who completed a college degree in six years after college entry. And 31% of 2-year degree completers and 29% of 4-year degree completers in the sample took a work-based course in six years after enrollment.

Figure 2.2 illustrates trends of work-based course-taking among 2-year and 4-year enrollees over time. That is, the figure depicts percentages of enrollees in the analysis sample seeking 2-year and 4-year credentials who took a work-based course in six years after college entry in each entry cohort. Results only show a slight downward trend among 2-year enrollees, and no significant variation of work-based course-taking over time among 4-year enrollees. Overall, the trend has been steady at around 11% among 2-year enrollees and 17% among 4-year enrollees in the sample.

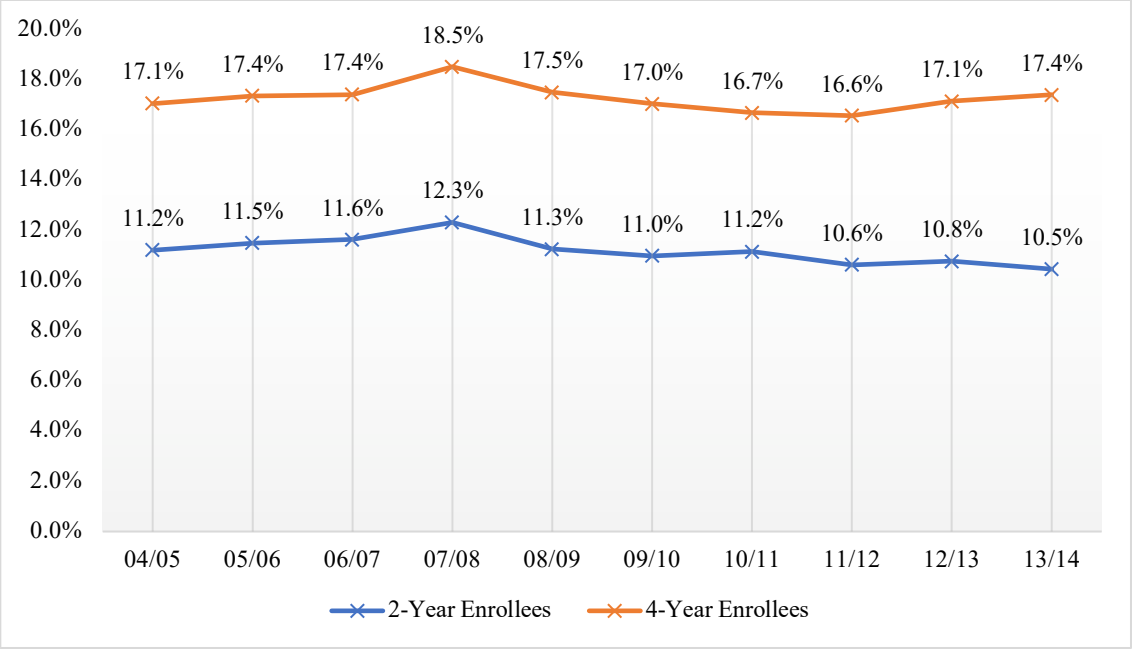



**Figure 2.2: Trends of Work-Based Course-Taking Over Time**

Note: The figure shows percentages of 2-year and 4-year enrollees in the analysis sample who took a work-based course in six years after college entry by entry cohort.

In contrast, work-based course-taking varies largely across fields of study. Figure 2.3 shows percentages of degree completers in the analysis sample who took a work-based course in six years after college entry by degree fields. The analysis is focused on students who completed a college degree in six years after college entry for two reasons: First, the field of study is not available for all the enrollees in the sample because students may not declare a major in earlier years of study; however, the field information is complete and accurate among degree completers in the degree data. Second, as students tend to take work-based courses in later years of their programs, limiting the analysis sample to degree completers would eliminate potential bias arising from different retention rates across fields of study.

Among 2-year degree completers in the sample, students in Education, Law, and Social & Behavioral Sciences are most likely to take a work-based course at college, followed by

students in Business and Trades & Repair Technicians. Among 4-year degree completers in the sample, 96% of students in Education took a work-based course within six years after college entry, indicating that the work-based course could be a degree completion requirement for a Bachelor's degree in Education. Bachelor's degree completers in Arts & Humanities, Health and Law are also more likely than students in other fields to take a work-based course. Among both 2-year and 4-year degree completers, students in Engineering and Natural Sciences & Math are relatively less likely to take a work-based course at college.

It is interesting that while 80% of 2-year degree completers in Social & Behavioral Sciences took a work-based course in six years after college entry, the work-based course-taking rate is only 27% among 4-year degree completers in this field area. The disparity may be due to differences in major distribution within the degree field area – the majority of 2-year students in Social & Behavioral Sciences completed the degree in Community Organization and Advocacy or Human Services, while the majority of 4-year students in the field area completed the degree in Psychology and Economics. Therefore, not only does the work-based course-taking rate vary across degree field areas, it may also vary across specific majors within a field area. In addition, not a high proportion of students in Health, especially among 2-year degree completers, took work-based courses in the sample. However, this does not imply that students in Health gain little experiential learning experience at college because the clinical requirement for Health programs is not always course-based or reflected in the transcript data. This study is limited to experiential learning experience that can be examined through transcript records only, but would be valuable to be expanded to a broader coverage when other data sources are available.

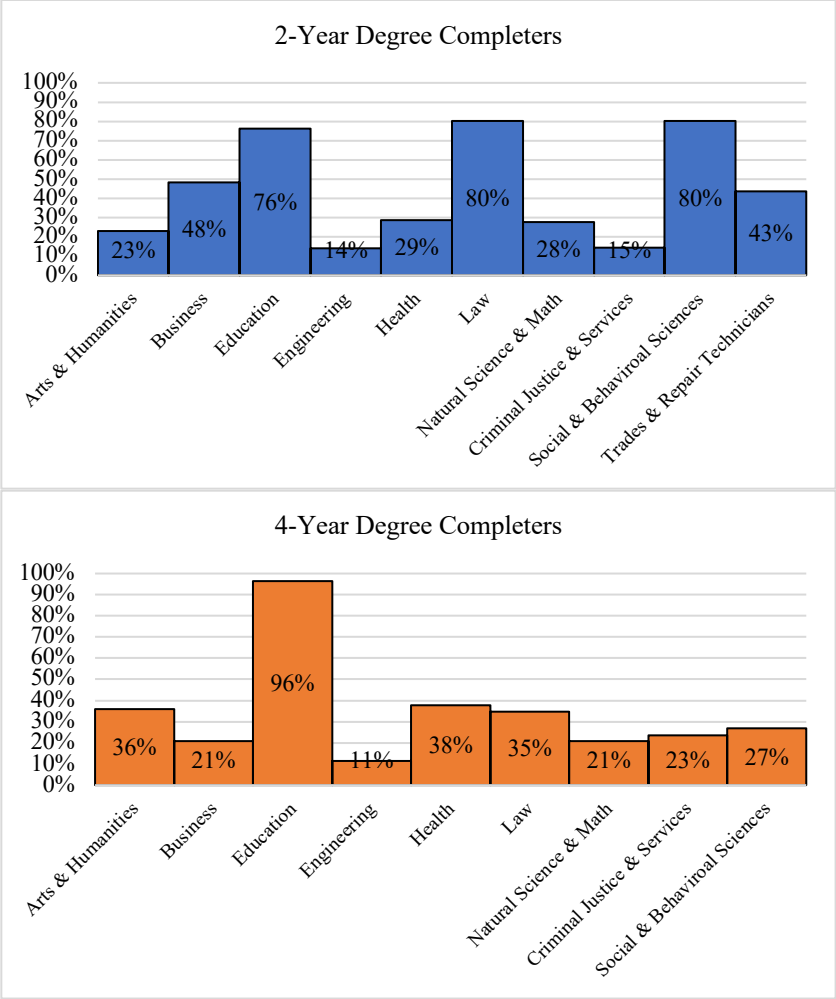
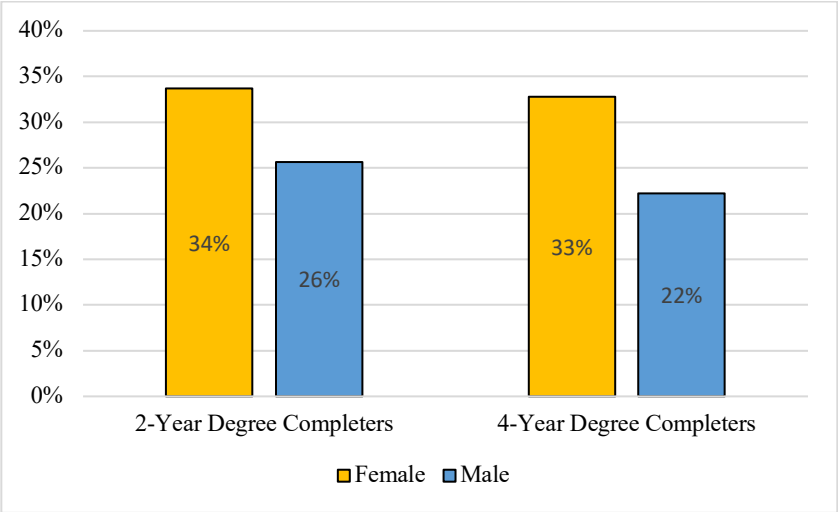



**Figure 2.3: Work-Based Course-Taking by Field of Study**

Note: The figures show percentages of 2-year and 4-year degree completers in the analysis sample who took a work-based course in six years after college entry by degree field area.

(A) Work-based course-taking by gender




(B) Work-based course-taking by race/ethnicity

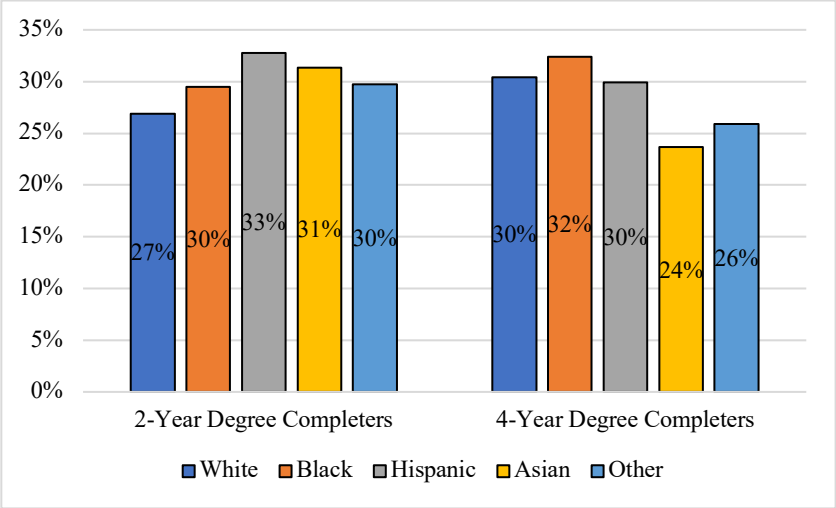



**Figure 2.4: Work-Based Course-Taking Across Gender and Racial Groups**

Note: The figures show percentages of 2-year and 4-year degree completers in the analysis sample who took a work-based course in six years after college entry by gender and race/ethnicity.

Last, I examine work-based course-taking across gender and racial groups. Similar to the analysis by field area, I focus on degree completers in the sample to eliminate potential bias due to different retention rates across student groups. Panel A of Figure 2.4 shows that female students are more likely than male students to take work-based courses at both 2-year and 4-year

institutions. Among 2-year degree completers in the sample, 34% of female students and 26% of male students took a work-based course within six years after college entry. Among 4-year degree completers in the sample, the work-based course-taking rate was 33% for female students and 22% for male students. The gender disparity in work-based course-taking may be at least partially explained by major differences across gender groups. Panel B of Figure 2.4 shows that there are no significant racial disparities in work-based course-taking at both 2-year and 4-year colleges. In particular, the rate of work-based course-taking among minority students is high, implying that work-based courses offer important experiential learning opportunities for underserved minority students, and may thus serve as an equity tool in both higher education and post-college workforce.

## 2.5 Labor Market Outcomes of Work-Based Course-Taking at College

### 2.5.1 Characteristics of Work-Based Course Takers

Table 2.1 presents summary statistics of the analysis sample to examine labor market outcomes associated with taking work-based courses at college. As discussed in Section 3, the sample includes first-time college entry cohorts of 2004/05-2008/09 who completed a college degree in six years after enrollment and were not enrolled after the degree completion in the data. The first column represents the overall sample, while the subsequent columns provide the same statistics for work-based course takers and non-takers separately. Work-based course takers are students in the sample who took a work-based course in six years after college entry, and non-takers are those who did not take work-based courses at college.

Overall, work-based course takers are more likely to be female than non-takers for both the 2-year and the 4-year samples. Among the 2-year sample, work-based course takers are also more likely to be Hispanic, enrolled full-time and not employed in the first term of study.

## Table 2.1: Summary Statistics

| Variable | 2-Year Degree Completers | | | 4-Year Degree Completers | | |
|---|---|---|---|---|---|---|
| | Total | Work-Based Course Takers | Non-Takers | Total | Work-Based Course Takers | Non-Takers |
| **Student Characteristics:** | | | | | | |
| Female | 60% | 66% | 57% | 59% | 66% | 56% |
| White | 22% | 18% | 24% | 33% | 35% | 32% |
| Black | 29% | 26% | 31% | 17% | 20% | 16% |
| Hispanic | 32% | 39% | 29% | 23% | 23% | 23% |
| Asian | 17% | 17% | 17% | 27% | 23% | 29% |
| Instate residency | 98% | 97% | 98% | 98% | 98% | 98% |
| Age at enrollment | 22 | 22 | 22 | 19 | 19 | 19 |
| Age at degree completion | 26 | 26 | 25 | 23 | 23 | 23 |
| Zip-code level average household income | 61955 | 60379 | 62636 | 70268 | 70785 | 70094 |
| US Born | 32% | 33% | 32% | 36% | 37% | 36% |
| Foreign Born | 35% | 41% | 32% | 28% | 25% | 29% |
| Birthplace Missing | 33% | 27% | 36% | 35% | 38% | 35% |
| **Academic and Labor Market Outcomes:** | | | | | | |
| Full-time enrollment in first term | 83% | 86% | 81% | 88% | 86% | 88% |
| Credits attempted in first term | 8.0 | 7.5 | 8.2 | 11.6 | 11.4 | 11.7 |
| Credits earned in first term | 7.4 | 6.9 | 7.6 | 11.2 | 11.0 | 11.3 |
| GPA in first term | 2.5 | 2.5 | 2.5 | 2.8 | 2.8 | 2.8 |
| Cumulative credits earned in last term | 70.0 | 70.2 | 69.9 | 127.2 | 127.5 | 127.1 |
| Cumulative GPA in last term | 2.9 | 3.0 | 2.9 | 3.1 | 3.2 | 3.1 |
| Employed in first term | 40% | 37% | 41% | 35% | 37% | 35% |
| Employed full-time in first term (Quarterly earning >= 4000) | 12% | 11% | 12% | 4% | 4% | 4% |
| Earning in first term (zeros excluded) | 3652 | 3462 | 3726 | 2179 | 2166 | 2184 |
| Post-Degree: | | | | | | |
| Employed, Quarter 1 | 52% | 51% | 53% | 58% | 62% | 57% |
| Employed, Quarter 2 | 54% | 54% | 54% | 61% | 65% | 60% |
| Employed, Quarter 3 | 55% | 55% | 56% | 62% | 67% | 61% |
| Employed, Quarter 4 | 57% | 57% | 57% | 64% | 68% | 62% |
| Employed, Year 1 | 67% | 67% | 67% | 74% | 78% | 72% |
| Employed, Year 2 | 66% | 65% | 66% | 73% | 77% | 72% |
| Employed, Year 3 | 66% | 65% | 66% | 73% | 76% | 71% |
| Employed, Year 4 | 66% | 65% | 66% | 72% | 75% | 71% |
| Employed, Year 5 | 65% | 63% | 66% | 73% | 75% | 72% |
| Earning, Quarter 1 (zero excluded) | 5039 | 4895 | 5100 | 5372 | 5183 | 5441 |
| Earning, Quarter 2 (zero excluded) | 5782 | 5594 | 5864 | 6417 | 6441 | 6408 |
| Earning, Quarter 3 (zero excluded) | 6232 | 5877 | 6386 | 6889 | 6926 | 6875 |
| Earning, Quarter 4 (zero excluded) | 6832 | 6301 | 7065 | 7505 | 7594 | 7472 |
| Earning, Year 1 (zero excluded) | 19661 | 18580 | 20131 | 21789 | 22000 | 21712 |
| Earning, Year 2 (zero excluded) | 25803 | 23639 | 26737 | 29988 | 30613 | 29760 |
| Earning, Year 3 (zero excluded) | 29934 | 27032 | 31174 | 35973 | 36485 | 35788 |
| Earning, Year 4 (zero excluded) | 33899 | 30214 | 35460 | 41809 | 42311 | 41630 |
| Earning, Year 5 (zero excluded) | 38287 | 33852 | 40134 | 47819 | 47294 | 48003 |
| **Observations (N)** | 9,164 | 2,779 | 6,385 | 26,401 | 6,664 | 19,737 |

Note: The analysis sample includes first-time college entry cohorts of 2004/05-2008/09 who completed a college degree in six years after enrollment and who were not enrolled after the degree completion in the state administrative data. 2-year degree completers include observations who completed a certificate or an Associate's degree, and 4-year degree completers include observations who completed a Bachelor's degree. Work-based course takers include observations who took any work-based courses in six years after college entry. And non-takers include observations who did not take work-based courses at college. Earnings are adjusted by CPI to 2015 dollar.

Differences in other student characteristics are not significant between the two groups. On average, work-based course takers are slightly less likely to be employed after graduation and have lower post-degree earnings than non-takers among 2-year degree completers in the sample. Among 4-year degree completers in the sample, however, both the probability of employment and post-degree earnings are higher for work-based course takers than non-takers.

In order to mitigate selection bias in the comparison of labor market outcomes between the two groups, I first examine selection into work-based course-taking at college. Appendix Table B1 shows levels of R-squared running OLS regressions with the dummy of taking work-based courses in six years after college entry as the dependent variable and different sets of independent variables. Specifically, I gradually include individual characteristics, ability/preference at enrollment, college and field of study[9] as independent variables, and changes in R-squared indicate the explanatory power of the independent variables on work-based course-taking. As Appendix Table B1 shows, the major predictors of taking work-based courses are college and field of study.

Table 2.2 displays estimates from a logit regression on taking work-based courses at college as specified in Eq. 2.1. Controlling for the effects of college and field of study, female students are still more likely to take work-based courses than male students among both the 2-year and the 4-year samples. Across racial groups, there is no significant racial disparity in work-based course-taking among the 2-year sample, while White and Black students are more likely to take work-based courses than Hispanic and Asian students among the 4-year sample.

[9] Field of study in Section 2.5 is based on 2-digit CIP codes of degree majors.

## Table 2.2: Selection into Work-Based Course-Taking

| | 2-Year Degree Completers | | 4-Year Degree Completers | |
|---|---|---|---|---|
| **Variable** | B | S.E. | B | S.E. |
| **Student Characteristics:** | | | | |
| Female | 0.298 | (0.066)*** | 0.400 | (0.035)*** |
| Black | 0.046 | (0.090) | 0.061 | (0.053) |
| Hispanic | 0.027 | (0.093) | -0.129 | (0.050)*** |
| Asian | -0.107 | (0.103) | -0.237 | (0.046)*** |
| Other race/ethnicity | 0.726 | (0.451) | -0.027 | (0.418) |
| Instate residency | -0.194 | (0.292) | 0.059 | (0.205) |
| Age at enrollment | -0.094 | (0.032)*** | -0.052 | (0.026)** |
| Age at enrollment squared | 0.001 | (0.001)** | 0.001 | (0.000)* |
| Zip-code level average household income | 0.000 | (0.000) | 0.000 | (0.000)*** |
| Foreign born | -0.196 | (0.077)** | -0.102 | (0.044)** |
| Missing birth place | -0.131 | (0.076)* | 0.056 | (0.039) |
| Degree Intent at enrollment: Bachelor's Degree | -0.064 | (0.129) | | - |
| Full-time enrollment in first term | 0.240 | (0.094)** | -0.053 | (0.090) |
| Credits attempted in first term | -0.041 | (0.022)* | 0.033 | (0.015)** |
| Credits earned in first term | 0.016 | (0.022) | -0.038 | (0.014)*** |
| GPA in first term | -0.044 | (0.046) | 0.198 | (0.032)*** |
| Missing first term GPA | -0.114 | (0.172) | 0.537 | (0.140)*** |
| Employed in first term | 0.004 | (0.079) | 0.092 | (0.045)** |
| Employed full-time in first term | 0.034 | (0.171) | -0.039 | (0.123) |
| Quarterly earning in first term | -0.000 | (0.000) | -0.000 | (0.000) |
| **Degree Program Area:** | | | | |
| Natural Resources and Conservation (03) | | - | 1.009 | (0.360)*** |
| Agriculture and Related Science (04) | | - | -3.552 | (0.332)*** |
| Area, Ethnic, Cultural, and Gender Studies (05) | | - | 0.588 | (0.248)** |
| Communications, Journalism, and Related Programs (09) | -0.108 | (0.443) | 1.616 | (0.171)*** |
| Communications Technologies/Technicians and Support Services (10) | 2.215 | (0.274)*** | 2.598 | (0.812)*** |
| Computer and Information Sciences and Support Services (11) | 1.436 | (0.166)*** | -0.434 | (0.192)** |
| Personal and Culinary Services (12) | 2.024 | (1.947) | | - |
| Education (13) | 1.945 | (0.180)*** | 3.785 | (0.230)*** |
| Engineering (14) | -3.078 | (1.054)*** | -3.006 | (0.444)*** |
| Engineering Technologies/Technicians (15) | 1.725 | (0.215)*** | -3.975 | (0.393)*** |
| Foreign languages, Literatures, and Linguistics (16) | | - | -0.087 | (0.241) |
| Family and Consumer Sciences/Human Services (19) | | - | 1.352 | (0.256)*** |
| Legal Professions and Studies (22) | 3.223 | (0.574)*** | -0.948 | (0.237)*** |
| English Language and Literature/Letters (23) | -1.605 | (0.675)** | -0.294 | (0.175)* |
| Liberal Arts and Sciences, General Studies and Humanities (24) | -0.304 | (0.123)** | -0.198 | (0.204) |
| Biological and Biomedical Sciences (26) | -1.357 | (0.600)** | -2.072 | (0.225)*** |
| Mathematics and Statistics (27) | | - | -0.039 | (0.224) |
| Multi/Interdisciplinary Studies (30) | | - | 0.540 | (0.286)* |
| Parks, Recreation, Leisure, and Fitness Studies (31) | | - | 5.783 | (1.032)*** |
| Philosophy and Religious Studies (38) | | - | -0.596 | (0.265)** |
| Physical Sciences (40) | 0.636 | (0.207)*** | -1.272 | (0.243)*** |
| Psychology (42) | -0.032 | (1.258) | -0.639 | (0.165)*** |
| Security and Protective Services (43) | -3.172 | (0.696)*** | -0.630 | (0.178)*** |
| Public Administration and Social Service Professions (44) | 4.320 | (0.349)*** | 3.324 | (0.301)*** |
| Social Sciences (45) | | - | -0.140 | (0.162) |
| Mechanic and Repair Technologies/Technicians (47) | 1.006 | (0.215)*** | | - |
| Transportation and Materials Moving (49) | | - | 0.601 | (0.374) |
| Visual and Performing Arts (50) | 1.127 | (0.173)*** | 0.076 | (0.177) |
| Health Professions and Related Clinical Sciences (51) | 0.917 | (0.139)*** | 0.410 | (0.174)** |
| Business, Management, Marketing and Related Support Services (52) | 1.777 | (0.128)*** | -0.562 | (0.163)*** |
| History (54) | | - | 0.149 | (0.200) |
| **Observations (N)** | 8,955 | | 25,957 | |

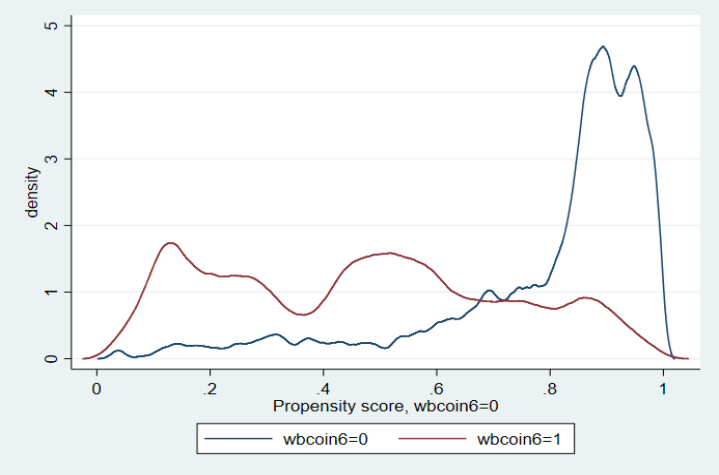
Note: The analysis sample includes first-time college entry cohorts of 2004/05-2008/09 who completed a college degree in six years after enrollment and who were not enrolled after the degree completion in the state administrative data. 2-year degree completers include observations who completed a certificate or an Associate's degree, and 4-year degree completers include observations who completed a Bachelor's degree. The analysis runs a logit model and the dependent variable is if the student took a work-based course in six years after college entry. The model also controls for college and cohort. ***p<0.01, **p<0.05, *p<0.1.

In addition, students who are younger at enrollment and US-born are generally more likely to take work-based courses than their counterparts. Among the 2-year sample, students who enrolled full-time in the first term are also more likely to take work-based courses than their part-time counterparts. Students who enrolled part-time in the first term at community colleges or who are older at enrollment in general are more likely to be working students who already have a full-time job when they first enroll at college. This may explain why they are less likely to take work-based courses at college than their counterparts. Foreign-born students are more likely to be non-US citizens, and face legal restrictions to work in the country. This may be a potential reason that they are less likely to take work-based courses than US-born students. Furthermore, after controlling for other student characteristics, had a job in the first term of study appears not to be a strong predictor of taking work-based courses among the 2-year sample, but is a positive predictor among the 4-year sample.

### 2.5.2 Common Support and Match Quality

To ensure the validity of PSM, Figure 2.5 provides evidence of common support. The common support assumption requires sufficient overlap across treatment and control groups to find adequate matches for a range of propensity scores. Figure 2.5 displays the estimated density of the predicted probabilities that work-based course takers (treatment group) and non-takers (control group) in the sample would not take work-based courses at college. Among both the 2-year and the 4-year samples, neither plot indicates too much probability mass near 0 or 1, and the estimated densities show a broad range of overlap between treatment and control groups. The density plots indicate there is no evidence that the common support assumption is violated (Busso, DiNardo, and McMrary, 2011).

(A) Common support check for 2-year degree completers




(B) Common support check for 4-year degree completers

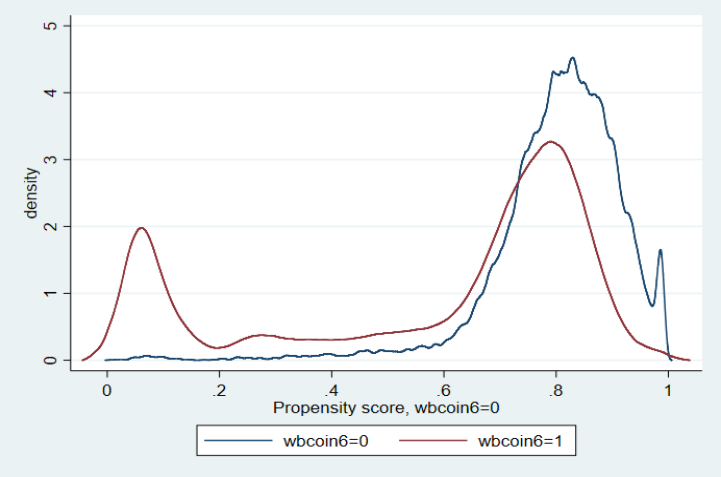


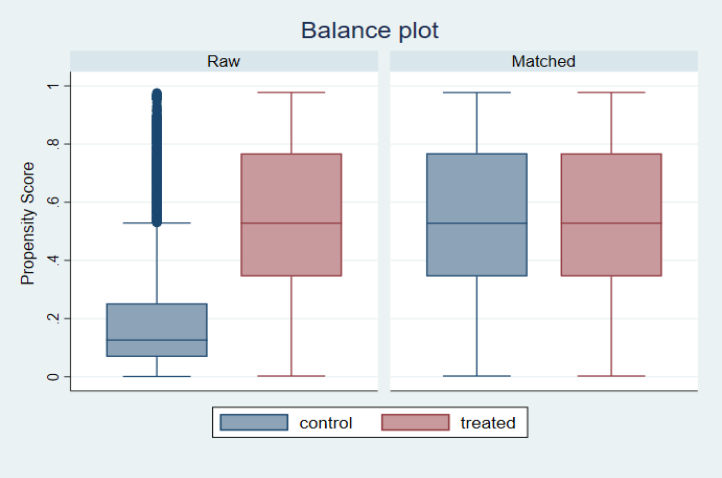

**Figure 2.5: Common Support Checks**

Note: The figures show the estimated density of the predicted probabilities that work-based course takers (wbcoin6=1) and non-takers (wbcoin6=0) in the analysis sample are assigned to not taking work-based courses.

In addition, I check the match quality by measuring the balance of the covariates across treatment and control groups in the study. Figure 2.6 displays balance plots of estimated propensity scores before and after the matching between work-based course takers and non-takers. As the raw plots show, there are large differences across the two groups prior to the matching. The differences suggest correlations between work-based course-taking and the

covariates, which lends credence to the concern that selection bias may confound findings from simple descriptive comparisons between work-based course takers and non-takers. And the box plots after the matching indicate that the matching has balanced the estimated propensity scores.

(A) Balance plot for 2-year degree completers




(B) Balance plot for 4-year degree completers

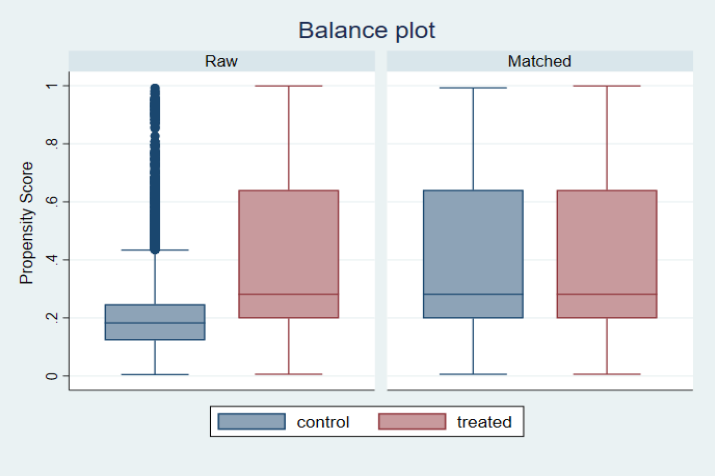



**Figure 2.6: Balance Plots**

Note: The figures show the balance of estimated propensity scores between work-based course takers (treated) and non-takers (control) in the analysis sample before and after the matching.

### 2.5.3 Main Results

Table 2.3 presents the main PSM results of post-degree labor market outcomes associated with taking work-based courses at college. Estimations are run separately among 2-year degree completers and 4-year degree completers in the sample. Panel A presents results on first-year post-degree outcomes, including the probability of being employed and earnings in each quarter of the first year after degree completion as well as the first year overall. And Panel B presents results on long-term post-degree outcomes, tracking students' employment and earning outcomes by year up to the fifth post-degree year.

**Table 2.3: Labor Market Outcomes of Taking Work-Based Courses at College**

| | 2-Year Degree Completers | | | 4-Year Degree Completers | | |
|---|---|---|---|---|---|---|
| **Outcome** | B | S.E. | Baseline | B | S.E. | Baseline |
| **A. First-year post-degree outcomes** | | | | | | |
| Probability of employment, Quarter 1 | 0.007 | (0.021) | 0.531 | 0.041 | (0.019)** | 0.572 |
| Probability of employment, Quarter 2 | 0.022 | (0.020) | 0.543 | 0.027 | (0.017) | 0.599 |
| Probability of employment, Quarter 3 | 0.022 | (0.021) | 0.558 | 0.035 | (0.017)** | 0.610 |
| Probability of employment, Quarter 4 | 0.029 | (0.020) | 0.570 | 0.043 | (0.015)*** | 0.625 |
| Earning, Quarter 1 | -193.028 | (175.142) | 2709.397 | -58.461 | (158.829) | 3103.196 |
| Earning, Quarter 2 | -241.284 | (183.577) | 3172.31 | 163.979 | (187.886) | 3831.425 |
| Earning, Quarter 3 | -279.196 | (181.855) | 3558.339 | 235.834 | (172.513) | 4188.687 |
| Earning, Quarter 4 | -378.460 | (204.840)* | 4019.812 | 290.490 | (166.000)* | 4662.968 |
| Probability of employment, Year 1 | 0.043 | (0.020)** | 0.669 | 0.044 | (0.016)*** | 0.728 |
| Earning, Year 1 | -1,091.967 | (671.507) | 13459.86 | 631.842 | (614.100) | 15786.28 |
| **B. Long-term post-degree outcomes** | | | | | | |
| Probability of employment, Year 2 | 0.033 | (0.021) | 0.665 | 0.040 | (0.014)*** | 0.720 |
| Probability of employment, Year 3 | 0.027 | (0.020) | 0.663 | 0.023 | (0.012)** | 0.718 |
| Probability of employment, Year 4 | 0.022 | (0.020) | 0.669 | 0.009 | (0.011) | 0.717 |
| Probability of employment, Year 5 | 0.018 | (0.020) | 0.666 | -0.016 | (0.010) | 0.722 |
| Earning, Year 2 | -1,758.193 | (833.730)** | 17769.57 | 2,081.044 | (744.369)*** | 21410.62 |
| Earning, Year 3 | -1,738.456 | (919.829)* | 20675.41 | 1,522.745 | (823.681)* | 25712.89 |
| Earning, Year 4 | -2,143.508 | (961.910)** | 23746.24 | 775.737 | (891.377) | 29856.61 |
| Earning, Year 5 | -2,319.445 | (1,024.628)** | 26701.77 | -328.593 | (928.819) | 34624.1 |
| **Observations (N)** | 8,955 | | | 25,957 | | |

Note: The table shows PSM results of post-degree labor market outcomes associated with taking work-based courses at college. The analysis sample includes first-time college entry cohorts of 2004/05-2008/09 who completed a college degree in six years after enrollment and who were not enrolled after the degree completion in the state administrative data. 2-year degree completers include observations who completed a certificate or an Associate's degree, and 4-year degree completers include observations who completed a Bachelor's degree. The analysis uses "teffects psmatch" command in STATA, with a logit model to estimate the propensity score, caliper of 0.05, with replacement and excluding observations that violate the overlap assumption, to execute the matching and the estimation. Earnings are adjusted by CPI to 2015 dollar; missing earning data are treated as unemployed quarters and are replaced with zeros. Baseline columns show outcomes among the control group who did not take work-based courses at college. ***p<0.01, **p<0.05, *p<0.1.

Among 2-year degree completers, taking work-based courses at college is associated with a higher employment rate during the first year after degree completion. Specifically, the probability of being employed in the first post-degree year is 4.3 percentage points higher for work-based course takers than non-takers from a baseline rate of 66.9%. Coefficients on the employment rate in each quarter of the first year are all positive, but not at a statistically significant level. However, taking work-based courses at college is not associated with higher post-degree earnings during the first year after graduation among 2-year degree completers. Coefficients on the first-year earning outcomes are mostly not statistically significant and with negative values, suggesting null to negative associations between taking work-based courses and the first-year post-degree earning outcomes. In the long term, coefficients on the employment outcomes from the second to the fifth post-degree year among the 2-year sample are positive but not statistically significant, and coefficients on the earning outcomes for the years are negative. Results suggest that while there is no significant difference in the probability of being employed between work-based course takers and non-takers among 2-year degree completers, students taking work-based courses at college earn less than their counterparts in the long run.

Among 4-year degree completers, results in Table 2.3 also show a positive association between taking work-based courses at college and the probability of being employed during the first post-degree year. Specifically, the first-year post-degree employment rate among work-based course takers is 4.4 percentage points higher than their counterparts from a baseline rate of 72.8%. Coefficients on the quarterly employment outcomes for the first post-degree year are also positive and mostly statistically significant, ranging from a size of 2.7-4.3 percentage points. Furthermore, the positive association between taking work-based courses at college and the post-degree employment rate lasts up to three years after graduation among 4-year degree completers.

As Panel B of Table 2.3 for the 4-year sample shows, the employment rates are 4 percentage points higher in the second year and 2.3 percentage points higher in the third year after degree completion for work-based course takers than non-takers. Last, coefficients on earning outcomes for the 4$^{th}$ quarter of the first post-degree year, the second and the third post-degree years are positive and statistically significant for the 4-year sample, suggesting a positive association between taking work-based courses at college and post-degree earning outcomes up to three years after graduation among 4-year degree completers.

### 2.5.4 Alternative Sample

In order to check the robustness of the results, as well as to further mitigate any confounding effects of the field of study, I run the PSM estimation with an alternative sample dropping students in fields with a work-based course-taking rate higher than 80% from the main analysis sample. That is, the alternative sample includes first-time college entry cohorts of 2004/05-2008/09 who completed a college degree in six years after enrollment, were not enrolled after the degree completion, and were in a field (based on the 2-digit CIP code) with less than 80% of the sample taking work-based courses.

Table 2.4 shows that results on post-degree labor market outcomes using the alternative sample are in general consistent with the main results. Among the 2-year alternative sample, the association between taking work-based courses at college and the probability of being employed after degree completion is positive and more significant than the main results. Specifically, the probability of being employed in the first post-degree year for work-based course takers is 5.3 percentage points higher than non-takers. The employment rates for each quarter of the first year are also statistically significantly higher for work-based course takers than non-takers, at a size of 3.5 to 5.6 percentage point. Furthermore, the positive association between taking work-based

courses at college and the probability of being employed lasts through the fourth post-degree year: The employment rate for work-based course takers is 4.6 percentage points higher in the second year, 3.5 percentage points higher in the third year, and 4 percentage points higher in the fourth year after degree completion than non-takers. Also, consistent with the main results for the 2-year sample, there is no significant association between taking work-based courses at college and post-degree earnings in the first year. And the associations with post-degree earnings in the second and the third years are negative, at a statistically significant level similar to the main results.

**Table 2.4: Labor Market Outcomes of Taking Work-Based Courses at College, Excluding Majors with High Work-Based Course-Taking Rate**

| | 2-Year Degree Completers | | | 4-Year Degree Completers | | |
|---|---|---|---|---|---|---|
| **Outcome** | B | S.E. | Baseline | B | S.E. | Baseline |
| **A. First-year post-degree outcomes** | | | | | | |
| Probability of employment, Quarter 1 | 0.042 | (0.020)** | 0.531 | 0.022 | (0.017) | 0.568 |
| Probability of employment, Quarter 2 | 0.048 | (0.020)** | 0.543 | 0.017 | (0.014) | 0.595 |
| Probability of employment, Quarter 3 | 0.035 | (0.019)* | 0.558 | 0.025 | (0.013)* | 0.607 |
| Probability of employment, Quarter 4 | 0.056 | (0.020)*** | 0.570 | 0.037 | (0.015)** | 0.621 |
| Earning, Quarter 1 | 140.180 | (135.594) | 2712.017 | -221.841 | (192.228) | 3092.034 |
| Earning, Quarter 2 | 29.402 | (160.396) | 3173.707 | 8.223 | (180.076) | 3812.312 |
| Earning, Quarter 3 | -163.371 | (194.993) | 3561.522 | 113.861 | (184.345) | 4169.824 |
| Earning, Quarter 4 | -214.391 | (220.102) | 4021.439 | 197.197 | (195.064) | 4638.372 |
| Probability of employment, Year 1 | 0.053 | (0.019)*** | 0.669 | 0.034 | (0.011)*** | 0.724 |
| Earning, Year 1 | -208.181 | (638.230) | 13468.69 | 97.439 | (704.634) | 15712.54 |
| **B. Long-term post-degree outcomes** | | | | | | |
| Probability of employment, Year 2 | 0.046 | (0.019)** | 0.665 | 0.034 | (0.013)*** | 0.716 |
| Probability of employment, Year 3 | 0.035 | (0.020)* | 0.663 | 0.023 | (0.011)** | 0.714 |
| Probability of employment, Year 4 | 0.040 | (0.020)** | 0.669 | 0.011 | (0.011) | 0.713 |
| Probability of employment, Year 5 | 0.025 | (0.020) | 0.666 | 0.005 | (0.011) | 0.718 |
| Earning, Year 2 | -1,754.393 | (872.171)** | 17776.19 | 1,747.208 | (986.630)* | 21301.27 |
| Earning, Year 3 | -1,716.212 | (932.807)* | 20694.12 | 939.394 | (856.973) | 25572.48 |
| Earning, Year 4 | -1,601.615 | (974.691) | 23764.83 | 643.155 | (917.053) | 29710.93 |
| Earning, Year 5 | -1,617.838 | (1,082.672) | 26730.51 | -651.180 | (923.072) | 34478.66 |
| **Observations (N)** | 8,818 | | | 25,065 | | |

Note: The table shows PSM estimation results of post-degree labor market outcomes associated with taking work-based courses at college dropping students in majors with more than 80% of the sample taking work-based courses. The analysis sample includes first-time college entry cohorts of 2004/05-2008/09 who completed a college degree in six years after enrollment, who were not enrolled after the degree completion, and who were in majors with a work-based course-taking rate lower than 80%, in the state administrative data. 2-year degree completers include observations who completed a certificate or an Associate's degree, and 4-year degree completers include observations who completed a Bachelor's degree. The analysis uses "teffects psmatch" command in STATA, with a logit model to estimate the propensity score, caliper of 0.05, with replacement and excluding observations that violate the overlap assumption, to execute the matching and the estimation. Earnings are adjusted by CPI to 2015 dollar; missing earning data are treated as unemployed quarters and are replaced with zeros. Baseline columns show outcomes among the control group who did not take work-based courses at college. ***p<0.01, **p<0.05, *p<0.1.

Results for the 4-year alternative sample are also consistent with the main results, showing positive associations with both post-degree employment outcomes and post-degree earnings. Specifically, work-based course takers have a 3.4 percentage points higher employment rate in the first post-degree year than non-takers, especially during the third (2.5 percentage points higher) and the four (3.7 percentage points higher) quarter of the first year. The positive association lasts through the second and the third post-degree year, at a size of 3.4 percentage points and 2.3 percentage points respectively. In addition, there is a positive and statistically significant association between taking work-based courses at college and earnings in the second post-degree year.

As noted, results should not be taken as rigorous causal evidence because the identifying assumption may not be fully satisfied. In particular, while students are matched by the 2-digit CIP code of the degree program area, there may exist program-specific factors that affect both the selection and the outcomes of work-based course-taking within a program area. For example, some programs may offer and/or require more work-based courses than other programs in the same program area. If 2-year students in these programs are more likely to transfer to a 4-year institution outside the CUNY system, pursue further training/apprenticeship, or go into unionized sectors where wages are traded off with non-wage benefits, they would have lower post-graduation earnings than students in other programs under the same program area. These are all factors that may drive the negative association between work-based course-taking and post-graduation earnings among 2-year students. That is, results do not indicate that taking work-based courses has adverse impact on 2-year students' labor market outcomes.

## 2.6 Discussion

The paper applies an innovative text mining technique to identify work-based courses in transcript records of a large urban college system, and examines patterns and post-degree labor market outcomes of taking work-based courses at college. The technique identifies that 11% of 2-year enrollees and 17% of 4-year enrollees take a work-based course in six years after college entry. Among degree-completers, 31% of 2-year graduates and 29% of 4-year graduates take a work-based course at college. The majority of students only take credit-bearing work-based courses, and generally take the courses in later years of their programs.

The analysis of work-based course-taking patterns highlights large differences across fields of study. In particular, students in Engineering and Natural Sciences & Math are less likely to take work-based courses than students in other fields. Programs in Engineering and Natural Sciences & Math may be traditionally focused more on academic learning. As the value of experiential learning at college has been increasingly endorsed by employers nowadays, colleges may consider incorporating more applied opportunities for students in Engineering and Natural Sciences & Math to prepare them better for the workforce. In addition, there is no significant racial disparity in work-based course-taking. Results suggest that work-based courses can open up an opportunity for underserved minority students to participate in experiential learning at college, and thus may serve as an equity tool in both higher education and the post-college labor market.

To understand the relationship between work-based courses and workforce preparation at college, the paper uses a PSM model to compare post-degree labor market outcomes between work-based course takers and non-takers. A key assumption of the PSM model is conditional independence, stating that the model must control for all the observable covariates that can affect

both the decision and the outcomes of the treatment (i.e. taking work-based courses at college). The study relies on the richness of the administrative data and includes a rich set of covariates for the matching. Nevertheless, there may remain unobserved factors that could potentially affect both the selection into work-based course-taking and post-degree labor market outcomes. The selection bias would be at least partially mitigated through the correlation between the unobserved factors and the observed covariates. However, we should still be cautious about interpretating the PSM results as rigorous causal evidence because the selection into work-based courses can be very non-random. I therefore take the PSM results as suggestive evidence, which still provides valuable insights for college administrators and future research.

First, results show a positive association between taking work-based courses at college and the probability of being employed after degree completion among both 2-year and 4-year degree completers. The association is strongest during the first year after college graduation, and can last through the third post-degree year. The results suggest that work-based course takers have better chances of being employed after college graduation than non-takers, probably as a result of networking opportunities, job skills, or signaling effects gained from taking work-based courses at college. In addition, there are some positive associations between taking work-based courses at college and post-degree earnings in the 4-year results. That is, work-based course takers at the Bachelor's degree level are also more likely to land a high paying job than non-takers.

In contrast, results among 2-year degree completers show null to negative associations between taking work-based courses at college and post-degree earnings. The results imply that work-based course takers at community colleges tend to have lower post-degree earnings than non-takers. As discussed, there may exist program-specific factors within a 2-digit CIP area

driving the negative association between taking work-based courses at community colleges and post-graduation earnings. To interpret the results, it is also important to note that a large proportion of community college students are already working while enrolled. It would be valuable for college administrators to understand how work-based courses relate to and interact with students' other working opportunities. For example, if there are any administrative barriers to enroll in work-based courses with some external job opportunities, students may opt out of work-based courses and take other higher-paying opportunities they find outside school. This may at least in part explain the higher post-degree earnings associated with students who did not take work-based courses at college. Colleges may consider addressing such barriers if exists and expanding job opportunities for work-based courses so that it would allow for a more effective combination of academic learning and experiential learning at college.

Also, the probability of employment and earnings are not the only measures of post-degree labor market success. Another possible reason that work-based course-taking at college is not positively associated with post-degree earnings is that there may be tradeoffs between earnings and a career of interest among work-based course takers. When colleges design and evaluate the effectiveness of work-based courses, it would be great if they incorporate students' interests and the nature of the job beyond average earnings.

As one of the first quantitative research on experiential learning at college, the study has several limitations and leaves open questions for future research. First, the study is focused on work-based courses that can be identified through course titles in transcript records. Experiential learning at college has a broader coverage, and may not always be course-based or reflected in the transcript data. Future research with access to other data sources may apply a similar text mining technique to include other types of experiential learning experience during college. Also,

while this study examines labor market outcomes associated with work-based course-taking at college, workforce preparation is definitely not the sole goal of college education. It is beyond the scope of this paper to examine how work-based courses interact with academic learning as well as other aspects of personal development, but would be interesting for future research to explore.

Last, the PSM model used in this paper may not provide a plausible estimate of the causal effect of taking work-based courses, especially among community college students of whom a large proportion were already working while enrolled. Future research may explore quasi-experimental opportunities for a more causal analysis. For example, the data shows variation in the rate of taking work-based courses across campuses, even after controlling for field areas of study. Among 2-year institutions in the CUNY system, LaGuardia Community College has a relatively higher rate of work-based course-taking than other colleges in several field areas, including Arts & Humanities, Business, and Natural Science & Mathematics. In the area of Business, students at Kingsborough Community College and Borough of Manhattan Community College are also more likely to take work-based courses than students at other 2-year colleges. Among 4-year institutions in the CUNY system, New York City College of Technology has a higher rate of work-based course-taking than other campuses in the areas of Arts & Humanities, Business, Natural Science & Mathematics, and Social & Behavioral Sciences. Students at John Jay College of Criminal Justice in Natural Science & Mathematics and students at Medgar Evers College in Social & Behavioral Sciences are also more likely to take work-based courses than students in the same field areas at other colleges granting 4-year degrees in the CUNY system. It would be interesting for future research to understand if the variation across campuses in taking work-based courses is due to exogeneous factors, such as different graduation requirements or

work-based course/program offerings across campuses. We may compare outcomes of similar students with different likelihoods of taking work-based courses as a result of such exogeneous factors for a more causal understanding of the relationship between work-based course-taking at college and students' outcomes. In addition, I checked trends of taking work-based courses over time of the top 10 majors/programs based on 6-digit CIP codes that enroll the largest number of students at 2-year and 4-year institutions in the CUNY system. The data shows an increase in the work-based course-taking rate among 2-year students in Teacher Assistant/Aide on and after the year of 2010, and a decrease in the work-based course-taking rate among 4-year students in Finance on and after the year of 2011. Exogeneous policy changes in work-based courses over time may offer quasi-experimental opportunities to study the causal relationship between taking work-based courses and students' outcomes within these majors. While other majors may not have an adequate sample size, we may combine majors/programs with similar labor market opportunities for the statistical analysis in future research.

# Chapter 3: Heterogeneity in Labor Market Returns to Master's Degrees - Evidence from Ohio[10]

## 3.1 Introduction

Graduate education is among the fastest growing segments of the U.S. higher educational system. While total undergraduate enrollment in degree-granting postsecondary institutions increased by 26 percent between 2000 and 2018, total post-baccalaureate enrollment increased by 41 percent over the same period (Hussar et al., 2020). This rapid growth in enrollment has been accompanied by an even faster increase in graduate school debt, which rose by 51 percent between 2000 and 2018. While the increase in graduate student debt has attracted policy concern (Douglas-Gabriel, 2020; Miller, 2020), less attention has been given to the other side of the equation: what returns students earn from a graduate degree. Most of the existing studies on this question use outdated sources of data, self-reported measures of labor market outcomes, and/or empirical strategies that can control only for selection on observable characteristics (e.g., Arcidiacono et al., 2008; Song et al., 2008; Stevenson, 2016; Tamborini et al., 2015; Titus, 2007). Two recent papers (Altonji & Zhong, 2021; Altonji & Zhu, forthcoming) provide more rigorous causal evidence on labor market returns to a wide range of graduate programs. These recent studies find substantial differences in returns to Master's degrees across fields, but still leave important open questions about the heterogeneity in labor market returns to graduate degrees across demographic groups and labor market contexts.

[10] The chapter is a joint work with Veronica Minaya and Judith Scott-Clayton. The authors gratefully acknowledge Lisa Neilson and the staff of the Ohio Education Research Center, who helped facilitate our use of the restricted data in this study. The opinions expressed are those of the authors and do not represent views of the Ohio Education Research Center.

In this paper, we provide up-to-date causal evidence on labor market returns to Master's degrees, and examine the returns by degree area, student characteristics and the macroeconomic condition into which students graduate. We use administrative data tracking graduate students enrolled in Ohio from 2000 to 2009, with quarterly earnings spanning historically for 20 years through the fourth quarter of 2019.

Our study will contribute in two primary ways. First, we examine labor market returns to Master's degrees using an individual fixed effects strategy similar to Altonji and Zhu (forthcoming), but with a sample from a different state (Ohio) spanning over a long time frame including the Great Recession and the steady recovery of the 2010s. Second, we examine heterogeneity in the returns by the macroeconomic condition at graduation. Increasing evidence has shown that the macroeconomic condition in which students enter the labor market matters for both the quality of their first job and their long-term career development (Altonji, Kahn and Speer, 2016; Kahn, 2010; Schwandt & von Wachter, 2019). Our study not only advances the literature to highly-skilled entrants with graduate degrees, it also informs more rational educational choices under a recession, with possible implications during the current pandemic for policymakers and students. To the best of our knowledge, we are the first to compare labor market outcomes under different economic conditions at graduation for highly skilled graduates.

On average, we find that obtaining a Master's degree increased quarterly earnings by about 12%. Consistent with past empirical evidence, the returns varied across fields. Our results suggest that while Master's degrees in Health increased earnings by approximately 25%, students could have nearly zero earnings gains from completing a Master's degree in Arts and Humanities. The magnitudes of the returns to obtaining a Master's degree in other fields are around 10%. We also find gender and racial disparities in the returns to Master's degrees, with

higher average returns for women than for men, and for White graduates relative to their Black counterparts. In addition, we compare returns among cohorts who graduated with the Master's degree before versus during the Great Recession. The returns remained strongly positive among cohorts who graduated during the recession (8%), but were about 6 percentage points lower than for those who graduated before the recession (14%). While obtaining a Master's degree on average can boost up earnings, it is important for individuals and policymakers to take into consideration the field of study of the degree, the timing to obtain the degree as well as long-term career development after graduate school.

The paper proceeds as follows. The next section reviews literature on labor market returns to graduate degrees and interactions between graduating under recessions and labor market outcomes. Section 3.3 describes the data and the analysis sample used in the study. Section 3.4 discusses the empirical framework to analyze labor market returns to graduate degrees. Section 3.5 presents empirical results, and the paper concludes with a discussion of the findings and future research in Section 3.6.

## 3.2 Previous Literature

The economic theory of human capital investment suggests that individuals invest in schooling until the marginal benefits in terms of future earnings and other non-monetary benefits are equal to its marginal costs. While costs of graduate education are relatively more straightforward, students have incomplete information about its payoffs. Moreover, Lindley and Machin (2016) warned against examining college graduates as a single group of workers in the labor market due to its increasingly heterogeneous nature between advanced degree holders and college-only workers. Therefore, providing information about returns to graduate degrees is critical to guide students and policymakers about investments in graduate schools.

There is a small but growing literature examining labor market returns to graduate degrees. For example, Titus (2007) employed a propensity score matching model among students who completed a Bachelor's degree in 1993, and found no significant returns to Master's degrees in fields other than Education and Business. Arcidiacono et al. (2008) used an individual fixed effects approach and found positive wage returns to an MBA program among students who registered to take the GMAT in 1990 and were followed up with surveys through 1998. Song et al. (2008) applied a two-stage regression model among Bachelor's degree recipients between 1963 and 1986, and found an estimated annualized return of 7.3% for Master's degrees, 16.6% for professional degrees, and 12.8% for doctoral degrees. Stevenson (2016) examined returns to quality in graduate education among students who received a Bachelor's degree in 1993, and found no returns to either degree completion or program quality in most graduate programs. Important exceptions include Master's programs in health, where completion substantially increased earnings, as well as MBA and professional degree programs, where program quality had a positive influence on earnings. While these studies shed light on returns to graduate degrees, they all used data from more than two decades ago. As both graduate education and the labor market have been developing and changing, the results may not generalize well to current conditions.

One important recent paper by Altonji and Zhong (2021) used fixed effects for combinations of college major and graduate field with more recent data from the National Survey of College Graduates (NSCG, 1993 to 2015) & the National Survey of Recent College Graduates (NSRCG, 1993 to 2010). The paper found differences across fields of graduate degrees in returns on earnings. One limitation of the study is that the analyses are based on self-reported earnings and oversampled individuals in Science & Engineering fields.

The most similar study to ours is Altonji and Zhu (forthcoming) examining labor market returns to various graduate degrees applying alternative fixed effects models with administrative data from Texas. Results also show variations across fields, with particularly high returns to graduate degrees in health-related fields. In addition, this paper found substantial differences across racial and gender groups, and studied how the returns differ by college GPA and major, and across graduate institutions. Our paper examines labor market returns to Master's degrees with administrative data from a different state (Ohio) using an individual fixed effects model. We show similar patterns in the returns under a different state context to findings from Altonji and Zhu (forthcoming). In addition, our data span over a long time frame and allow us to compare labor market returns to Master's degrees among students who graduated before and during the Great Recession.

Our paper also relates to the literature on the effects of entering the labor market during a recession. Prior literature has documented that initial labor market experiences have large and persistent impacts on long-term career progression and success (Oreopoulos, Von Wachter, & Heisz, 2012; Kahn, 2010; Liu, Salvanes, & Sorensen, 2016; van den Berge, 2018). For example, both the quality of the first employer and the first job match have been shown to have lasting impacts on students' future employment and earnings. Therefore, the macroeconomic condition that students graduate and enter the labor market into can have critical implications for their earnings trajectories.

Indeed, there is increasing evidence showing that labor market entrants under a bad economic condition suffer adverse consequences on labor market outcomes. Kahn (2010) showed that graduating under the recession in the early 1980s led to earnings losses persisting up to 15 years among college graduates. Schwandt and Von Wachter (2019) studied young workers

who entered the labor market in the U.S. from 1976 to 2015, and found that entrants under times of high unemployment experienced substantial and long-lasting employment and wage reductions. These effects were particularly large among nonwhite student groups, partly driven by greater losses in employment. Altonji, Kahn and Speer (2016) examined labor market outcomes of U.S. college graduates from the classes of 1974 to 2011, and also concluded that a large recession at graduation substantially reduced initial earnings. Furthermore, the effects differed by field of study: the losses were smaller among students in fields predicted with higher earnings.

Yet, less is known about how entering the labor market during a recession interacts with labor market outcomes among highly-educated workers with graduate degrees. Human resources with graduate-level training are scarcer in the labor market. Thus, workers with graduate degrees are potentially less vulnerable to adverse conditions in the labor market than their lower-educated counterparts. Our paper intends to not only provide the most up-to-date causal evidence on labor market returns to graduate degrees using administrative data from Ohio, we also examine changes in the returns surrounding the Great Recession to better understand the transition from graduate schools to the labor market under economic downturns.

## 3.3 Data & Sample

For our empirical analysis, we use two administrative data sources from the state of Ohio. De-identified data were provided by the Ohio Education Research Center (OERC) under a limited-use, restricted data agreement. The OERC assembles data from multiple state agencies,

including the Ohio Board of Regents (OBR) and the Ohio Department of Job and Family Services (ODFJS), into a repository known as the Ohio Longitudinal Data Archive (OLDA).[11]

The first dataset includes demographic and term-level academic information for all the students attending any public institutions of higher education in Ohio between the years of 2000 and 2011. We can identify students' demographic characteristics, school enrollment and degree outcomes from the dataset. The second dataset includes quarterly earnings records of all the employees subject to Unemployment Insurance (UI) contributions in Ohio between the years of 1999 and 2019. The earnings records can be linked to the education data through an individual identifier. Thus, we can identify students' quarterly earnings before enrollment at graduate school as well as after the completion of a graduate degree. The data also include the county of the employer, which allows us to link county-level unemployment rates from the Bureau of Labor Statistics (BLS) to each quarter and every worker in the data, serving as a proxy for the macroeconomic condition that the individual worked under.

For this study, we restrict the analysis sample to Master's degree holders who first enrolled and obtained a Master's degree in Ohio's public college system from Fall 2000 to Fall 2009. This restriction allows us to track and compare each individual's quarterly earnings from six quarters before graduate school enrollment to ten years after completion. Individuals who obtained Master's degrees older than 50 are excluded.

[11] The following acknowledgement is required to be stated on any materials produced using workforce or higher education data accessed from the OLDA: This workforce solution was funded by a grant awarded to the U.S. Department of Labor's Employment and Training Administration. The solution was created by the Center for Human Resource Research on behalf of the Ohio Department of Job and Family Services and does not necessarily reflect the official position of the U.S. Department of Labor. The Department of Labor makes no guarantees, warranties, or assurances of any kind, express or implied, with respect to such information, including any formation on linked sites and including, but not limited to, accuracy of the information or its completeness, timeliness, usefulness, adequacy, continued availability, or ownership. This solution is copyrighted by the institution that created it. Internal use, by an organization and/or personal use by an individual for non-commercial purposes, is permissible. All other uses require the prior authorization of the copyright owner.

Since our fixed effects approach compares an individual's earnings before and after obtaining the graduate degree, one would ideally like to focus on Bachelor's degree holders who actively participated in the labor market before graduate school enrollment. Yet, a limitation of the data is that we cannot observe Bachelor's degree completion for all individuals in our sample. To address this, we limit the sample to individuals who had at least four quarters of earnings higher than the state minimum wage at an age older than 22 within six quarters before their first graduate enrollment.[12] Furthermore, we limit the sample to individuals who have at least one quarter of earnings higher than the minimum wage within ten years after completion. This ensures that individuals who did not work actively for UI-covered employers in the state after Master's completion are excluded as we are not able to identify their post-Master's degree earnings.

Table 3.1 displays summary statistics of our analysis sample. Approximately 65% are women and 84% are White students.[13] The average student in our sample first enrolled in graduate school at the age of 30 and earned a Master's degree at the age of 32. Degrees in the fields of Education, Business, Social & Behavioral Sciences and Health are the most common. And we can track approximately an average of six quarters of earnings before graduate enrollment and 33 quarters of earnings post Master's degree completion for individuals in the sample.

[12] We cannot capture Bachelor's degree completion for everyone, since some may have earned it prior to our data window, or may have done so at an out-of-state or private institution not covered in our data. Earnings above the minimum wage are approximated using information from U.S. Department of Labor, State Minimum Wage Rate for Ohio [STTMINWGOH], retrieved from FRED, Federal Reserve Bank of St. Louis: https://fred.stlouisfed.org/series/STTMINWGOH. The minimum wages are converted to 2019Q4 dollars using the Consumer Price Index for All Urban Consumers, U.S. City Average (CPI-U), in the same way as the earnings data.

[13] The racial distribution of our sample corresponds to Ohio's overall demographics - approximately 76% of the state's population are White based on census estimates in 2019.

**Table 3.1: Summary Statistics**

| | Analysis Sample | National Comparison Samples | | | |
|---|---|---|---|---|---|
| | Master's Degree Holders in Ohio | NPSAS: GR 2004 | NPSAS: GR 2008 | NPSAS: GR 2004 Public Schools | NPSAS: GR 2008 Public Schools |
| **Gender** | | | | | |
| Male | 34.6% | 42.8% | 34.3% | 35.1% | 37.0% |
| Female | 65.3% | 57.2% | 65.7% | 64.9% | 63.0% |
| **Race/Ethnicity** | | | | | |
| White Non-Hispanic | 84.1% | 74.6% | 68.1% | 73.3% | 71.6% |
| Black Non-Hispanic | 8.0% | 12.4% | 16.3% | 13.1% | 11.9% |
| Hispanic | 1.2% | 6.1% | 8.8% | 5.9% | 8.3% |
| Asian or Pacific Islander | 1.5% | 5.2% | 5.3% | 7.1% | 6.3% |
| Other | 5.2% | 1.7% | 1.5% | 0.6% | 1.9% |
| **Age** | | | | | |
| Graduate School Enrollment | 30 | | | - | |
| Master's Degree Completion | 32 | 35 | 35 | 34 | 34 |
| **Master's Degree Field** | | | | | |
| Arts & Humanities | 3.9% | 6.0% | 3.8% | 5.5% | 3.6% |
| Business | 20.2% | 30.3% | 25.9% | 22.6% | 17.2% |
| Education | 46.8% | 33.1% | 38.9% | 38.7% | 37.9% |
| Engineering | 3.3% | Math/Engineering/CS: 11.2% | Math/Engineering/CS: 5.5% | Math/Engineering/CS: 7.6% | Math/Engineering/CS: 9.5% |
| Health | 9.4% | 4.6% | 9.0% | 4.9% | 9.0% |
| Natural Science & Mathematics | 2.8% | Life sciences: 1.9% | Life sciences: 1.8% | Life sciences: 3.7% | Life sciences: 2.8% |
| Services | 1.9% | - | - | - | - |
| Social & Behavioral Sciences | 11.6% | 3.4% | 4.3% | 3.8% | 3.7% |
| Other | - | 9.6% | 10.9% | 13.1% | 14.6% |
| **School Type** | | | | | |
| Public School | 100% | 41.6% | 44.6% | 100% | 100% |
| Private Not-For-Profit | 0% | 49.4% | 42.0% | 0% | 0% |
| Private For-Profit | 0% | 9.0% | 13.4% | 0% | 0% |
| Sample Size (N) | 29,247 | - | - | - | - |

Note: (1) Master's degree holders in Ohio include individuals who first enrolled in graduate school and obtained a Master's degree from Fall 2000 to Fall 2009 in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is also limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. (2) National comparison samples include master's degree students in NPSAS: GR 2004 and NPSAS: GR 2008 who have earned Bachelor's degree, had job prior to enrollment, and completed the master's degree program in 2003-2004 and 2007-2008 respectively. Summary statistics among students attending public schools in the samples are shown separately. Age at Master's degree completion was calculated as of 12/31/03 for students in NPSAS: GR 2004 and 12/31/07 for students in NPSAS: GR 2008. The statistics were downloaded using NCES PowerStats.

Table 3.1 also compares our analysis sample with national samples of Master's students in NPSAS: GR 2004 and NPSAS: GR 2008 who have earned a Bachelor's degree, had a job

prior to graduate enrollment and completed the Master's degree program in 2003-2004 and 2007-2008 respectively. As our analysis sample includes students in the public college system in Ohio only, summary statistics for students in the national samples attending public institutions are shown separately. Compared to Master's students in the nation, our analysis sample has a slightly higher proportion of women and White students. Students in our analysis sample completed a Master's degree at a relatively younger age, and their degrees are more likely to be in the field of Education. Overall, summary statistics are similar between our analysis sample and the national comparison samples, indicating that conclusions from our analysis among Master's degree holders in Ohio may be directly generalizable to the nation.

An important limitation of our earnings data is that we are unable to distinguish between quarters when an individual was unemployed, not in the labor force, worked in another state or under non-UI-covered employers in Ohio. We therefore exclude quarters of missing earnings from the analysis. All the earnings are converted to 2019Q4 dollars using the Consumer Price Index for All Urban Consumers, U.S. City Average (CPI-U), and the highest 1% are top-coded. Figure 3.1 shows the earning trajectory of the analysis sample. Specifically, the figure depicts average quarterly earnings of six quarters before graduate enrollment, shown as negative relative quarters, and ten years after graduate degree completion, shown as positive relative quarters. The figure displays a sharp earning jump right after the completion of the Master's degree, and the earnings continued to rise gradually over the next ten years. Also, the figure shows a slight earning dip one and two quarters before the first enrollment term at graduate school, indicating possible existence of the "Ashenfelter dip" before graduate entry.


**Figure 3.1: Wage Trajectories for Master's Degree Holders in Ohio**

## 3.4 Empirical Methodology

To estimate the labor market returns to Master's degrees, we employ an individual fixed effects approach comparing pre- and post-graduate education earnings within the same individual among Master's degree holders in our analysis sample. For each individual, we use quarters of earnings within six quarters before the first graduate enrollment as pre-earnings, and quarters of earnings within ten years after the completion of the Master's degree as post-earnings. Only earnings higher than state's minimum wage earned at an age older than 22 are used in the estimation. Specifically, we estimate:

$$Lnearn_{it} = \alpha + \beta Madeg_{it} + \theta_1 Age_{it} + \theta_2 Age_{it}^2 + \gamma Prern_{it} + \delta Demog_i * YQ_t + \lambda Field_i * YQ_t + \pi Cohort_i * YQ_t + \varphi Unempr_{it} + \eta_i + \varepsilon_{it} \quad (3.1)$$

The dependent variable $Lnearn_{it}$ is the log-transformed real quarterly earning for individual *i* at quarter t. $Madeg_{it}$ is a dummy variable indicating if individual *i* has obtained a

Master's degree in quarter *t*. That is, quarters of pre-earnings have a value of zero for this variable and quarters of post-earnings have a value of one.

The underlying age (experience) - earnings profile is accounted for by $Age_{it}$ and $Age_{it}^2$. $Prern_{it}$ is a set of two dichotomous variables, which equal to one for the time period one and two quarters before first graduate enrollment, respectively. These two variables control for possible pre-enrollment dips in earnings (Ashenfelter's dip) that we observed shortly before first graduate enrollment in Figure 3.1. $Demog_i * YQ_t$ is a vector of time-variant person-specific demographics including a time trend interacted with race/ethnicity, gender and age at first enrollment in graduate school. We also control for $Field_i * YQ_t$, a time trend interacted with fields of study declared at first graduate school enrollment, and $Cohort_i * YQ_t$, a time trend interacted with cohorts of entry at graduate school. These interaction terms control for linear time trends specific to individual characteristics, and accounts for the fact that underlying earnings trajectories may be different by these characteristics. $Uempr_{it}$ is the county-level, quarter-specific unemployment rate to control for the effect of the macroeconomic condition that individual *i* works under. $\eta_i$ controls for individual fixed effects, and thus eliminates the risk of a bias due to any omitted factors that stay constant across time for each individual. Our estimation includes data from post- Master's degree quarters to account for time-variant factors and underlying earnings trajectories by individual characteristics. However, the time trends may pick up some effect of the Master's degree on earnings growth, especially when returns change with post-graduate school experience.

The coefficient of interest measures the change to earnings within an individual from before graduate school to after graduate degree receipt, compared to the trends we would expect for similar students. Thus, the coefficient can be interpreted as the causal effect of Master's

degrees on log earnings. Since the analysis sample includes individuals who eventually obtained a Master's degree and quarters of earnings higher than the minimum wage only, the estimated return is the treatment on the treated effect conditional on active employment.

The empirical methodology is based on several assumptions. First, since the degree data do not track students beyond the year of 2011 but the earnings data expand to 2019, we could not consistently identify a control group of individuals who never obtained a Master's degree over the entire time frame of the earnings data. As a result, our sample does not include a "pure" control group but includes only those who earned a Master's degree at some point by 2011. We compare graduates' earnings trajectories before versus after degree completion, assuming that the counterfactual earnings in the absence of the graduate degree can be modeled based on the earnings trajectories of similar individuals who eventually completed a Master's degree but hadn't yet enrolled in graduate school during the quarter. The model controls for any individual-specific variations in the outcome that stay constant across time, important time-variant factors such as the individual's age and county-level unemployment, as well as time trends in earnings by race/ethnicity, gender, field of study, age and cohort of entry in graduate school. For the same reason, quarters from 2011 to 2019 are exclusively post-graduation earnings in our data. We therefore do not control for time fixed effects because it will absorb some of the returns to Master's degrees. Instead, we include controls for flexible time trends, which we allow to vary by age at entry, race/ethnicity, gender, and field of study.

Finally, since we do not have undergraduate records for students who obtained college degrees outside of Ohio's public college system or beyond the time coverage of the education data, we do not control for individual undergraduate variables in the model, such as college major. This is not a concern for our estimation, as any time-invariant effects of college-related

variables are controlled by the individual fixed effects, and our specification also already includes graduate-field-of-study-specific time trends.

## 3.5 Results

### 3.5.1 Labor Market Returns to Master's Degrees

Table 3.2 reports estimates of the effects of obtaining a Master's degree on log earnings. Column 1 shows the average return to any Master's degree. Since Master's degree holders in different fields of study enter different labor markets, we categorize Master's degrees into eight areas based on CIP codes: Arts & Humanities, Business, Education, Engineering, Health, Natural Sciences & Math, Services, and Social & Behavioral Sciences.[14] We conduct subgroup analyses by field area and show estimates in Column 2-9.

The coefficient for the estimated return (in logs) to any Master's degree is 0.116. This indicates that on average a Master's degree increased quarterly earnings by about 12%. That is, obtaining a Master's degree increased earnings by approximately $1,400 per quarter from an average baseline quarterly earning of $11,433.

Although the average return to a Master's degree is positive and statistically significant, Table 3.2 shows a large variation in returns by graduate fields. Ranked from the highest to the lowest, the estimated returns to a Master's degree are approximately 24% for Health, 13% for Education, 12% for Natural Sciences & Math, 11% for Engineering, 9% for Services, 8% for Business, 7% for Social & Behavioral Sciences, and essentially zero for Arts & Humanities. To allow for comparison across graduate fields, the returns are shown in percentages as baseline earnings also vary across fields. Also, the returns do not take into account direct and indirect

[14] Top majors (CIP codes) within each field area are listed in Appendix Table C1.

costs of completing the Master's degree. These results thus suggest that the net value of a Master's degree in Arts & Humanities could be negative.

**Table 3.2: Individual Fixed Effects Estimates of Labor Market Returns to Master's Degrees**

| **OUTCOME: Log Real Quarterly Earnings** | **Any Master's Degree** | **Master's Degree Field** | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | Arts & Humanities | Business | Education | Engineering | Health | Natural Sciences & Math | Services | Social & Behavioral Sciences |
| **VARIABLES** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Master's Degree | 0.116 | 0.002 | 0.079 | 0.128 | 0.109 | 0.243 | 0.118 | 0.085 | 0.072 |
| | (0.004)*** | (0.021) | (0.009)*** | (0.004)*** | (0.021)*** | (0.016)*** | (0.024)*** | (0.030)*** | (0.012)*** |
| Observations (N) | 1,058,874 | 36,811 | 198,369 | 533,655 | 31,387 | 99,245 | 28,102 | 18,923 | 112,376 |
| Students (N) | 29,144 | 1,148 | 5,884 | 13,656 | 952 | 2,745 | 827 | 553 | 3,378 |
| Baseline Quarterly Earnings | $11,433 | $9,463 | $15,535 | $10,364 | $13,566 | $12,199 | $11,068 | $10,133 | $9,797 |

Note: Each column represents a separate regression showing individual fixed effects estimates. The analysis sample includes Master's degree holders in Ohio who first enrolled in graduate school and obtained a Master's degree from Fall 2000 to Fall 2009 in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is further limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. All the wages are converted to 2019Q4 dollars using the Consumer Price Index for All Urban Consumers, U.S. City Average (CPI-U), and the highest 1% wages are top-coded. Wages that are missing/below minimum wage/earned before age 22 are excluded. All the regressions are also controlled for: (i) individual's age at each quarter; (ii) a dummy that is equal to one for the time period one quarter before first enrollment at graduate school, and another dummy that is equal to one for the time period two quarters before first enrollment at graduate school; (iii) a set of demographic and personal characteristics, including race/ethnicity, gender, age at first graduate school enrollment, graduate school entry cohort, and major declared at first graduate school enrollment, all interacted with time trends; (iv) county unemployment rate. Baseline quarterly earnings show geometric mean of earnings before graduate enrollment. Robust standard errors clustered at individual level in parentheses, ***p<.01, **p<.05, *p<.1.

Table 3.3 illustrates heterogeneity in returns to Master's degrees by gender and race/ethnicity. Results for gender disparities indicate that the returns are higher for women than for men. On average, a Master's degree increased quarterly earnings by about 7% for men and 14% for women. Before graduate enrollment, men in our sample had an average baseline earning of $13,380 while women had only $10,511. Obtaining a Master's degree increased quarterly earnings for men by $950 and $1,500 for women. Results demonstrate that women benefit more

from obtaining a Master's degree than men, suggesting that the gender wage gap would be narrower but still not fully closed among these highly skilled graduates.

In terms of racial disparities, Table 3.3 shows that returns are the highest among White degree holders, and relatively lower among Black degree holders. Specifically, obtaining a Master's degree increased quarterly earnings by about 12% for White students and only 8% for Black students. In addition, the estimated returns are about 11% for Hispanic degree holders, and 7% for Asian degree holders. However, results for Hispanic and Asian students are estimated on a small sample and thus should be interpreted with caution.

**Table 3.3: Gender and Racial Disparities in Return to Master's Degrees**

| **OUTCOME: Log Real Quarterly Earnings** | **Gender** | | **Race/Ethnicity** | | | |
|---|---|---|---|---|---|---|
| | Male | Female | White | Black | Hispanic | Asian |
| **VARIABLES** | (1) | (2) | (3) | (4) | (5) | (6) |
| Master's Degree | 0.073 | 0.137 | 0.124 | 0.080 | 0.109 | 0.073 |
| | (0.006)*** | (0.004)*** | (0.004)*** | (0.012)*** | (0.030)*** | (0.032)** |
| Observations (N) | 359,065 | 698,744 | 905,289 | 79,331 | 12,622 | 13,917 |
| Students (N) | 10,082 | 19,034 | 24,518 | 2,331 | 360 | 443 |
| Baseline Quarterly Earnings | $13,380 | $10,511 | $11,260 | $11,246 | $11,775 | $15,778 |

Note: Each column represents a separate regression showing individual fixed effects estimates. The analysis sample includes Master's degree holders in Ohio who first enrolled in graduate school and obtained a Master's degree from Fall 2000 to Fall 2009 in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is further limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. All the wages are converted to 2019Q4 dollars using the Consumer Price Index for All Urban Consumers, U.S. City Average (CPI-U), and the highest 1% wages are top-coded. Wages that are missing/below minimum wage/earned before age 22 are excluded. All the regressions are also controlled for: (i) individual's age at each quarter; (ii) a dummy that is equal to one for the time period one quarter before first enrollment at graduate school, and another dummy that is equal to one for the time period two quarters before first enrollment at graduate school; (iii) a set of demographic and personal characteristics, including race/ethnicity, gender, age at first graduate school enrollment, graduate school entry cohort, and major declared at first graduate school enrollment, all interacted with time trends; (iv) county unemployment rate. Baseline quarterly earnings show geometric mean of earnings before graduate enrollment. Baseline quarterly earnings show geometric mean of earnings before graduate enrollment. Robust standard errors clustered at individual level in parentheses, ***p<.01, **p<.05, *p<.1.

To further understand possible interactions between graduate fields, gender and race, Appendix Table C2 compares distributions of fields between men and women, as well as White

and Black students. First, men are relatively more likely to graduate in Business and Engineering, while women are more likely to graduate in Education and Health. As shown in Table 3.2 and discussed above, a Master's degree in Health or Education has the highest returns among all graduate fields, and a Master's degree in Business or Engineering has a relatively lower return. Also, compared to White degree holders, Black students are more likely to obtain the Master's degree in Social & Behavioral Sciences, which has a relatively lower return than other fields. Thus, results suggest that graduate fields serve as a contributing factor to the gender and racial disparities in returns to Master's degrees.

### 3.5.2 Labor Market Returns to Master's Degrees and the Great Recession

In order to examine heterogeneity in returns to Master's degrees by the macroeconomic condition at graduation, we compare individuals who graduated with a Master's degree before and under the Great Recession. To ensure the comparability of the two groups, we construct a recession comparison sample by further restricting our analysis sample to individuals who graduated from 2004 to 2009 within three years after they first enrolled in graduate school. Among them, we compare students who graduated from 2004 to 2007 (before the recession) with those who graduated from 2008 to 2009 (under the recession).[15] The majority (84%) of the sample who graduated under the recession started graduate school before the recession (before 2008). Figure 3.2 shows the wage trajectories of the two graduation cohorts separately. Similar to the full analysis sample shown in Figure 3.1, students' earnings increased sharply right after

[15] The National Bureau of Economic Research (NBER) defines a recession as "a period of falling economic activity spread across the economy, lasting more than a few months, normally visible in real GDP, real income, employment, industrial production, and wholesale-retail sales", and estimates that the Great Recession began in December 2007 and ended in June 2009. Since our study focuses on labor market outcomes, and unemployment rates continued to increase through the end of 2009, we include all of 2009 in the recession. Appendix Table C3 compares summary statistics between students who graduated with a Master's degree before and under the Great Recession in the recession comparison sample. There is no significant difference in student compositions across the two cohorts.

completing a Master's degree and further gradually increased over the next ten years for both groups.


**Figure 3.2: Wage Trajectories for Master's Degree Holders in Ohio, by Graduation Cohort**

Focusing on the recession comparison sample, we examined and compared returns to Master's degrees among students who graduated before and during the Great Recession using the same individual fixed effects model. In particular, we add to the estimation an interaction term of a flag for graduating with the Master's degree under the recession and the dummy variable for post-Master's degree quarters. Individuals who graduated in 2008 or 2009 get a value of one for the recession flag, and individuals who graduated before 2008 get a value of zero. The coefficient on the interaction term would estimate the variation in the return to a Master's degree completed under the Great Recession from the return to the degree completed before the recession.

Table 3.4 shows estimated coefficients on the dummy variable for post-Master's degree quarters and the interaction term of the dummy and the recession flag. The coefficient on the dummy variable indicates returns to Master's degrees completed before the Great Recession, and the sum of the coefficients on the dummy variable and the interaction term indicates returns to degrees completed under the recession. Overall, a Master's degree completed under the recession had a lower return than before the recession. Specifically, a Master's degree increased quarterly earnings by about 14% for students who graduated before the recession, and by only about 8% for those who graduated under the recession. While returns to Master's degrees under the Great Recession were still positive, returns for those graduating during the recession were about 6 percentage points lower.

Table 3.4 also shows returns to Master's degrees among graduates before and under the Great Recession by graduate field. This specification breaks the estimations down to smaller samples, resulting in weaker statistical power. None of the coefficients of interaction terms appear positive, indicating that returns to Master's degrees completed under the Great Recession were lower than degrees completed before the recession across fields. Returns completed under the recession were lower than returns completed before the recession by about 12 percentage points for Master's degrees in Services, and 6 to 7 percentage points for a degree in Business, Social & Behavioral Sciences, and Education. The coefficients of interaction terms are not statistically significant for Arts & Humanities, Engineering, Health, and Natural Sciences & Math. Except for Engineering, the coefficients also have a small magnitude, suggesting that returns to Master's degrees in these fields did not vary significantly with the recession.

**Table 3.4: Interactions Between Return to Master's Degrees and the Great Recession**

| **OUTCOME: Log Real Quarterly Earnings** | **Any Master's Degree** | **Master's Degree Field** | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | Arts & Humanities | Business | Education | Engineering | Health | Natural Sciences & Math | Services | Social & Behavioral Sciences |
| **VARIABLES** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Master's Degree | 0.137 | 0.015 | 0.114 | 0.155 | 0.094 | 0.242 | 0.137 | 0.110 | 0.089 |
| | (0.005)*** | (0.029) | (0.011)*** | (0.006)*** | (0.030)*** | (0.021)*** | (0.034)*** | (0.043)** | (0.016)*** |
| Master's Degree * Degree under the Great Recession | -0.060 | -0.015 | -0.067 | -0.061 | -0.062 | -0.027 | -0.012 | -0.118 | -0.064 |
| | (0.006)*** | (0.035) | (0.014)*** | (0.008)*** | (0.039) | (0.027) | (0.043) | (0.057)** | (0.021)*** |
| Observations (N) | 673,238 | 22,952 | 134,863 | 328,952 | 19,123 | 66,949 | 16,053 | 12,512 | 71,834 |
| Students (N) | 18,371 | 711 | 3,935 | 8,340 | 570 | 1,846 | 478 | 366 | 2,125 |
| Baseline Quarterly Earnings | $11,239 | $8,991 | $15,376 | $10,152 | $12,870 | $11,951 | $10,285 | $10,004 | $9,505 |

Note: Each column represents a separate regression showing individual fixed effects estimates. The analysis sample includes individuals who obtained a master's degree from 2004 to 2009 within three years after they first enrolled in graduate school in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is also limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. Individuals who obtained the master's degree from 2004 to 2007 are master's degree holders before the Great Recession, and individuals who obtained the master's degree from 2008 to 2009 are master's degree holders under the Great Recession. All the wages are converted to 2019Q4 dollars using the Consumer Price Index for All Urban Consumers, U.S. City Average (CPI-U), and the highest 1% wages are top-coded. Wages that are missing/below minimum wage/earned before age 22 are excluded. All the regressions are also controlled for: (i) individual's age at each quarter; (ii) a dummy that is equal to one for the time period one quarter before first enrollment at graduate school, and another dummy that is equal to one for the time period two quarters before first enrollment at graduate school; (iii) a set of demographic and personal characteristics, including race/ethnicity, gender, age at first graduate school enrollment, graduate school entry cohort, and major declared at first graduate school enrollment, all interacted with time trends; (iv) county unemployment rate. Baseline quarterly earnings show geometric mean of earnings before graduate enrollment. Robust standard errors clustered at individual level in parentheses, ***p<.01, **p<.05, *p<.1.

Table 3.5 compares interactions between returns to Master's degrees and the Great Recession by gender and race/ethnicity. Returns to Master's degrees completed under the recession were lower for both genders and with a similar magnitude (6 percentage points). Unlike gender, returns to Master's degrees did not vary consistently with the recession across racial groups. Table 3.5 suggests that while returns to Master's degrees were lower under the

recession across all the racial groups, the difference was larger for White and Hispanic degree holders than for Blacks and Asians. Distributions of graduate fields by gender and race are shown in Appendix Table C4. Across gender groups, the distribution did not vary much between the two cohorts. Across racial groups, Black students were less likely to graduate in Education under the recession than before the recession while the distribution did not change significantly with the recession for White students.

**Table 3.5: Interactions Between Return to Master's Degrees and the Great Recession by Gender and Race/Ethnicity**

| **OUTCOME: Log Real Quarterly Earnings** | **Gender** | | **Race/Ethnicity** | | | |
|---|---|---|---|---|---|---|
| | Male | Female | White | Black | Hispanic | Asian |
| **VARIABLES** | (1) | (2) | (3) | (4) | (5) | (6) |
| Master's Degree | 0.095 | 0.157 | 0.146 | 0.094 | 0.144 | 0.096 |
| | (0.008)*** | (0.006)*** | (0.005)*** | (0.017)*** | (0.045)*** | (0.048)** |
| Master's Degree * Degree under the Great Recession | -0.057 | -0.057 | -0.059 | -0.036 | -0.096 | -0.026 |
| | (0.011)*** | (0.008)*** | (0.007)*** | (0.023) | (0.051)* | (0.050) |
| Observations (N) | 227,666 | 444,859 | 576,765 | 48,804 | 7,925 | 9,930 |
| Students (N) | 6,355 | 11,998 | 15,471 | 1,420 | 220 | 313 |
| Baseline Quarterly Earnings | $13,210 | $10,312 | $11,036 | $11,076 | $11,931 | $16,112 |

Note: Each column represents a separate regression showing individual fixed effects estimates. The analysis sample includes individuals who obtained a master's degree from 2004 to 2009 within three years after they first enrolled in graduate school in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is also limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. Individuals who obtained the master's degree from 2004 to 2007 are master's degree holders before the Great Recession, and individuals who obtained the master's degree from 2008 to 2009 are master's degree holders under the Great Recession. All the wages are converted to 2019Q4 dollars using the Consumer Price Index for All Urban Consumers, U.S. City Average (CPI-U), and the highest 1% wages are top-coded. Wages that are missing/below minimum wage/earned before age 22 are excluded. All the regressions are also controlled for: (i) individual's age at each quarter; (ii) a dummy that is equal to one for the time period one quarter before first enrollment at graduate school, and another dummy that is equal to one for the time period two quarters before first enrollment at graduate school; (iii) a set of demographic and personal characteristics, including race/ethnicity, gender, age at first graduate school enrollment, graduate school entry cohort, and major declared at first graduate school enrollment, all interacted with time trends; (iv) county unemployment rate. Baseline quarterly earnings show geometric mean of earnings before graduate enrollment. Robust standard errors clustered at individual level in parentheses, ***p<.01, **p<.05, *p<.1.

## 3.6 Robustness Checks

Appendix Table C5 displays results for a robustness check regarding how we treat missing earnings, which could reflect non-employment but could also indicate migration out of state, self-employment, or employment in a non-UI covered position. As discussed, the main analysis excludes quarters with missing earnings as we could not identify the reason for the missing data. However, our results would overestimate returns to Master's degrees if missing earnings are mostly due to unemployment. To check if the exclusion of missing earnings biases the findings, we conduct a robustness check limiting the analysis sample to individuals with no employment gaps. We estimate returns for individuals who have earnings data that could be used in the analysis for all the six quarters before graduate enrollment and every quarter throughout the ten years after completion of a Master's degree.

Overall, our findings are robust to missing earnings data. Column 1 of Appendix Table C5 shows that the average labor market return to a Master's degree among individuals with no employment gaps (10%) was slightly lower than but close to the findings using the full analysis sample (12%). Results of how the returns varied with the Great Recession shown in Column 2 are also consistent with our main results.

As discussed above, 16% of the recession comparison sample who graduated with the Master's degree under the recession also entered graduate school under the recession. Their enrollment decisions, such as major choice, may be influenced by the recession, which have implications for their returns to Master's degrees. We therefore test whether the results are sensitive to dropping students who entered graduate school after 2008. Among students who entered graduate school before the recession, we compare students who completed a Master's degree before and under the recession. Appendix Table C6 & Table C7 shows results of this test.

These results are consistent with our main results, which reassures that our estimation results on how returns to Master's degrees varied with the Great Recession were not driven by selection bias.

## 3.7 Discussion

The paper exploits state administrative data with an individual fixed effects model, and provides up to date causal evidence on labor market returns to Master's degrees. On average, obtaining a Master's degree increased earnings by about 12% or $1,400 per quarter. According to data from NPSAS: GR 2008, the average cumulative amount borrowed for graduate school is $26,263 at Ohio's public institutions.[16] This suggests that graduate debt would be paid off in about 5 years with the estimated average quarterly return to Master's degrees. Although attending graduate school also entails indirect costs such as "forgone earnings," graduate education still seems like a good investment in the long term on average. However, the returns vary largely across graduate fields. While obtaining a Master's degree could be a great investment for students in certain graduate fields (e.g. Health), it may not be for other fields (e.g. Arts & Humanities). Policy discussions on the payoff of Master's degrees and human capital investment decisions on graduate education should take into account differences in returns across graduate fields.

Although we use a different approach to categorize Master's degree fields from Altonji and Zhu (forthcoming)'s study based on Texas administrative data, results are similar across several graduate fields such as Engineering, Health and Social & Behavioral Sciences. However, our estimate is lower in Business and higher in Education than results in Altonji and Zhu

[16] Calculated by authors using NCES PowerStats among borrowers who enrolled in and completed a Master's degree in 2007-2008 at a public institution in Ohio.

(forthcoming).[17] The difference in returns to Business could be due to different labor market conditions at graduation or different quality of MBA programs offered in the two states. And the difference in Education may be explained by differences in teacher salary schedule between the two states.[18] Therefore, returns to graduate degrees could be sensitive to state conditions and regulations, which should be taken into consideration when generalizing our results to a different state.

Our paper also shows that the average labor market return to a Master's degree is higher for women than for men, and for White than for Black students. We find that differences in graduate fields may contribute to the gender difference and the Black-White gap in returns to Master's degrees. Determining what drives these gaps is beyond the scope of this paper, but should be an important priority for future research.

Finally, we compare returns to Master's degrees among students who graduated before versus during the Great Recession to examine heterogeneity in the returns by the macroeconomic condition at graduation. Our findings suggest that economic downturns may reduce but not eliminate the positive returns to Master's degrees. Also, the decrease in returns to Master's degrees under the recession was larger among White students than Black students. While prior research showed that economic downturns affect labor market outcomes among non-white labor

[17] Specifically, our estimate for a Master's degree in Engineering is 0.109, among which 22% are in Mechanical Engineering; and Altonji & Zhu's individual fixed effects estimate for a graduate degree in Mechanical Engineering is 0.125. We estimate that a Master's degree in Health, mostly in Nursing, has a return of 0.243; and their estimate for a degree in Nursing is 0.26. Also, our estimate for a degree in Social & Behavioral Sciences, represented largely by Social Work, is 0.072; and their estimate for a degree in Social Work is 0.097. While Altonji & Zhu show that an MBA degree has a return of 0.194, our estimate for a Master's degree in Business, among which 66% are in Business Administration and Management, is only 0.079. A Master's degree in Education has a return estimated at 0.128 in our study, represented by 24% in Curriculum & Instruction and 15% in Education Administration and Leadership, but is low for both majors in Altonji & Zhu's results (0.033 for a degree in Education Administration, and -0.005 for a degree in Curriculum & Instruction).

[18] In Ohio, the minimum teacher salary increases with the highest degree level of the teacher. In Texas, the minimum salary only depends on years of experience, and teachers with an advanced degree may be compensated with a stipend by districts.

market entrants more negatively overall (Schwandt & non Wachter, 2019), our results indicate that the adverse impact may be no larger for highly-skilled Black entrants with a Master's degree than for Whites.

Our study has important implications for policies and future research on graduate education. First, the paper provides credible causal evidence on labor market returns to Master's degrees and highlights heterogeneity in the returns. Policymakers and students should take into account the field of study of the graduate degree, students' characteristics, and the macroeconomic context at graduation when evaluating educational investment in graduate school. Costs of graduate schools and non-monetary returns to graduate degrees are also critical factors in determining optimal investment decisions in graduate education. While our study focuses on examining returns to Master's degrees on earnings, it has important implications for future research with access to data on costs, debt and non-monetary outcomes associated with graduate education. Last, since our study sample is limited to individuals who work both before and after graduate school, attend public institutions and work a UI-covered position in Ohio, the findings may have limited generalizability to individuals who enroll in graduate school directly after college, complete Master's degrees at private/for-profit colleges, and work non-UI-covered positions or in another state with different labor market conditions. Our study could be extended with different samples/contexts, in order to provide a more thorough understanding about returns to graduate school investments.

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# Appendix A

**Appendix Table A1: States with a 4-Math HSGR by High School Graduation Class of 2019**

| State | Reform Year |
|---|---|
| Alabama | 2000 |
| South Carolina | 2001 |
| Rhode Island | 2008 |
| Arkansas | 2009 |
| Mississippi | 2009 |
| West Virginia | 2010 |
| Delaware | 2011 |
| Washington, D.C. | 2011 |
| Florida | 2011 |
| Michigan | 2011 |
| Georgia | 2012 |
| Louisiana | 2012 |
| Arizona | 2013 |
| New Mexico | 2013 |
| North Carolina | 2013 |
| Tennessee | 2013 |
| Ohio | 2014 |

Note: The table lists states that have required four years of math in the HSGR by high school graduation class of 2019, as well as the reforming year of increasing the requirement to four years of math.

**Appendix Table A2: C&S Estimates of the Math Reforms' Impact on Racial Composition**

| OUTCOMES | White | Minority |
|---|---|---|
| | (1) | (2) |
| | -0.010 | -0.004 |
| | (0.010) | (0.009) |
| Baseline rates: Class of 2006 | 0.708 | 0.271 |
| N | 87,366 | |

Note: The table shows estimated impact of the math reforms increasing the statewide HSGR to four years of math on racial composition using the C&S estimator aggregated to a single summary parameter. The full analysis sample includes individual aged 22 in the 2010-2019 ACS data among states recently reformed the HSGR to four years of math. Each column represents a separate estimation. Baseline rates are measured among the high school graduation class of 2006. All the observations are weighted by the person weight. Standard errors are calculated using the cluster-robust multiplier bootstrap procedure clustered at state level. *** p<0.01, ** p<0.05, * p<0.1.

**Appendix Table A3: TWFE Estimates of the Math Reforms' Impact on High School Graduation and College Outcomes**

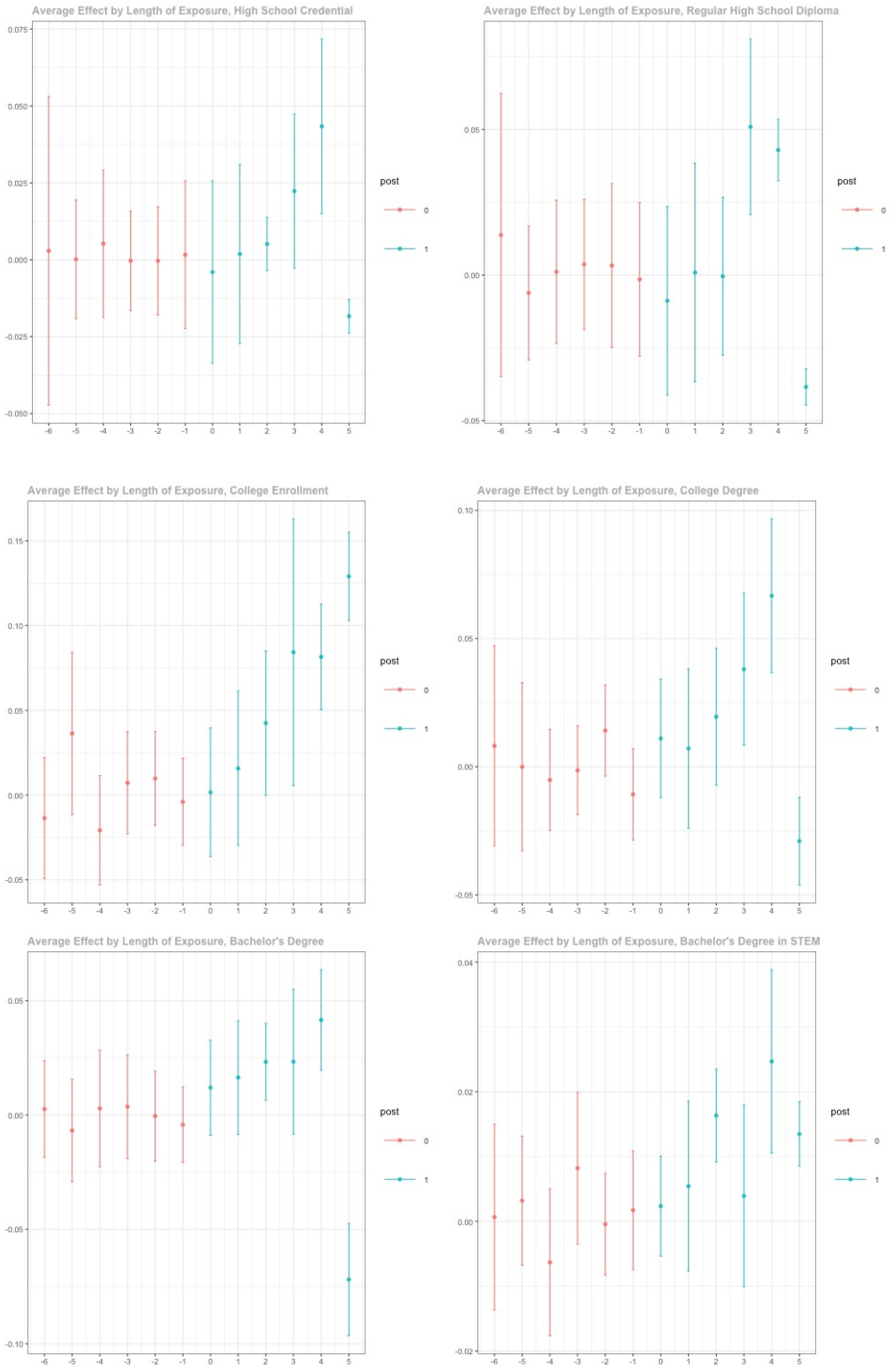
| OUTCOMES | Outcomes by Age 22 | | | | | |
|---|---|---|---|---|---|---|
| | High School Credential | Regular High School Diploma | College Enrollment | College Degree | Bachelor's Degree | Bachelor's Degree in STEM |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***All Races*** | -0.001 | -0.006 | 0.006 | 0.006 | 0.008 | 0.004 |
| | (0.007) | (0.009) | (0.012) | (0.009) | (0.005) | (0.003) |
| Baseline rates: Class of 2006 | 0.888 | 0.838 | 0.587 | 0.177 | 0.110 | 0.024 |
| N | 87,366 | | | | | |
| ***White*** | 0.000 | 0.000 | 0.006 | 0.006 | 0.008 | 0.003 |
| | (0.008) | (0.009) | (0.010) | (0.010) | (0.003)** | (0.002) |
| Baseline rates: Class of 2006 | 0.913 | 0.864 | 0.619 | 0.204 | 0.133 | 0.029 |
| N | 60,068 | | | | | |
| ***Black*** | -0.010 | -0.021 | -0.013 | -0.020 | -0.009 | -0.009 |
| | (0.014) | (0.016) | (0.028) | (0.011)* | (0.013) | (0.004)** |
| Baseline rates: Class of 2006 | 0.827 | 0.772 | 0.508 | 0.090 | 0.045 | 0.005 |
| N | 17,979 | | | | | |
| ***Hispanic*** | 0.021 | -0.015 | 0.021 | 0.045 | 0.014 | 0.013 |
| | (0.040) | (0.041) | (0.048) | (0.037) | (0.038) | (0.009) |
| Baseline rates: Class of 2006 | 0.857 | 0.807 | 0.539 | 0.165 | 0.071 | 0.009 |
| N | 7,471 | | | | | |

Note: The table shows estimated impacts of the math reforms increasing the statewide HSGR to four years of math on high school graduation and college outcomes using the two-way fixed effects estimator. The full analysis sample includes individual aged 22 in the 2010-2019 ACS data among states recently reformed the HSGR to three or more years of math. Each column represents a separate estimation, and each panel represents a separate sample. All the educational attainment outcomes are measured at age 22. Baseline rates are measured among the high school graduation class of 2006. All the observations are weighted by the person weight. Standard errors are calculated using the bootstrap procedure clustered at state level. *** p<0.01, ** p<0.05, * p<0.1.

**Appendix Table A4: High School English Course-Taking Patterns, High School Graduation Class of 2004 and 2012**

| | High School Graduation Class of 2004 | | High School Graduation Class of 2012 | |
|---|---|---|---|---|
| | Average units in English | N | Average units in English | N |
| Full Sample | 4.2 | 3,710 | 4.4 | 7,070 |
| White Sample | 4.2 | 2,350 | 4.2 | 3,920 |
| Black Sample | 4.2 | 590 | 4.7 | 800 |
| Hispanic Sample | 4.1 | 310 | 4.7 | 810 |

Note: Statistics for high school graduation class of 2004 are based on the sample of Spring 2004 senior cohort members with transcript data available for four academic years in the ELS:2002 sampled members. And statistics for high school graduation class of 2012 are based on the sample of students with transcript data available for four academic years in the HSLS:09 sampled members. The table shows the average number of English courses by race in the reforming states that recently increased the HSGR to four years of math. Results for the high school graduation class of 2004 are calculated from the following variable in the student file: units in letters/English, last transcript school state and students' race/ethnicity-composite, weighted by cross-sectional high school transcript weight. And results for the high school graduation class of 2012 are calculated from the following variables in the student file: credits earned in English, state code for school, students' race/ethnicity-composite, weighted by student high school transcript weight. The table shows unweighted sample sizes rounded to the nearest ten, and suppresses cells in which there are fewer than three (3) respondents.




**Appendix Figure A1: Average Treatment Effects by Length of Exposure**

Note: The figures show average treatment effects by length of exposure to the math reforms using the C&S estimator and the main analysis sample (ACS). Standard errors are calculated using the cluster-robust multiplier bootstrap procedure clustered at state level.

# Appendix B

**Appendix Table B1. Selection into Work-based Course-Taking, R-squared**

| | | | | |
|---|---|---|---|---|
| **R-squared** | | | | |
| 2-Year Degree Completers | 0.0301 | 0.0455 | 0.2067 | 0.335 |
| 4-Year Degree Completers | 0.0136 | 0.0176 | 0.0688 | 0.2114 |
| **Independent Variables** | | | | |
| Individual Characteristics | X | X | X | X |
| Ability/preference at enrollment | | X | X | X |
| College | | | X | X |
| Field of study (2-digit CIP code) | | | | X |

Note: The table shows levels of R-squared running OLS with the dummy of taking work-based courses as the dependent variable and different sets of independent variables. Individual characteristics include: gender, race, age at enrollment, in-state residency, birthplace (US-born vs. foreign born), and zip-code level average household income. Ability/preference at enrollment controls include: full-time enrollment in the first term, credits attempted & earned in the first term, GPA in the first term, intent for Bachelor's degree at enrollment, flags for employment and full-time employment in the first term, quarterly earning in the first term, and cohort.

# Appendix C

## Appendix Table C1. List of Top Majors by Master's Degree Field Area

| | | | |
|---|---|---|---|
| **Arts & Humanities (N=1,152)** | | | |
| CIP Code | | CIP Description | % |
| | 230101 | English Language and Literature, General | 37.07 |
| | 500901 | Music, General | 7.64 |
| | 90101 | Speech Communication and Rhetoric | 6.77 |
| | 500903 | Music Performance, General | 5.9 |
| | 160905 | Spanish Language and Literature | 5.12 |
| **Business (N=5,919)** | | | |
| CIP Code | | CIP Description | % |
| | 520201 | Business Administration and Management, General | 66.21 |
| | 520301 | Accounting | 5.96 |
| | 520801 | Finance, General | 4.73 |
| | 520101 | Business/Commerce, General | 4.6 |
| | 521401 | Marketing/Marketing Management, General | 3.73 |
| **Education (N=13,673)** | | | |
| CIP Code | | CIP Description | % |
| | 130301 | Curriculum and Instruction | 24.22 |
| | 130401 | Educational Leadership and Administration, General | 15.13 |
| | 130101 | Education, General | 9.62 |
| | 131001 | Special Education and Teaching, General | 6.74 |
| | 250101 | Library and Information Science | 5.79 |
| **Engineering (N=965)** | | | |
| CIP Code | | CIP Description | % |
| | 141901 | Mechanical Engineering | 21.66 |
| | 143501 | Industrial Engineering | 9.64 |
| | 140801 | Civil Engineering, General | 9.43 |
| | 141001 | Electrical and Electronics Engineering | 9.33 |
| | 40201 | Architecture | 9.12 |
| **Health (N=2,756)** | | | |
| CIP Code | | CIP Description | % |
| | 511601 | Nursing/Registered Nurse | 30.88 |
| | 511699 | Nursing, Other | 7.51 |
| | 512201 | Public Health, General | 6.57 |
| | 511603 | Adult Health Nurse/Nursing | 4.46 |
| | 511599 | Mental and Social Health Services and Allied Professions, Other | 4.35 |
| | 512308 | Physical Therapy/Therapist | 4.35 |
| **Natural Science & Mathematics (N=831)** | | | |
| CIP Code | | CIP Description | % |
| | 270101 | Mathematics, General | 14.92 |
| | 110101 | Computer and Information Sciences, General | 13.72 |
| | 260101 | Biology/Biological Sciences, General | 9.27 |
| | 400601 | Geology/Earth Science, General | 7.46 |
| | 400501 | Chemistry, General | 6.38 |
| **Services (N=557)** | | | |
| CIP Code | | CIP Description | % |
| | 430104 | Criminal Justice/Safety Studies | 49.91 |
| | 310501 | Sports, Kinesiology, and Physical Education/Fitness, General | 17.59 |
| | 310504 | Sports and Fitness Administration/Management | 13.11 |
| | 310599 | Sports, Kinesiology, and Physical Education/Fitness, Other | 7.9 |
| | 310505 | Exercise Science and Kinesiology | 7.36 |
| **Social & Behavioral Sciences (N=3,393)** | | | |
| CIP Code | | CIP Description | % |
| | 440701 | Social Work | 40.73 |
| | 440401 | Public Administration | 18.6 |
| | 540101 | History, General | 3.57 |
| | 420401 | Community Psychology | 3.54 |
| | 309999 | Multi-/Interdisciplinary Studies, Other | 3.07 |

**Appendix Table C2. Distribution of Graduate Fields by Gender and Race**

| | Gender | | Race | |
|---|---|---|---|---|
| **Master's Degree Field:** | Male | Female | White | Black |
| Arts & Humanities | 4% | 4% | 4% | 2% |
| Business | 36% | 12% | 19% | 13% |
| Education | 32% | 54% | 49% | 44% |
| Engineering | 7% | 1% | 3% | 2% |
| Health | 4% | 12% | 10% | 9% |
| Natural Science & Mathematics | 5% | 2% | 3% | 1% |
| Services | 2% | 2% | 2% | 3% |
| Social & Behavioral Sciences | 9% | 13% | 10% | 26% |
| **N** | 10,132 | 19,087 | 24,592 | 2,342 |

Note: The analysis sample includes individuals who first enrolled in graduate school and obtained a Master's degree from Fall 2000 to Fall 2009 in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is also limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree.

## Appendix Table C3. Summary Statistics by Graduation Cohort

| | Master's Degree Holders in Ohio | |
|---|---|---|
| | Degree Before Recession | Degree Under Recession |
| **Gender** | | |
| Male | 34.8% | 34.3% |
| Female | 65.2% | 65.6% |
| **Race/Ethnicity** | | |
| White Non-Hispanic | 84.3% | 83.9% |
| Black Non-Hispanic | 7.8% | 7.5% |
| Hispanic | 1.2% | 1.3% |
| Asian or Pacific Islander | 1.5% | 2.2% |
| Other | 5.2% | 5.2% |
| **Age** | | |
| Graduate School Enrollment | 30 | 30 |
| Master's Degree Completion | 32 | 32 |
| **Master's Degree Field** | | |
| Arts & Humanities | 3.9% | 3.7% |
| Business | 21.8% | 20.8% |
| Education | 46.7% | 42.5% |
| Engineering | 2.8% | 3.8% |
| Health | 9.5% | 11.3% |
| Natural Science & Mathematics | 2.6% | 2.6% |
| Services | 1.9% | 2.3% |
| Social & Behavioral Sciences | 10.9% | 13.0% |
| **Sample Size (N)** | 12,498 | 5,911 |

Note: The analysis sample includes individuals who obtained a master's degree from 2004 to 2009 within three years after they first enrolled in graduate school in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is also limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. Individuals who obtained the master's degree from 2004 to 2007 are master's degree holders before the Great Recession, and individuals who obtained the master's degree from 2008 to 2009 are master's degree holders under the Great Recession.

**Appendix Table C4. Distribution of Degree Fields by Graduation Cohort, Gender & Race**

| | Gender | | | | Race | | | |
|---|---|---|---|---|---|---|---|---|
| | Degree Before Recession | | Degree Under Recession | | Degree Before Recession | | Degree Under Recession | |
| | Male | Female | Male | Female | White | Black | White | Black |
| **Master's Degree Field:** | | | | | | | | |
| Arts & Humanities | 4% | 4% | 3% | 4% | 4% | 2% | 4% | - |
| Business | 39% | 13% | 37% | 12% | 21% | 14% | 19% | 17% |
| Education | 32% | 54% | 29% | 50% | 49% | 46% | 45% | 32% |
| Engineering | 6% | 1% | 9% | 1% | 3% | - | 4% | 4% |
| Health | 5% | 12% | 4% | 15% | 10% | 8% | 12% | 9% |
| Natural Science & Mathematics | 4% | 2% | 5% | 2% | 3% | - | 2% | - |
| Services | 2% | 2% | 2% | 2% | 2% | 3% | 2% | 4% |
| Social & Behavioral Sciences | 8% | 12% | 11% | 14% | 10% | 24% | 11% | 29% |
| **N** | 4,348 | 8,142 | 2,025 | 3,876 | 10,537 | 979 | 4,957 | 445 |

Note: Cells with a sample size smaller than 10 students are suppressed to protect data privacy. The analysis sample includes individuals who obtained a master's degree from 2004 to 2009 within three years after they first enrolled in graduate school in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is also limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. Individuals who obtained the master's degree from 2004 to 2007 are master's degree holders before the Great Recession, and individuals who obtained the master's degree from 2008 to 2009 are master's degree holders under the Great Recession.

**Appendix Table C5. Labor Market Returns to Master's Degrees Among Individuals with No Employment Gaps**

| **OUTCOME: Log Real Quarterly Earnings** | Full Sample with No Employment Gaps | Recession Comparison Sample with No Employment Gaps |
|---|---|---|
| **VARIABLES** | (1) | (2) |
| Master's Degree | 0.102 | 0.123 |
| | (0.005)*** | (0.007)*** |
| Master's Degree * Degree under the Great Recession | - | -0.058 |
| | - | (0.009)*** |
| Observations (N) | 383,559 | 239,130 |
| Students (N) | 8,732 | 5,362 |
| Baseline Quarterly Earnings | $12,906 | $12,847 |

Note: Each column represents a separate regression showing individual fixed effects estimates. The full sample includes Master's degree holders in Ohio who first enrolled in graduate school and obtained a Master's degree from Fall 2000 to Fall 2009 in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. The recession comparison sample is further restricted to individuals who obtained the master's degree from 2004 to 2009 within three years after they first enrolled in graduate school. And only individuals with no earnings that are missing/lower than minimum wage/younger than 22 for the six quarters before graduate enrollment and the 10 years after obtaining Master's degree are included in the estimations. All the wages are converted to 2019Q4 dollars using the Consumer Price Index for All Urban Consumers, U.S. City Average (CPI-U), and the highest 1% wages are top-coded. Wages that are missing/below minimum wage/earned before age 22 are excluded. All the regressions are also controlled for: (i) individual's age at each quarter; (ii) a dummy that is equal to one for the time period one quarter before first enrollment at graduate school, and another dummy that is equal to one for the time period two quarters before first enrollment at graduate school; (iii) a set of demographic and personal characteristics, including race/ethnicity, gender, age at first graduate school enrollment, graduate school entry cohort, and major declared at first graduate school enrollment, all interacted with time trends; (iv) county unemployment rate. Baseline quarterly earnings show geometric mean of earnings before graduate enrollment. Robust standard errors clustered at individual level in parentheses, ***p<.01, **p<.05, *p<.1.

## Appendix Table C6. Interactions Between Return to Master's Degrees and the Great Recession Among Students Enrolled Before the Recession, by Degree Field

| **OUTCOME: Log Real Quarterly Earnings** | **Any Master's Degree** | **Master's Degree Field** | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | Arts & Humanities | Business | Education | Engineering | Health | Natural Sciences & Math | Services | Social & Behavioral Sciences |
| **VARIABLES** | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Master's Degree | 0.141 | 0.016 | 0.125 | 0.156 | 0.101 | 0.244 | 0.133 | 0.132 | 0.094 |
| | (0.005)*** | (0.029) | (0.012)*** | (0.006)*** | (0.030)*** | (0.021)*** | (0.035)*** | (0.041)*** | (0.016)*** |
| Master's Degree * Degree under the Great Recession | -0.051 | 0.002 | -0.056 | -0.055 | -0.06 | -0.023 | -0.021 | -0.123 | -0.064 |
| | (0.007)*** | (0.036) | (0.015)*** | (0.009)*** | (0.040) | (0.027) | (0.044) | (0.059)** | (0.022)*** |
| Observations (N) | 637,194 | 22,390 | 126,449 | 308,438 | 18,388 | 66,006 | 15,522 | 11,786 | 68,215 |
| Students (N) | 17,419 | 696 | 3,688 | 7,835 | 550 | 1,821 | 462 | 344 | 2,023 |
| Baseline Quarterly Earnings | $11,200 | $8,913 | $15,333 | $10,114 | $12,854 | $11,948 | $10,294 | $9,912 | $9,486 |

Note: Each column represents a separate regression showing individual fixed effects estimates. The analysis sample includes individuals who enrolled before 2008 and obtained a master's degree from 2004 to 2009 within three years after they first enrolled in graduate school in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is also limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. Individuals who obtained the master's degree from 2004 to 2007 are master's degree holders before the Great Recession, and individuals who obtained the master's degree from 2008 to 2009 are master's degree holders under the Great Recession. All the wages are converted to 2019Q4 dollars using the Consumer Price Index for All Urban Consumers, U.S. City Average (CPI-U), and the highest 1% wages are top-coded. Wages that are missing/below minimum wage/earned before age 22 are excluded. All the regressions are also controlled for: (i) individual's age at each quarter; (ii) a dummy that is equal to one for the time period one quarter before first enrollment at graduate school, and another dummy that is equal to one for the time period two quarters before first enrollment at graduate school; (iii) a set of demographic and personal characteristics, including race/ethnicity, gender, age at first graduate school enrollment, graduate school entry cohort, and major declared at first graduate school enrollment, all interacted with time trends; (iv) county unemployment rate. Baseline quarterly earnings show geometric mean of earnings before graduate enrollment. Robust standard errors clustered at individual level in parentheses, ***p<.01, **p<.05, *p<.1.

## Appendix Table C7. Interactions Between Return to Master's Degrees and the Great Recession Among Students Enrolled Before the Recession, by Race & Gender

| OUTCOME: Log Real Quarterly Earnings | Gender | | Race/Ethnicity | | | |
|---|---|---|---|---|---|---|
| | Male | Female | White | Black | Hispanic | Asian |
| **VARIABLES** | (10) | (11) | (12) | (13) | (14) | (15) |
| Master's Degree | 0.099 | 0.161 | 0.149 | 0.098 | 0.139 | 0.122 |
| | (0.008)*** | (0.006)*** | (0.005)*** | (0.017)*** | (0.046)*** | (0.049)** |
| Master's Degree * Degree under the Great Recession | -0.049 | -0.049 | -0.05 | -0.027 | -0.112 | -0.023 |
| | (0.012)*** | (0.009)*** | (0.008)*** | (0.025) | (0.051)** | (0.052) |
| Observations (N) | 214,924 | 421,557 | 545,868 | 46,114 | 7,521 | 9,265 |
| Students (N) | 6,005 | 11,396 | 14,675 | 1,341 | 210 | 293 |
| Baseline Quarterly Earnings | $13,172 | $10,278 | $11,001 | $11,006 | $11,861 | $15,893 |

Note: Each column represents a separate regression showing individual fixed effects estimates. The analysis sample includes individuals who enrolled before 2008 and obtained a master's degree from 2004 to 2009 within three years after they first enrolled in graduate school in the Ohio data. Individuals who obtained the master's degree older than 50 are excluded. The sample is also limited to individuals who have at least 4 quarters of earnings higher than the state minimum wage at an age older than 22 within 6 quarters before graduate enrollment, and at least 1 quarter of earnings higher than minimum wage within 10 years after obtaining the master's degree. Individuals who obtained the master's degree from 2004 to 2007 are master's degree holders before the Great Recession, and individuals who obtained the master's degree from 2008 to 2009 are master's degree holders under the Great Recession. All the wages are converted to 2019Q4 dollars using the Consumer Price Index for All Urban Consumers, U.S. City Average (CPI-U), and the highest 1% wages are top-coded. Wages that are missing/below minimum wage/earned before age 22 are excluded. All the regressions are also controlled for: (i) individual's age at each quarter; (ii) a dummy that is equal to one for the time period one quarter before first enrollment at graduate school, and another dummy that is equal to one for the time period two quarters before first enrollment at graduate school; (iii) a set of demographic and personal characteristics, including race/ethnicity, gender, age at first graduate school enrollment, graduate school entry cohort, and major declared at first graduate school enrollment, all interacted with time trends; (iv) county unemployment rate. Baseline quarterly earnings show geometric mean of earnings before graduate enrollment. Robust standard errors clustered at individual level in parentheses, ***p<.01, **p<.05, *p<.1.