A STAGGERED DIFFERENCE-IN-DIFFERENCE ANALYSIS OF THE EFFECT OF FREE COLLEGE PROGRAM ADOPTION ON SUB-BACCALAUREATE COLLEGE COMPLETIONS

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ABSTRACT

Over the past two decades, free college programs, colloquially referred to as promise programs, have emerged as a solution to address increasing postsecondary education costs and increase the number of educated workers in regional workforces. Past research on promise programs has provided evidence that these programs are effective at significantly improving the student outcomes of enrollment and graduation. However, there has yet to be a study that could provide a standard, nationwide estimate for the effect of these programs. I utilize a staggered difference-in-difference (sometimes referred to as an event study or heterogenous treatment-timing difference-in-difference design) to estimate the effect of promise program adoption on the number of sub-baccalaureate awards that publicly controlled, sub-baccalaureate award granting institutions produce. I examine this outcome across two research questions that consider program length and field of study. My research question concerning program length examines all sub-baccalaureate awards, associate's degrees, all credentials, credentials that take between 1 and 2 years to complete, and credentials that take less than one year to complete. My research question concerning program length examines awards across the fields of Science, Technology, Engineering, and Math (STEM); trades; liberal arts; human services; and professional services programs.

Analysis of pre-treatment trends data found that all outcomes mentioned above were appropriate for analysis through a staggered difference-in-difference approach with the exception of awards in the field of professional services. I find evidence that promise programs do increase the number of awards produced by institutions for the outcomes

that are appropriate for my analytical approach. However, this estimate varies by both length of program and I find no statistically significant changes when examining programs by academic area of study. My research has implications for and ongoing institutional, state, and federal level discussions over pending decisions to alter existing or adopt new free college promise programs. It also provides evidence that these free college programs accomplish the key goal of these programs, to increase the number of educated workers in labor markets served by institutions with promise programs.

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CHAPTER 1

Over the past 40 years, public officials have attempted to combat the rising costs of postsecondary education in the United States. Officials have primarily relied on increasing access to portable student aid and award amounts in the form of in-kind money such as grants and scholarships as well as loans which must be paid back. The most recent type of aid to emerge has been place-based scholarships, otherwise referred to as free college or promise programs. Promise programs are place-based scholarships that are differentiated from traditional portable financial aid awards in that they promise funds to students before applying to college. While criteria for specific promise programs can vary and may include specific attributes such as an income requirement, all promise programs restrict potential awards to students from specific geographic areas and where students can utilize promise scholarship funds. Further, promise programs share a common purpose in that they attempt to increase postsecondary access, build a college-going culture, and focus on local community and economic development (Miller-Adams, 2015).

In this manuscript, I examine the national effects of promise program adoption on the production of undergraduate credentials that take less than two years to complete and associate degrees. Hereon, I refer to these awards collectively as sub-baccalaureate awards. I study these sub-baccalaureate awards because they represent the approximately 40% of postsecondary awards produced in the United States but also because some past research has found promise programs do not significantly increase student completion at the sub-baccalaureate level while other studies have (Carruthers et al., 2023; Bartik et al., 2021; National Center for Education Statistics, 2024a; Swanson & Ritter, 2020).

However, these studies that find null effects for sub-baccalaureate award production used less than ideal research designs to reach this conclusion.

The past studies that found null-effects for completions at the sub-baccalaureate level have only considered promise programs that allow for students to choose whether they attend sub-baccalaureate or baccalaureate-granting institutions and thus have a limitation in that students may be substituting their enrollment into more expensive/longer academic programs. Thus, these past estimates of the effect promise programs have on sub-baccalaureate award production could leave the field with an incomplete understanding of the effect of promise programs. In these past studies, students who would have graduated from the sub-baccalaureate without access to promise funds may have instead graduated from four-year institutions with access to promise funds. My research design only considers promise programs that limit their awards to the sub-baccalaureate level to avoid this problem and leverages a national data set to get an average expected effect from promise program adoption. An additional advantage of this focus on the sub-baccalaureate is that my outcome variable's shorter time-to-award lends itself to establishing more observations for my data set (National Center for Education Statistics, 2024b).

I utilize a staggered difference-in-difference design that uses a national inventory of promise programs to assign treatment. The inventory is maintained by CollegePromise.org and represents the most complete inventory available of active promise programs in the United States. Because of this design, my study is poised to

uniquely contribute empirical evidence that provides the first national estimate on the effect of adopting a free college program on sub-baccalaureate award production.

Background of the Study

As of the 2023-2024 academic year, the costs of tuition and required fees on average for community and technical colleges are approximately 1.51 times higher after adjusting for inflation than it was thirty years ago in 1993-1994 (Ma & Pender, 2023). Additionally, living expenses such as room and board for students have also increased over the past thirty years, and new costs have emerged such as the need to access technology such as computers, course software, and the internet as an all-but-required utility for student learning. Regarding the cost of attendance for students, the literature establishes that the more affordable postsecondary enrollment is for students, the more likely students are to participate in postsecondary education and potentially graduate with an award (Dynarski et al., 2023; Goldrick-Rab et al., 2016; Nguyen et al., 2019).

Because of this relationship to degree attainment, financial aid programs have become positioned as a key policy lever to influence degree production as costs have risen over the past 30 years. This has major implications for the needs of the US economy as student access to financial aid has become increasingly important for meeting the workforce's demand for educated workers. Carnevale et al. (2023) predict that by 2031, 72% of the projected 171 million jobs in the United States will require some form of postsecondary training. Of these 171 million jobs 29% will require at least some college or an associate's degree while 42% will require at least a bachelor's degree. Meanwhile, only 28% of jobs will require the equivalent of a high school diploma or less. With nearly

a third of jobs requiring education provided by community and technical colleges and associates degrees providing an affordable start on the path to the 42% of jobs that require a bachelor's degree or higher, community and technical colleges will continue to operate as an important path through higher education to meet workforce needs.

Problem Statement

From 2009 through 2019, the graduation rates within 150% of the time-to-degree for associates degree programs has ranged from 21.1% to 31.4% (National Center for Education Statistics, 2024d). For public institutions that offer credential programs, completion rates for 150% of time-to-degree ranged from 66.6% to 74.6%, reaching a twenty-year low of 66.6% in 2019 (National Center for Education Statistics, 2024e). These completion rates are of concern not only for meeting workforce needs but also to individual students. A 1-in-3 chance of not completing a credential and a 2-in-3 chance of not completing an associate's degree do not constitute favorable odds for students looking to better their economic opportunities, especially in a climate where an increasing number of students, families, and other stakeholders question the value of higher education (Fry, Braga, & K., 2024).

Further complicating these matters are that public, sub-baccalaureate programs represent one of the most affordable paths to postsecondary education in the United States. This affordability and their open-access enrollment practices contribute to these institutions serving a larger share of lower-income students than more selective and expensive institutions. For example, the National Center for Education Statistics (2022) found public, sub-baccalaureate granting institutions enrolled 51% of the students in the

lowest income quintile and 48% of the second lowest income quintile of 2009 ninth graders enrolled by 2016. Meanwhile, only 35% and 18% of the students who enrolled in any postsecondary education from the second highest and highest quintiles respectively were enrolled at public, sub-baccalaureate granting institutions.

Beyond enrollment, these differential outcomes by socio-economic status are also found in completion rates. Using a sample of high school sophomores from 2002, a majority of students from the highest income tercile obtained bachelor's degrees by 2012 (National Center for Education Statistics, 2022). Meanwhile, the majority of students from the lowest income tercile were found to have either only completed some postsecondary education or had no award or less by 2012.

Here, the problem that I identify is a system where the most affordable path for upward economic mobility among lower income students is one through sub-baccalaureate programs where completing a sub-baccalaureate award is not guaranteed. Scholars have established that the cost of attendance influences both enrollment decisions and completion rates. This is because many students struggle to afford the costs of postsecondary education (Goldrick-Rab et al., 2019; Hanushek et al., 2019; Jury et al., 2017).

Over the past 20 years, promise programs emerged and expanded rapidly as a strategy to increase postsecondary affordability and generate new additional graduates (Perna & Leigh, 2017). Studies have individually examined the effects of a limited selection of these programs finding largely positive evidence on their effectiveness (Dynarski, Page, & Scott-Clayton, 2023). However, there is currently no single estimate

for the potential effect of these programs. This is important not only as state and institutional policy makers consider adopting new or altering existing promise programs but also has implications at the national policy level as attempts to institute a national free college model have been repeatedly proposed by federal policy makers (Carnevale, Sablan, Gulish, Quinn, & Cinquegrani, 2020).

Purpose of the Study

This study seeks to provide the first national estimate on the effect of promise programs on sub-baccalaureate awards produced by postsecondary institutions. To accomplish this, I will utilize a staggered differences-in-differences design to examine pre- and post- promise program adoption award counts as compared to non-adopters for promise programs at colleges with sub-baccalaureate programs. I utilize completions award count data from the Integrated Postsecondary Education Data System (IPEDS) from 2000 through 2022 as my outcome variable to examine changes in number of awards produced from sub-baccalaureate programs. I utilize a treatment data set constructed from CollegePromise.org as the most holistic inventory of college promise programs to date to identify treated units. To identify the first treatment year of treated units, I independently identified the adoption date of each promise program through searching institutional and program press releases and local newspapers reporting on program adoption. Later in Chapter 3, I provide further detail on how I constructed my data set containing both treatment and outcome data.

In addition to examining sub-baccalaureate production, I also want to attend to two additional dimensions of interest: program length and academic area of focus. I find

these issues to be important caveats to include in my study due to the aforementioned issues with student substitution behavior in postsecondary education choice. In regards the program length, it is important to understand and assess for effects on both credentials and two-year degrees separately to understand if when a path to a free two-year degree becomes available, credential program completions are influenced. In regard to program area of focus, it is important to understand if students adjust their courses of study when presented with free college programs. Thus, including these two dimensions help me to better understand how free college programs affect sub-baccalaureate completions beyond a simple measure if they effect the number of awards produced as a single aggregate.

Research Questions

The research questions of this study are:

- What effect does the adoption of a free college program have on postsecondary program completions by program length (less than 1 year, 1 year to 2 years, and 2 years)?
- What effect does the adoption of a free college program have on postsecondary program completions by program's academic focus?

Significance of the Study

While promise program research has been an area of active research, there has yet to be a national level estimate of the effect of promise program adoption on program completions. The lack of this type of study is likely in part due to promise programs being implemented (i.e., treatment turning on) at different times and colleges having

different unit-specific time trends as treated and non-treated units. However, recent advances in difference-in-difference designs have allowed for projects to investigate heterogenous treatment adoption and unit-specific time trends.

This study will utilize these recent advancements in staggered difference-in-difference designs to account for these data concerns. This approach will allow me to provide a national estimate on the effect of promise program adoption on completion counts for sub-baccalaureate awards. This estimate will offer considerable utility to both policymakers and institutional leaders.

This is because despite these programs being billed as free college programs, these programs are not free to run. When a college, institutional system, or state sector of higher education adopts free college for students, the cost of their education is effectively off boarded to another entity. For example, the Kalamazoo Promise does this through a fund started by anonymous donors while Tennessee Promise covers costs through a combination of federally awarded aid such as Pell grants and funds generated through a state lottery.

When promise programs were newly emerging, it was difficult to know how free college might affect student outcomes. Currently, the possibility of finding an estimate would be useful to leaders in postsecondary education where this knowledge could be utilized by communities, states, and the nation as leaders consider the benefits and costs of adopting a free college program. This is of increased importance as promise programs continue to proliferate with 83 percent of promise programs identified for inclusion of this study being started during or after 2015.

Using the sample identified for this study, I identified 73 operating promise programs in 2015, the year that Tennessee launched its state-wide promise program and promise programs became a regularly reoccurring federal education policy issue of interest. Presently, I identified 449 promise programs for my sample that were in operation as of 2022 for this study. This represents approximately a 615% increase in the number of operating promise programs from 2015 to 2022, a seven-year time span. This growth represents an excellent opportunity to examine the effect of promise programs at a national level as the sample of treated units has widely expanded recently. Further, as these programs continue to increase in popularity, it would be critical to examine the effects these programs have on students in aggregate.

Chapter 1 Summary

As the United States economy's demand for educated workers continues to grow, colleges serve an important function in maintaining workforce supply. Rising costs in postsecondary education limit who can take advantage of educational opportunities and thus who can access these jobs which have increasing demand and typically higher wages. Free college or promise programs emerged as a financial aid tool to address issues pertaining to cost. However, there has yet to be a national study of the effect of these programs regarding the number of credentials and degrees produced.

In this manuscript, I conduct a staggered difference-in-difference study to examine the effect of promise program adoption on the number of sub-baccalaureate credentials and degrees produced. In Chapter 2, I provide a review of financial aid policy and empirical research on financial aid tools including grant aid, loans, and merit

scholarships in addition to promise programs to introduce context surrounding these programs. Following this I present my empirical strategy for examining this issue. This is followed by a presentation of my results and discussion of those results in the context of research and practice in Chapter 4 and Chapter 5 respectively.

CHAPTER 2

This chapter reviews literature and research relevant to this study's examination of the effect of free college on sub-baccalaureate award completion. This chapter begins with a broad review of the landscape and research regarding financial aid programs including need-based (e.g., grant), merit, and loan-based aid programs as a means for students to afford higher education in the United States. Following this, I narrow in focus to examine promise programs as a financial aid tool.

Review of the Progressing Need for Student Financial Aid

Before examining the effect of promise programs on award completion, it is important to establish why various financial aid programs and specifically promise programs emerged to address the affordability of postsecondary education in the United States. I open Chapter 2 with a review of both the federal and state governments' roles in shaping financial aid policy to broadly introduce the financial aid programs discussed in this chapter. This is then followed by a separate discussion on relevant empirical research on these financial aid programs. I did this to provide a discussion of empirical research that is focused on methodological approaches and causal effects where I did not need to introduce each type of financial aid program studied.

This method also provided additional benefit in that readers that are more familiar with these programs could move ahead to sections discussing the empirical research on the effect of financial aid programs. I then also repeated this strategy for promise programs in the second half of this chapter. While my study focuses on promise programs only, I chose to provide a detailed historical overview of these other programs because a

significant portion of the body of research on financial aid has been on these programs. Further, given the wide range of differences between individual promise programs in my sample (e.g., last dollar vs. first dollar), it would be beneficial to understand the effects and influences of as many aid programs as possible for my study sample. Finally, the students within my treated and non-treated units will have received other forms of financial aid such as a Pell grant, merit aid, and loans. Thus, reviewing the established literature on these other forms of aid provides an excellence reference of past research for teasing out the potential effects from my staggered difference-in-difference models.

The Federal Government's Role in Student Financial Aid

The authorization of the Servicemen's Readjustment Act (1944) and changing views on who should attend higher education in the United States following World War II marked a notable point of interest for examining how students afford higher education. As early as 1942, the federal government was examining the impact of an estimated 15 million men and women re-entering the US workforce following the end of the war. Before this, students were largely reliant on isolated financial aid systems at institutions that provided scholarships, loans, or other forms of charity/sponsorship if they were not able to afford higher education outright (Fuller, 2014). Until the authorization of the Serviceman's Readjustment Act, federal involvement in higher education had been limited to the Morrill Land Grant Act (Bickell, 2022) which provided a means for states to finance public education systems through the selling of federally owned lands.

Following World War II, Congress passed a suite of benefits for veterans to facilitate a smooth transition back to civilian life. This was done to prevent an influx of

available labor comprised of unemployed servicemen from causing a second economic depression through an over-availability of laborers in the labor market. Colloquially known as the G.I. Bill of Rights, The Serviceman's Readjustment Act of 1944 provided benefits to veterans including healthcare, housing/business loans, and education. Specifically, this educational benefit spurred postsecondary enrollment and graduates to nearly double from 1.15 million to 2.45 million between 1940 and 1950 and then grow to 3.6 million by 1960 (Labaree, 2016; Snyder, 1993).

This approach to funding students directly rather than institutions established a framework for funding the cost of higher education that has persisted over the following 80 years. The federal government would introduce additional campus-level aid packages outside this framework in the form of Federal Work-Study in 1964, Supplemental Educational Opportunity Grants in 1972, and Perkins Grants in 1984 (Carl. D. Perkins Career and Technical Education Act, 1984; Economic Opportunity Act, 1964; Higher Education Act, 1972). However, spending on these programs are less that other programs discussed in this section. For example, in fiscal year 2024, the US Department of Education requested $32,478 million for Pell grants and $26,592 million for Federal direct loans while only requesting $910 million for Supplemental Education Opportunity Grants and $1,230 million for Federal Work-Study (United States Department of Education, 2023). Because of the respective current scope of these programs, I chose to primarily focus on portable-student aid such as the Pell grant, loans, and some but not all promise programs during the sections reviewing empirical financial aid research.

Following this, the federal government became further entangled in the financing of postsecondary education during the 1960s. President Johnson's suite of Great Society initiatives included the passage of The Higher Education Authorization Act (1965). This bill and its following reauthorizations have brought with it Title IV funds for students and eligibility requirements for institutions to receive these federally provided student dollars. Title IV introduced federally provided grants and loans to students. Grants were provided through programs proposed by local campuses for how institutions would spend money locally on authorized programs including Higher Education Opportunity Grants and College Work Study. Meanwhile, federally provided loans were portable with respect to students meaning that students applied directly for funds and took these funds with them to their institution of attendance, echoing the government's previous approach to financing postsecondary education for servicemen following World War II.

Hearn (2001) examined historical documents on the debates regarding expansion of portable student aid and grants provided to campuses leading up to the 1972 reauthorization of The Higher Education Act. Hearn describes the two primary opposing views on this debate as:

1. portable aid advocates argued increased student portable aid would increase student choice between institutions by having the government calculate a student's eligibility for aid and then offer the student a lump sum they could take to any eligible institutions while
2. campus grant advocates believed that the government's role in providing financial aid should be that of a facilitator rather than funder and provide an opportunity for

equal access to postsecondary education through specifically targeting the lowest income students.

Following debate, congress responded to increasing advocacy for student choice by shifting toward portable aid leading up to the second authorization of The Higher Education Act. This effectively decided that the federal government had a role in regulating how the aid monies it provided were dispersed to students through its existing loan programs and the then newly introduced Pell Grant. While campus grant programs such as federal work study remained intact, Hearn's (2001) review of federal spending levels across aid programs from 1963 to 1990 following this philosophical shift toward student choice in 1972 found that student participation and the costs of portable aid quickly outpaced campus provided grants.

Following this, the 1970s saw renewed discussion about if the federal government should support higher education by financing students' costs or by providing institutional funds through grants. These approaches were introduced by the Senate and House respectively during Nixon's presidency. The Senate's bill introduced the basic educational opportunity grant that would later become the Pell Grant program. Following a report from the National Commission on the Financing of Postsecondary Education which found higher enrollments were tied to portable student aid rather than institutional support. The Senate's and House's bills were reconciled to further push federal involvement in postsecondary finance toward portable student aid. At this time, the amount of portable grants as compared to loans for the number of enrolling students was

high and gaps in postsecondary enrollment by race nearly disappeared (St. John, Daun-Barnett, & Moronski-Chapman, 2018).

During this time the price of higher education began to rise, and President Carter signed off on postsecondary education tax credits for families of students middle-income and higher-income students in 1978. This resulted in students from middle-income and higher-income families accessing more expensive institutions using a combination of guaranteed student loans and the tax credit to access higher education more affordably than before. As institutions shifted this aid away from low-income students to middle-income students, racial enrollment gaps reappeared in United States postsecondary education (St John et al., 2018).

Following this period of expansion for federal student funding in higher education and its subsequent increased cost, President Reagan claimed that universities had pursued increased profits by raising prices billed to students who were using the federal government's portable aid. This belief, which is known as the Bennett Hypothesis, has received some challenges to its legitimacy from empirical research. For example, Kelchen (2020) found little consistent evidence of increased tuition and fee billing when using an interrupted time series approach to study the effect of access to student aid following an expansion for federally provided graduate student aid in 2006.

More broadly, other researchers found results ranging from null evidence to moderate evidence in support of the Bennett hypothesis or a modified version of it as provided by Gillen in 2012 (Barid et al., 2019; Cellini & Goldin, 2014; Gillen, 2012; Lau, 2014; Lucca, Nadauld, & Shen, 2015; Rizzo & Ehrenberg, 2003; Singell & Stone, 2007;

Miller-Adams, 2021). Under the guidance of the claim that colleges were wasteful, the federal government shifted away from funding students through Pell Grants toward federal and private loans for students to afford higher education. More recently, incremental policy changes such as a 1992 change in financial need calculations that introduced an exclusion for home equity from calculations have directed more federal dollars toward middle-income and higher-income students (Schenet, 1993).

St. John et al. (2018) described this shift from a post-War World II, structural model with grants provided to institutions to the current, neo-liberal free-market model where students apply directly for funding that they take with them to institutions in further detail than is necessary to review here. However, a key-take away of their historical review is that while the free-market model has coincided with increased enrollment by headcount as higher education access has expanded, there has also been a downside in that this access to a postsecondary education and its subsequent degree production have been inequitably distributed across racial and socio-economic lines.

State Governments' Role in Student Financial Aid

While the federal government's involvement in financing postsecondary education is limited to student aid and federally provided grant-related activities, states play a more direct role in financing higher education. Historically, states provide the land, facilities, and appropriations for public colleges and universities to operate (Dougherty, 2004). Although, through the 19th century, much of this state-provided revenue did originate with the federal government's Morrill Land Grant Act of 1862 which allowed states to sell land to afford operating their land-grant institutions (Croft, 2019).

Consequently, access to this funding stream was primarily provided to now currently predominately White institutions (PWI) which were previously racially segregated during this time. This history contributes to current financial inequity between PWI land-grant campuses and public, historically Black colleges and universities today.

Regarding higher education, this system primarily financed a specific institution designated as a land-grant institution in each state for the purposes of instruction in agricultural and mechanical programs. At this time revenue streams for institutions were limited, and institutions charged nominal but non-zero tuition amounts. Thus, by merit of existence these charges could preclude poorer students from attendance due to cost. As researched by Heller (2006), tuition and fee rates during the mid-1800s included charges of $12 at the University Wisconsin in 1855, $10 at the University of Tennessee in 1866, $5 at the University of Illinois in 1868, $15 at Ohio State University in 1874, and $5 at the University of Missouri in 1874. During this time, a limited number of scholarships were issued by institutions. However, these opportunities were limited by a low demand fueled by a fledgling primary/secondary education system, a lower economic demand for college graduates, and unequal opportunities for accessing postsecondary education across racial and socio-economic groups (Goldin & Katz, 1999).

This of course changed following the introduction of the Serviceman's Readjustment Act of 1942 and the subsequent Baby Boom of the 1950s/60s that drove increased enrollments and federal grant/loan investments in higher education and its students. At this time, state policy favored spending funds so that all students wishing to pursue higher education were subsidized by low costs insured through state

appropriations (Fuller, 2014). However, this was followed by what Heller (2006) describes as a boom or bust cycle between 1966 and 1996 where state appropriations per student fluctuated, falling from a high-water mark between $4,750 and $5,000 per student in 1974 to between $4,250 and $4,500 in 1996. More recently, this downward trend was exacerbated by an economic recession in 2008 that resulted in a 23% decrease in state appropriations per full-time equivalent student (Mitchell, Palacious, & Leachman, 2014).

The 2008 Recession stagnated state spending on postsecondary education appropriations per full-time equivalent (FTE) student. While the State Higher Education Executive Officers Association found that educational appropriations increased by 4.9 percent beyond inflation in 2022, they simultaneously reported this is the first time that per-FTE spending was higher than pre-recession appropriations (Kunkle & Laderman, 2023). The reduction and stagnation in state spending on higher education was long standing with 10 years later the average state per-FTE spending being $8,686 in 2017 compared to $9,752 in 2007 (Kunkle & Laderman, 2023). Compared to the pre-2008 recession levels, postsecondary education appropriations in 2022 were on average approximately $304 more per FTE student (Kunkle & Laderman, 2023). However, more than half of the states have not recovered from recession era cuts with 28 states still spending less on educational appropriations per FTE than before the Great Recession (Kunkle & Laderman, 2023).

These inconsistent levels of per-student state spending dovetail with the federal government's transition to primarily fund portable student aid, resulting in the foundations of a postsecondary education system that is primarily funded by students as

consumers of education rather than a government-provided service. As of fiscal year 2022, the national average of educational appropriations was $10,141 for 2-year colleges and $9,596 for 4-year colleges while the national average for state-level student aid spending was $562 and $1,216 for two-year and four-year colleges respectively (Kunkle & Laderman, 2023). Growth in state spending on financial aid per full time equivalent student increased by 79.3 percent since 2001 from an average of $552 to $990 (Kunkle & Laderman, 2023).

However, average educational appropriations per full time equivalent student experienced a 6.4 percent decrease from $10,983 per full time equivalent student in 2001 to $10,237 per full time equivalent student in 2022 (Kunkle & Laderman, 2023). Simultaneously, the average net tuition revenue obtained from students grew by 62.9 percent since 2001 from $4,447 to $7,244 (Kunkle & Laderman, 2023). In sum, as the cost of higher education increased, state spending did not keep pace with growing cost during and after the Great Recession until this most recent, past fiscal year. In the meantime, the cost of higher education has been passed onto students and their families through cost of attendance.

State policy has also shaped this branch of financial aid significantly. During this time, most states maintained affordable postsecondary education primarily through a combination of low tuition rates and need-based scholarships (Dynarski, 2004; Heller & Marin, 2002). Also, state financial aid programs were incentivized to be need-based due to the State Student Incentive Grant Program, a federal matching program that offered additional funds to states that operated locally focused, need based aid programs

(Archibald, 2002; Fuller, 2014). In 1993, Georgia changed this trend through the introduction of a lottery-funded, merit scholarship for state residents to use at in-state institutions. By 2021, 16 other states had adopted this merit scholarship model which are often funded from sales for a state-run lottery, land leases, or taxes on substances such as tobacco (Jamieson et al., 2021). The Education Commission has identified 40 state financial aid programs with some type of merit component across 32 states which suggests that merit-aid has become increasingly popular following its adoption by Georgia in the 1990s. Further, the state investment in merit aid has outpaced need-based aid in terms of growth. Over the past decade, need-based aid increased by 37.2% while non-need-based aid in the United States increased by 41.8% (National Association for State Student Grant & Aid Programs, 2023). Currently, non-need-based aid such as merit or lottery scholarships have grown to represent a little over a quarter of all aid funding at 26.6% of all financial aid (National Association for State Student Grant & Aid Programs, 2023).

Heller and Marin (2002) stated that this shifting from need-based to merit aid represents a “shifting [of] funds from Blacks, Latinos, and Native Americans to Whites and Asians, from city and rural residents to suburban residents, from children from one-parent families to those who have two parents” (p. xiii). Arguably, this represents a socially regressive reallocation of state managed student portable aid. Under the rise of merit-aid during the late 1990s and 2000s, state aid was directed toward better resourced students who had the means and support to readily meet merit aid scholarship requirements.

The Current Reality of Postsecondary Affordability

This consumer-centric model for affording postsecondary education places an increased importance on financial aid as public institutions increase prices in response to lower levels of per-student state support and rising operating costs (National Center for Education Statistics, 2024c; Weerts & Ronca, 2006). While students from higher-income families will continue to access postsecondary education without issue, lower- and middle-income families experience intensified pressure to close the gap between institutional costs and their provided aid packages. This affordability issue impacts students in two critical ways. First, it can affect who decides to attend postsecondary education in the United States. Second, it affects the ability of students to persist to a degree while enrolled. In the following sections on different aid types, I review how this first factor affects enrollment for each aid type and close this section out by briefly outlining the issues related to persistence, completion, and other post-graduation outcomes.

Many low-income students, particularly those enrolled in community college, attempt to address this gap by balancing employment on top of academics to offset costs of living and enrollment (McCormick, Moore, & Kuh, 2010). Student responses to the National Postsecondary Student Aid Study (2020) indicate that 69.5% of undergraduate students that enrolled mostly full-time as students at public, two-year or less institutions had some form of employment, excluding work-study or fellowships. Horn et al. (2006) found over half of working undergraduates express their employment as a necessity, suggesting that many students cannot afford these costs without funds generated from

extracurricular employment. Horn et al. (2006) also found that a quarter of students in their study describe themselves as employees first and students second.

Further, higher income students struggle less than their low- and middle-income peers to absorb these increased costs. In a Brookings Institution report, Whitmore Schanzenbach et al. (2017) identified that nearly a quarter of low-income students at four-year institutions stop-out by the end of their second year. These findings were more ubiquitous at community colleges, with 49% of students who stopped out in 2023 citing a need to work as the leading reason they discontinued enrollment (Nguyen & Cheche, 2024). This aligns with existing research that higher education students who work less than 20 hours per week earn significantly better grades than students who work more than 20 hours per week (Pike, Kuh, & Massa-McKinley, 2009).

These costs burden students and families, placing them in precarious financial situations. The Hope Center for College, Community, and Justice (2021) found that 3 in 5 students from a 2020 sample of 195,000 experienced a basic needs insecurity (i.e., lack of access to food, housing, utilities, clothing, etc. due to a lack of funds). This sample consisted of students who were actively enrolled and responded to the survey invitation at 130 two-year colleges and 72 four-year colleges and primarily measured access to food, access to housing, and homelessness. Of these sampled students, 39% and 29% were affected by food insecurity at two-year and four-year colleges respectively. Regarding housing, 48% were affected by housing insecurity and 14% were experiencing homelessness.

In addition to the Hope Center's basic needs survey results, the National Postsecondary Student Aid Study provides a national level data set on student basic needs. The National Postsecondary Student Aid Study (2020) estimates 23% of all postsecondary students experienced food insecurity respectively within 30 days of survey. This survey also found that 7.3 percent reported experiencing homelessness within 30 days of being surveyed (National Postsecondary Student Aid Study, 2020).

This reflects the reality of a consumer-centric model of higher education in a country where better-paying jobs often require a postsecondary credential or degree. When costs increase in this system, they are passed onto lower-income students who must make difficult choices about whether to pursue or continue pursuing higher education. Through access to families' financial resources, middle-income and higher-income students likely experience less of a need to work while attending school or basic needs insecurities than their lower-income peers. Further, it is important to note the looming student loan crisis still affects over 43 million Americans with over $1.7 trillion in debt (Board of Governors of the Federal Reserve System, 2024). These symptoms of higher education's affordability crisis such as basic needs insecurity and loan debt, are outcomes of the policies used to finance public higher education in the United States and contribute to an increased importance for in-kind financial aid such as the Pell Grant and promise programs such as those studied in my study.

Empirical Research Examining Financial Aid and Student Outcomes

In this section, I provide a review of empirical research for financial aid and the student outcomes such as enrollment and degree completion. While my study does not

consider enrollment as an outcome of interest, I include it in this review to communicate how financial aid shapes the whole of the postsecondary student experience from the decision to enroll through persisting to degree completion. Following this section, I examine promise programs separately as a more recent evolution in the landscape of financial aid.

I have included research on grant, merit, and loan-based financial aid in this review of student outcomes. I choose to do this because promise program students may still receive other non-promise funds. Because students represented in my sample by institutional completion counts have received varying types of aid, this addition helps to more holistically understand the effect student aid has on student outcomes.

Financial Aid and Enrollment

Efforts to research the relationship between postsecondary enrollment, cost of attendance, and financial aid have been long-standing for over 40 years across a variety of different aid types with early research lacking the experimental design necessary to address omitted variable bias or reverse causation (Dynarski, 2002; Leslie & Brinkman, 1988). Dynarski et al. (2023) notes that more recent methodological approaches using experimental and quasi-experimental methods offer findings that are more sound than past research regarding the effects of financial aid on student outcomes. In this review, I center these studies as the best evidence to date that informed my study.

Grant-Based Aid (Enrollment)

Using difference-in-difference analysis of the now-canceled Social Security Student Benefit Program, Dynarski (2003) estimated that an increase of $1,000 in grant

aid can correspond to a 3.6% increase in the probability of a student attending college. To examine the loss of $6,700 received annually by students of deceased, disabled, or retired Social Security beneficiaries while enrolled full-time in college, Dynarski utilized the death of a parent during childhood as a proxy for program participation before its closure in 1982. This allowed Dynarski to compare students' decisions to enroll in postsecondary education based on eligibility (i.e., intent to treat) before and after program closure while examining the proxy as an indicator for program eligibility.

Using difference-in-difference, Abraham and Clark (2006) as well as Kane (2007) studied Washington, D.C.'s Tuition Assistance Grant which covers the difference between out-of-state and in-state tuition at primarily public institutions. D.C. has three public institutions with a combined enrollment of less than 6,000, two of which are limited in enrollment to a special focus such as national security. This means that without this program, most D.C. residents would need to pay a higher out-of-state tuition to access public postsecondary education. Both of these D.C. Tuition Assistance Grant studies take advantage of the start of this program for D.C. students and compare them with students from the bordering states of Virginia or Maryland (Kane T. , 2007) and of similar cities (Abraham & Clark, 2006). These studies found that each $1,000 of increased aid increased enrollment by between 5 and 6 percentage points for D.C. residents when compared to Maryland or Virginia (Kane T. , 2007) and approximately 3.6 percentage points for D.C. residents when compared to similar cities (Abraham & Clark, 2006).

However, findings from the literature on studies on the country's largest financial aid grant program are more mixed (Nguyen, Kramer, & Evans, 2019). Researchers experience some difficulty in teasing out the causal effects of the Pell grant because studies have utilized relatively small changes in potential award amounts, approximately $600 according to Nguyen et al.'s (2019) meta-analysis, as the treatment variable. As of the 2023-2024 academic year, the Department of Education awarded a maximum Pell grant of $7,395. With researchers often measuring the impact of Pell grant awards in percent change per $1,000 received in Pell grant aid, observational data does not provide a complete picture for the potential impact of increasing Pell grant aid. Further, the tendency for students to substitute away other forms of aid such as loans or reduce their work hours further complicates empirical research on this topic (Evans B. J., 2019).

Early research on the Pell grant could not identify a clear effect on enrollment by studying trends in family income before and after the program introduction (Hansen, 1983; Kane, 1996). Carruthers and Welch (2019) utilized regression discontinuity to identify that Pell grants did not produce a significant effect on postsecondary enrollment but rather influenced student decisions on where they would enroll. Carruthers and Welch found that compared to similar students that were not Pell eligible that Pell eligible students on average attended institutions that charged $116 more tuition per $1,000 in Pell grant received.

Another study using regression discontinuity found that students who receive the Pell Grant are 4 to 7 percentage points more likely to enroll full-time in college than peers that are close to the Expected Family Contribution cut-off for the award but did not

receive the Pell grant award (Park & Scott-Clayton, 2018). Both the comparison group and treatment group for this study were within $2,000 of the EFC cut-off. The treated group received an average award of $1,261 from Pell funds. While limited to a single, undisclosed state, Park and Scott-Clayton were able to find this effect by isolating students who had Expected Family Contributions that were within $2,000 of that year's cutoff for Pell Grant eligibility. This then provided Park and Scott-Clayton with financially similar students for analysis from which they could control for the influence of other observable characteristics such as race/ethnicity, gender, age, dependency status, and academic preparedness.

Merit-Based Aid (Enrollment)

While older programs such as the Pell grant utilize estimated student need in their award structures, many newer programs focus more heavily on merit. These aid programs center the higher education affordability concerns for middle-income and higher-income families. These merit-aid policies have in effect directed state funds toward middle-income students and families that may not qualify for federally provided need-based awards.

Since the early 2000s, researchers have studied merit aid programs such as Georgia's HOPE scholarship using rigorous methods. An early study by Dynarski (2000) found that each $1,000 in merit aid received through the Georgia HOPE scholarship in the 1990s increased college attendance rates by 3.7 to 4.2 percentage points. Within these findings, these increases in enrollment were most concentrated among students from upper- and middle-income families. Dynarski (2000) also concluded that the introduction

of this merit aid program widened enrollment gaps for Black and White students from low- and high-income families. Despite this, other states adopted similar programs through the policy diffusion process (Baum & Schwartz, 1988; Dynarski, 2004).

Bruce and Caruthers (2014) later studied Tennessee's iteration of the HOPE scholarship using a Regression Discontinuity. They found no evidence that Tennessee's HOPE scholarship affected the college-going of those who barely qualified for the scholarship with those who barely did not. However, Bruce and Carruthers did find evidence that students who just qualified for the HOPE scholarship utilize that aid to attend four-year colleges at a higher rate than their non-eligible peers who enroll at higher rates in more affordable community colleges. This echoes previously established findings with the Georgia HOPE Scholarship, that merit aid largely did not generate new enrollments from increasing affordability but rather altered the enrollment patterns of students who were already likely to enroll in higher education (Cornwell, Mustard, & Sridhar, 2006).

Loan-Based Aid (Enrollment)

Researchers investigating loans and college enrollment largely study the effect of student access to credit on college enrollment. Further, this access to credit does not need to be restricted to a traditional student loan in order to affect enrollment. For example, Lovenheim (2011) found that students from families with access to liquid housing equity enrolled in college at a rate 0.7 percentage points higher for each $10,000 available in liquid equity on average than others without access to this credit. Lovenheim also found enrollment increased by 5.7 percentage points per $10,000 increase among students from

low- and medium-income families. This difference between the overall average and low- and middle-income effects reflects the reality that access to credit is an issue that most heavily influences the enrollment decisions of students who are not higher-income.

Regarding student loans specifically, access to loan credit has been found to influence enrollment decisions. In 1992, a reauthorization of the Higher Education Act created the Stafford Unsubsidized Loan Program to expand loan access to students and families that were previously ineligible to federal student loans. Long (2007) utilized a difference-in-difference approach to examine the effect of this increase in credit access. Long utilized home values as a proxy for the degrees of which families were previously and currently eligible for federal aid before the policy change in 1992. Long (2007) found that per $10,000 in home value, enrollment rates increased 1.4 percentage points following increased loan access that was no longer constricted to lower-income students. Long's (2007) findings also align with earlier research on the same policy change where Dynarski (2003) found that this increase in loan access may have shifted higher-income students toward private, four-year colleges.

This shift also appears to have encouraged additional full-time enrollment with a 2.24 percentage point increase per $10,000 in increased home value following the policy change. Wiederspan (2016) studied the decision of community colleges to opt out of the Stafford Loan program using administrative records from more than 50 community colleges in an instrumental variable design. In this study, the decision to participate in the Stafford Loan program was used as an instrument for predicting the probability of a student accepting a Stafford Loan. Wiederspan (2016) found that Pell-eligible students at

community colleges that offered Stafford Loans were 7.6 percentage points more likely to borrow than the same population of students at community colleges that did not have access to the Stafford Loan. These students with access to Stafford Loans earned 1.4 more credits than peers without access to Stafford Loans.

Overall, research shows that access to student aid funding can positively affect enrollment regarding the effect of student loans on enrollment intensity and credits earned. However, it is also important to highlight two examples where researchers found no effect on the decision to enroll when access to loans were considered. Marx and Turner (2019) studied the effect of including student loans in a student's award letter for a large US community college. While Marx and Turner found that increased access to loan awards were linked to a 30 percent increase in student GPA and number of credits earned, there was no effect on enrollment. After randomly assigning students between groups that received a loan offer and a control group with no loan offer, Marx and Turner found that the inclusion of the loan offer did not affect the enrollment rates of new or returning students.

Additionally, Denning and Turner (2019) utilized difference-in-difference design to examine the effect of the Graduate PLUS Student Loan program on access to high-earnings graduate programs in Texas and found that access to additional loan awards did not affect the enrollment of students overall or in underrepresented groups to academic programs associated with high-earnings. These studies which examine the effect of loan program expansion on generating new enrollments are important to note here as they suggest that other studies here inadvertently found changes in existing patterns of

enrollment rather than the generation of additional, new student enrollment due to loan access.

Financial Aid, Graduation, and Other Outcomes

While the effects of financial aid on enrollment are noteworthy, its effects on post enrollment outcomes such as graduation rates and credit attainment are also important as prerequisites to access the benefits associated with postsecondary credential/degree attainment. More recently, these post-enrollment outcomes have been grouped together under the label of student success with institutions increasingly tracking the metrics related to these outcomes such as on-time graduation, retention to degree attainment, and credit accumulation. Like enrollment, researchers studying postsecondary education have conducted numerous rigorous, quasi-experimental studies to examine not only for the relationship between aid and these outcomes but also for the estimated effect of dollars spent on graduation.

Grant-Based Aid (Graduation and Other Outcomes)

Focusing on grant-based aid, the positive effects of this type of student aid are well documented. Nguyen et al. (2023) conducted a meta-analysis of 43 studies using causal inference methods to identify an increase between 1.5 and 2 percentage points in likelihood for student persistence to degree completion per $1,000 awarded under an assumed linear relationship. This relationship represents a viable policy lever to increase degree completion as Nguyen et al.'s model estimated that spending an additional $50,000 - $67,000 in aid spent can produce an additional graduate. As costs for higher

education continue to grow, increases in grant-based aid represent a potential strategy to maintain socio-economic diversity and by extension racial diversity in higher education.

While the return on investment in increased degree production may appear low at a 2 percentage point increase, the government's return on investment for the use of tax dollars on grant aid money has been documented to produce $85,000 in post-graduation savings per graduate which is greater than the expenditures on higher education, roughly $74,500 per graduate (Trostel, 2010). Further, Trostel (2010) estimated that a weighted average of graduates by degree type generated an extra $470,000 in income, payroll, property, and sales-tax revenue over their lifetime as compared to non-graduates. In summary, need-based grant aid represents an investment in students that returns a consistent albeit modest return in degree production, but produces strong financial returns per degree produced in saved spending and future potential tax revenue.

Regarding the Pell Grant specifically, researchers have repeatedly identified positive effects for receiving this aid on outcomes leading to graduation. Bettinger (2004) studied student persistence in returning Pell Grant recipients using discontinuities in awards. Bettinger found that a $1,000 increase in Pell grants is associated with a 3-percentage point reduction in the likelihood a student would stop-out and 4 percentage point reduction in the likelihood a student would formally withdraw from the university.

Later, both Park and Scott-Clayton (2018) and Denning et al. (2019) utilized regression discontinuity based on the cut-off for Pell Grant eligibility. These studies both found that students receiving the grant compared to similar students that do not receive the scholarship decrease the hours worked each week while increasing their credit hour

enrollment for that semester. In particular, Denning et al. (2019) found within a sample of students from Texas public colleges that Pell grant eligibility resulted in a 1.5 percentage point increase in on-time graduation rates and 3.3 percentage point increase for graduation rates within 150% of the estimated length of the program.

This study by Denning et al. (2019) also shared similarities to another study by Eng and Matsudaira (2021) which also utilized the same Pell grant income cut-offs to establish similar effect sizes. Research teams found increases in both degree attainment and post-college earnings. In a sample limited to students enrolled in Texas public schools, Denning et al. (2019) found that for each $1,000 in grant aid the probability of graduating increased 5 percentage points (a 15% increase) and that post-graduation earnings increased $1,000 in later-year earnings (a 4.8% increase). In a nationwide sample, Eng and Matsudaira (2021) found each $1,000 increase in grant aid resulted in a 1.5 percentage point increase in degree attainment (0.5% increase) and a decrease in earnings of 12.3 percent. However, Eng and Matsudaira do note that state-to-state variation in the award systems of their nationwide sample as compared to Denning et al.'s (2019) Texas-only sample may contribute to these differences.

Also, of interest regarding Eng and Matsubaira's (2021) nationwide study is the use of the 2002 through 2010 entry cohorts of postsecondary first time in college students to examine post-graduation income while not noting that the earliest graduates of this sample graduated in 2006 shortly before an economic recession began in 2007. While this recession was declared over in 2009, its impacts on unemployment and wages affected the labor market for years to come. It would be 7 years later, after many students within

Eng and Matsubaira's sample had entered the workforce before total employment would reach pre-recession levels in the US economy (US Bureau of Labor Statistics, 2023). By comparison, Denning et al.'s (2019) sample utilized students from the 2008 through 2011 entry cohorts which mostly graduated into a recovering labor market. However, it was not until 2017 that enough new workers were able to re-enter the economy to account for retiring and laid off-workers to return the nation to pre-recession labor market-conditions (Whitmore Schanzenbach et al., 2017). This is an important and surprising caveat that goes unaddressed by Eng and Matsudaira (2021) where tough labor market conditions for workers could have biased earning data downward in their study.

Despite these promising results, Dynarski et al. (2023) posits that complex relationships between educational costs and grant aid can reduce or otherwise obscure effects on student outcomes. An example of this is noted by Turner (2014) that found private institutions responded by awarding students with Pell grants less institutional financial aid as compared with students who barely do not qualify for the grant. Later, Marx and Turner (2019) found students that are not Pell eligible received more financial aid from all potential sources combined than Pell eligible students. Additionally, Bettinger and Williams (2014) found that states have responded to increases in Pell funding by changing state awards; usually reducing students' state awards when Pell awards increased. Further Park and Scott-Clayton (2018) found that community colleges move state grant dollars toward students that are not Pell eligible to assist with their costs rather than directing aid toward Pell eligible students. Both Marx and Turner (2019) as well as Park and Scott-Clayton (2018) suggest that additional aid is crowded out by the

Pell grant as institutions attempt to offer the more moderate aid packages across as many potential students as they can to maximize enrollment yield.

Moving beyond student outcomes for grant aid, some studies have examined the post-college outcomes of grant aid receipt. Bettinger et al. (2019) utilized regression discontinuity to study California's Cal Grant, an award that is simultaneously need- and merit-based. They find when compared to similar students that did not qualify for the Cal Grant, that compared to students near the GPA and income at cutoffs, participants had not only a higher rate of degree attainment but also higher rates of graduate degree attainment and post-college earnings. Anderson et al. (2020) studied the Wisconsin Scholars Grant that offers up to $17,500 over five years for Pell eligible students at Wisconsin public colleges. This grant is privately funded in contrast to the other grant aid reviewed in this proposal. Due to the design of the program which functions like an extension of Pell funds for eligible students, I choose to include it here in my review of grant aid impacts. Anderson et al. (2020) found that the grant funding had no effect on persistence, completion, or graduate enrollment as a post-college outcome.

However, this could be due the fact that students eligible for this grant were already eligible for other types of student aid such as loans. In a previous study on the Wisconsin Scholars Grant, Goldrick-Rab et al. (2016) note that grant recipients reduced their loan uptake by accepting newly offered grant dollars. This is an important caveat to consider as these students' award amounts essentially did not change after they opted to accept grant aid without relying on loans which would place the students into debt. This finding is similar in nature to the crowding-out phenomenon noted in the earlier-

mentioned studies by Marx and Turner (2019) as well as Park and Scott-Clayton (2018). Except instead of an institution attempting to maximize yield across as many less generous aid packages as possible, students in the Wisconsin Scholars Grant effectively left aid money on the table by allowing grant-aid to crowd out loan-based aid to reduce or eliminate their potential debt balances.

Merit-Based Aid (Graduation and Other Outcomes)

Researchers studying the outcomes of merit-based aid such as state-wide scholarships tied to high school GPA and standardized test scores have mostly investigated differences in outcomes between recipients and non-recipients. Often using regression discontinuity to examine merit aid scholarships, researchers construct a treatment and control group of similar students by limiting their sample around the thresholds of the scholarship's initial award and renewal requirements. Additionally, while limited in number, some researchers have also studied the effects of losing merit aid on student outcomes.

Welch (2014) also examined the impact of merit aid using Tennessee's HOPE scholarship at the state's community colleges. Welch utilized a regression discontinuity design to compare students on either side of the scholarship's ACT score criteria to establish an effect for receiving the Tennessee HOPE scholarship. This study found that the Tennessee HOPE scholarship had no statistically significant impact on the number of semesters enrolled or the number of credits accumulated following a student's first year of enrollment. It did have a significant impact on hours attempted by the second year with

students who were HOPE eligible earning approximately 3.4 more credit hours over the course of two years than students who were not HOPE eligible.

An inherit goal of merit-aid scholarship is talent retainment for states. Dynarski (2004) noted this about merit aid programs created following the adoption of Georgia's HOPE scholarship. Here policy makers assumed that students planning to go out of state for college may choose to stay closer to home by reducing the cost of attendance at in state institutions. In addition to enticing students to stay in-state for college, policy makers hoped that this would make it harder for students to leave the state following graduation. This would allow the state to retain skilled laborers in which the state had likely been investing in since the student's enrollment in primary school. Thus, merit aid exists not only as a financial aid source but also as a state talent retention policy (Harrington, Muñoz, Curs, & Ehlert, 2016).

Scott-Clayton and Zafar (2019) utilized regression discontinuity to examine the post-college effects of West Virginia's merit-based scholarship program[1] on recipients. While Scott-Clayton and Zafar found West Virginia merit aid recipients did not significantly affect year-round employment, recipients earned more after graduation on average at $2,724. Scott-Clayton and Zafar calculate this to be a 7% increase over the mean earnings of students in their comparison group. Recipients are also more likely to earn graduate degrees, own a home, and live in neighborhoods with a higher average

[1] While this merit aid scholarship is referred to as the West Virginia Promise Scholarship, it is important to note that the scholarship's award criteria make it more similar to programs such as Tennessee and Georgia's HOPE scholarships rather than a traditional college promise program such as the Tennessee Promise or Kalamazoo Promise.

adjusted gross income when compared to similar non-recipients. Further, recipients borrowed $1,172 (17%) less and their families borrowed $1,020 (26%) less after four years.

Scott-Clayton and Zafar (2019) find no evidence that state merit-aid recipients in their sample are more likely to leave the state but are also not the first scholars to look at inter-state migration for merit-aid recipients. Sjoquist and Winters (2014) examined the effects of merit-aid scholarships across 25 states on post-college inter-state migration. Across all state programs, Sjoquist and Winters found merit aid programs increase the state's retention of post-graduate employees by 2.8 percentage points. This effect varies with it being larger in states with higher award amounts than in states with lower award amounts as an earlier study by Sjoquist and Winters (2013) limited to only Georgia's HOPE scholarship which noted increased retention by 3.5 percentage points following adoption of the scholarship.

A similar regression discontinuity study by Harrington et al. (2016) on the Missouri Bright Flight Program found similar albeit larger results. Harrington et al. (2016) found a 4.3 percentage point increase in the likelihood of employment in students' home state of Missouri when they received the Bright Flight merit scholarship. Also continuing early commentary on who gets access to merit aid, Harrington et al. note that the Bright Flight program mostly benefited White and higher income students with 89% of recipients being White and 71% coming from families that earned more than $60,000.

Regarding merit-based aid research on the effects of losing this additional aid, the research found results similar to Dynarski's (2003) study on the loss of grant aid that the loss of merit aid results reduced metrics for student outcomes.

Cummings et al. (2022) utilized regression discontinuity to study the loss of Tennessee's merit-aid HOPE scholarship. Cummings et al. found that loss of the HOPE scholarship due to failing to maintain the requisite GPA after the first time that the grades were checked for the scholarship resulted in increased rates of stop-out among higher-income White students and increased rates of transfer to a community college for Black students. Scholarship loss did not impact on-time graduation but did cause a decrease in the probability of graduation within 150% of expected time-to-degree. This aligns with earlier research on the effects of losing Georgia's Hope scholarship where recipients who lost the scholarship saw a reduction in persistence and graduation rates to levels on par with similar students who originally did not receive the scholarship (Henry, Rubenstein, & Bugler, 2004).

Loan-Based Aid (Graduation and Other Outcomes)

Early quasi-experimental research on the effects of student loans on outcomes largely found that receipt of student loans does increase persistence, graduation rates, and other outcomes of interest. Velez (2013) utilized a difference-in-difference strategy comparing similar community college students between institutions that did and did not offer Stafford student loans. Velez identified that access to the Stafford loan increased borrowing by $262 per year and increased the likelihood of transferring to a four-year institution by 5.6 percentage points. Velez (2013) also found that loan access decreased

the probability of working while in college at a rate of 7 percentage points. Further, while loan access did not have a statistically significant positive effect on degree attainment for all students, it did have a significant impact on specific sub-populations. For Black women as well as low-income students, access to student loans increased transfer rates to four-year institutions to 8.8 and 7.6 percentage points respectively. Unlike when groups were aggregated together, increased access to loans did increase the graduation rates from four-year institutions following transfer from a community college for these groups at 8.7 and 9.7 percentage points respectively (Velez, 2013).

The previously mentioned Marx and Turner (2019) randomized control trial studying the effect of listing loan amounts in financial aid award letters resulted in a 30 percent increase in earned credits and an 11-percentage point increase in transfer to four-year colleges. In a randomized control trial, Barr et al. (2021) found that loan counseling before accepting a loan resulted in a 7 percent reduction in the accepted unsubsidized loan amount for the community college students studied, a reduction of $194 from a comparison average of $2,792. However, this experiment found that community college students who declined unsubsidized loan aid were 4.2 percentage points less likely to successfully complete coursework and 3 percentage points more likely to fail a course during the term following a reduction in accepted loan aid. This in turn reduced degree attainment by 0.9 percentage points. These findings are in line with findings from Denning et al. (2019), who found that when lower-income students gained access to additional loans, they were 1.8 percentage points more likely to complete their degrees by the time they were 24 years old.

A working paper by Black et al. (2023) examines the outcomes of students in Texas who borrowed during their first year of enrollment at public two- and four-year colleges. In line with other research projects, Black et al. (2023) identify a positive effect on degree completion but also identify positive effects on graduate program enrollment and lifetime earnings at increases of 12 and 2.8 percentage points, respectively. Further, rates of loan delinquency were reduced between 0.9 and 1.4 percentage points. Black et al. (2020) identified that increases in acceptance of these state loans did not meaningfully affect home ownership which conflicts with prior findings from Mezza et al. (2020). This earlier study by Mezza et al. (2020) that found that each $1,000 increase in loan debt decreased home ownership by 1.8 percentage points.

To explain this discrepancy, Black et al. (2023) suggested that, unlike Mezza et al.'s (2020) study on student loans and home ownership, their study did not investigate this relationship in response to increasing educational costs but rather investigated responses to an investment in human capital. Regarding the effects of student loans following graduation on indicators of quality of life, Di Maggio et al. (2020) found that for private, non-federal loans borrowers who received a $7,000 discharge from their loan balance were less likely to be delinquent on another loan, more geographically mobile, and earned approximately $3,000 more than the comparison group. In this study, Di Maggio et al. (2020) used a difference-in-difference design and suggested that a reduction in the debt balance allowed borrowers to take better advantage of career opportunities than those without this discharge of $7,000.

Promise Programs as a Contemporary Financial Aid Award

Broadly research on grant-, merit-, and loan-based aid suggests that the more access to additional capital a student has for financing their postsecondary education, the more likely they are to enroll and graduate from college. As noted in an earlier section of this chapter, these policies have a long-standing history dating back to the 1970s, 1990s, and 1950s respectively. More recently, a new strategy for financing higher education emerged through promise programs. Due to policy diffusion over the past 20 years, promise programs rapidly proliferated and have been broadly operationalized as a type of program that provides financial aid to students within a specific locale which can be utilized for at least one institution of higher education within that geographic area (Carnevale et al., 2020; Campaign for Free College Tuition, n.d.; Kanter, et al., 2021).

Miller-Adams (2015) identified the Kalamazoo Promise as the first student aid program of its kind. The Kalamazoo Promise was founded by donors who wished to remain anonymous to revitalize the economically ailing city of Kalamazoo, Michigan. The program offers graduates of the district's public high schools 100% of the cost of tuition at 61 eligible Michigan postsecondary institutions. However, some might identify other longer-standing aid programs such as Indiana's 21st Century Scholars which started in the 1990s as the first promise program. Furthermore, College Promise, a non-profit that inventories existing promise program has identified the 21st Century Scholars program as a promise program housed in their list of existing programs.

This discrepancy may have arisen because Miller-Adams (2015) constructed one of the most focused definitions of promise programs. The Miller-Adams definition

included three major prongs. This definition states that promise programs attempt to increase postsecondary access, build a college-going culture, and focus on local community and economic development (Miller-Adams, 2015). Because of the focus on the local community, Miller-Adams may have excluded existing programs that serve larger areas such as entire states. While the reason for Miller-Adams, a leading scholar on promise programs, identified the Kalamazoo Promise as the first promise program may be due to specific technicalities in program design, the moniker of promise program has gained popularity and is now used beyond Miller-Adam's narrow definition.

As of 2022, College Promise (n.d.) inventoried 441 operating promise programs in the United States. The popularity of the promise naming convention following the initial excitement of the Kalamazoo Promise may have contributed to this expansion. Because these programs are operated by variety of stakeholders the promise program moniker has come to refer to a large range of programs that my not fit solidly under Miller-Adams's (2015) definition. However, these programs are promise-like enough that adding the title of promise helps convey that these scholarships help to provide free college.

Promise programs have been broadly operationalized beyond Miller-Adam's (2015) definition by institutions, state governments, and lay-people to broadly refer to programs that guarantee at least tuition free college to students from a specific geographic area to certain institutions within said area. This broader definition has informed my study as I have identified CollegePromise.org's inventory of programs as the most holistic and encompassing of existing programs. In the coming section of this

literature review, I review variations of promise programs and empirical research on promise programs similarly to how I did in the first half of this review for other forms of financial aid.

Today, 33 states have similar statewide programs with the non-profit organization, CollegePromise.org identifying at least 441 free college programs across all 50 states in existence as of 2022, just 17 years after the start of the Kalamazoo Promise. As of fall 2023, CollegePromise.org (n.d.) provided an inventory of college promise programs of which 309 are locally offered programs and 132 are classified as statewide programs. These 132 statewide programs encompass state ran programs and include privately funded and public-private partnership programs. The 309 locally focused programs have service areas as large as several municipalities and as small as a specific selection of local high schools. These 309 locally focused programs also include institutional promise programs only offered at a single university or university system. While specific criteria for the financial award offered and eligibility criteria vary greatly, these programs oftentimes borrow the word “promise” from the original Kalamazoo Promise. Thus, this broad category of aid has come to colloquially be referred to as college promise programs.

Promise Programs Compared to Other Aid Types

As an in-kind award that does not have to be repaid, it is clear that promise programs fundamentally differ from loans. However, distinctions between merit-aid and need-based-aid are less clear especially when one considers the place-based nature of promise programs where place-based refers to the geographic conditions that must be met

in order to be eligible for a promise program. For example, state-merit aid programs are often place-based. The Tennessee HOPE and Georgia HOPE scholarships both require a student have attended high school in the state for a certain length of time prior to graduation to be eligible for the scholarship. If a student does not meet that criterion, then they are not eligible for the scholarship even if they may meet the academic requirements for the merit aid program.

However, the academic requirements to receive place-based merit-aid are also the features that result in these scholarships being established as a separate type of scholarship as compared to promise programs. As Miller-Adams identified, promise programs are focused on increasing high school completion and contributing to economic development in an area by removing financial barriers to accessing postsecondary training and education. While merit-based programs may also claim to do this, their initial requirements may discourage and/or inhibit students from making progress toward achieving these awards. Thus, these criteria may result in a particular distribution of awards that favors students from families with more access to academic and financial resources.

Of these three major financial aid types, it is more difficult to establish promise programs as a separate entity from grant programs. After all, one might pithily suggest that if the Pell grant is limited to residents of the United States and does not have a merit component that it could be considered a Promise program of sorts. However, it is important to know that no students are promised a Pell grant before they apply or enroll in college and complete the FAFSA. Rather, students must apply for financial aid and

have their family's assets be entered into a formula that determines eligibility for the Pell grant. Here, the major difference between a promise program and programs like the Pell grant is that there is no guarantee when a student applies to the FAFSA that they will be awarded a Pell grant or another grant when they apply for state or institutional aid.

For example, before a student applies to FAFSA they might have the information necessary to use the program's official estimate calculator for federal student aid. This tool is useful as it helps students determine their Student Aid Index (or formerly their Expected Family Contribution) and see what aid they may be eligible for, but this is not a guaranteed award for the student. Meanwhile, a promise program clearly lays out its requirements and guarantees the award when those criteria are meet. When a student applies for the promise program there is not a question of whether the student will receive the funds or how much funding they will receive from the program if the student knows they meet the criteria beforehand.

Miller-Adams attributed an early commitment of funds to prospective students as a key feature of promise programs. An excellent example in practice of this early commitment from an institution can be observed in the University of Michigan's High Achieving Involved Leader (HAIL) scholarship. Launched in 2015, this scholarship was intwined with an evaluation of the effects of providing students with highly publicized, clear communication of an early commitment of funds, and the results for the program were significantly positive (Baum, et al., 2021). Dynarski et al. (2021) found that the promise of funds from the HAIL scholarship resulted in an initial increase of 8 percentage points on number of applicants, 2.8 percentage points of more admissions,

and 2.7 percentage points of more enrollments from the scholarship's target population of students from Michigan families earning less than $65,000.

Types of Promise Programs

With over 400 programs currently identified under CollegePromise,org's broad definition, variations between these programs have naturally emerged to reflect the ideals and wants of local stakeholders served by their promise programs (Billings, 2018). For empirical evidence of program variation please see Perna et al.'s (2017) typology study of promise programs. Perna et al.'s study provides an overview of promise program variation that remains useful in understanding the ways in which promise programs may vary from one another. This typology has provided a strong framework for understanding and organizing promise programs that remains useful to researchers and policy makers.

Most succinctly, promise programs vary in who they serve geographically and where their students are able to attend given the generosity of their awards. These variations between programs are important to identify as they introduce challenges to over-arching national level research on promise programs and have contributed to research projects that largely study one promise program at a time. Because of this, research is limited to institution-level analysis of student-level behaviors and outcomes which can be appropriate for examining for effect on institutions but introduces difficulty with examining student-level outcomes between program types (Gandara & Li, 2020). These data considerations likely limit research that can make conclusions about student-level effects between programs to future meta-analyses that have yet to be conducted on promise programs. Later in my section reviewing the empirical research on promise

programs, I note the limited number of examples regarding studies conducted across multiple promise programs (see Gándara and Li, 2020 and Li and Gándara, 2020) or Swanson et al.'s (2020) meta-analysis of multiple other previous studies.

Variations in Place.

Miller-Adams (2021) broadly categorizes promise programs by service area and places programs into three main types: state-level, community-based, and institution-based programs. Among state-level programs, the Tennessee Promise was a notable, early state-wide free college promise program that served as the inspiration for former President Obama's proposal for national free community college (Office of the Press Secretary, 2015). It utilized a local community-based program serving Knoxville, Tennessee as its inspiration. Since then, 33 states have adopted a similar state-wide promise program. Some states, such as Tennessee, operate both a place-based merit scholarship and place-based promise program. The key distinction of these programs is that the place-based merit scholarship rewards students for academic success in high school while the place-based promise program seeks to enable postsecondary workforce development by making all in-state, public community and technical colleges tuition and fee free.

The UpJohn Institute or PENNAhead promise data set (n.d.) specifically tracks smaller, community-based programs like the Kalamazoo Promise and has most recently identified 210 of these programs as of November 2022. These programs generally operate within the following set of three goals: build a college-going culture within local school districts, increase access to postsecondary awards by reducing the costs of postsecondary

education, and promoting economic development in the community by increasing the number of skilled workers available in the local area. Within these programs, there is variation in whether the award can be utilized only at a single institution, a certain type of institution (two-year, CTE-focused institution, four-year institution, publicly controlled, privately controlled, etc.), or any institution within a certain geographic area up to as large as a state.

Finally, institution-based promise programs are often the smallest in scope compared to state-wide and community-based programs. While all students in the state or community are eligible for a said promise program upon meeting certain criteria, only students that apply to, are accepted to, and enroll at the specific institution or system offering an institutional promise may receive aid from these programs. Because of this prerequisite, one could argue that institutional promise programs at selective institutions might be akin to merit aid as a student must meet an institution's academic requirements for admission to receive this promise aid even if there are no academic requirements for the promise program beyond successful admission. However, these promise programs such as the HAIL scholarship are still understood by the field to operate as promise programs although the use of the term pledge to describe these programs has become increasingly common (Dynarski et al., 2023).

Variations in Award.

While service area effectively determines who is eligible for a promise program, variations in promise program awards are an important aspect in considering the potential effects of these programs. There are three major ways in which promise programs vary.

First, these programs vary among their selves in what billable items are covered by the program. For example, one program might only cover tuition while another covers tuition and fees and yet other covers costs beyond tuition and fees such as housing, meal plans, and/or school supplies.

Second, only particular institutions may be eligible recipients of a student's promise funds. Some programs like the Kalamazoo Promise generously allow recipients to use their awards for less than two-year credentials, at two-year colleges, and four-year colleges in both the public and private colleges. Others, such as the Tennessee Promise may only allow use at public two-year colleges and for shorter-term credentials. Still other promise programs may only allow students to use promise funds at a specific awarding institution.

Third, these programs vary in how their award funds are applied to covered items. Promise programs are considered either first- or last-dollar. First-dollar promise programs are considered more generous than last dollar programs as their funds are applied to a student's bill prior to other financial aid such as a state merit scholarship or Pell grant. In many cases this means that the student receives back their non-promise aid as a refund check from their institution to afford other living expenses. This contrasts with last-dollar scholarships which allow a student's non-promise aid to disperse to their bills before the promise program's funds are applied.

This means when a student using a last dollar promise program receives enough non-promise aid that they might receive $0 in promise program funds because no balance remains for the items covered by the program. A notable example of this can be found in

a report tracking the first and second cohorts of students in the Tennessee Promise, a last-dollar, statewide program, after their first five semesters of enrollment in the program. While all program students were stated to receive a tuition and fee free access to community college, 43.6% of the first cohort's 13,287 students (Tennessee Board of Regents, 2018) and 46% of the second cohort's 13,793 (Tennessee Board of Regents, 2019) students received $0 in aid from the state of Tennessee. This is because for these students, other financial aid such as the Pell grant or the state's merit aid scholarship completely covered the students' tuition and fee bill for the program. Essentially this means that any effects on enrollment and consequential increases on graduation could be due to messaging around and promotion of the program rather than a change in award for lower income students.

These variations introduce challenges in conducting research that compares the effects of promise programs where the impact of these variations is likely felt by individual students. However, empirical research investigating promise programs at this level are limited with few studies comparing outcomes across programs or examining student experiences. Further, there have been no studies examining the experiences of promise students across varying program types at this time. Instead, most studies focus on examining for a specific program's effects on student outcomes such as enrollment or award attainment.

Empirical Research Examining Promise Programs and Student Outcomes

Evidence from research on the effects of promise programs on student outcomes echo the findings on other aid types. The more access to funding that students, especially

low-income students, have through promise programs, the more likely students are to enroll in postsecondary education, persist with better grades, graduate, and achieve better post-graduation outcomes (Andrews et al., 2010; Bartik et al., 2021; Bell & Gàndara, 2021; Bozick et al., 2015; Gonzalez et al., 2014; Page et al., 2019; Ruiz et al., 2020; Swanson & Ritter, 2020). See Swanson et al. (2020) for a systematic review of college promise programs that found null to positive effects for the postsecondary outcomes of programs included for study.

Studies comparing the receipt of promise funds between promise programs would likely reflect issues mentioned in other studies on the effects of aid due to the potential for promise funds to crowd out or be crowded out by other aid types of aid depending on their status as first- or last-dollar promise programs. Research to date has found evidence that promise funds do not crowd out other forms of in-kind aid such as the Pell grant but have found evidence that loan aid is crowded out (Bartik, Hershbein, & Lachowska, 2021; Odle, Lee, & Gentile, 2021). However, these findings have not considered how differences in program design may affect these findings. For example, last dollar promise program award amounts are crowded out by other types of aid with documented instances of $0 awards in Tennessee.

Here the difference in timing of the award disbursement is critical to consider as it determines whether a promise program contributes to a larger award sum of in-kind funding as found by Bartlik et al. (2021) or is potentially crowded out by the other in-kind funding as found in the aforementioned report on Tennessee Promise (Tennessee Board of Regents, 2019, 2018). While a meta-analysis of existing quasi-experiments

would be necessary to confirm potentially different effect on outcomes, Carruthers et al. (2023) called attention this when comparing results of their study on a local, last-dollar scholarship to a second local, first-dollar scholarship effects in findings from the aforementioned meta-analysis on grant aid from Nguyen et al. (2019). Carruthers et al. (2023) noted this difference in effectiveness between promise and grant funding to be between 0.5 to 0.8 percentage points.

As Swanson et al. (2020) and Dynarski et al. (2023) note there has been a numerous quasi-experimental research inquiries into promise programs. However, two issues contribute a lack of clarity to these programs. First is research on these programs is limited to the past two decades. For example, an early study on the earlier mentioned 21st Century Scholars program in Indiana identified the positive results of promise programs as early as 2003 (St. John, Musoba, & Simmons, 2003). Second, research has mostly centered the well-publicized early programs such as the Kalamazoo Promise and Tennessee Promise without attempts to consolidate research regarding these programs through more rigorous methods such as a meta-analysis.

This created a challenge in assembling relevant promise program literature for this review. Studies on college promise programs studying either enrollment or other post-secondary outcomes are few in number and often study a limited number of longer established programs such as the Kalamazoo Promise and Tennessee Promise programs. While the scope of promise program research is narrow, researchers have still conducted numerous quasi-experimental studies that provide strong causal inferences regarding the effect of promise programs.

Enrollment Related Outcomes

Since the launch of the Kalamazoo Promise, studies on the effect of promise program adoption on enrollment have been a popular subject for researchers. Studies on changes to enrollment can be implemented more quickly than studies on graduation or post-graduation outcomes. This is due to the time-lag needed measure enrollment only being one admissions cycle whereas award counts may require 3 to 6 or more years before the evidence of this longer-term student outcome emerges.

Positive effects on four-year enrollment through promise programs that can be utilized at a student's choice of four-year or sub-baccalaureate institutions have been identified (Bartik, Hershbein, & Lachowska, 2021; Bifulco, 2019). Additionally, multiple studies have found that when only for use within the sub-baccalaureate that promise programs have a positive effect, increasing enrollment (Gurantz, 2020; House & Dell, 2020; Nguyen, 2019). However, it should be noted that current promise program research draws heavily upon a handful of long-standing programs such as the Kalamazoo Promise, Tennessee Promise, Excelsior Scholarship, and El Dorado scholarships which may not be representative of all other promise programs. Thus, it is important to examine the effects for these programs in aggregate as my study does for the best understanding of promise program effects.

Studies Comparing Multiple Promise Programs

Working toward establishing this aggregate effect for sub-baccalaureate enrollment, Li and Gándara (2020) utilized difference-in-difference methods to analyze enrollment data across 32 institutional-level promise programs at 31 two-year, public

institutions. These programs were identified using the PENNAhead College Promise Programs inventory (Perna & Leigh, 2016) which is a small sub-set of privately managed promise programs such as the Kalamazoo Promise that fit the Miller-Adams (2015) definition of promise programs. In this study, Li and Gándara constructed multiple comparison groups that fall under one of two groups. The first considered both geographically and assumably similar institutions. Within the second group consisting of four comparison groups, Li and Gándara utilized state-level samples from surrounding and non-promise program offering states to account for potential regional-level enrollment changes caused by an institution with a promise program that was geographically close to an institution that did not offer a promise program.

Using difference-in-difference to account for fixed-effects in variation between years, Li and Gándara (2020) were able to identify the interaction between treatment (having a promise program at an institution) and amount of time in operation to find effects on enrollment among first-time-in-college, full-time students and how effects may vary across program type. Across all promise program offering institutions the average increase to enrollment following treatment was a 22% increase of approximately 180 additional students. Regarding differences in effect between program types, the effect of program adoption was 20% lower when programs introduced need-based requirements to receive aid as compared to programs without a need-based requirement. Programs that did not require additional student engagement with support services had a 22% higher increase in enrollment than programs that required engagement with student support

services. Li and Gándara (2020) found no statistically significant difference in effect on enrollment between last-dollar and first-dollar programs.

However, these findings are complicated by the caveat that comparisons between colleges with these additional requirements and those that did not were conducted across differing comparison groups. Li and Gándara (2020) suggest that an over-arching, simpler message promoting free college could be the most effective strategy to increasing enrollment. Conversely, they found the inclusion of additional requirements embedded in promise programs can reduce the positive effect that these programs have on increasing postsecondary enrollment.

A second study by these two researchers leverages a sub-set of this data focused on the El Dorado Promise program to examine two new research questions. This second study asks how 1) effects of promise program adoption vary across racial/ethnic and gender groups and 2) how effects on these groups varied across program differences. Across all programs, increases in enrollment where most pronounced for Black and Hispanic students. Black, male enrollment increased by 47% while Black, female enrollment increased by 49%. This is an increase of approximately 21 Black, male and 27 Black, female students across the average institution in the treatment group. For Hispanic, males the increase was approximately 40% or 20 additional students per institution offering a promise program and 52% or 30 students for Hispanic, females.

While White, male and White, female students were found to have smaller increases of 37% and 27% respectively, Gándara and Li (2020) found non-significant increases on enrollment for students from Asian and Pacific Islander backgrounds.

Additional analysis of pre-event trends through an event study model included by Gándara and Li show no signs of pre-treatment trends affecting these findings. However, this event study approach did find statistically significant increases for Asian and Pacific Islander students albeit the increases are at a level that this group did not increase their share of enrollment in community colleges with promise programs.

Gándara and Li's (2020) analysis of differences between promise program designs found non-significant differences in enrollment increases between first dollar and last-dollar programs for all racial groups except White students. This suggests that while non-White groups are similarly receptive to promise programs regardless of type, White students respond more through increased rates of enrollment to more generous, first-dollar programs. Income-requirements resulted in statistically significant, decreases in the overall positive effect of promise programs. This statistically significant decrease in effectiveness was observed across all racial/ethnic and gender subgroups except Black, males who experienced non-significant decreases.

Regarding promise program award amounts, only Asian and Pacific Islander students responded with increased enrollment to more generous promise programs that covered full tuition costs as opposed to partial tuition costs. Other groups responded with non-significant differences between promise programs with varying levels of generosity. Finally, programs that required differences in enrollment between programs that required additional student support services and those that did not were statistically insignificant across all groups except Black, females who responded with higher rates of enrollment in programs that required participation in addition support services. Both the Li and

Gándara (2020) and Gándara and Li (2020) studies are notable in the scope of the number of promise programs studied. While these two studies concerning effects on enrollment offer a unique sample of multiple promise programs, these studies were conducted on a smaller sample that my present study which captures a wider range of free college programs and constructs a broad comparison group nationally.

Sub-Baccalaureate Promise Program Studies

Narrowing in focus to the sub-baccalaureate specifically, an early study by House and Dell (2020) on the state-wide Tennessee Promise also utilized a difference-in-difference design to compare changes in enrollment between Tennessee's community colleges, bordering states' community colleges, community college in states within the southern region, and community colleges nationally. House and Dell (2020) identify positive effects for students overall but that for the Tennessee promise that results were most strongly concentrated among White students with weaker but also still significant results among Black students. Also, while not an aspect of their statistical analysis, House and Dell (2020) noted that enrolling community college students in Tennessee had higher adjusted gross incomes and greater expected family contributions following adoption of the Tennessee promise than before the program's adoption. While studies that center student income as an independent variable of interest have yet to be conducted, this means that the Tennessee promise may have not caused new enrollment but rather changed the enrollment patterns of students who were likely to already go to four-year colleges but have now chosen to attend a two-year college due to its free cost to them as a student. A study by Nguyen (2020) finds similar albeit slightly different results when

employing a difference-in-difference design using non-Tennessee community colleges in the US as a comparison group and a generalized synthetic control method. Largely, Nguyen's (2020) findings reconfirm House and Dell's (2020) findings but also Nguyen found a statistically significant increase for the enrollment of Hispanic students in addition the increase for White and Black students found by House and Dell (2020).

Currently, a majority of promise program studies focus on outcomes for community college students which is reflective of the fact that most promise programs serve community college students. The inventory of programs provided by College Promise notes 182 of the 409 inventoried promise programs can be used at four-year colleges, while 315 can be utilized at 2-year colleges and 399 can be used for Career and Technical Education (College Promise, n.d.)

Retention, Completion, and Post-Graduation Outcomes

In this section, I discuss relevant empirical findings linked to both graduation and degree attainment as well as other non-graduation and attainment outcomes such as employment and migration.

Graduation and Attainment Outcomes

Like other forms of financial aid, research on promise programs generally point to a positive relationship with award attainment. Bartik et al. (2021) conducted one of the first studies on the effect of promise programs on graduation rate using difference-in-difference design to study the Kalamazoo Promise program. They found that the adoption of the Kalamazoo Promise resulted in a 12-percentage point increase in attaining any type of postsecondary award within six years. However, award production did not

signifigantly increase for the sub-baccalaurate despite increasing four-year award production.

Swanson and Ritter (2020) provide initial results studying the El Dorado Promise program another local program in a working paper. Using difference-in-difference methods, Swanson and Ritter found that promise program implementation resulted in a 14-percentage point increase in completion but only statistically significant increases for four-year degrees. Here again, I highlight that these two studies suffer from a potential subsitution effect were students that were already planning to attend postsecondary education may have substituted away from the sub-baccularate when presented with these promise programs that allow for enrollment at either level of postsecondary education.

Turning to sub-baccalaureate only promise programs, I highlight Knox Achieves, which eventually became the model program that Tennessee Promise was based on. Carruthers et al. (2023) find that participants in a promise program with specialized advising requirements were 24 percent more likely to attain an associate's degree within 9 years of graduating high school as compared to students that were not able to access the studied promise program. However, their use of a two-way fixed effects model used to identify intent-to-treat of program eligibility found non-significant positive returns on bachelor's degree attainment. This suggests that in certain scenarios, promise programs can significantly increase sub-baccalaureate award production which in turn justifies the need to further explore the effect of promise programs at a national level, especially in regard to credential awards which researchers have not attended to in these studies.

Referencing Nguyen et al.'s (2019) estimation that each $1,000 increase in grant aid raises degree attainment by 1.8 to 2.2 percentage points, Carruthers et al. (2023) divide estimated percentage-point gains in any credential by average annual participant award amounts to create a comparison between the less expensive predecessor to Tennessee Promise and the more generous first-dollar Kalamazoo Promise program. They note that per $1,000 spent in their studied program that associate's degree attainment rose by 1.3 to 1.4 percentage points.

Other Non-Graduation and Attainment Outcomes

Hershbein et al. (2021) utilized difference-in-difference methods to examine the effect of the Kalamazoo Promise program on degree attainment and earnings, comparing eligible students from the Kalamazoo district to ineligible students pre- and post-treatment. Hershbein et al. (2021) found null results for the effect of the Kalamazoo Promise receipt on rates of unemployment and inconclusive results regarding potential impact on earnings. However, recipients were also 9 and 17 percentage points more likely to remain in or 11 to 19 percentage points more likely to live within 20 miles of Kalamazoo, Michigan following high school graduation.

Hershbein et al. (2021) suggested that this effect on migration could limit the program's economic uplift to the community but also represented evidence that the Kalamazoo Promise did achieve its goal of reducing the migration of young workers out of the community. While post-graduation migration is not a question considered by this study, it is important to note that educated laborers are more likely to migrated across state lines for employment opportunities. While this post-graduation migration for

employment affects all graduates, I note this potential reduction in migration for promise participants as worth consideration for future researchers because Scott-Clayton and Wen (2019) note these migration patterns can bias estimates for effects on earnings and employment records as post-graduation outcomes which have become of increasing interest to policy leaders.

Summary of Research Gap

As presented in this chapter, researchers studying promise programs have largely studied the effects of increasing college affordability through increased financial aid as other researchers have done for need-, merit, and loan-based aid. These projects have largely studied individual programs which can affect as few as one institution to as many as an entire sector of a state's higher education system. However, researchers have yet to investigate for a national-level estimate for the effect of promise programs on sub-baccalaureate completions. Additionally, past research has not attended to the credential production of the sub-baccalaureate and focused almost entirely on two-year degrees. Further, findings on two-year degree production are conflicting. I believe conflicting evidence on two-year degree production may stem from confounding effects of certain promise program design features such as the ability for an award to be utilized at either the four-year or sub-baccalaureate level. Therefore, I remove these programs from my study to better understand award production at the sub-baccalaureate level.

From a methodological perspective there have been several barriers to conducting a study such as my own that have only recently been addressed. In the following chapter, I present a strategy for working through these barriers using recent advancements in

econometric design. Currently, the use of staggered difference-in-difference designs to study promise programs has been limited to the Gándara and Li (2020) as well as the Li and Gándara (2020) event study projects that only studied enrollment in response to promise program adoption. Additionally, these studies relied on the more limited PENNAhead data set where as my CollegePromise.org data set more holistically captures what is colloquially considered to be promise programs. Thus, I identify a considerable gap in the area of promise program research where there is a lack of an aggregate estimate of effect on completions (the goal of promise programs), conflicting evidence regarding effect on completion within the sub-baccalaureate, and an opportunity to further apply staggered difference-in-difference design to study completions beyond enrollment as an outcome.

CHAPTER 3

In the previous chapter, I mentioned that there have been methodological barriers to establishing a national estimate for the potential effects of promise programs. Additionally, the recent proliferation of programs also lends itself to only recently establishing an appropriate sample size for a national-level study. Only recent advances in econometric design and associated statistical software packages have allowed for the type of analysis needed to establish these estimates. Here, I briefly review these issues and how I address them in this study. Following this, I provide a review of how other researchers have utilized this method, my research questions, study sample, data analysis, and data collection. I close the chapter by laying out my studies assumptions and limitations.

Research Design – Staggered Difference-In-Difference

At its simplest explanation, my project investigated for a change in the number of graduates (a count of awards conferred) after an institution adopts a promise program (i.e. free college program). Using a difference-in-difference design offered me the ability to use an established framework for analysis that provides two advantages to me. First, this approach allows for comparisons between units before and following policy adoption (i.e., a single treatment across two discrete time periods). Second, it allows for causal inference using a comparison group to net out changes that would have occurred regardless of treatment. Even though comparison groups in these quasi-experimental designs are not identical to the treatment group's outcome variable prior to treatment, comparisons between these groups may be made if the two groups' pre-treatment trends

over time are similar enough to meet a parallel trends assumption – discussed later in the assumptions section of this chapter.

This comparison of trends in outcome variables allows for the identification of an estimate that is an average treatment effect (ATE). This represents the average difference between the comparison and treatment groups. Scholars often estimate this ATE through a use of regression that resembles the basic regression that I provide in formula 1 where β_3 represents an ATE:

$$y_{it} = \alpha + \beta_1 treat_i + \beta_2 post_i + \beta_3(treat_i * post_t) + \epsilon_{it} \quad (1)$$

In this formula adapted for use to my study, y_{it} represents my outcome variable at time *t* for institution *i* and α represents the y-intercept in this specification. The β coefficients represent the change in y_{it} for when dummy variables indicating treatment group (*treat*) and post-treatment period (*post*) are equal to one which represents a group being treated for *treat* and it being the post-treatment for *post.* When institutions are not treated and/or it is prior to the treatment period, *treat* and *post* are equal to zero. Among these coefficients β_1 represents the change in *y* due when an institution is in the treated group, β_2 represents the change in *y* due when an institution is in the post-treatment time period, and $\beta_3(treat_i * post_t)$ represents when an institution is both in the treatment group and in a period following promise adoption. Finally, ϵ_{it} represents an error term that contains both the fixed effects of institutions over time and error within the model itself as it fits a linear projection through my data set.

A major advantage of this regression-based approach is its flexibility for adding additional units, periods, and covariates for analysis which allows for an expansion of

this method beyond a classic 2x2 design that compares a single pre- and post-treatment comparison across two groups. Past research has established that this linear regression estimation and the algebraic 2x2 design which calculates unit differences between treatment and control groups in both the pre- and post-treatment groups are equivalent (Roth J. , 2024). This regression-based expression can also be captured through a two-way fixed effects (TWFE) expression that assigns unit and period effects to α and λ respectively. This can be represented by a regression where $D_{it} = treat_i * post_t$ and $\delta^{DD} = \beta_3$.

$$y_{it} = \alpha_i + \lambda_t + \delta^{DD} D_{it} + \epsilon_{it} \quad (2)$$

However, this TWFE approach has been found to be problematic in settings with heterogenous treatment adoption. Of primary concern is the interpretability of δ^{DD} as it is weighted average of individual treatment effects across each potential period of treatment adoption within a sample. This creates situations where weighted averaging can contribute to an δ^{DD} that potentially has an opposite sign of the effect of the true ATE or average treatment effect on the treated or ATT (Athey & Imbens, 2017; Borusyak & Jaravel, Consistency and inference in bartik research designs, 2017; Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Sun & Abraham, 2021).

With this caveat in mind, recent advances have provided methods for researchers to address the shortcomings of the TWFE model through a variety of strategies. I have identified several approaches to assist me in addressing concerns in identifying my estimator of interest: imputation-based (Borusyak, Jaravel, & Spiess, 2024), interaction-weighted (Sun & Abraham, 2021), and decomposition (De Chaisemartin &

d'Haultfoeuille, Two-way fixed effects estimators with heterogeneous treatment effects, 2020).

I identified these estimators for use over other potential other emerging estimators including Wooldridge's Extended TWFE (2021) and Callaway and Sant'Anna's (2021) group-time average because of their ability to account for unit-specific time trends in their set-ups using currently available STATA packages. These models produce outputs that are unable to establish the absence of significant pre-treatment parallel trends while not being compatible with the Rambachan and Roth (2023) *honestdid* package to assess for when it is appropriate to still conduct a staggered difference-in-difference. While these models have been found to produce similar results, specific differences remain due the uniqueness of my data set's unit-specific time trends (Baker, Larcker, & Wang, 2022). These unit-specific time trends occur because institutional and system level decisions to adopt a promise program may be influenced by preceding enrollment and graduation trends, the inclusion of the ability to account for unit-specific time trends will be important to account for non-zero pre-treatment trends in the treated sample (Billings, 2018).

Research Questions

The research questions for this study are:

1) What effect does the adoption of a free college program have on postsecondary program completions by program length (less than 1 year, 1 year to 2 years, and 2 years)?

2) What effect does the adoption of a free college program have on postsecondary program completions by program's academic focus?

Study Sample

Because this study is a national analysis of the effect of promise programs on sub-baccalaureate award production, my sample's unit of analysis are institutions that grant sub-baccalaureate awards. To simplify analysis, I study institutions that were open for the entirety of my sample's time range of 2000 through 2022 (23 years). Once, I downloaded the completed data files for completions from IPEDS for 2000 through 2022, I removed award levels above the associates degree and appended these 23 files together into a single panel data set.

The complete data files when downloaded from IPEDS have each level of award for each institution as their own separate row or observation in the data file. When an institution does not offer awards at this level, the institution does not have an observation row for that level of award. This means that when I removed baccalaureate awards and higher from my sample, I in effect limited the complete data file downloads from IPEDS to institutions that award sub-baccalaureate awards. The total number of institutions at this point was 8,928 institutions.

I then removed institutions that did not have observations at each year in the data set (2,648 institutions in my sample). For sub-baccalaureate only granting institutions, this effectively removed instances of institutional openings and closings. By doing this, I avoided issues where institutions that closed down throughout the sample may bias my estimates. For four-year institutions that may offer sub-baccalaureate awards, this also

effectively removed instances where a four-year institution begins to offer a sub-baccalaureate award between 2000-2022, which would be similar to an institutional opening and thus desirable to remove.

While removing institutions that opened throughout my sample could potentially bias estimates of effect downwards, I opt in favor of a potentially more conservative first national estimate for the effect of promise programs. Additionally, as I am concerned with the number of graduates that an institution with a promise program produces, removing newer opening institutions allows me to consider the potential influence of new institutions opening on the enrollment and subsequent graduation as a year-to-year market force captured in my model's year-to-year fixed effects. I then also removed institutions from US territories in the sample (2,575 institutions in sample) and institutions treated by multiple promise programs or similar unclear treatments.

Later in this chapter, I will describe my process for identifying treatment. This merging in of treatment variables resulted in a final total of n = 2,525 institutions in my final sample containing public and both for-profit as well as non-profit private institutions. I then created three sub-samples for modeling purposes. These three sub-samples consisted of a sub-sample excluding for-profit private institutions, a second sub-sample excluding all private institutions, and a third sub-sample of only state supported public institutions. I present distributions of these institutional types in **Table 1** (*n* = 2,525 for all sub-baccalaureate institutions, *n* = 1,786 for non-profit sub-baccalaureate institutions, *n* = 1,322 for public sub-baccalaureate institutions, and *n* = 1,173 for state-

Table 1

Distribution of Institutions in Sample

	All Sub-Baccalaureate Institutions	For-Profit Sub-Bacc. Institutions.	Non-Profit Sub-Bacc. Institutions	All Public Sub-Bacc. Institutions	Stated Funded Sub-Bacc. Institutions
Total	2,525	739	464	1,322	1,173
Less Than 2-Year Institutions	713	448	27	158	80
2-Year Institutions	1,195	197	44	799	755
4-Year Institutions	1,190	94	393	365	338

funded, public sub-baccalaureate institutions). In **table 2**, I provide an overview of treatment and comparison group assignment across these groups.

Here, I pause to discuss an example of the difference between a public sub-baccalaureate award granting institution and state-funded sub-baccalaureate award granting institution. The former receives state funding from the state that provides oversight and management to these public institutions while the latter does not receive state funds despite any other potential relationships. I included these institutions in the modeling phase in determining an appropriate sample due to the presence of these institutions in states that provide a free college program to state-funded institutions but not these local institutions. An example of this can be found in the Otsego Area Boards of Cooperative Educational Services of New York State which is a publicly controlled sub-baccalaureate award granting institution but is not supported by New Yorks state appropriations as reported by IPEDS. Instead, it is locally funded and not eligible for the state's free college program, the Excelsior Scholarship. Due to situations such as this, I include this sub-group for consideration when identifying a final sample for this study.

Note that the institutional descriptions that I report in **table 1** are produced via the sector descriptive variable reported in IPEDS. In addition to all sub-baccalaureate granting institutions sample (n = 2,525), I identify the following sub-samples of sub-baccalaureate granting institutions: non-profit institutions (n = 1,786), public institutions (n = 1,322), and state funded institutions (n = 1,173).

Each of these cuts to the sub-baccalaureate granting institutions sample offers some analytical utility. Removing private, for-profits for the sample of 1,786 institutions

Table 2

Sample by Postsecondary Control and Model Used for Analysis

	All (For-profits & Non-Profits)	*For-profits Only*	*Non-profits Only*	*Public, Non-profits Only*	*State Funded Public, Non-profits*
Total Sample	2,525	739	464	1,322	1,173
Treated Group	464	1	2	491	465
Untreated Group	1,322	738	462	831	708

allows for removal of many institutions that are not treated by promise programs which are often associated with public institutions while also removing institutions that operate with different financial goals than most of my treated units. This is reflective of my identification of only one instance of a private, for-profit institution being affected by a promise program as shown in **table 2**. Removing private, non-profits also refined my comparison group to more accurately reflect my treatment group by further restricting my sample to the 1,322 institutions most similar to treated institutions. The final sub-sample of 1,173 removes instances of locally funded public institutions that do not receive state support.

To decide upon which sample to use for my primary analysis in this study, I examined outputs for each sample's pre-treatment parallel trends. I discuss the approaches used for this later on more formally within the assumption's sub-section. To briefly explain here, this process involved me examining the pre-treatment trends of my regression models for each sample and my understanding of my data sub-samples to identify a sample that could produce consistent interpretable results in a staggered difference-in-difference design while also maintaining an appropriate comparison group construction.

I limited my sample to the number of sub-baccalaureate awards produced. It should be noted that the institutions included in my four above mentioned samples are not only community or technical colleges but also include four-year colleges. This is because an increasing number of four-year institutions have begun to offer two-year awards and an increasing number of two-year institutions are now offering four-year awards (Field,

2024; Weissman, 2023). Because these programs contribute to sub-baccalaureate award production, I have opted to include these institutions in my models if they had observations for all years in my sample. If affected by a promise program, I include them in the treatment group. If not, I include these institutions in the comparison group. However, these sub-baccalaureate granting institutions represent a relatively small proportion of the treated group population at only 43 four-year institutions present in the treated sample.

Data Collection

Regarding how I formatted my outcome variables, I collected the number of completions per year per institution in my sample, by program length and by Classification of Instruction Program (CIP) code. For my outcome variable I downloaded IPEDS complete data files for awards/degrees conferred by program (6-digit CIP code) and merged this with researcher collected treatment data. In this section I outline the protocol utilized to establish my data sample for this study.

Outcome Data Collection and Preparation

I obtained completion data using the option for complete data downloads from IPEDS. For my first research question concerning program length, I separated sub-baccalaureate awards by program length into the following groups: associates degrees (programs that take a least two years to complete), credentials that take between one-to-two years to complete, and credentials that take less than one year to complete. In this calculation, I only utilized the grant total of awards given in each category of program length summing all CIPs together.

For my second research question, which considers program area of study, I organized the 46 CIPs into five broad categories based on similarity in program focus. I did this due to limited sample size among various CIP codes at the two-digit classification level (i.e., the broadest existing classification level), I combined similarly focused two-digit CIP codes into five broad groups that I refer to as super-CIP codes. These outcome variables were summed for all the awards across program length for each of the five categories that I identified.

These super-CIP codes and the two-digit codes making up these groups are presented in **Table 3**. Constructing my own classifications of super-CIP codes was necessary because currently there is no crosswalk that accounts for both the CTE and non-CTE awards given at sub-baccalaureate granting institutions. The closest option was the Perkins Collaborative Resource Network that offers a 2020 crosswalk for CIP codes and the 16 registered career clusters promoted by Advance CTE which is an organization comprised of the CTE directors of each state in the United States. However, because my study does not separate CTE aligned awards from the sub-baccalaureate awards that fall outside this crosswalk, I had to develop my own crosswalk presented here to proceed with this study. Future studies that separate CTE from non-CTE degrees may benefit from this crosswalk and its widely accepted clusters of academic programs. For my study, I had to rely on organizing CIPs into categories that most resembled each other.

I accomplished this by utilizing the complete data files for awards/degrees conferred by program at the 6-digit CIP code where I utilized total degree counts by CIP code. While CIP codes were collected at the 6-digit level, data preparation and analysis of

Table 3

Super-CIP Codes and Corresponding 2 Digit CIP Codes

Super-CIP Code	Corresponding 2 Digit CIP Codes
Science, Technology, Engineering, and Math (STEM)	01 Agriculture, Agriculture Operations, and Related Sciences 03 Natural Resources and Conservation 11 Computer and Information Sciences and Support Services 14 Engineering 15 Engineering Technologies/Technicians 26 Biological and Biomedical Sciences 27 Mathematics and Statistics 40 Physical Sciences 41 Science Technologies/Technicians
Trades	46 Construction Trades 47 Mechanic and Repair Technologies/Technicians 48 Precision Production 49 Transportation and Materials Moving
Liberal Arts	05 Area, Ethnic, Cultural, and Gender Studies 16 Foreign Languages, Literatures, and Linguistics 23 English Language and Literature/Letters 24 Liberal Arts and Sciences, General Studies and Humanities 38 Philosophy and Religious Studies 39 Theology and Religious Vocations 42 Psychology 30 Multi/Interdisciplinary Studies 45 Social Sciences 50 Visual and Preforming Arts 54 History
Human Services	12 Personal and Culinary Services 19 Family and Consumer Sciences/Human Sciences 31 Parks, Recreation, Leisure, and Fitness Studies 32 Basic Skills 33 Citizenship Activities 34 Health-Related Knowledge and Skills 35 Interpersonal and Social Skills 36 Leisure and Recreational Activities 37 Personal Awareness and Self Improvement 53 High School/Secondary Diplomas and Certificates
Professional Services	04 Architecture and Related Services 09 Communication, Journalism, and Related Programs 10 Communications Technologies/Technicians and Support Services 13 Education 22 Legal Professions and Studies 24 Library Science 28 Reserve Officer Training Corps (JROTC, ROTC) 29 Military Technologies 43 Security and Protective Services 44 Public Administration and Social Service Professions 51 Health Professions and Related Clinical Services 52 Business, Management, Marketing, and Related Support Services

the CIP codes was done only at the 2-digit level with observations at the 6-digit level collapsed into the 2-digit level. CIP codes that changed at the 2-digit level during the 2000 to 2010 and 2010 to 2020 typologies were put into a crosswalk that updated all CIP codes to the current 2020 framework. I collected outcome data from 2000 to 2022 for 23 years of data and appended this data together into a panel data format. My outcome data set included the following variables unique unit identification (UnitID) codes, CIP codes, award level indicators, and count of completions for that observation.

Following this, directory information from the institutional characteristics and some financial data for public institutions from IPEDS surveys were merged into the data set. This additional data was utilized for identifying institutional characteristics and sorting institutions into various models explored in this study respectively. This merging of data from IPEDS enabled me to refine my samples based on institutional control as well as access to state appropriations to construct my four sample groups.

Treatment Data Collection and Preparation

Utilizing College Promise's MyPromise tool, I created a list of the 441 recorded promise programs in existence as of July 2022. I then conducted an internet search to identify the start date of each of these promise programs. Start dates for programs were identified via local newspapers reports, press releases, and institutional websites. These start dates were added to the list of promise programs for use later as a treatment adoption date variable. Of the 441 programs, I was able to identify start dates for 411 promise programs.

Among this list of 411 promise programs, I identified 310 programs that could be utilized for a sub-baccalaureate award while others could only be utilized for four-year degrees. These 310 promise programs affected 645 institutions. However, not all 645 instances of treatment were appropriate for inclusion in this study. Of the 310 promise programs, 72 programs affecting 595 unique sub-baccalaureate granting institutions had potential to elicit a scattering effect on student enrollment. I identified two potential cases of a scattering effect in my data.

First, I removed any programs that gave students a choice to use the award for either a bachelor's degree or sub-baccalaureate program. This is because eligible students for these programs might scatter across sub-baccalaureate and baccalaureate level programs. To answer the research questions of this study, I opted to remove these programs from both treatment and comparison groups to simplify the process of estimating the effect of free college adoption on sub-baccalaureate award production. Second, if a promise program affected high school graduates in a specific local area such as city or county but allowed for those students to enroll in any institution across the state, those students may scatter across the state in a way that is difficult to account for in my models with institutional level data. Given my lack of access to student-level data to account for this, I opted to remove these 72 promise programs and the sub-baccalaureate institutions they affected from both the treatment and comparison groups of my study.

Another 55 promise programs were removed from both the treatment and comparison groups for the following reasons:

- The data of the programs' served institutions were not reported in IPEDS (two programs).
- The programs were actually early postsecondary education opportunities for high school students such as dual enrollment (three programs).
- The programs were in operation prior to my sampled timeframe (four programs).
- The programs did not have a findable start date (five programs).
- The programs were merit scholarships (six programs).,
- There were instances where multiple programs were affecting a single unit in a way that it was unclear what program assisted which students (ten programs). As an example for this comparatively complex reason for removal, Cerra Cossa Community College in California is served by three active promise programs that each served a specific sub-population within Cerra Cosa Community College: Cerro Coso Promise starting in 2017, Inyo Promise starting in 2017, and the Mammoth Lakes Foundation Scholarship (Mono Promise) starting in 2014. Because of this, there is no way to identify which programs were linked to which students that completed their degrees. Due to this complexity, I removed these programs and their affected institutions from my sample.
- There was an expansion of a pre-existing program that did not differ from students utilizing a prior program (26 programs). Unlike instances of multiple programs simultaneously serving multiple sub-populations of students within a college, these examples of removal are best represented by an example using KnoxAchieves and the Tennessee Promise. When Tennessee first launched the

Tennessee Promise, KnoxAchieves had already been operating and served as a prototype/pilot of the statewide promise with relatively minor differences between the two. In situations like this, I removed the later adopted program and treatment date while leaving the earlier adopted program as the singular treatment date for these programs.

This allowed institutions served by programs that were adopted through an expansion to remain in the treatment group. In this scenario, only the later adopted promise programs and not the institutions were removed from my data. These institutions were assigned to the treatment group as with other institutions affected by promise programs. If removed for any other of my above criteria, institutions in addition to the promise programs meeting these criteria were removed from both the treatment and comparison groups. Following these eliminations, my final list for treatment assignment consisted of 198 programs that affected 580 institutions. Institutions that had programs which went into effect following my sample's final collected year of data (2022) are considered as comparison group institutions.

Merging Treatment and Outcome Data

When merging, I had observed outcome observation data for 2,525 institutions and treatment data for 198 programs affecting 580 of those 2,525 institutions (for the sake of simplicity, I only discuss my all-institutions sample here). Merging treatment and outcome data resulted in 5 unmatched promise programs across 36 institutions present in the treatment data that was not present in the outcome data. These 36 institutions were institutions that were absent from the IPEDS completion data either entirely or due to not

having an observation for one of the 23 periods in my sample. These 36 institutions that were removed during the preparation of the outcome data and thus were dropped following the merge. As mentioned above, I also removed 47 instances where an institution was treated by a promise program prior to the start of my sample date to simplify analysis. I also removed institutions when a promise program began between 2000 and 2004. By doing this, I established a five-year period in my sample where all institutions were not treated regardless of assignment to the treatment or comparison groups. These eliminations resulted in a sample of n = 2,525 where 188 promise programs resulted in assigning treatment group status to 494 institutions.

Of the 2,525 institutions in the sample, 2,031 units were untreated units. Among for-profit private institutions, there were 738 comparison group units and one treated unit in my final sample. There were 462 comparison group and two treated non-profit private institutions in my sample. There were 831 comparison group and 491 treated public institutions in my sample. Regarding state-funded public institutions only, there were 815 untreated institutions and 489 treated institutions.

I run multiple models restricting the sample comparison's group. These models and the sample sizes utilized in these models are:

- model including for-profit private, non-profit private, and non-profit public institutions (n = 2,525)
- model excluding for-profit private institutions while including non-profit private and non-profit public institutions (n = 1,786)

- model excluding for-profit private and non-profit private institutions while including non-profit public institutions (n = 1,322)
- model excluding for-profit private, non-profit private institutions, and locally funded public non-profit institutions (i.e., institutions that do not receive state appropriations) while including only state funded public institutions (n = 1,173).

When I restrict the comparison group to public institutions, I remove institutions in both the treatment and comparison group that are not public institutions. The distinction between the models that include only nonprofit public institutions and only state-funded public non-profits is an important inclusion because in states where there are state-ran promise programs, these institutions that are locally funded are not served by these state programs. Including this fourth model allows for these locally funded institutions to be removed. Including these four models of varying sample size by institutional characteristics alters the comparison group of the sample.

While my more specific models reduce my comparison group size, these models also offer increased similarity between treatment and non-promise program sub-baccalaureate institutions. For example, the sample size of the model excluding for-profit private, non-profit private institutions, and locally funded public non-profit institutions while including only state funded public non-profit public institutions is the smallest of my samples. However, this sample also provides a model where institutions in the treatment and comparison groups are most similar.

I identify the public institutions only sample as my preferred sample for this study (n = 1,322 with 831 comparison group units and 491 treated units). I do this because

doing this ensures that dissimilar for-profit and private non-profit sub-baccalaureate institutions are not in my comparison group while also preserving the size of my comparison group through the inclusion of public institutions that are locally funded. I provide an overview of treatment assignment by year for each of my models in **Figures 1 through 4**. **Figure 3** contains the overview of treatment assignment by year for the preferred sample of this study which provides a balance of a sufficiently large comparison group and sufficiently similar institutions to the treatment group.

The sub-samples for each of these models was prepared through a Stata .do file provided in Appendix A. This project utilized a total of two STATA .do files. I utilized a loop to repeat my empirical analysis for each sub-sample. This allowed me to only utilize two .do files to manage this project. In addition to my data preparation .do file in Appendix A, I provide my analysis .do file in Appendix B.

Assumptions

Roth et al. (2023) provide a robust review on the state of difference-in-difference designs including the necessary assumptions to conduct a basic difference-in-difference model as well as the assumptions needed for more advanced models. These more advanced models require researchers to account for issues such as heterogenous treatment, treatments that may turn on or off over time, and/or unit-specific time trends (Roth, Sant'Anna, Bilinski, & Poe, 2023). Assumptions for basic difference-in-difference models include:

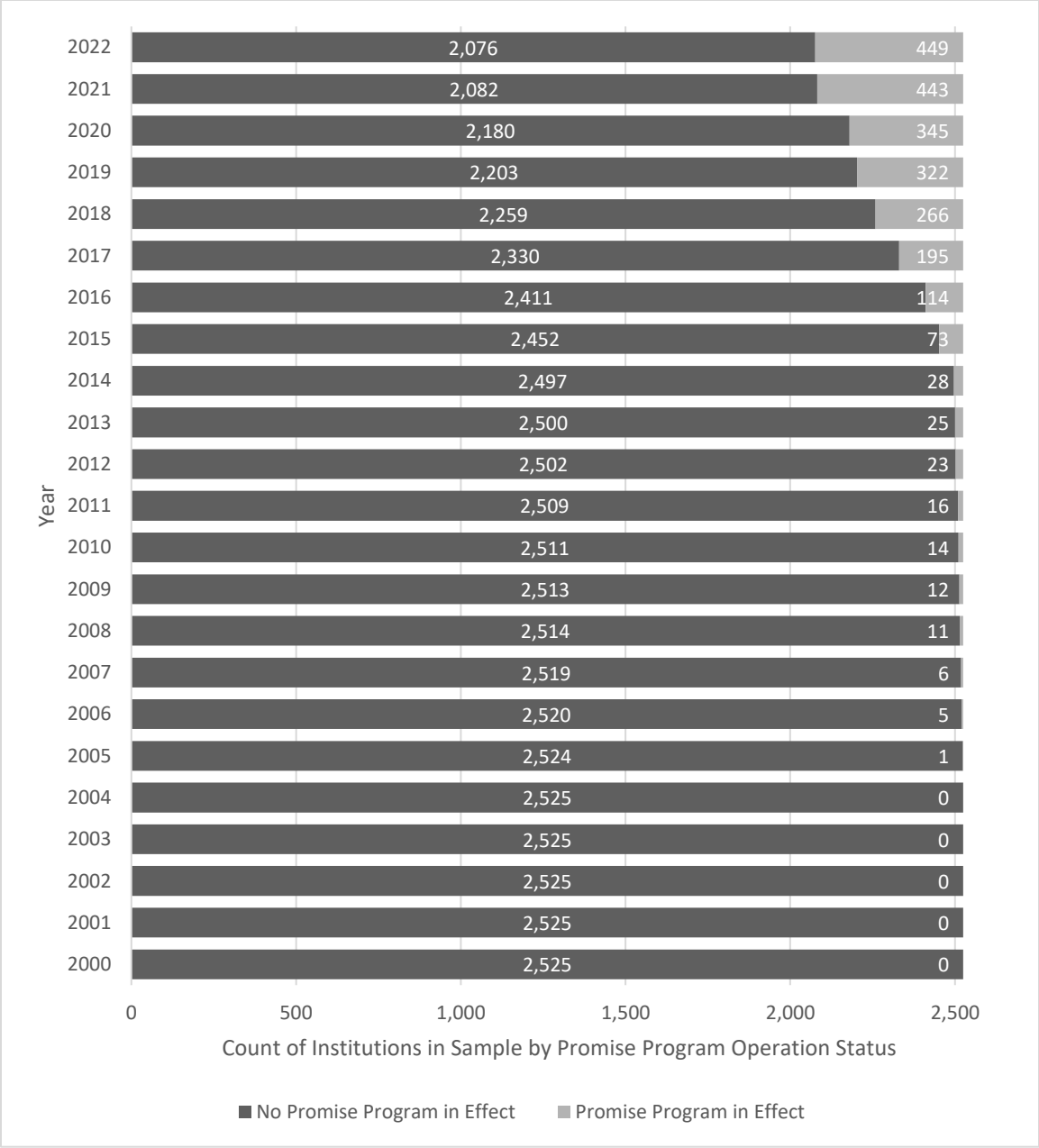

Figure 1

Treatment Assignment Count Among Sample Institutions Per Year (All: For-profits & Non-Profits)

This figure depicts distributions of treated and comparison group assignments overtime for a sample that includes all sub-baccalaureate granting institutions. Treatment group assignment does not change after treatment is turned on in this sample.

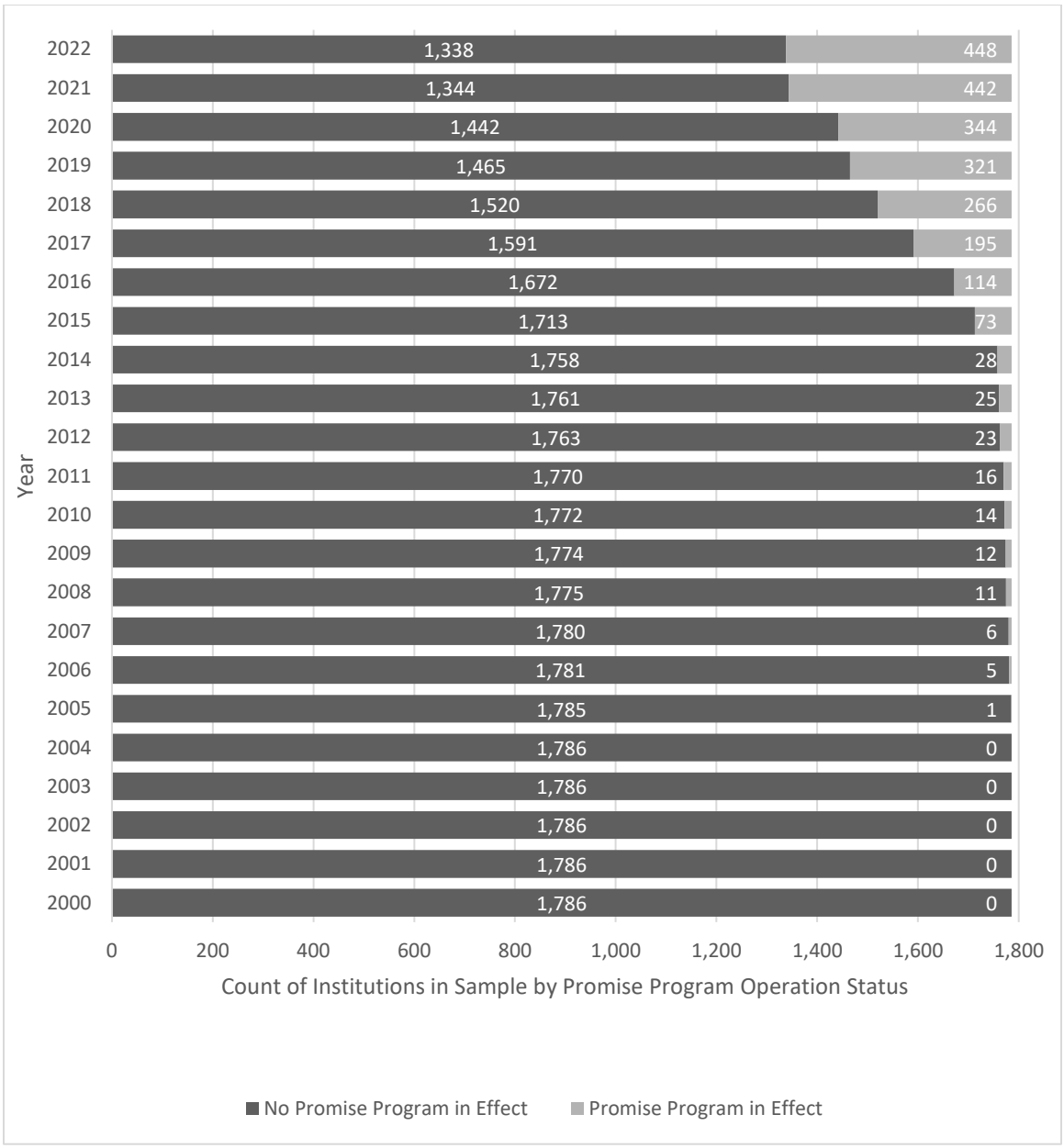

Figure 2

Treatment Assignment Count Among Sample Institutions Per Year (Non-Profits Only)

This figure depicts distributions of treated and comparison group assignments overtime for a sample that includes only non-profit sub-baccalaureate granting institutions. Treatment group assignment does not change after treatment is turned on in this sample.

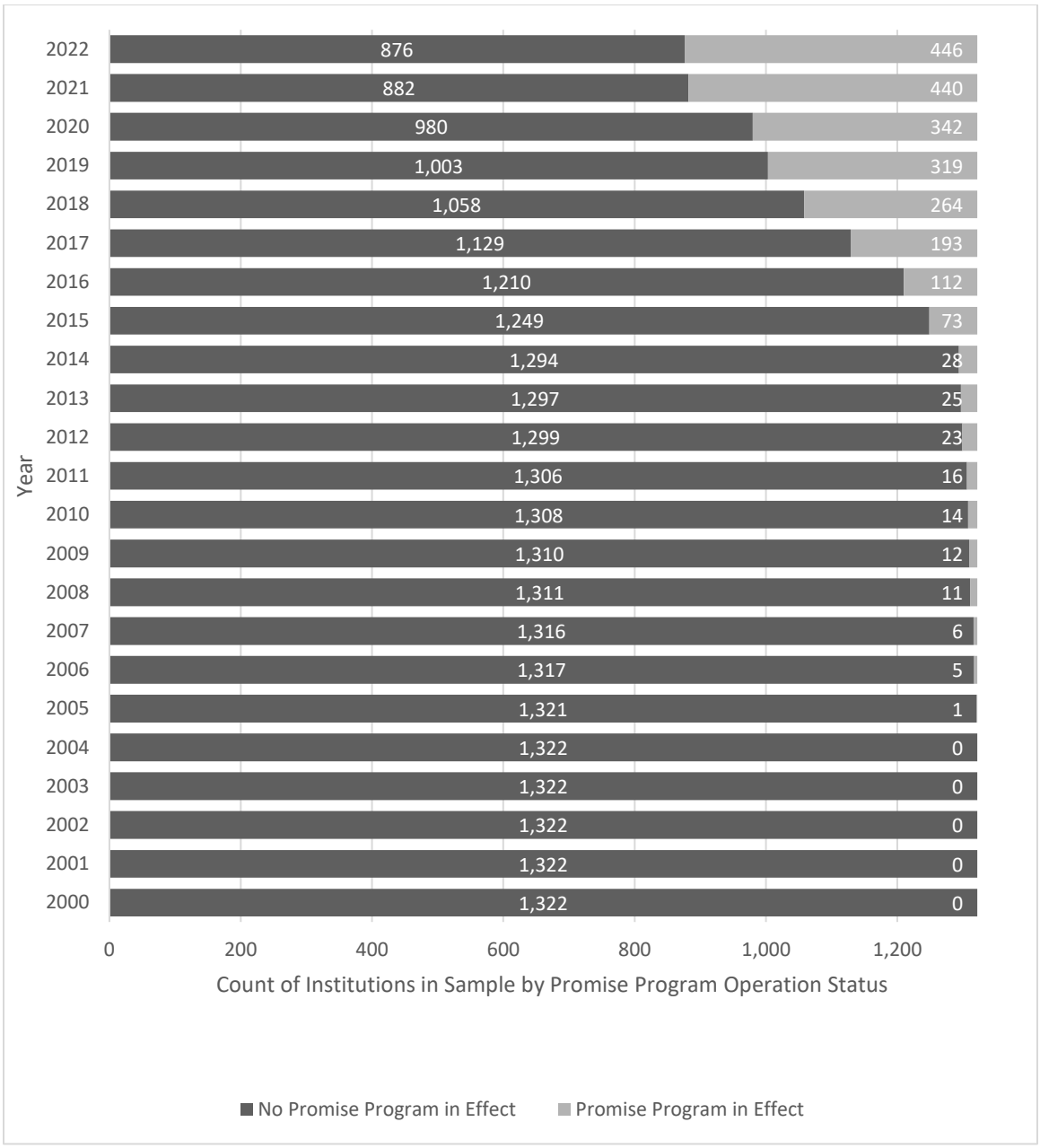

Figure 3

Treatment Assignment Count Among Sample Institutions Per Year (Public, Non-Profits Only)

This figure depicts distributions of treated and comparison group assignments overtime for a sample that includes only public sub-baccalaureate granting institutions. Treatment group assignment does not change after treatment is turned on in this sample.

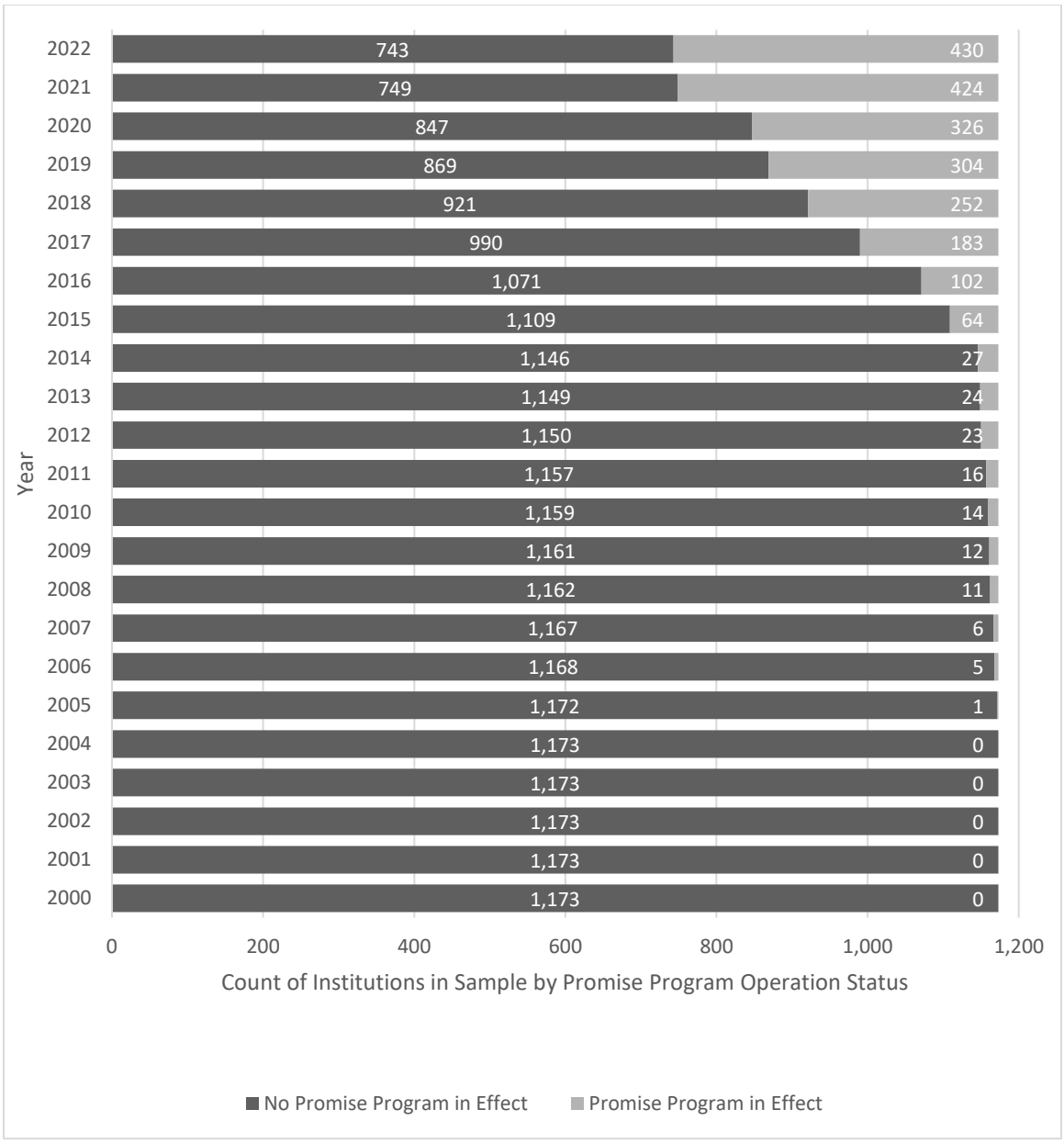

Figure 4

Treatment Assignment Count Among Sample Institutions Per Year (State-funded, Public, Non-Profits Only)

This figure depicts distributions of treated and comparison group assignments overtime for a sample that includes only state-funded sub-baccalaureate granting institutions. Treatment group assignment does not change after treatment is turned on in this sample.

(1) that pre-treatment trend lines for the outcome should be close to parallel,

(2) there should be no anticipatory effects, and

(3) a sample is independent and identically distributed.

Assumptions for more advanced models that examine heterogenous treatment assignment such as my specified model include:

(4) that in the counterfactual where treatment had not occurred, the average outcomes for all adoption groups would have evolved in parallel and

(5) if the unit is untreated in period *t*, their outcome does not depend on what time period they will be treated in the future.

In the coming sections, I review these assumptions for both the basic difference-in-difference specification and the additional assumptions required for my model using heterogenous treatment groups.

Unit-Specific Time Trends & Parallel Pre-Treatment Trends

It is important that my selected estimators are able to handle further complexity such as unit-specific time trends. This is because institutions that adopt promise programs may have pre-existing trend coefficients in enrollment and graduation counts that could be significantly different from zero which would bias comparisons made between the pre- and post-treatment groups. In this empirical design, establishing that the pre-treatment trends for the treated and comparison groups are not significantly different from one another establishes that parallel pre-treatment trends assumption.

For example, an institution struggling with enrollment and graduation counts may be motivated to secure funding for a free college program as a strategy to boost

enrollment and stabilize institution finances. This type of pre-treatment trend would contribute to a significantly non-zero beta coefficient for an outcome of interest. This introduces complications to my ability to draw out causal estimates between the comparison and treatment groups across the pre- and post-treatment periods which requires consideration for unit-specific time trends in my empirical strategy.

Applying a Staggered Difference-In-Difference Design

Basic difference-in-difference has frequently been utilized to research promise programs (Swanson, Watson, & Ritter, 2020). As seen in the literature review and motivation for this study to establish a national estimate of the effect of promise programs on graduation, previous difference-in-difference studies on promise programs have mostly only studied a singular program and largely studied effects on enrollment with few studying effects on program completion. Based on my review of the literature to prepare for this study, I could only identify the Li and Gándara (2020) and Gándara and Li (2020) studies as having used some form of staggered difference-in-difference approach to study promise programs however their sample utilized a much smaller range of treated groups ($n = 32$) and smaller comparison group that is unspecified in size but limited to the nearest 5-7 institutions of each treated college. Additionally, these event studies focus on enrollment rather than completion. My study builds upon this earlier work by applying event study designs to the postsecondary outcome of completion.

Additionally, the use of staggered difference-in-difference to study varying policy adoption timings (heterogeneous treatment) is useful in quasi-experimental settings because treated units such as states or institutions often adopt policies in a non-uniform

manner. These studies where treatment may switch on in a non-uniform manner in a difference-in-difference model are also occasionally referred to as event studies. Outside of education they have been applied to designs studying other areas such as automotive plant closures, family planning access, and healthcare reform (Venkataramani, Bair, O'Brien, & Tsai, 2020; Bailey & Lindo, 2018; Dimitrovová, Perelman, & Serrano-Alarcón, 2020). Regarding education, staggered difference-in-difference has been utilized to study postsecondary finance policy, university reform, and intergenerational mobility (Kelchen & Pingel, 2024; Ortagus, Rosinger, Kelchen, Voorhees, & Chu, 2023; Suhonen & Karhunen, 2019).

Regarding student outcomes specifically, staggered difference-in-difference has been utilized to study the effects of state performance policies on student loan debt and underrepresented minority student enrollment (Rosinger, Ortagus, J., Cassell, & Brown, 2021; Kelchen, Ortagus, Rosinger, & Cassell, 2022). However, examples of using staggered difference-in-difference are limited due to the relative newness of relevant statistical packages and on-going discussions as the field identifies the best staggered difference-in-difference estimator for given situations (Roth, Sant'Anna, Bilinski, & Poe, 2023). As packages are developed to support recent econometric developments, various approaches to staggered difference-in-difference are limited and subject to future updates. For example, the recently identified extended two-way fixed effects estimator from Wooldridge utilizes sample heterogeneity by interacting cohort effects (adoption year) with time specific effects which is ideal for establishing an average treatment effect on

the treated; however, current packages for these extended-TWFE (ETWFE) models are not able to account for unit-specific time trends.

Another leading estimator from Callaway and Sant'Anna (2021) also presents current software package limitations that do not allow for a specification that accounts for unit-specific time trends. In selecting an appropriate estimator, I constructed Appendix C which highlights strengths and weaknesses of current emerging estimators for staggered difference-in-difference. Given my samples issues with unit-specific time trends, I provide **table 4** to track which models are capable for accounting for my sample's issue of unit-specific time trends and identify whether that model is utilized in my data analysis strategy.

Data Analysis

With only the interaction weighted (Sun & Abraham, 2021) and imputation (Borusyak, Jaravel, & Spiess, 2024) approaches being able to unit-specific time trends with current packages, I selected these two approaches as my estimators for this dissertation. I selected the interaction weighted estimator as my preferred approach because the imputation estimator has a noted issue of showing a jump in time of treatment even in the absence of a treatment effect which results in unreliable visual heuristics tests for parallel pre-treatment trends (Roth, 2024). I still choose to include this approach as the next best fit for one estimator among several emerging estimators in the field.

Table 4

Selected Staggered Difference-in-Difference Estimators for Present Study

Staggered DiD Estimator	Current Packages Can Account for Unit-Specific Time Trends	Produces a Composite ATT	Standalone Estimator / Not a Diagnostic Tool
Classic TWFE	No	Yes	Standalone
Interaction Weighted	Yes	No	Standalone
Doubly Robust	No	No	
Decomposition (weighted average of all possible 2x2 estimators)	n/a	Yes	Primarily a Diagnostic Tool
Decomposition (weighted average of all ATEs across group-time combinations)	n/a	Yes	Primarily a Diagnostic Tool
Imputation	Yes	No	Standalone
Regression Adjustment	No	Yes	Standalone
Extended TWFE	No* (not with current packages)	Yes	Standalone

While Roth (2024) also notes that Sun and Abraham's model struggles with a similar issue, there are built-in specification tool options that allow for this concern to be addressed that I use per Roth's guidance. Because this issue with Sun and Abraham's estimator is addressable, I opt to make it my preferred estimator. However, I still chose to consider the Borusyak et al. estimator for a secondary analysis of the robustness of my findings due to evidence that this imputation-based estimator can offer more precision that Sun and Abraham's interaction-based estimator (de Chaisemartin & D'Haultfoeuille, 2023). Of note however is that these gains in precision can bias the outputs utilized to establish a parallel trends assumption when compared to other estimators under a dynamic effects assumption with longer time frames such as those found in my study (De Chaisemartin & d'Haultfoeuille, 2023). Ultimately, while the utility of the imputation-based estimator is limited in my study, it represents the second-best available strategy to examine the validity of my findings. Thus, I include it in this manuscript for transparency and documentation of an attempt to examine the robustness of my results.

While I will primarily focus on my Sun and Abraham estimator in Chapters 4 and 5, I will still report the outputs produced by Borusyak et al.'s imputation estimator and a basic TWFE estimator. This inclusion of the Borusyak et al. estimator will serve as a robustness test of the Sun and Abraham estimator. I also included de Chaisemartin & D'Haultfœuille's (2020) decomposition approach as a robustness test of my results and to examine potential issues with negative weighting.

Negative weighting is a problem in complex difference-in-difference designs caused by the ordinary least squares (OLS) and two-way fixed effect (TWFE) models

impose strong restrictions on the homogeneity of treatment effect (Borusyak, Jaravel, & Spiess, 2024). This negative weighting problem can result in not only biased estimation results but has also been associated with findings that are of the wrong sign (Roth, Sant'Anna, Bilinski, & Poe, 2023). This is because when treatment effects are heterogenous, OLS and TWFE models still create what the literature refers to as forbidden comparisons between already treated units where there are different cohorts of treatment. In other words, forbidden comparisons are comparisons using already treated units with to-be-treated units rather than untreated units. Using the de Chaisemartin and D'Haultfœuille's (2020) decomposition approach provides me a diagnostic tool for assessing negative weighting in my data sample which can still affect my Sun and Abraham estimator (De Chaisemartin & d'Haultfoeuille, 2023; Sun & Abraham, 2021). This decomposition approach allows for a means to assess for issues of negative weighting that could be present within my models. Thus, this decomposition approach is valuable not only as a robustness check but also as a *post hoc* strategy to ensure that my estimations are appropriate.

Identifying Models Against Potential Violations of Assumptions

The primary assumption that requires testing to determine if any difference-in-difference model is an appropriate empirical strategy is whether the data present satisfies a pre-treatment parallel trends assumption. There is no single agreed upon test for parallel trends or significant pre-treatment trends in staggered difference-in-difference design (Roth, 2022). However, researchers have established the examination of the significance of pre-treatment coefficients as an additional reference point to consider in determining if

a difference-in-difference design is appropriate (Marcus & Sant'Anna, 2021). These strategies are an examination of if individual pre-treatment period coefficients significantly differ from zero at the $p > 0.05$ level and an examination of if the pre-treatment period coefficients significantly differed from zero in a joint test.

I found that a staggered difference-in-difference approach was appropriate for the majority of Sun and Abraham event study estimator models in my public institutions only sample. Using this test, I found inconsistent evidence across my samples for whether the assumption of parallel pre-treatment trends held. I present these p-values in **table 5 & 6** for individual and joint tests of significance respectively of the public institutions only sample of pre-treatment trends. Note that in **table 5**, there are multiple values present for each sample (the columns) and outcome variable (the rows). These values are the p-values for individual pre-treatment coefficients arranged in ascending order from the fifth period before treatment to the period to the period immediately before treatment.

Because the Sun and Abraham approach does not calculate a coefficient for the period immediately before treatment, there are only four p-values presented for each of those regressions. As I have now presented the first outputs of my study in order to examine my pre-treatment parallel trends assumption, I want to note that I have provided terminal output logs for my study's .do files in Appendix D. I also provide versions of **table 5 and 6** for my other three sample groups in Appendix E to allow for comparisons of how each sample holds when inspected for significant pre-treatment parallel trends.

Table 5

Significance Pre-treatment Parallel Trends – Individual Period Test at Public Institutions

	Ordinary Least Squares	Sun & Abraham (Event Study Interact)	Borusyak et al (DID Imputation)
All Awards	0.376 0.964 0.945 0.487 0.701	0.903 0.261 0.060 0.285	0.541 0.335 0.394 0.340 0.085
Associates Only	0.034* 0.174 0.195 0.203 0.168	0.094 0.297 0.944 0.543	0.284 0.189 0.139 0.073 0.005**
All Credentials	0.204 0.032* 0.081 0.225 0.344	0.171 0.014* 0.007** 0.080	0.436 0.816 0.451 0.398 0.869
One-to-Two Year Credentials	0.105 0.009** 0.006** 0.042* 0.049*	0.052 0.001*** 0.000*** 0.000***	0.479 0.399 0.387 0.363 0.840
< 1 Year Credentials	0.298 0.138 0.048* 0.287 0.053	0.229 0.070 0.003** 0.035*	0.676 0.882 0.821 0.972 0.698
STEM	0.923 0.472 0.765 0.895 0.704	0.431 0.030* 0.041* 0.110	0.477 0.592 0.511 0.490 0.814
Trades	0.478 0.291 0.163 0.819 0.495	0.341 0.169 0.097 0.605	0.042* 0.013* 0.073 0.059 0.025*
Lib Arts	0.002** 0.002** 0.003** 0.001*** 0.000***	0.010* 0.011* 0.072 0.015*	0.000*** 0.000*** 0.000*** 0.000*** 0.000***
Pro. Services	0.021* 0.002** 0.004** 0.031* 0.077	0.024* 0.000*** 0.000*** 0.001***	0.110 0.139 0.067 0.089 0.268
Human Services	0.741 0.320 0.161 0.245 0.226	0.816 0.548 0.148 0.163	0.614 0.513 0.493 0.690 0.935

* p<0.05, **p<0.01, ***p<0.001

Table 6

Significance Pre-treatment Parallel Trends – Joint Period Test at Public Institutions

	Ordinary Least Squares	Sun & Abraham (Event Study Interact)	Borusyak et al (DID Imputation)
All Awards	0.6003	0.3169	0.0647
Associates Only	0.3787	0.2940	0.0086**
All Credentials	0.268	0.0593	0.0622
One-to-Two Year Credentials	0.1104	0.0016**	0.8119
< 1 Year Credentials	0.1802	0.0679	0.1823
STEM	0.5887	0.2212	0.4646
Trades	0.3225	0.4669	0.0261*
Lib Arts	0.04*	0.0322*	0.0000***
Pro. Services	0.7939	0.0006***	0.2603
Human Services	0.7445	0.5730	0.7548

* p<0.05, **p<0.01, ***p<0.001

There have been documented issues with the use of significance tests on individual pre-treatment coefficients for violations of the pre-treatment parallel trends assumption in a staggered difference-in-difference setting (Roth, 2022). It has been found that these tests can be weak against detecting violations of this assumption. Thus, I supplement this examination for pre-treatment trends violations with a joint test of whether all coefficients are significantly different from zero. Roth (2022) notes that while there is no agreement within the field regarding the use of the joint test or relying solely on individual tests of significance for coefficients, that they have both been relied upon by researchers to establish parallel pre-treatment trends. Thus, I leverage both tests of significance in my study to examine pre-treatment trends.

I consider a nonsignificant result for both the coefficient individually and the joint test to support the establishment of a parallel pre-treatment trends assumption. I consider instances where one or more individual pre-treatment coefficients returns as significant result while the joint test returns a nonsignificant result as weakly supportive of conclusions from an examination of visual heuristics. Instances where both examination of individual coefficients and the joint test return a significant difference from zero are considered to be unsupportive of whether the assumption that the parallel pre-treatment trends has been satisfied.

In tandem with an examination of visual heuristics I found that my staggered difference-in-difference strategy was appropriate for seven of my ten outcome variables. Four of these seven models produced strong evidence that the parallel trends assumption had been met when examining individual and joint tests of significance for pre-treatment

coefficients – these four models examined all awards, associates degrees, awards in the trades, and awards in the human services field. The other three models found somewhat supportive evidence that this assumption was met. These three models examined all credentials, credentials from programs shorter than 1 year in length, and awards in the STEM field. These three models were supported by only the joint test of significance. I provide these visualizations in **Figures 5 through 11** for these seven Sun and Abraham event study estimators. I provide all visualizations using the event study interaction approach for the public institutions sample in Appendix F. I provide the output for the OLS and for Borusyak et al.'s imputation estimators in Appendices G and H respectively.

While the three models of credentials that take one-to-two years to complete, liberal arts awards, and professional services awards were not supported by an individual or joint tests of significance of pre-treatment coefficients, I do find evidence using an additional smoothness restriction test that the one-to-two year credentials and liberal arts outcomes are suitable for inclusion in my analysis. This test exploits post-treatment violations of parallel trends that differ from a linear extrapolation of my pre-treatment trend line as described by (Rambachan & Roth, 2023). I provide the visualizations of pre- and post-treatment regression coefficients for these to outcome variables in **Figures 12 and 13** as I did with the other models. I also provide my robust confidence intervals for this smoothness restriction test in **Figure 14** for one-to-two year credentials and **Figure 15** for Liberal Arts awards. These figures plot the robust confidence intervals utilized in the sensitivity analysis in **figures 14 and 15** prepared using the *honestdid* package.

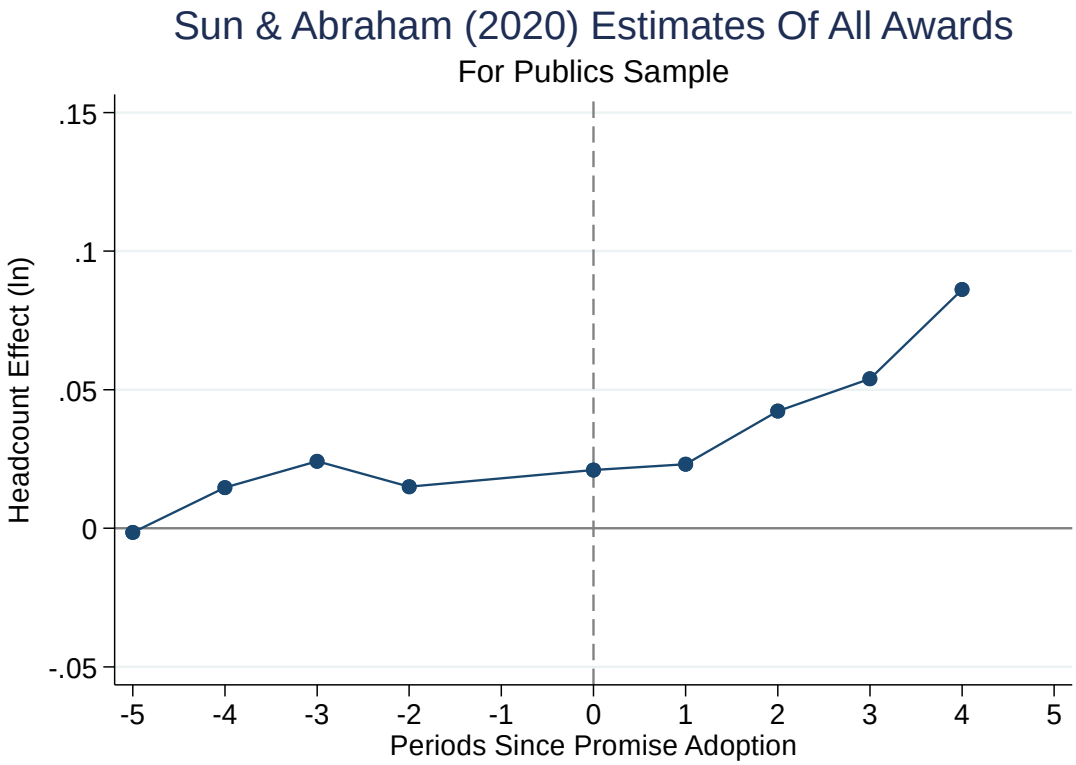

Figure 5

All Awards Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

Natural log difference between treatment and comparison groups of all sub-baccalaureate awards granted within the public institutions only sample. This event study visualization utilized the Sun & Abraham *eventstudyinteract* command. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

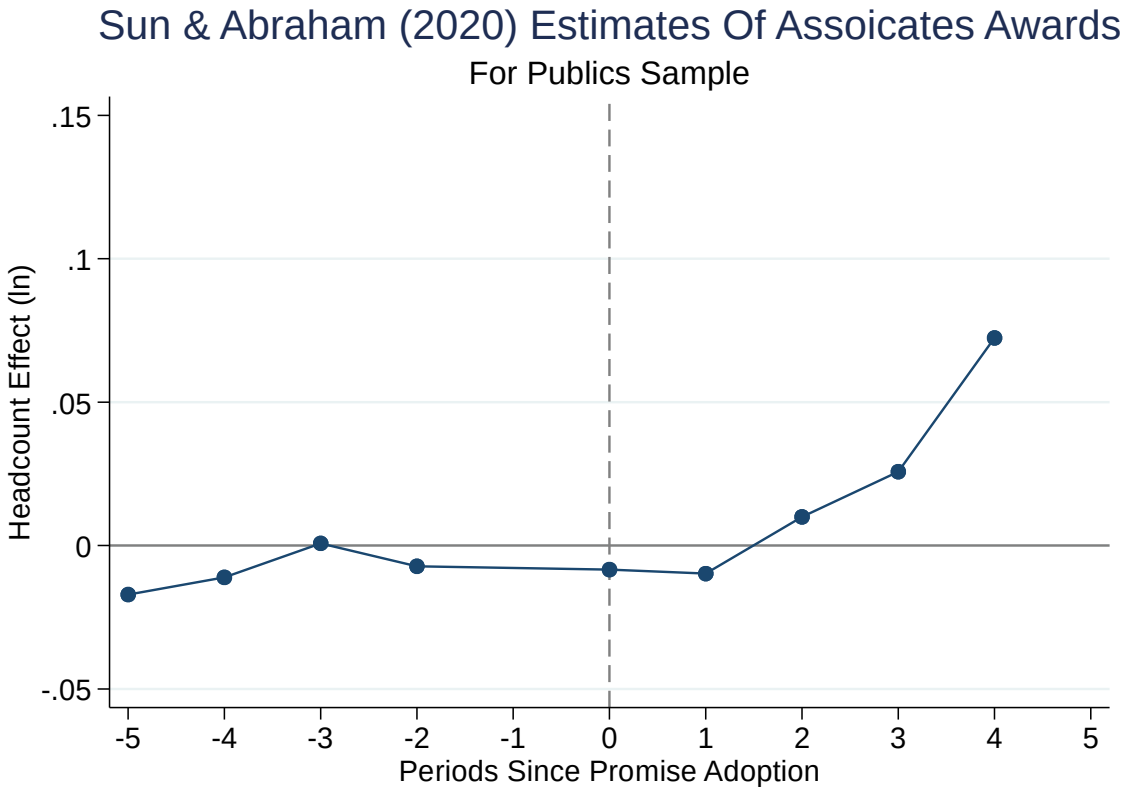

Figure 6

Associates Degree Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

Natural log difference between treatment and comparison groups of associate degree awards granted within the public institutions only sample. This event study visualization utilized the Sun & Abraham *eventstudyinteract* command. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

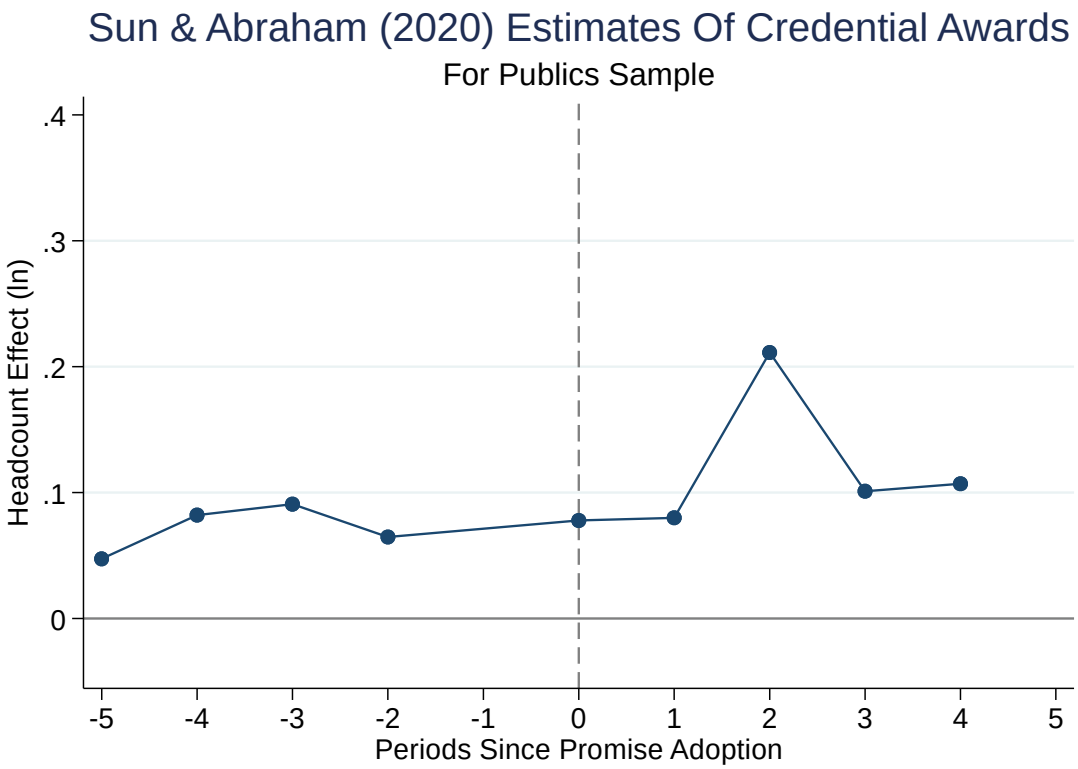

Figure 7

All Credentials Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

Natural log difference between treatment and comparison groups of all credential awards granted within the public institutions only sample. This event study visualization utilized the Sun & Abraham *eventstudyinteract* command. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

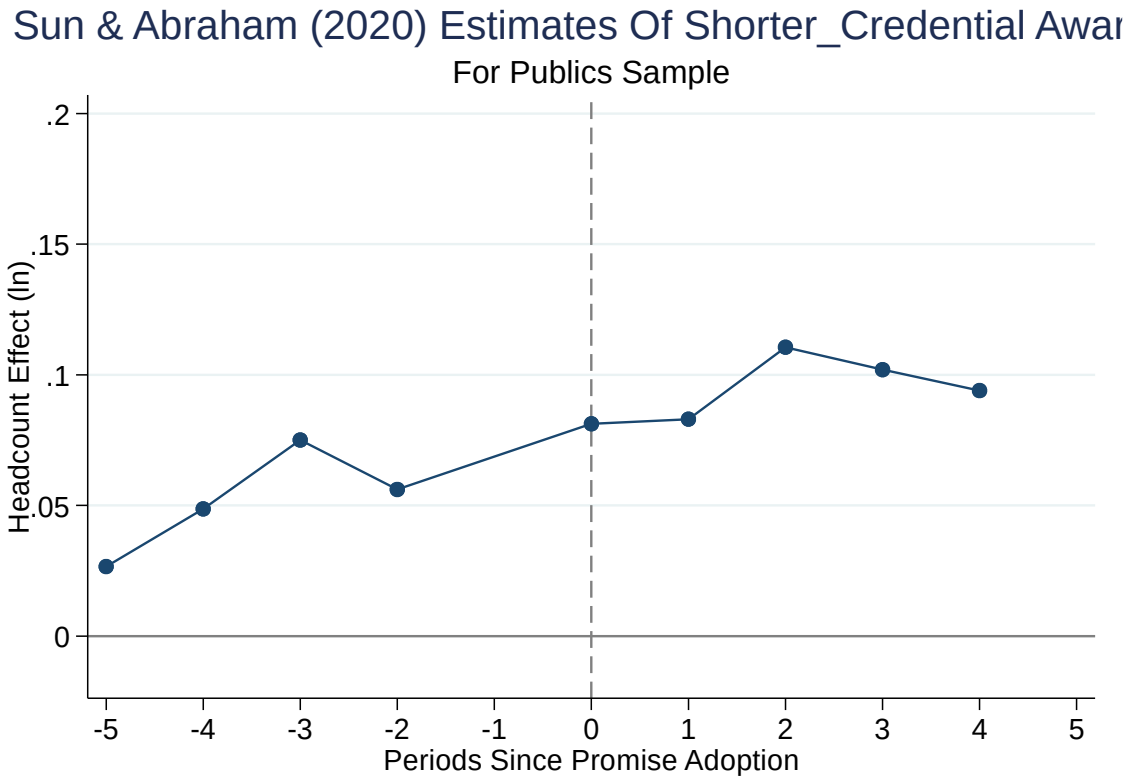

Figure 8

Less than One Year Credentials Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

Natural log difference between treatment and comparison groups of credential awards that take less than one year to complete that were granted within the public institutions only sample. This event study visualization utilized the Sun & Abraham *eventstudyinteract* command. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

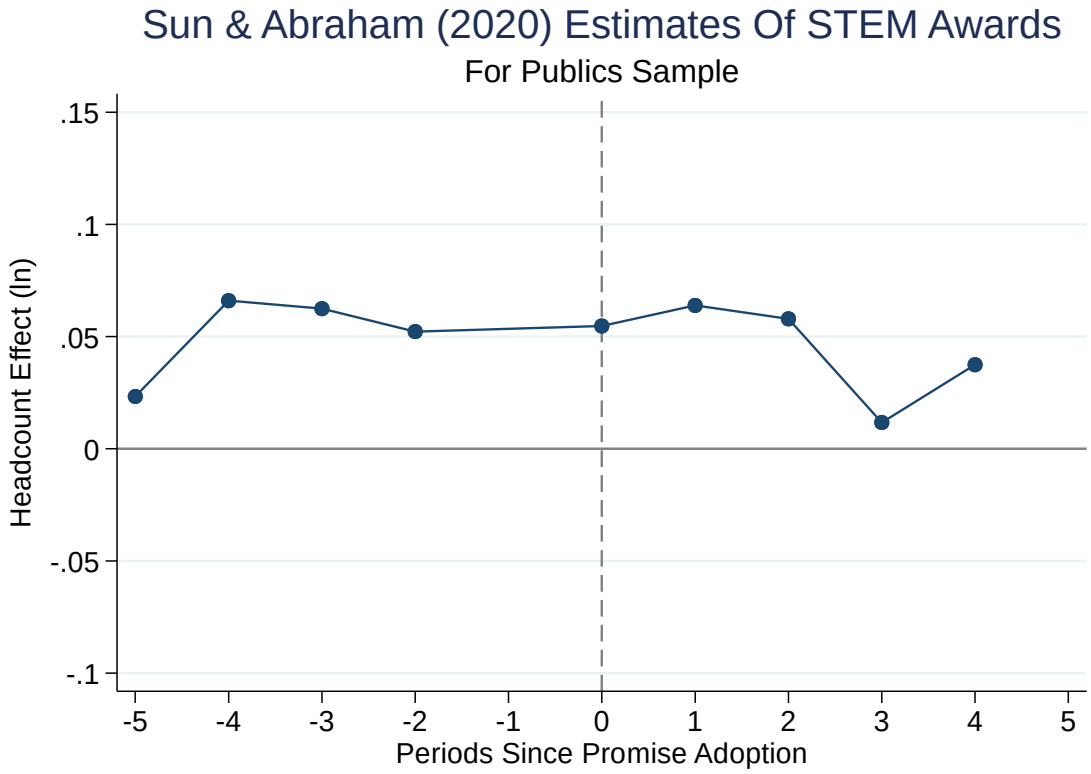

Figure 9

STEM Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

Natural log difference between treatment and comparison groups of awards from sub-baccalaureate STEM programs that were granted within the public institutions only sample. This event study visualization utilized the Sun & Abraham *eventstudyinteract* command. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group. STEM programs include the following programs from the 2-digit CIP designations: Agriculture, Agriculture Operations, and Related Sciences, Natural Resources and Conservation, Computer and Information Sciences and Support Services, Engineering, Engineering Technologies/Technicians, Biological and Biomedical Sciences, Mathematics and Statistics, Physical Sciences, and Science Technologies/Technicians.

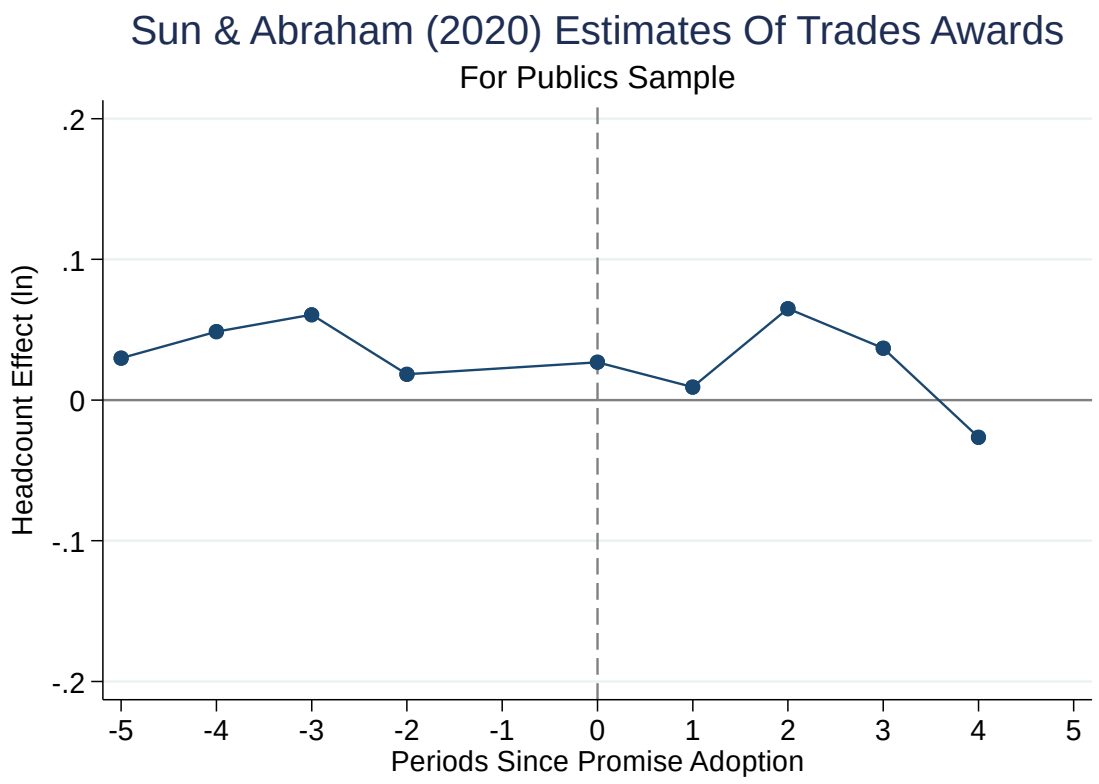

Figure 10

Trades Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

Natural log difference between treatment and comparison groups of awards from sub-baccalaureate trades programs that were granted within the public institutions only sample. This event study visualization utilized the Sun & Abraham *eventstudyinteract* command. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group. Trades programs include the following programs from the 2-digit CIP designations: Construction Trades, Mechanic and Repair Technologies/Technicians, Precision Production, and Transportation and Materials Moving.

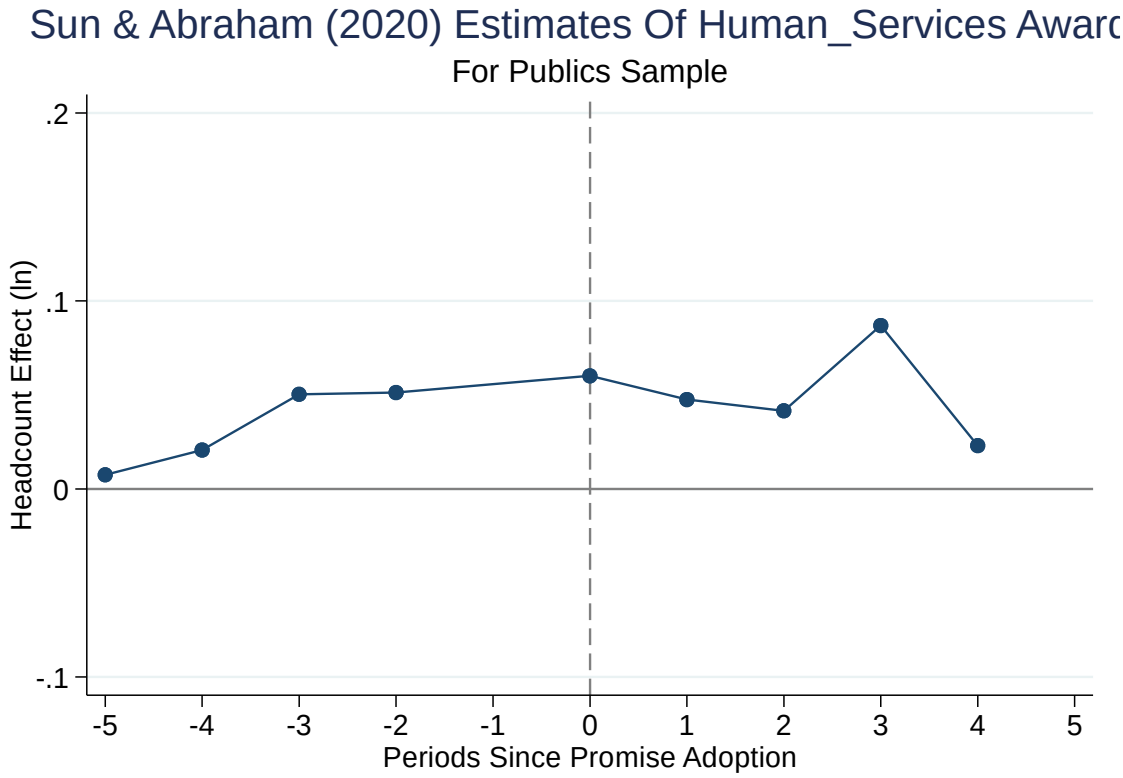

Figure 11

Human Services Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

Natural log difference between treatment and comparison groups of sub-baccalaureate awards from human services programs that were granted within the public institutions only sample. This event study visualization utilized the Sun & Abraham *eventstudyinteract* command. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group. Human services programs include the following programs from the 2-digit CIP designations: Personal and Culinary Services, Family and Consumer Sciences/Human Sciences, Parks, Recreation, Leisure, and Fitness Studies, Basic Skills, Citizenship Activities, Health-Related Knowledge and Skills, Interpersonal and Social Skills, Leisure and Recreational Activities, Personal Awareness and Self Improvement, and High School/Secondary Diplomas and Certificates.

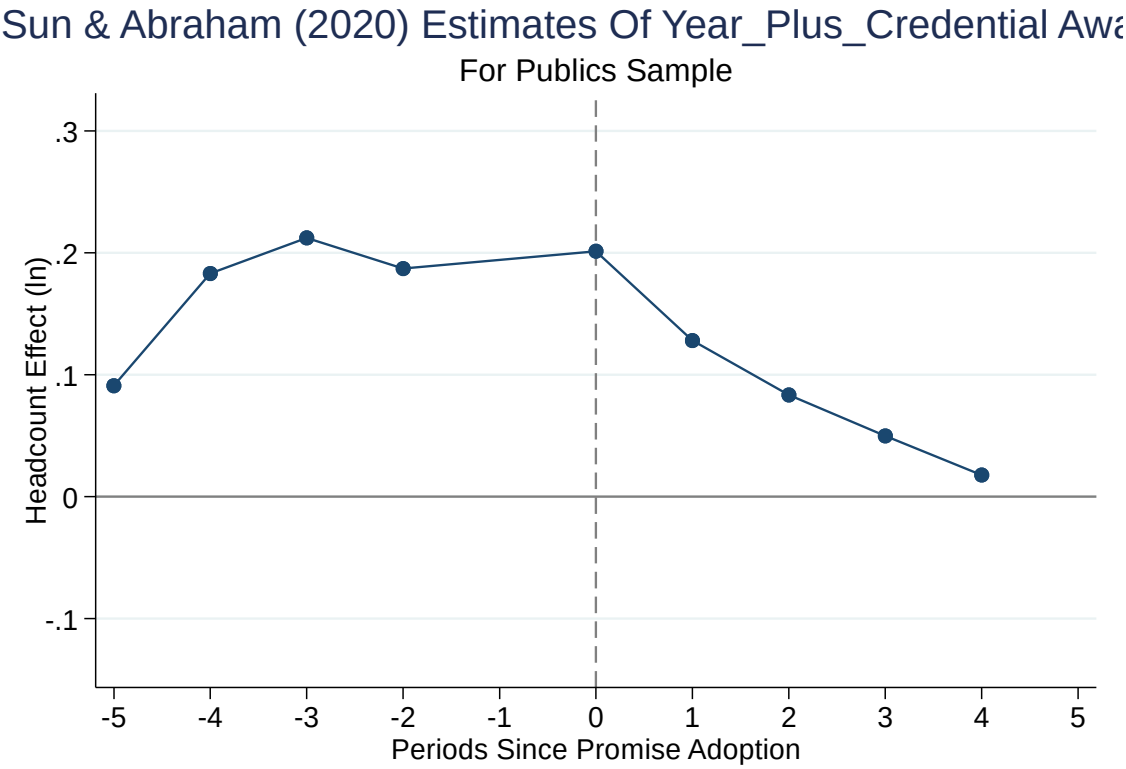

Figure 12

One-to-Two Year Credentials Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

Natural log difference between treatment and comparison groups of credential awards that take between one and two years to complete that were granted within the public institutions only sample. This event study visualization utilized the Sun & Abraham *eventstudyinteract* command. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

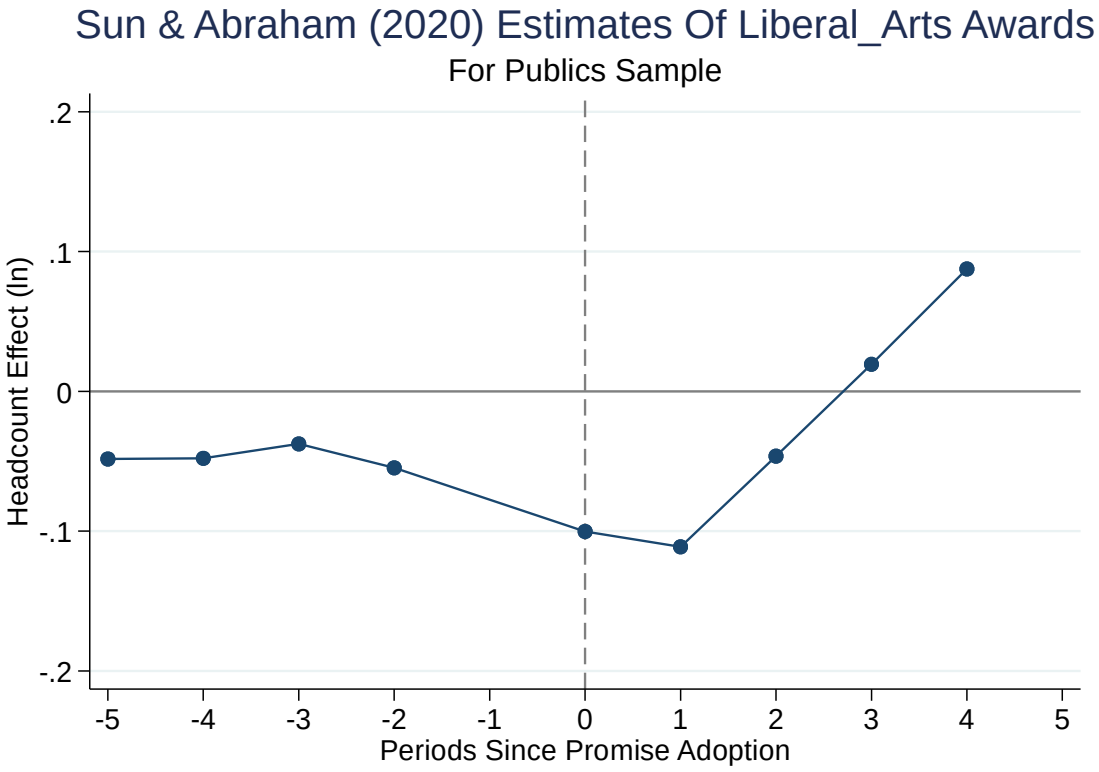

Figure 13

Liberal Arts Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

Natural log difference between treatment and comparison groups of awards from sub-baccalaureate liberal arts programs that were granted within the public institutions only sample. This event study visualization utilized the Sun & Abraham *eventstudyinteract* command. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group. Human services programs include the following programs from the 2-digit CIP designations: Area, Ethnic, Cultural, and Gender Studies; Foreign Languages, Literatures, and Linguistics; English Language and Literature/Letters; Liberal Arts and Sciences, General Studies and Humanities; Philosophy and Religious Studies; Theology and Religious Vocations; Psychology; Multi/Interdisciplinary Studies; Social Sciences; Visual and Preforming Arts; and History.

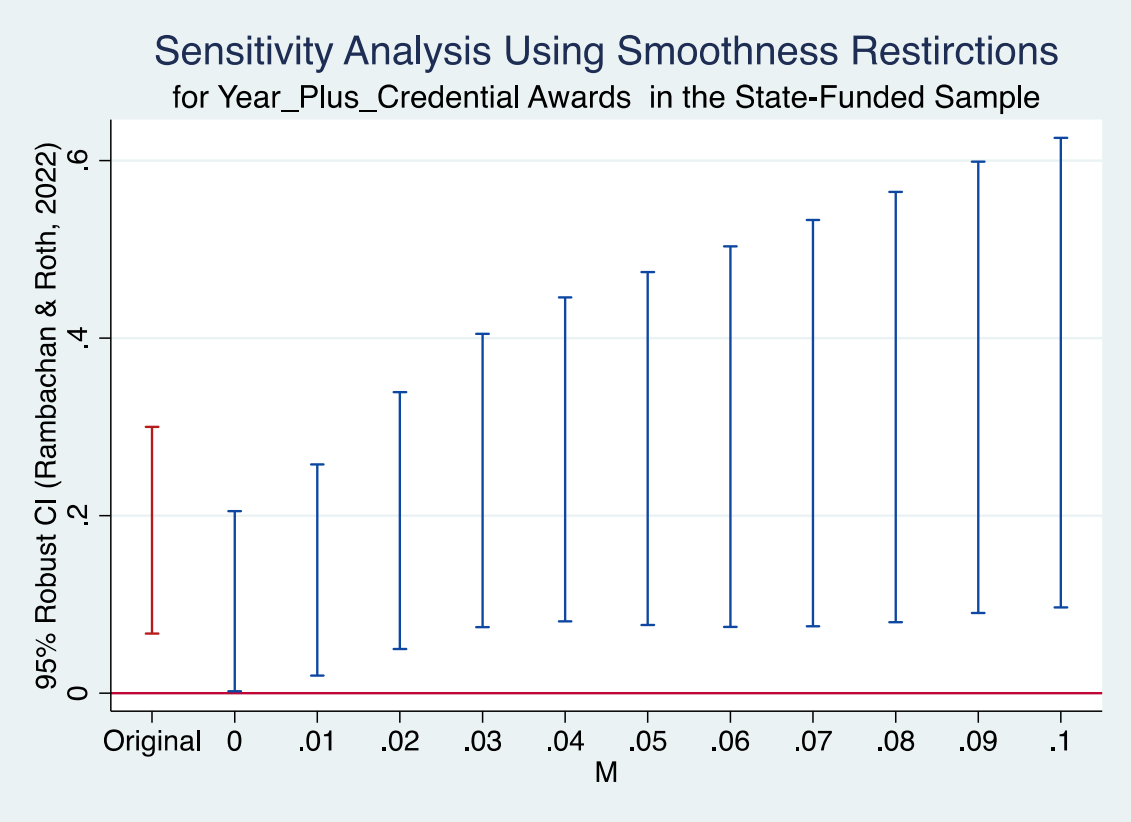

Figure 14

Smoothness Restriction Output for One-to-Two Year Credentials

Sensitivity analysis utilizing a smoothness restriction for credential awards that take between one and two years to complete within the public institutions only sample. This analysis reveals a breakdown value for M that is greater than M=.01 which indicates that a staggered difference-in-difference analysis is appropriate.

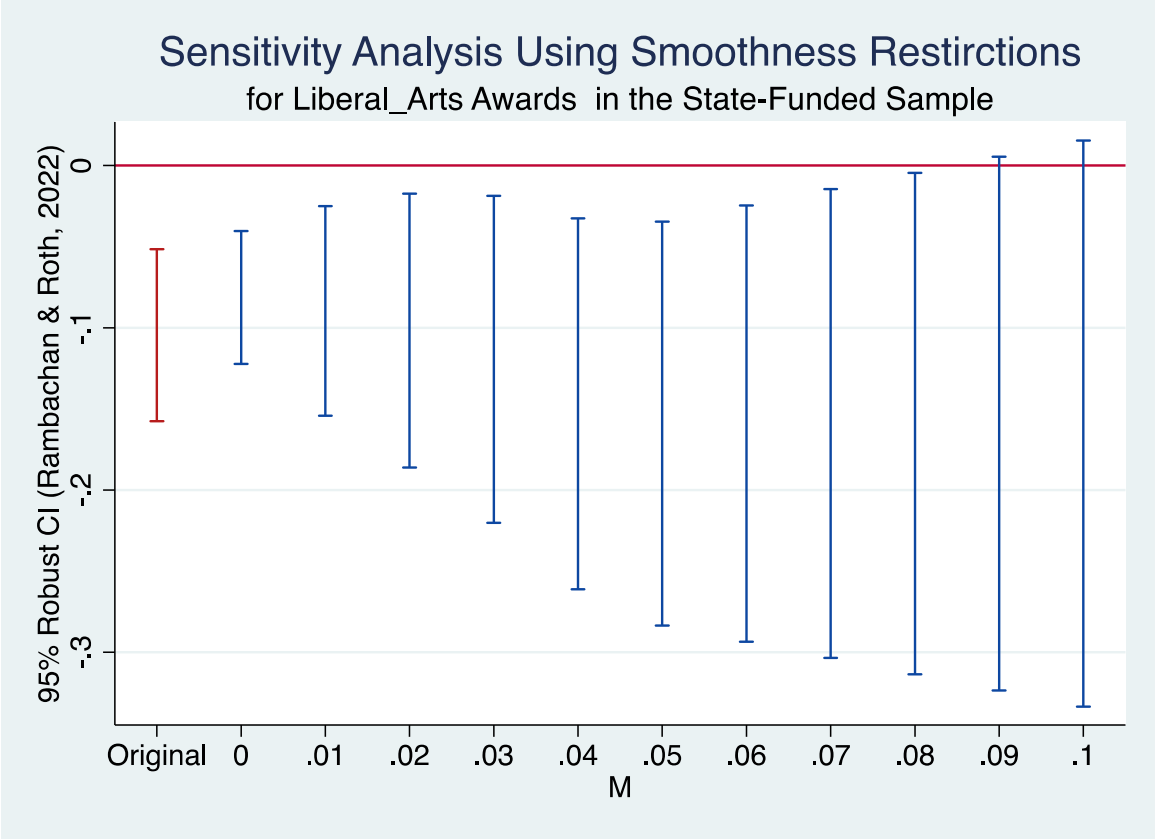

Figure 15

Smoothness Restriction Output for Liberal Arts Programs

Sensitivity analysis utilizing a smoothness restriction for sub-baccalaureate liberal arts program awards within the public institutions only sample. This analysis reveals a breakdown value for M that is greater than M=.01 which indicates that a staggered difference-in-difference analysis is appropriate. Liberal arts programs include the following programs from the 2-digit CIP designations: Area, Ethnic, Cultural, and Gender Studies; Foreign Languages, Literatures, and Linguistics; English Language and Literature/Letters; Liberal Arts and Sciences; General Studies and Humanities; Philosophy and Religious Studies; Theology and Religious Vocations; Psychology; Multi/Interdisciplinary Studies; Social Sciences; Visual and Preforming Arts; and History.

The *honestdid* package utilizes a robust inference approach proposed by Rambachan and Roth (2023) to establish a confidence interval that captures an estimation of treatment in an event study within a 95% confidence interval. It exploits intuition that the post-treatment trends of a difference-in-difference model can be informative about the pre-treatment trends of a model by splitting the model into two smaller difference-in-difference models. The first model compares the pre-treatment period(s) to the period of adoption and the second model compare the period of adoption to the post-treatment period(s). From these estimates, an estimate of an aggregate estimator is possible which can then be leveraged to assess for situations where use of a staggered difference-in-difference analysis is appropriate despite failing to establish traditional measures associated with meeting the parallel pre-treatment trends assumption.

I utilize the *honestdid* package to identify at what point null effect of M "broke down" or crossed zero as seen in **figures 14 and 15**. In otherwards, I am searching for at what point for M that the confidence interval estimating the effect of treatment straddles zero. This becomes the point at which I can no longer fail to reject a null effect for significant pre-treatment trends hypothesis. Note that the confidence intervals for one-to-two-year credentials do not breakdown for less than M=0.1. The breakdown for liberal arts programs occurred near M=0.08. This means that I can accept these two models as satisfying pre-treatment parallel trends as being robust against linear and non-linear violations of parallel trends (Kessler & Bruce, 2024; Rambachan & Roth, 2023).

For transparency, there is not an established or agreed upon level of M where results are deemed appropriate for staggered difference-in-difference approaches. Rather,

this test is about identifying how much non-linearity your model can take on before you fail to reject a null hypothesis of parallel pre-treatment trends based on a researcher's understanding of their data. Using this analysis under conditions where parallel trends are not met allows us ideally to establish a value of M that is greater than zero. Finding my values of M that are greater than zero, I accept that I meet the parallel trends assumption for these two variables in addition to my other seven outcome variables.

It should be noted that Rambachan and Roth (2023) describe two tests using their robust inference approach. The first utilizes bounds on relative magnitude of violations of the parallel trends assumption. The approach exploits logic that post-treatment violations of parallel trends should not be greater than those in the pre-treatment period. Then this approach projects these violations onto the confidence intervals calculated by Rambachan and Roth (2023) to define $\bar{M}$. This approach then calculates $\bar{M}$ at various levels based on how much a researcher allows post-treatment trends to violate the parallel trends assumption as compared to the pre-treatment period. This approach is most appropriate for analyses that examine industries that experience non-simultaneous industry shocks (Rambachan & Roth, 2023). Here, we identify what Rambachan and Roth refer to as the breakdown value of $\bar{M}$ where the robust confidence intervals cross zero. This value of $\bar{M}$ reflects the amount by which post-treatment violations can be larger than the pre-treatment violation of the parallel trends assumption before we can no longer accept that the pre-treatment trends are non-significantly different from zero.

The second test is more suited to data sets such as my own where the sample is contained within a single industry (i.e., postsecondary education) that experiences

similarly trending forces but have stronger unit-level, unique characteristics that might affect how they experience these shocks. This second approach utilizes a sensitivity analysis using a smoothness restriction that utilizes the changes in slope between the pre-treatment through adoption period and adoption through post-treatment periods to make conclusions about the appropriateness of applying staggered difference-in-difference analysis (Rambachan & Roth, 2023). This approach identifies the confidence intervals at *M* values as a measure of how much non-linearity we allow the post-treatment trends to violate the parallel trends assumption based on violations in the pre-treatment trends. Similarly to $\bar{M}$, the point at which a confidence interval crosses zero for *M* is also the point where a researcher must reject the null hypothesis that the studied pre-treatment trends are not significantly different from zero.

I opted to utilize the smoothness restriction test rather than the alternate examination of bounds of relative magnitude due to it being the option more suited to similarly behaving units over time (e.g., firms within one industry) that may have unit-specific time trends. The examination of bounds of relative magnitude are more appropriate for samples that experience asynchronous external shocks throughout the sample (e.g., firms across multiple industries affected by industry level shocks).

In the following results chapter, I will explore regression outputs for these nine models using Sun and Abraham's event study interaction estimator for my sample of public institutions. Regression outputs for the outcomes that did not meet the parallel pre-treatment trends assumption as well as models from other samples and estimators will be included in the appendices of this manuscript as specified in Chapter 4.

Study Limitations

I did not disaggregate my outcome variables by student demographic nor establish specific covariates to account for variations in promise program design. This is limiting in two ways. First, because I did not include an analysis of student demographics, I am not able to speak to potential differences in the effect of free college programs by student groups which has been documented to exist in other studies. Second, I am not able to conclude if there are differences in effect size between promise programs regarding variations in eligibility by geographic place, what the award covers, or how awards are distributed. Currently, this data exists both on individual promise program websites but remains to be properly organized and collected. A future project would benefit from spending time collecting this data to include in future analysis. However, at this time this was not feasible for the completion for my present project. Despite this, I find my study is still valuable as it uniquely discusses sub-baccalaureate awards in discreet categories by length and area of study which other studies have not done. This provides the field with unique utility in that I am able to offer estimates of how free-college programs affect completions.

By doing so, researchers will be able to establish how effective promise tools are at addressing long-standing inequities which would be of interest given sub-baccalaureate institutions traditionally serve a large number of students affected by these inequities. Additionally, due to limitations in my empirical and sample design, I only estimate the causal effect of promise program adoption for the five years following adoption. I want to note that it is possible to produce estimates beyond this five-year horizon. However, due

to a majority of promise programs being adopted relatively recently, I opt to limit my horizon to prevent long-standing promise programs from over contributing to my estimates due to a lack of observations for many promise programs beyond the five-year mark.

I am also not able to create a single aggregate estimate of effect. As a reminder, the programs capable of producing such a composite ATT such as the decomposition estimator are not able to account for my unit-specific time trends and thus not appropriate for causal analysis in my study. Compared to my preferred Sun & Abraham estimator, these decomposition-based estimators over-estimate the effect of promise program by around ten percentage points. Limited to my present event study design by current statistical packages, I can only offer ranges of increased completions following program adoption as I have done here.

Further, my design where I examine completions by program area of focus was potentially flawed. Because my sample summed together awards by academic area of focus with no attention paid to program length, there are instances where an outcome variable is influenced by the proportion of certain award lengths within a distribution of my academic program groups. It is possible that this contributed to non-significant results for completions by program focus. Based on observations of post-treatment p-values for liberal arts programs, I believe future studies could potentially further explore this issue by better considering the distribution of program length within the distributions of awards by academic program. Because this could reduce the number of awards within each outcome variable, future researchers should be attuned to potential issues with cell size in

projects that examine awards by academic program within discreet award types by program length. While such additional work falls out of the immediate scope of my project and its research questions, this represents a promising area for additional study.

Finally, my findings are limited to sub-baccalaureate awards with the majority of my institutions comprised of public community and technical colleges in both my preferred sample's treatment and comparison groups. This is problematic in two ways. First, it is beyond the scope of this study to understand how free college might vary in effect for longer four-year awards or in the for-profit or non-profit private postsecondary sectors. However, given my explicit focus on clarifying the effect of promise program adoption on sub-baccalaureate award production, this is of little concern to this present study. Instead, the second and more concerning feature of this research design choice is that like the studies that I critiqued earlier run the risk of constructing my sample in such a way that student enrollment substitution influences my findings.

Because I limit my sample to promise programs that only cover sub-baccalaureate awards, students affected by these programs may be substituting their enrollment away from four-year institutions to enroll in treated sub-baccalaureate institutions which could bias my findings in the opposite direction of past research by Bartik et al. (2021) as well as Swanson and Ritter (2020). While I cannot completely account for this potential, I do acknowledge that it is likely present in my design as evidenced by past research on sub-baccalaureate only promise programs (Carruthers & Fox, 2016; House & Dell, 2020; Nguyen, 2019). However, my design still offers utility despite this limitation. My study adds to the body of knowledge on college affordability, student outcomes, and promise

programs while highlighting a need for future work to address these enrollment substitution issues that are due to student choice as a behavior influenced by free college programs.

CHAPTER 4

In this chapter I present findings from my staggered difference-in-difference analysis. I focus my presentation of results on my preferred sample of public sub-baccalaureate granting institutions using Sun and Abraham's (2021) event study estimator and present findings for each of my research questions. I close the chapter with findings from robustness tests that utilize the imputation-based estimator and a decomposition-based approach. My research questions for this study are:

- What effect does the adoption of a free college program have on postsecondary program completions by program length (less than 1 year, 1 year to 2 years, and 2 years)?
- What effect does the adoption of a free college program have on postsecondary program completions by program's academic focus?

To answer these research questions, I examined the number of degrees a college produced (dependent variable) before and after promise program adoption (treatment). This was done across all awards produced and nine sub-groupings of the outcome variable of interest that meet the pretreatment parallel trends assumption. The sub-groupings by program length were the total of sub-baccalaureate awards, associates degrees, all credentials under 2 years in length, credentials that take between One-to-two years to complete, credentials that take less than a year to complete. The sub-groupings by program academic focus followed the five CIP codes displayed in **Table 2**. These sub-groupings are science, technology, engineering, and math (STEM); trades; liberal arts; and human services programs. To answer these research questions, I utilized a nationwide

sample from 2000-2022 of institutional sub-baccalaureate program completions from IPEDS as my outcome variable and assigned treatment from a promise program using the CollegePromise.org's inventory of promise programs. As a reminder, the professional services sub-group did not meet the parallel pretreatment trends assumption and thus will not be discussed here. Results for both sets of these sub-groups are presented in the relevant research question findings sections based on their examination of program length and academic area of focus respectively.

Findings Research Question #1

Research question one examined the effect of promise program adoption by program length. As a reminder the first research question of this study was: *What effect does the adoption of a free college program have on postsecondary program completions by program length (less than 1 year, 1 year to 2 years, and 2 years)?* Using available data from IPEDS for my sample period, I was able to separate sub-baccalaureate awards by program length into the following groups: associates degrees (programs that take a least two years to complete), credentials that take between one-to-two years to complete, and credentials that take less than one year to complete. I also included regressions that considered the total of all sub-baccalaureate awards produced and a measure that combined both sets of credentials for a total of five models that examine award production by program length. The total sub-baccalaureate count included both associates degrees and all credentials that take less than two years to complete. The total credentials count included credentials that take between one-to-two years to complete and credentials

that take less than a year to complete. This was done to construct a more holistic view of how credentials may vary from two-year degrees.

I present my regression outputs for this research question in **Table 7** from the Sun and Abraham (2021) event study interaction specification for each of my five sets of program lengths. Following the evidence presented in Chapter 4 regarding the pre-treatment trends analysis, I know that all five of these outcome variables are appropriate for analysis. The outcomes for all awards, associates degrees, all credentials, and credentials that take less than one year to complete show evidence of pretreatment trends that were not significantly different from zero.

For the three outcome variables where analysis using staggered difference-in-difference was not supported by evidence that the parallel trends assumption was met, I utilized the *honestdid* package to further examine these outcome variables. Using a sensitivity analysis of smoothness restriction, I was able to compare the slope of my sample within the pre-treatment period and compare that the slope of my sample within the post-treatment period. I then examined the robust confidence intervals which examine what degree of non-linearity my model's pre-treatment trends can accommodate before pre-treatment trends become significant.

As for one-to-two year credentials, a smoothness restriction analysis of the robust confidence intervals using the honestdid package as described by Rambachan and Roth (2023) indicated that a regression examining this is robust up to and beyond M=0.1 suggesting that my results are able to hold even when allowing for the deviations for parallel trends suggested by the joint and individual coefficient tests of significance. As a

Table 7

Regression of Outputs for Awards by Program Length

	All Awards	Associate's degrees	All Credentials	One-to-Two Year Cred.	Less than 1 Year Cred.
Pre-5	-.0015	-.0171	.0474	.0910	.0266
	(.012)	(.0102)	(.0346)	(.0467)	(.0221)
Pre-4	.0147	-.0111	.0822*	.1830**	. 0487
	(.0131)	(.0107)	(.0333)	(.0548)	(.0268)
Pre-3	.0242	.0008	.0909**	.2122***	.0751**
	(.0128)	(.0109)	(.0337)	(.0529)	(.0255)
Pre-2	.0150	-.0072	.0647	.1870***	.0561*
	(.0140)	(.0119)	(.0370)	(.0524)	(.0266)
Post-0	.0210	-.0084	.0778	.2013***	.0813**
	(.0167)	(.0146)	(.0425)	(.0576)	(.0305)
Post-1	.0231	-.0098	.0800	.1280*	.0830*
	(.0181)	(.0168)	(.0480)	(.0631)	(.0323)
Post-2	.0423*	.0100	.2113**	.0834	.1106**
	(.0212)	(.0197)	(.0620)	(.0742)	(.0385)
Post-3	.0540*	.0257	.1011	.0498	.1020*
	(.0227)	(.0215)	(.0723)	(.0819)	(.0416)
Post-4	.0862**	.0724*	.1070	.01772	.0940*
	(.0250)	(.0228)	(.0756)	(.0813)	(.0414)

* p<0.05, **p<0.01, ***p<0.001

reminder, the Sun and Abraham (2021) estimator did not produce coefficient estimates for the pre-treatment period immediately prior (pre-1) to the first period after treatment (post-0).

Examining individual post-treatment period coefficients for each of the five outcome variables in this Sun and Abraham specification offered an inconclusive look at whether there is an actual effect on treated institutions. Therefore, I supplemented this analysis with a joint test of significance of the post-treatment coefficients to examine whether they significantly varied jointly from zero. I provide this joint test in **Table 8** where I present joint test results for posttreatment coefficients with no lag, a one-year lag for shorter term credentials, and two-year lag for associates degrees. However, on-time completion is not the norm for sub-baccalaureate students with only 35% of students from 2-year institutions graduating within 3-years and 65% of students graduating from their less than 2-year programs within time-and-a-half of their program's length (National Center for Education Statistics, 2024d).

Because of this, I utilized all three joint tests to shape my interpretation of findings for credential awards. As for associates degrees, I was limited by the recent adoption of many promise programs and was unable to include an adequate lag for these awards. Instead, a study that included potential late graduations for associates degree students will have to wait until a later time when these promise programs have been in operation for a longer period of time.

Table 8

Joint Test of Post-Treatment Outputs

	All Awards	Associate's degrees	All Credentials	One-to-Two Year Cred.	Less than 1 Year Cred.
No Lag	0.031*	0.004**	0.014**	0.010**	0.053
1 Year Lag	0.016**	0.002**	0.007**	0.328	0.040*
2 Year Lag	0.008**	0.003**	0.003**	0.705	0.026*

* p<0.05, **p<0.01, ***p<0.001

Examination of the joint test of significance for post-treatment periods found that the coefficients for the effect of treatment over the five periods following promise program adoption were significant for the outcome variables of all awards, associates degrees, all credentials, and one-to-two-year credentials. However, less than one-year credentials were not jointly significant without a lag year despite each period being independently significantly different from zero.

Credentials that required less than one year to complete would be a group that naturally made the most sense to not require a lag given that a student could enroll in and likely complete a shorter-term credential within the first year of adoption. However, I did not believe the lack of a jointly significant difference from zero when I examined for no lag should be considered as problematic. This was because students who enroll in these short-term programs are historically from low-income and other underrepresented groups (Carnevale, Van Der Werf, Quinn, Strohl, & Repnikov, 2018; Marcotte, Bailey, Borkoski, & Kienzl, 2005).

I adopted the following strategy for interpreting significant post-treatment coefficients. For all awards, I considered the lagged coefficients following one and two years after adoption as potentially informative about post-treatment effects. For associates degrees, I only considered the coefficients from the third post-treatment period onward as informative about post-treatment effects. For credentials, I only considered coefficients from the second post-treatment period and on as informative about post-treatment effects.

With these lags in mind and evidence of significant findings, I identified a positive 2.3% - 8.6% increase in the number of sub-baccalaureate awards produced by treated colleges when compared to colleges without promise programs depending on the period after treatment examined. For associates degrees only, this increase in completions ranged from about 1.0% - 7.2%. These wide gaps in treatment effect offered some potential insight into the effect of these programs for increasing awards overall and associates degrees awarded. As the periods progressed, toward five years following adoption, the coefficient increased. While this study was not set up to examine why this increase may occur, there are a variety of potential reasons such as increased awareness of the program leading to more participation and students making different decisions regarding their postsecondary enrollment in light of new information or students that taking longer than expected to complete their program graduating.

As for credentials, the number of all credentials awarded increased 8.0% - 21.1%. However, unlike associates degrees or all awards, this increase was not consistently rising throughout the post-treatment period. After the third post-treatment period, the coefficient decreased to around 10% for the remainder of the periods. Meanwhile, because I did not find evidence of a consistent significant change in the number of awards from one-to-two-year length credential programs, I cannot confidently say free college programs significantly increased this subsection of credentials. As for short-term credentials that take less than one year to complete, I found evidence of an increased number of credentials awarded by 8.3% - 11.1% that reached a peak during the third period before falling to 9.4% and 10.2% in the final periods.

These increases provided evidence that free college programs can increase the number of graduates at the sub-baccalaureate level. Further, increases at the associates, credential level, and overall levels were a surprisingly optimistic finding that suggested that it is possible that the change in number of associates degrees awarded was not only driven by students substituting shorter term credentials for associates degrees that take longer to earn. Instead, this lack of an overall decrease to shorter term credentials when institutions offer free college programs points toward the potential that not only more students are completing a sub-baccalaureate postsecondary education overall following free college adoption, but that these additional completions are distributed across both degree and credential programs.

Findings Research Question #2

Research question two examined the effect of promise program adoption by field of study. As a reminder the second research question for this study was: *What effect does the adoption of a free college program have on postsecondary program completions by program's academic focus?* Due to concerns over program graduation count sizes, I consolidated academic programs into five major families based on similarities in their CIP codes after pulling available data from IPEDS for my sample period. The major groups I found were organized into the groups provided in **Table 2** in Chapter 3. The groups I identified are organized into the following broad academic fields: STEM, trades, liberal arts, human services, and professional services. However, testing of the pre-treatment parallel trends assumption found that the field of professional services was not

appropriate for analysis using my staggered difference-in-difference design. Analysis of these award groups was done without consideration to program length.

I presented my regression outputs for this research question in **Table 9** from the Sun and Abraham event study interaction specification for each of my five sets of program lengths. Because the fifth outcome variable of awards under the professional services area were not appropriate for analysis, I did not present this outcome variable in **Table 9**. I included the model for the outcome of all awards as done in research question one.

The outcomes for all awards, STEM, trades, and human services awards provided evidence of pre-treatment trends that were not significantly different from zero. As for liberal arts awards, a smoothness restriction analysis of the robust confidence intervals using the honestdid package as described by Rambachan and Roth (2023) indicated that a regression examining liberal arts awards was robust up to and beyond M=0.1. This means that my assumption that parallel pre-treatment trends were meet could hold even when allowing for the deviations in parallel trends suggested by the significant outputs from the joint and individual coefficient tests of significance.

For these models, I found a lack of compelling evidence that the adoption of a promise program significantly increased awards within specific academic sub-areas. Examination of significance among individual post-treatment coefficients provided some evidence of change significantly different from zero and negative in trend for Liberal Arts awards in the first two post-treatment periods. However, many of these Liberal Arts awards were from two-year associates degree programs.

Table 9

Regression of Outputs for Awards by Program Length

	All Awards	STEM	Trades	Liberal Arts	Human Services
Pre-5	-.0015	.0233	.0298	-.0484*	.0075
	(.0122)	(.0296)	(.0313)	(.0186)	(.0323)
Pre-4	.0147	.0660*	.0486	-.0479*	.0207
	(.0131)	(.0304)	(.0354)	(.0187)	(.0345)
Pre-3	.0242	.0625*	.0606	-.0376	.0504
	(.0128)	(.0304)	(.0365)	(.0209)	(.034775)
Pre-2	.0150	.0522	.0183	-.0547*	.0513
	(.0140)	(.0327)	(.0355)	(.0225)	(.0367)
Post-0	.0210	.0547	.0269	-.1002***	.0601
	(.0170)	(.0354)	(.0445)	(.0264)	(.0394)
Post-1	.0231	.0639	.0092	-.1112***	.0475
	(.0181)	(.0378)	(.0437)	(.0281)	(.0425)
Post-2	.0423*	.0579	.0650	-.0463	.0416
	(.0212)	(.0455)	(.0527)	(.0347)	(.0456)
Post-3	.0540*	.0117	.0369	.0194	.0869
	(.0227)	(.0495)	(.0570)	(.0347)	(.0496)
Post-4	.0862**	.0375	-.0265	.0876*	.0231
	(.0250)	(.0511)	(.0615)	(.0371)	(.0521)

* p<0.05, **p<0.01, ***p<0.001

Thus, the first two post-treatment periods were less informative about the overall effect of promise program adoption for this set of programs.

A caveat that requires future study would utilize a design that considers program features such as if promise program adoption affected all students enrolled or just new, incoming students. This would allow researchers to consider variation in whether a program affects only current and future in-coming students at time of adoption or affects all students currently enrolled in addition to future students. Including this level of analysis in a future study could allow for further clarity on whether immediate effects should be expected following program adoption. However, this type of analysis is beyond the scope of this initial project. Turning the joint tests to see if the post-treatment overall varies significantly from zero, I observed that liberal arts awards but not STEM, trades, or human services programs vary significantly from zero. These joint tests results are presented below in **Table 10**.

These results suggested that there were not significant increases regarding the number of students who graduated from specific program areas of study at institutions with free college programs when compared to institutions without these programs. These findings were not entirely contradictory to findings from research question one and instead merit additional study in the future. I suggest that my regression specifications returned non-significant effects because the distribution of programs of study across program length did not exist in a manner that is uniform. Returning to liberal arts for example, a majority of graduates in this academic area came from associates degree

Table 10

Joint Test of Post-Treatment Outputs

	All Awards	STEM	Trades	Liberal Arts	Human Services
No Lag	0.031*	0.382	0.413	0.000***	0.403
1 Year Lag	0.016**	0.285	0.320	0.000***	0.386
2 Year Lag	0.008**	0.350	0.229	0.002**	0.267

* p<0.05, **p<0.01, ***p<0.001

programs while a majority of trades program graduates came from credential length programs.

Robustness Tests

I utilized both my imputation models and estimates from de Chaisemartin and D'Haultfœuille's (2020) decomposition approach to test the robustness of my results. I present the results of these approaches in this section. I then close this chapter with a summary before moving on to Chapter 5 where I discuss implications for my study's results.

Imputation Approach Model Results

While Borusyak et al.'s (2020) imputation approach was not selected as the preferred model for this study, I believed that the output of these models may still be informative regarding the validity of the estimates produced by the Sun and Abraham models. Referencing **Tables 6 and 7** in Chapter 3 revealed a similar pattern in what models I can interpret as meeting the pre-treatment parallel trends assumption. However, this imputation-based model found that the models for associates degrees, trades, and liberal arts did not provide evidence of meeting the parallel trends assumption. The use of the *honestdid* package did not identify any of these three outcome variables that were appropriate for analysis through a staggered difference-in-difference approach. Examining the outputs for these imputation-based models as provided in Appendix I revealed issues regarding the robustness of my findings using Sun and Abraham's (2021) event study estimates. This was because the Borusyak et al. (2021) model did not find

that the adoption of a free college program increased the number of sub-baccalaureate awards for any of my outcome measures.

Below I present combined event study plots of the coefficients for both the Imputation and OLS models for comparisons of my estimators. Providing these joint event study plots allowed for the comparison of effect sizes; however, with differences between tests of significance between my event study and imputation-based models, my findings face considerable limitations. I provide these joint event study plots in **figures 16 through 25** for my dependent variables in the following order: all awards, associates degrees, all credentials, credentials from programs that last one-to-two years, credentials from programs that take less than a year to complete, STEM programs, trades programs, liberal arts programs, human services programs, and professional services programs.

While awards from the professional services lacked evidence to meet the parallel trends assumption, I include them in these figures for transparency. I include OLS estimates not for purposes of analysis but rather as an additional reference point of how these other two estimates differ from each other. Outputs for each of these combined event plots for the three samples other than the public institutions sample have been included in Appendix J for my primary, public institutions only sample, Appendix K for the sample restricted to state-funded public institutions, Appendix L for the sample only including non-profits, and Appendix M for the sample including for-profits.

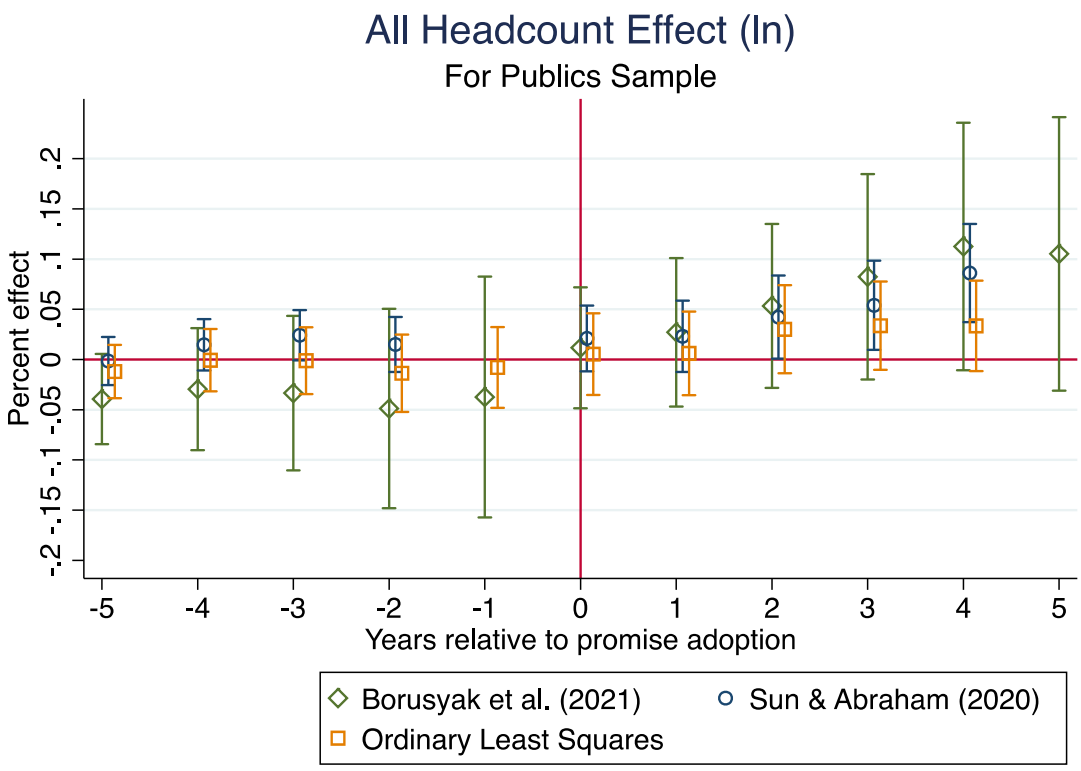

Figure 16

Combined Event Plot: All Awards

Natural log difference between treatment and comparison groups of all sub-baccalaureate awards within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

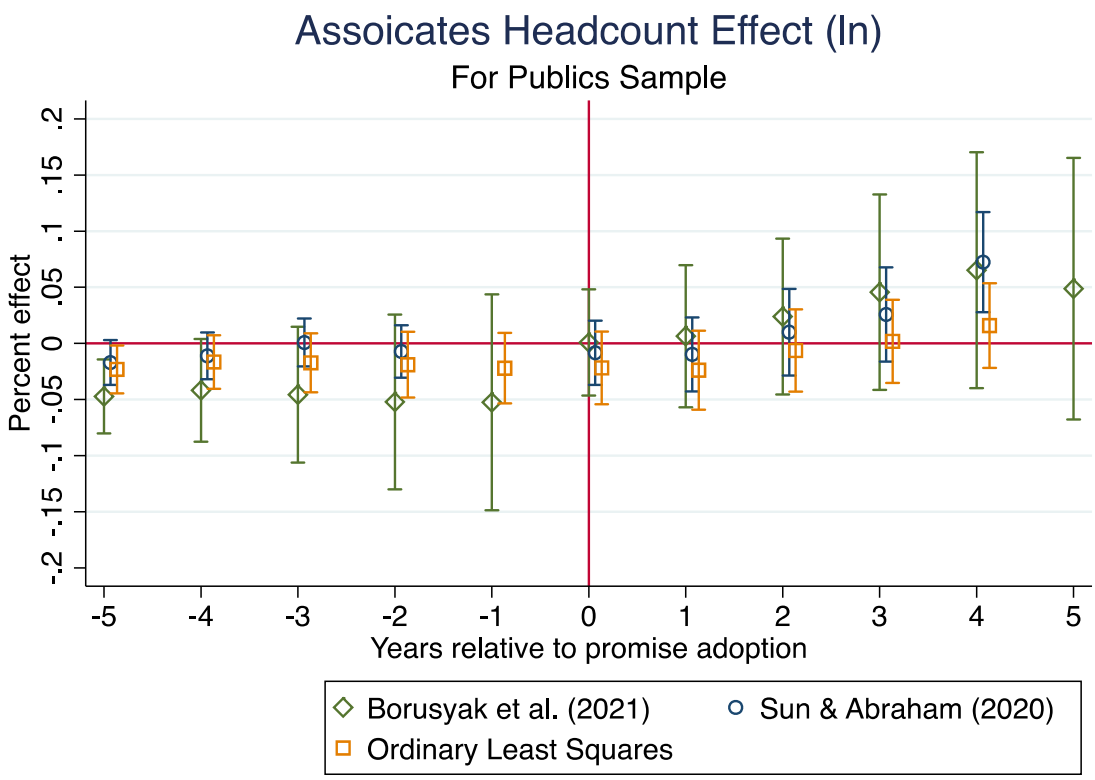

Figure 17
Combined Event Plot: Associate degrees
Natural log difference between treatment and comparison groups of associates degree awards within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

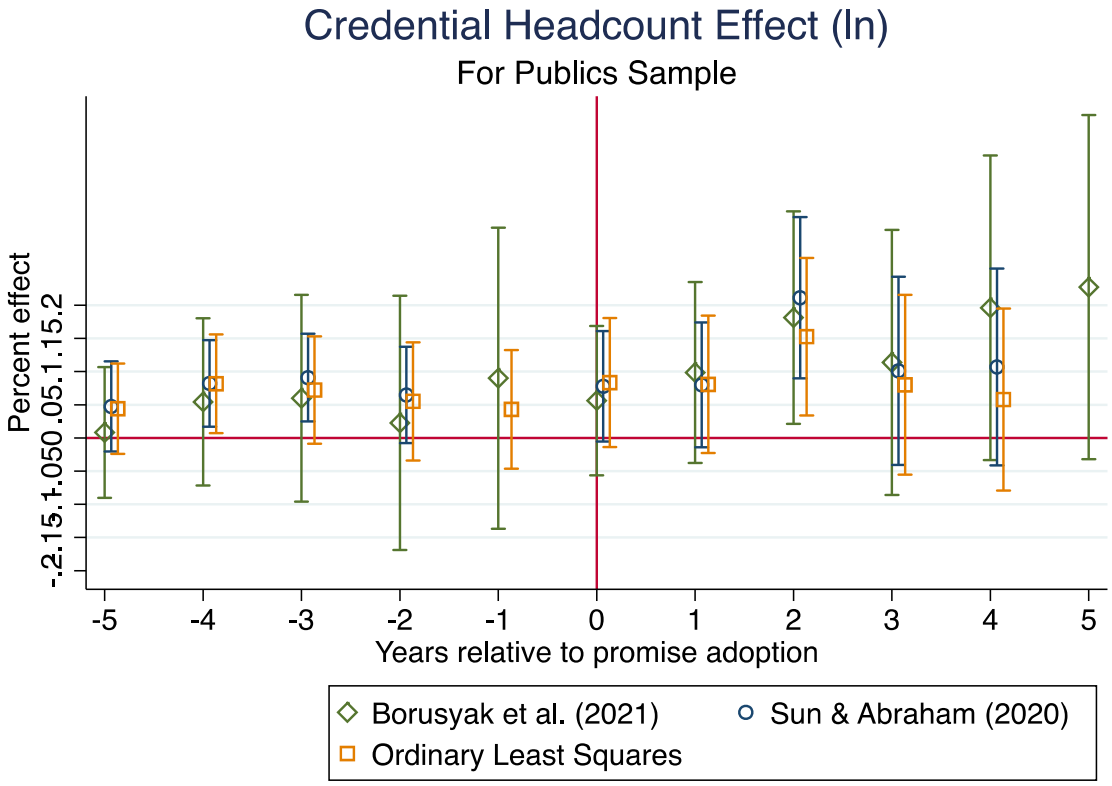

Figure 18

Combined Event Plot: All Credentials

Natural log difference between treatment and comparison groups of all credential awards that were granted within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

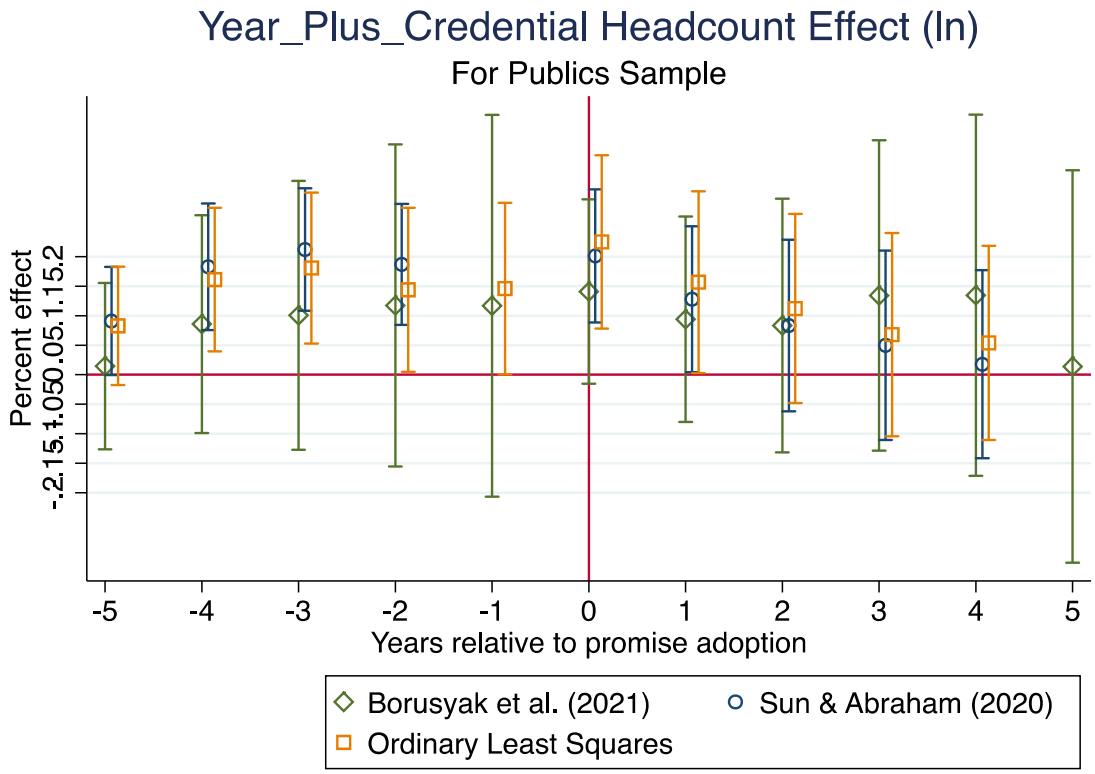

Figure 19

Combined Event Plot: One-to-two Year Credentials

Natural log difference between treatment and comparison groups of awards from credential awards that take between one and two years to complete that were granted within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

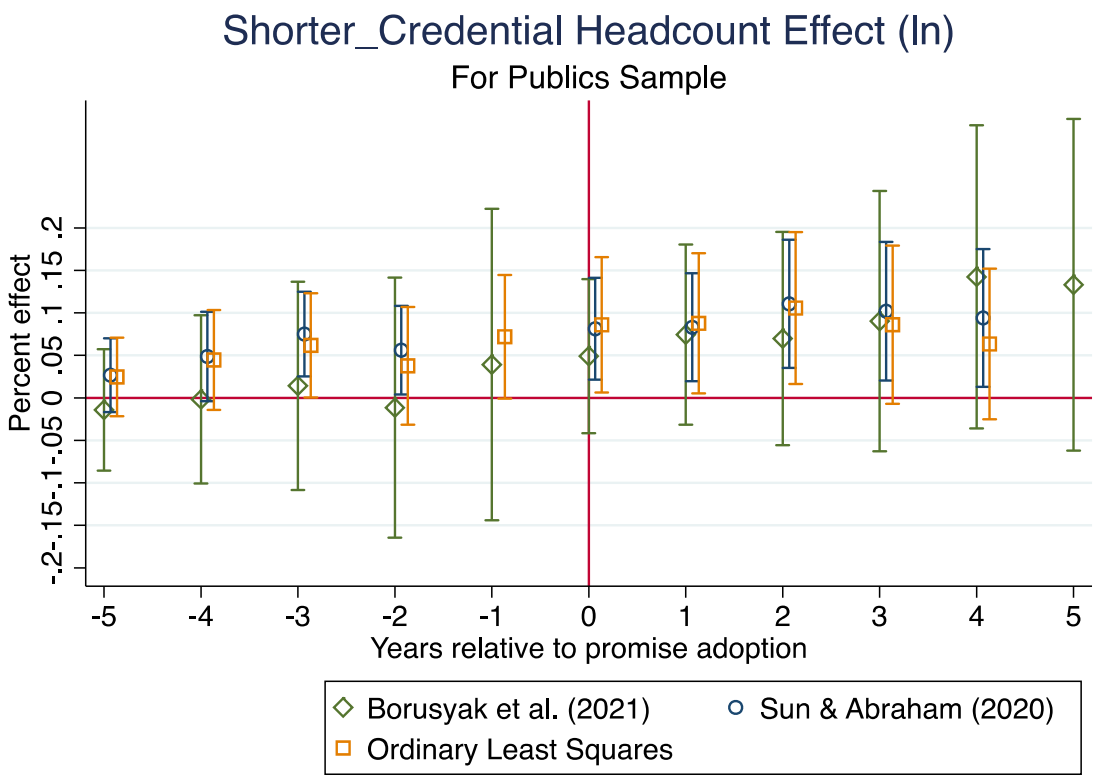

Figure 20

Combined Event Plot: Less Than One Year Credentials

Natural log difference between treatment and comparison groups of awards from credential awards that take less than a year to complete that were granted within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group.

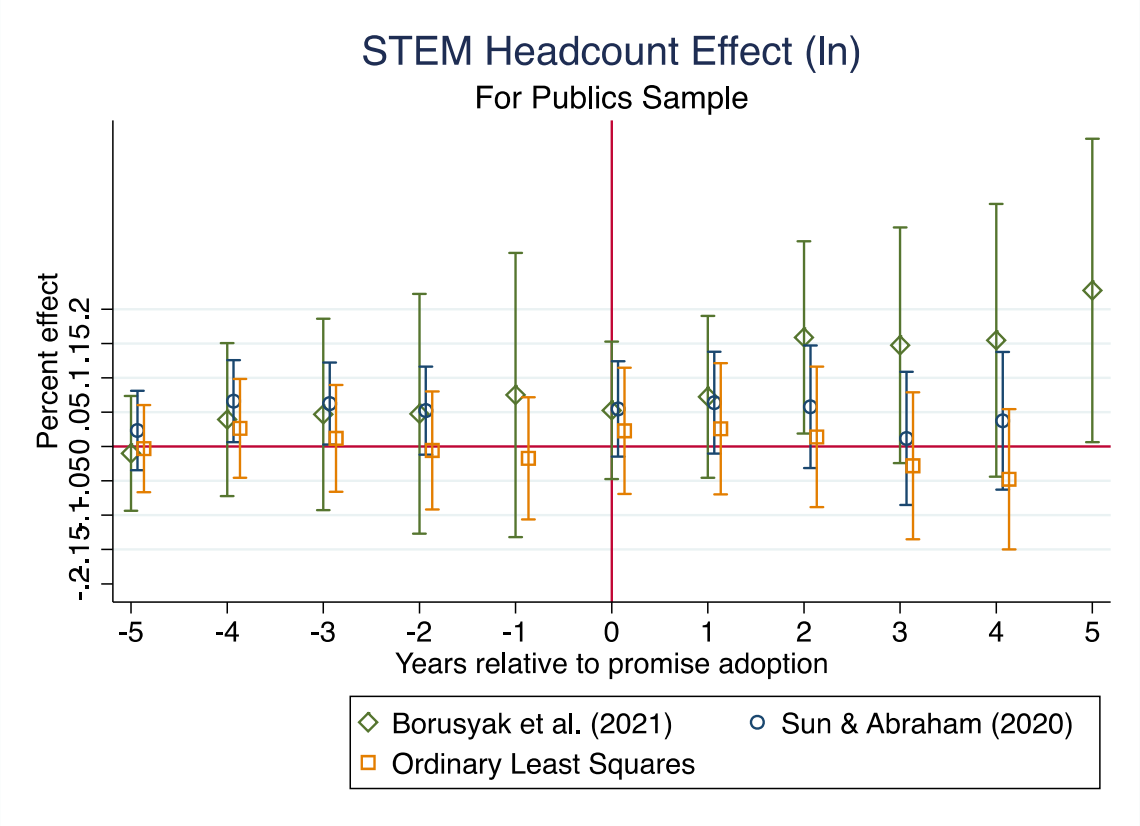

Figure 21

Combined Event Plot: STEM Awards

Natural log difference between treatment and comparison groups of awards from sub-baccalaureate STEM programs that were granted within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group. STEM programs include the following programs from the 2-digit CIP designations: Agriculture, Agriculture Operations, and Related Sciences, Natural Resources and Conservation, Computer and Information Sciences and Support Services, Engineering, Engineering Technologies/Technicians, Biological and Biomedical Sciences, Mathematics and Statistics, Physical Sciences, and Science Technologies/Technicians.

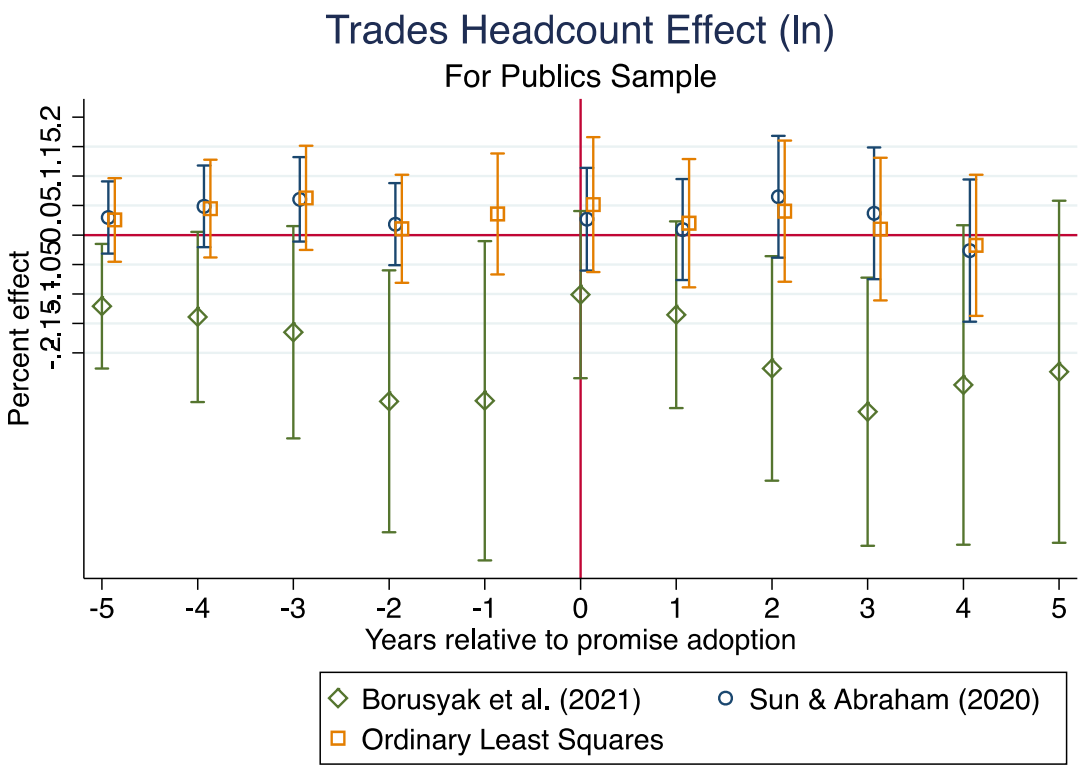

Figure 22

Combined Event Plot: Trades Awards

Natural log difference between treatment and comparison groups of awards from sub-baccalaureate trades programs that were granted within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group. Trades programs include the following programs from the 2-digit CIP designations: Construction Trades, Mechanic and Repair Technologies/Technicians, Precision Production, and Transportation and Materials Moving.

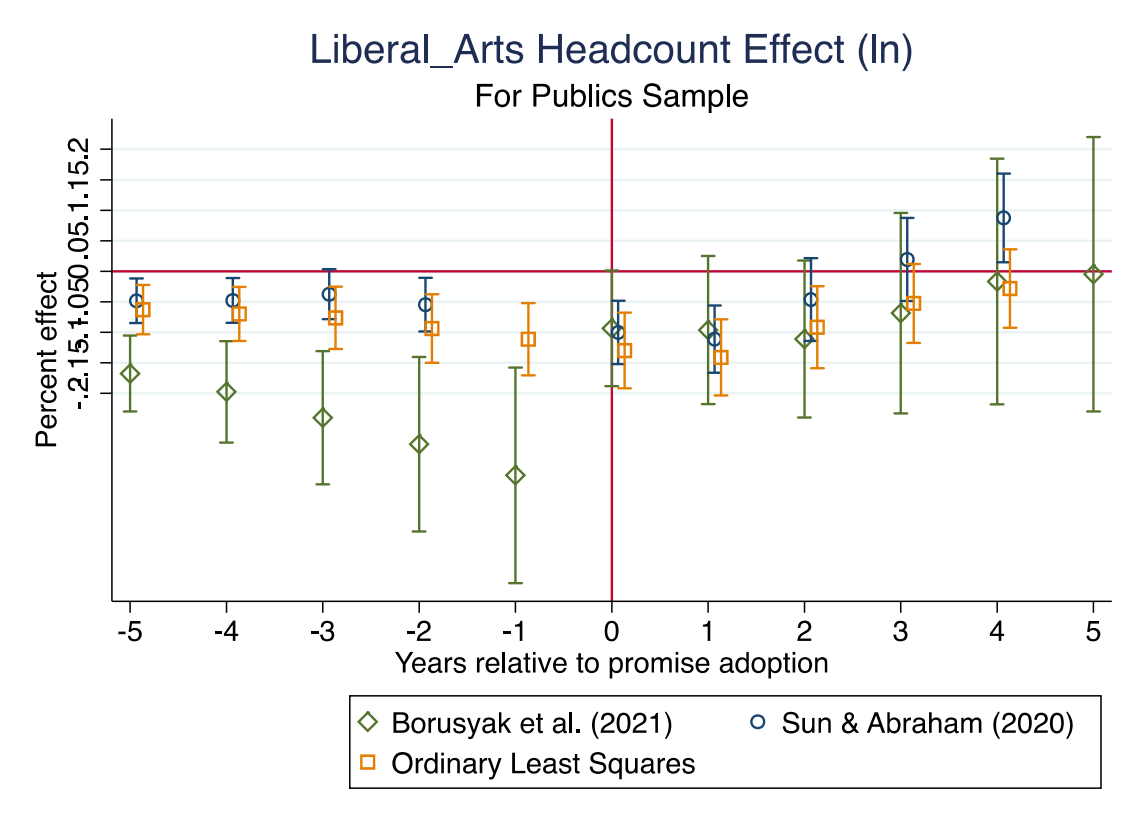

Figure 23

Combined Event Plot: Liberal Arts Awards

Natural log difference between treatment and comparison groups of awards from sub-baccalaureate liberal arts programs that were granted within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group. Liberal arts programs include the following programs from the 2-digit CIP designations: Area, Ethnic, Cultural, and Gender Studies; Foreign Languages, Literatures, and Linguistics; English Language and Literature/Letters; Liberal Arts and Sciences, General Studies and Humanities; Philosophy and Religious Studies; Theology and Religious Vocations; Psychology; Multi/Interdisciplinary Studies; Social Sciences; Visual and Preforming Arts; and History.

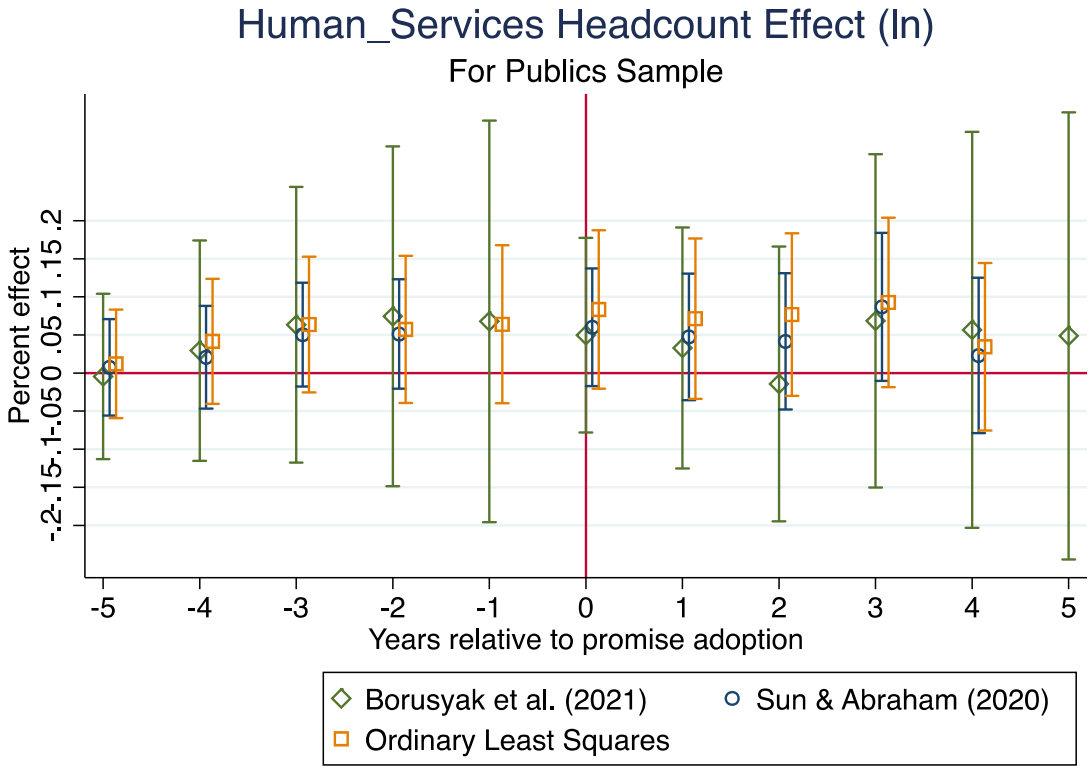

Figure 24

Combined Event Plot: Human Services Awards

Natural log difference between treatment and comparison groups of awards from sub-baccalaureate human services programs that were granted within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group. Human services programs include the following programs from the 2-digit CIP designations: Personal and Culinary Services, Family and Consumer Sciences/Human Sciences, Parks, Recreation, Leisure, and Fitness Studies, Basic Skills, Citizenship Activities, Health-Related Knowledge and Skills, Interpersonal and Social Skills, Leisure and Recreational Activities, Personal Awareness and Self Improvement, and High School/Secondary Diplomas and Certificates.

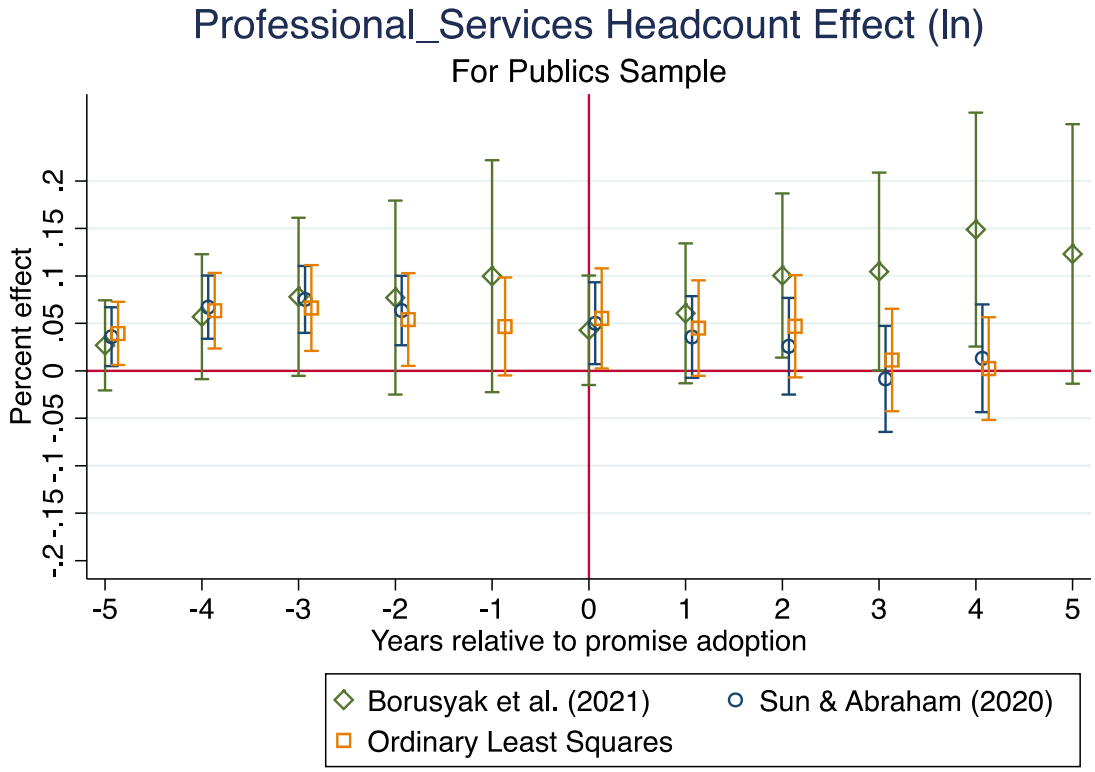

Figure 25

Combined Event Plot: Professional Services Awards

Natural log difference between treatment and comparison groups of awards from sub-baccalaureate professional services programs that were granted within the public institutions only sample. This event study visualization combines multiple estimators. The Borusyak et al. *did_imputation* command is visualized in green. The Sun & Abraham *eventstudyinteract* command is visualized in blue. The OLS *reghdfe* command is visualized in orange. Period zero indicates the year of promise program adoption and each period on the x-axis reflects a year. The y-axis reflects treatment effect on the treated group compared to the comparison group. Professional services programs include the following programs from the 2-digit CIP designations: Architecture and Related Services; Communication, Journalism, and Related Programs; Communications Technologies/Technicians and Support Services; Education; Legal Professions and Studies; Library Science; Reserve Officer Training Corps (JROTC, ROTC); Military Technologies; Security and Protective Services; Public Administration and Social Service Professions; Health Professions and Related Clinical Services; and Business, Management, Marketing, and Related Support Services.

The finding that none of the models that meet the parallel trends assumption across both the event study and imputation approaches to my study produced results that served to confirm the robustness of either mode is concerning. It suggests that my findings were not robust and that there may have been concerns regarding external validity. However, given the documented issues with Borusyak et al.'s (2020) imputation approach in estimating correct coefficients and my model's complexity due to unit-specific time trends, I found it important to not totally discount the findings of my preferred Sun and Abraham model.

Decomposition Approach Results

I utilized de Chaisemartin and D'Haultfœuille's (2020) decomposition approach as a second method to examine the robustness of my findings while also diagnosing potential issues with negative weighting in my data. De Chaisemartin & D'Haultfœuille posited that the conventional DID estimator could be expressed as a weighted average of causal effects (ATEs) across group-time cells, some of which are assigned negative weights. I used this approach to arrive at estimates of effect in a staggered difference-in-difference design which was useful in two ways. First, it was an additional way to examine average effect on the treated. Albeit this estimate was produced as a singular average treatment effect or ATT on the treated that was not bound to a window on pre-treatment and post-treatment periods. This meant that an ATT produced via this decomposition approach featured influence from the later post-treatment periods of early treated units and earlier pre-treatment periods of later treated units. Second, it also provided a means to not only identify the number of negative weights created in my

estimation of an effect but also how strong those weights were which is informative regarding negative weights issues common to staggered difference-in-difference designs. I provide ATT estimates, negative weight counts, and the sum of those negative weights in **Table 11**.

For each of these outcome variables, I found the models constructed through the decomposition approach did not present concerns regarding negative weights with only two-to-four of the 171 ATTs calculated through this model being assigned a negative weight of less than -0.0002 or -0.0001 for each outcome. Because the sum of all weights added up to one, even the model with the largest sum of negative weights only contributes 0.2% of the final ATT estimate. This suggested that my data and de Chaisemartin and D'Haultfœuille's decomposition output which was a weighted average of ATTs across all instances of treatment did not suffer from problems concerning negative weights found in staggered difference-in-difference models.

However, these estimates of the ATT were considerably higher than my earlier Sun and Abraham event study estimates. This could be in part due to the decomposition approaches accounting for all potential treatment year and pre/post-treatment periods. Additionally, the size of these coefficients closely resembled the outputs of variables for models that did not consider my data sample's issues with unit-specific time trends. Based on the foundational work for the *twowayfeweights* package, there was no indication that this package can account for issues of unit-specific time trends (De Chaisemartin, & d'Haultfoeuille, 2020). Therefore, caution should also be applied when

Table 11

Outputs from de Chaisemartin & D'Haultfœuille Decomposition

	ATT	Negative Weight Count	Sum of Negative Weights
All Degrees	0.2397	2	-0.0001
Associate's degrees	0.2314	2	-0.0001
All Credentials	0.3778	2	-0.0001
One-to-two Year Credentials	0.3214	2	-0.0001
Less than One Year Credentials	0.3627	4	-0.0002
STEM Awards	0.3763	2	-0.0001
Trade Awards	0.2552	2	-0.0001
Liberal Arts Awards	0.1826	2	-0.0001
Human Services Awards	0.2535	2	-0.0001
Professional Services Awards	0.2274	2	-0.0001

considering the implications of this second robustness test which generated considerably higher estimates of effect on my treated sample.

Summary of Findings

I utilized two strategies in a robustness check of my findings produced via the Sun and Abraham interaction-based estimator. However, my results for both of these robustness check strategies differed from those of my preferred model. Both robustness checks found coefficients of the same sign which provides some supportive evidence regarding my findings but are far from entirely supportive. While I believed it is important to consider this as reason to interpret the results of my preferred model with due caution, I also found these differing results as more reflective of issues that led to both approaches not being selected as my primary analysis strategy for this study.

The imputation approach did not meet the parallel pre-treatment trends assumption for three of my ten outcome variables. Further, it did not produce significant results for the models that did meet the assumption of parallel pre-treatment trends. The decomposition approach appeared to not account for unit-specific time trends and thus provided upwardly biased estimates of graduates that reflected an increasing number of graduates over the past two decades rather than an effect induced by promise program adoption. These shortcomings tempered my interpretation of these robustness checks as not directly contradictory to my main estimators found via Sun and Abraham's (2021) event study approach. Instead, it should be considered that the shortcomings in these approaches reiterated that the Sun and Abraham interaction-based event study approach was the best model for this study given the state of staggered difference-in-difference

methods. To summarize the findings of the Sun and Abraham event study approach, I found evidence that promise programs do increase sub-baccalaureate awards at the associates degree and short-term credential level. I estimated that the effect of promise program adoption increases award production at institutions by up to 8.6% for all sub-baccalaureate awards.

CHAPTER 5

My purpose in conducting this study was to estimate the effect of promise programs on sub-baccalaureate completion rates utilizing a staggered difference-in-difference design. Providing a national estimate for the effect of promise programs holds significance to the field of postsecondary education policy because as the cost of college continues to increase, these free college programs have proliferated. In response states, non-profits, and postsecondary institutions have invested the funding necessary to provide students with access to free college during this proliferation of college promise programs over the past twenty years.

While these programs vary in what is covered and how they affect student charges, the message of free college for students has been found to increase student outcomes such as enrollment and completion (Bartik et al., 2021; Bifulco, 2019; Burland et al., 2023; Carruthers and Fox, 2023; Daughtery & Gonzalez, 2016; Dynarski et al., 2023; Gándara & Li, 2020; Li & Gándara, 2020; Nguyen, 2019; Nguyen et al., 2019; St. John et al., 2003; Swanson et al. 2020). However, the adoption of these promise programs has been non-uniform and spread organically as states, non-profits, and institutions secure the funding and support to fund the costs that are off boarded from students when a promise program is adopted (Billings, 2018; Perna & Leigh, 2018). This natural proliferation has created considerable challenges to establishing a quasi-experimental design that can handle the staggered adoption of these programs.

Using recent advances in econometric design for staggered difference-in-difference or event study designs, I provided estimates for how promise programs affect

award production. This is an important issue to explore as the number of jobs that require some form of postsecondary education are predicted to increase through 2030 (Carnevale, 2023). Failing to produce a supply of educated workers to meet this demand not only limits the economic potential of individual workers without a postsecondary education but also can result in less-than-optimal performance for the economy writ large (Handel, 2024; Strohl, Mable, & Campbell, 2024).

Free college or promise programs which largely apply to sub-baccalaureate postsecondary opportunities, represent a means to respond to the demand for educated workers. The theory of action for these programs is that by reducing the barrier of cost, more students will enroll and then graduate from postsecondary programs. These students then will go on to fill the demand of the labor market and hopefully access better economic opportunities than available to them without a postsecondary education (Miller-Adams, 2021).

I utilized a staggered difference design to examine award production at institutions in attempt to estimate an aggregate causal effect of promise program adoption. To accomplish this, I constructed the following two research questions:

1) What effect does the adoption of a free college program have on postsecondary program completions by program length (less than 1 year, 1 year to 2 years, and 2 years)?
2) What effect does the adoption of a free college program have on postsecondary program completions by program's academic focus?

Review of Empirical Strategy

To answer these research questions, I constructed a national sample spanning 23 years that combined the sub-baccalaureate award production for institutions with records of promise program adoption. These records of promise program adoption served as my independent treatment variable. I organized my outcome variable of institutional awards into ten sums of sub-baccalaureate awards to answer my two research questions.

To answer my first research question, I examined the number of all sub-baccalaureate awards, number of associates degrees, number of any credential that takes under two years to obtain, number of credentials that take one-to-two years to obtain, and number of credentials that take less than one year to obtain. As a reminder, I examined the total of all sub-baccalaureate awards and a sub-total of all credentials in addition to associates degrees and each of my two credential types in an attempt to thoroughly understand how program length could influence the effect of promise programs on award production.

To answer my second research question, I examined five categories of academic programs: STEM, trades, liberal arts, human services, and professional services. I constructed these five broad categories of my own creation from available CIP codes due to a lack of a crosswalk that could be used simultaneously for CTE and non-CTE awards to group CIP codes into fewer categories for statistical analysis.

To conduct a difference-in-difference analysis, there must be evidence of parallel pre-treatment trends. Using my preferred sample of public institutions and preferred empirical strategy of Sun and Abraham's interaction-based estimator, I found that each of

my outcome variables excluding awards form the professional services field were appropriate for my analysis. As a reminder, I controlled for unique institutional trends or what could be referred to as unit-specific time trends in completion through the inclusion of an interaction between a specific unit and a time-trend variable.

While my research design was not equipped to get at issues of students substituting into longer term academic programs as they become subsidized by free college programs, it would appear that this concern was mitigated by student enrollment and subsequent increased program completion patterns following promise program adoption. Simultaneously, I wished to provide caution that this sub-baccalaureate only sample may be influenced by the issue of downward substitutions in enrollment noted in my limitations section. I did not find evidence that promise program adoption increases award production in specific program areas of focus. However, this could be reflective of my quasi-experimental design not considering uneven distributions of program area awards across program length.

Now aware of this potential issue, I would recommend that future examinations of award production carefully consider program length and program area of focus together. This construction of the outcome variables of interest may provide future researchers with a more appropriate design that can accurately estimate effect and potential p-values or alternatively replicate my present findings. One point of potential interest that I would direct future researchers to is the trends in significance and coefficient signs for liberal arts programs where I observe negative coefficients that differ significantly from zero in the two treatment periods following promise adoption.

However, after that second observation period which coincidently is the length of time that an associate's degree level liberal arts student would need to complete their program, this negative coefficient becomes positive but not significantly different from zero. While not both positive and significantly different from zero, this finding suggests that promise programs may work to stave off declining completions for programs that were graduating fewer and fewer students prior to promise programs. In effect, potentially creating a stabilizing effect on student completions despite a lack of a positive increase.

It may be possible that newer packages and estimation tools may emerge in the near future that are better suited for a study such as mine but at this point of time, my Sun and Abraham models are the only available estimator that can account for unit-specific time trends and does not feature identified issues with biasing post-treatment estimates. Thus, I found the results of my preferred Sun and Abraham approach as both supportive of a causal effect of college promise programs on the number of graduations and evidence that the difference-in-difference approaches still have areas for further development in exploring issues of staggered treatment when unit-specific time trends exist.

Findings: Research Question #1 Examining Completions by Program Length

I found evidence that promise program adoption increases sub-baccalaureate award production. Regarding program length, I found that this increase is significant not only for sub-baccalaureate awards but also for the sub-groups of associates degrees and credentials. While I found a significant increase for credentials when unseparated by

length of program, I found when separated into credential programs between one-to-two years in length and less than one year in length that only credential programs that were less than one year in length returned a significant effect.

This is a promising finding for the effect of promise programs. Within the first five periods of adoption, promise programs increased sub-baccalaureate award production at treated institutions by between 4.2% to 8.6%. For my horizon of five years following adoption, the increase of 8.6% was found during the fifth period following adoption, the latest period examined on my horizon. This trend in increasing award production also held for associates degrees with an estimated effect of an increase by 7.2% by the fifth post-treatment period. Increases for credentials that take less than a year to complete were found to be around 9% to 11% with a slight decrease found after reaching their highest increase on awards in the third year following promise program adoption. While credentials that take between one-to-two years to complete were not found to produce a significant increase, a measure that considered both credential lengths together did return a significant increase in award production that also reached a peak in the third post-treatment year of 21% before declining to an increase of about 10% by the fifth post-treatment period.

In the results section for this research question, I noted that for short-term credentials that take less than a year to complete, a lag was observed before significant effects were observed. I believe that this could be due to a lack of access to funds to live on while enrolled in a free college program, these students may not be immediately able to afford the time off work to engage in a short-term program even if it is offered at no

cost to them because of foregone wages. Thus, a lag being needed to meet joint significance here may be representative of two factors. First is that program awareness could be building for the program as news of free college proliferates within communities and students adjust their postsecondary plans accordingly or in other scenarios if the first year of a program was a limited pilot before larger scale adoption. The second is in regard to the potential that lower income students may take slightly longer to respond as they secure the necessary resources to support their selves and potentially their families while enrolled in a short-term program.

Moving back to consider findings for question one broadly, comparisons to the findings of other studies are limited due to empirical design and the uniqueness of my explored topic. Because my data is constructed from institutional level data from IPEDS, I was unable to provide estimates of how much more likely students that attend an institution with a promise program are to complete their program as other scholars working with student-level data have been able to do so (Carruthers et al., 2023; Gershenfield et al., 2019). However, I was still able to compare aggregate increases in the number of completions to existing literature.

My findings somewhat align with previous estimates of individual programs in terms of scale of general increases in awards but depart from past research findings on sub-baccalaureate outcomes. Bartik et al. (2021) found that the Kalamazoo Promise increased postsecondary completion by 12% but simultaneously found no increase for associates degree completion. Swanson and Ritter (2020) found the adoption of the El Dorado promise program resulted in a 10.7% percent increase in awards. However, my

results also departed from the Swanson and Ritter findings which found a non-statistically significant effect on associates degree completion.

However, an important caveat mentioned throughout this dissertation is that these studies examined the El Dorado Promise and Kalamazoo Promise programs which can be used at either two-year or four-year institutions. I discuss this caveat in greater detail in my implications section. This is a type of promise program that I exclude from my sample due to a potential scattering effect. Thus, their lack of significant findings for sub-baccalaureate awards may be reflective of students substituting away from the sub-baccalaureate sector as the four-year sector becomes more affordable – something my study is specifically designed to avoid. My study is unique in that it does not consider these student scattering promise aid awards that could be utilized at both the two-year and four-year levels but still finds a similar levels of increased completions to these two studies which examined promise programs that happen to also affect four-year colleges.

While it is possible that my study also was biased by student choice driving enrollment substitutions in the opposite direction of the above mentioned two projects, my estimates still offer utility to the field of postsecondary education research. First, my sub-baccalaureate only promise program findings highlight a pressing need for future research designs to consider student choice in future studies on promise programs. Second, is that my study offers a window into a deeper understanding of the effects of promise programs on workforce education. By identifying that there is a difference in effect for student completions as determined by promise program features, policy and institutional leaders can use this knowledge to better construct promise programs that

reflect relevant opportunities for the communities and areas that they desire a promise program to serve. For example, if a state or area's economic needs for four-year graduates was not being met and the local labor market had a surplus of sub-baccalaureate educated workers then it may be beneficial for a promise program to apply to four-year institutions, so students substitute up into four-year programs. However, if a local labor market was over-saturated with workers educated at the four-year level and higher, then a promise program limited to the sub-baccalaureate could potentially reduce this over-saturation by directing incoming workforce participants into the sub-baccalaureate.

In sum, I found this to be encouraging evidence that even when controlling for an institution's unique trends (i.e., unit-specific time trends) in student completion that promise programs do increase sub-baccalaureate student completions. Worthy of future examination, my findings provided evidence that completions at two-year colleges can be positively increased by free college programs when a program does not cover four-year programs. These results indicated that promise program adoption can lead to a larger number of sub-baccalaureate graduates that then can hopefully utilize their degrees in the labor market to access the increasing number of opportunities an award provides them in the workforce.

Findings: Research Question #2 Examining Completions by Program Area of Study

Examination for parallel pre-treatment trends for completions by program of study for my five super CIP-code categories found that my empirical strategy was appropriate for the following outcome variables: completions for STEM programs,

completions for trades programs, completions for liberal arts programs, and completions for human services programs. While all coefficients were positive for the effect of promise program adoption on completion for these four outcome variables, the results indicated that this increase was not significantly different from zero.

I posit that these results do not completely conflict with my findings from the first research question due to the unique distribution of awards by program area and program length. For example, trades programs would be predominately isolated to shorter-term credentials while liberal arts programs would be found predominately at the associates degree level. Continuing with this example, these distributions were reflective of the intention of these programs. Shorter term trades-focused credentials were reflective of preparing students for immediate careers following program completion while two-year liberal arts programs often align with the transfer missions of community colleges.

Conclusions on the Effect of Promise Programs

In line with past literature on both financial aid broadly and promise programs specifically, I found that making college more affordable for students increases student outcomes. This is significant for both students individually and society writ large. Even though the return on investment has become increasingly questioned, evidence persists that the individual life-time earnings of having a postsecondary award is significant (Carnevale, Smith, Van Der Werf, & Quinn, 2023).

However, broad increases in any postsecondary awards are not completely desirable. A recent report found that sub-baccalaureate award production in the United States is disjointed from the actual demands of local labor markets. Strohl et al. (2024)

found that recent community and technical college graduates were not graduating from programs that were in demand in their local labor markets. In fact, Strohl (2024) found that to meet this demand that one half of students would need to change their academic program of focus for community and technical colleges to align with local workforce needs.

As an increasing number of states specifically adopt promise programs to help meet workforce needs, this was an important consideration to flag. Free college programs can help to facilitate graduation but are not the only strategy that should be considered critical for meeting workforce needs. Instead, I recommend that free college programs be considered as a catalyst that drives and supports students in completing programs and suggest that policy makers and institutional leaders consider the areas of student recruitment, academic advising, and career services as pivotal in ensuring that students graduate into labor markets that value their postsecondary award (Lynch & Lungrin, 2018). In some ways this reflects the overall trajectory of contemporary postsecondary education in the United States. As institutions and policy makers have shifted from enrollment being of chief concern to also consider student success, it would be a natural evolution to consider the post-graduation outcomes of earnings and job outlook as an emerging frontier in postsecondary accountability. In fact, examples of this transition exist at both the state and federal levels. A number of states have or are moving toward adopting frameworks that assess sub-baccalaureate granting institutions not only on traditional measures of student success such as retention and graduation but also post-graduation outcomes such as job demand and earnings (National Conference on State

Legislatures, 2023). Regarding the federal government, the adoption of the College Scorecard, allows for transparency in debt and earnings outcomes for institutions and their programs (Mabel, Libassi, & Hurwitz, 2020). With this in mind, a future study worth recommending is one that takes advantage of these frameworks and available state occupational/earnings data to examine labor market outcomes for graduates.

Thus, I recommend that the potential excitement around estimating a national-level average effect for promise programs on completion be tempered with a reminder that much must be done to advance student outcomes. Even when college is free-to-students, there are still issues such as housing insecurity, food insecurity, and other financial or life barriers experienced by postsecondary students (Broton & Goldrick-Rab, 2018; Center for Community College Student Engagement, 2020; Evans, Kearney, Perry, & Sullivan, 2020; Mayer, et al., 2019; Perin, 2018; The Hope Center for College, Community, and Justice, 2021). Especially, the students of sub-baccalaureate programs who serve the largest proportions of students who are first-generation, low-income, and from historically marginalized groups. It is important to understand that increased postsecondary affordability can increase completions and by result the number of educated workers in the United States. However, leveraging promise programs as a singular solution for our workforce needs will likely not fully enable students and society the enjoy the benefits of a more educated workforce.

Future Implications for Research & Policy

As noted earlier in Chapter 3, there are several limitations in this study that suggest this area of research remains ripe for future exploration. Future studies would

benefit from further refining both my treatment variable and outcome variables in a number of ways to better examine this topic. Improvements to my treatment variable could include explorations of how different program designs (e.g., first-dollar vs last-dollar program designs) influence findings. This would by its merit also include an analysis of programs by state and institutional variables that include program scope, state/institutional support of the program, and potentially student-level variables given the correct study design. Improvements to outcome variables could examine for difference in effect between student demographic groups such as adult and traditional age students, gender, or race/ethnicity.

More pressingly and of impact to the research of promise programs is that this study establishes it is possible to leverage staggered difference-in-difference or event study designs to examine the effect of promise programs in a staggered adoption setting. While this study is not the first to employ a staggered difference-in-difference design to examine postsecondary education policy, it establishes a successful empirical strategy for studying an outcome variable that in the past has proved difficult to examine due to unit-specific time trends. By including an interaction between a time trend variable and institutions, it was possible to establish the necessary parallel pre-treatment trends to examine this issue. Thus, this approach could be applied to other instances of similar research designs. These similar designs could add to the literature by studying student enrollment, completion rates, and/or for potential differences in effect within student demographic sub-groups.

Additionally, while I designed this study's purpose and questions to examine graduation production by award length and program focus separately, I believe that a future study that considers both of these dimensions simultaneously may be able to better tease out findings regarding changes in sub-baccalaureate completions by academic program. I believe that because my design does not simultaneously consider program length and program area of focus that these models are biased by uneven distributions awards by area of focus across program length where my current models only consider program area of focus at all sub-baccalaureate awards level.

For institutional leaders and policy makers, my findings reconfirm previous studies albeit on a larger national scale. This aggregate estimate of the effect of promise program adoption would be of use to policy makers. Notably, it would be of potential explicit interest to federal policy makers who regularly examine and make campaign promises regarding national free college programs (Carnevale, Sablan, Gulish, Quinn, & Cinquegrani, 2020). This research provides evidence that the message of free college is an effective way to increase the supply of educated workers. However, simultaneously completion rates even at a time-and-a-half remain low in the United States at 35% of students from 2-year institutions graduating within 3-years and 65% of students graduating from their less than 2-year programs within time-and-a-half of their program's length (National Center for Education Statistics, 2024d; National Center for Education Statistics, 2024e).

This is indicative that while free college is effective at increasing completions, it is not a panacea to ensuring that our supply of educated workers meets workforce needs

and improves opportunities for all individual students. Many issues such as academic preparedness, food/housing insecurity, and access to advising remain important barriers to student success and eventually graduation, especially in the community and technical college sector (Broton & Goldrick-Rab, 2018; Center for Community College Student Engagement, 2020; Evans, Kearney, Perry, & Sullivan, 2020; Mayer, et al., 2019; Perin, 2018). In sum, my national staggered difference-in-difference analysis of sub-baccalaureate completions found that promise programs keep their promise of producing educated workers but also illuminates that there is much still to be done within area of postsecondary education policy research.

Conclusion

I now will close this dissertation by returning to and discussing the caveat I flagged in my discussion of research question one regarding differences in sample selection between my research and past research. I find this caveat important to discuss because my study found that completions increased for my sample of promise programs limited to sub-baccalaureate programs while past literature that only considered programs that could be used at any level of undergraduate education found null effects at the sub-baccalaureate level for promise program adoption. I place importance on this issue because the sub-baccalaureate and baccalaureate postsecondary ecosystem in the United States has been noted to be a two-tiered system (Carnevale et al., 2018). Students from less advantaged backgrounds are overwhelmingly sorted into the lower preforming sub-baccalaureate sector due to a variety of reasons including cost.

Thus, promise programs that only allow for utilization at the sub-baccalaureate versus those that allow for utilization at either level undoubtably shape the choices available to students. Considering that completion and career outcomes for four-year students are consistently higher than sub-baccalaureate students, the narrowing of postsecondary choice that programs in my study contribute to is potentially concerning (Carnevale et al., 2024). This is not to say that the students who finish their sub-baccalaureate studies gain nothing. In fact, their outcomes are stronger than those of just high school graduates (Carnevale et al., 2023). The point of this acknowledgement is to highlight the reality that promise programs include a diverse range of programs and that the goals of these aid programs are not entirely consistent among one another (Perna & Leigh, 2018; Perna et al., 2021).

Instead, each of these programs serve the purposes and needs of their local communities as interpreted by leaders that construct, advocate, and/or facilitate these programs (Billings, 2018). Thus, it will be critical for policy makers and institutional leaders to orient themselves toward identifying and clearly communicating the purpose, opportunities, and benefits of promise programs to their stakeholders. After doing so, leaders will be able to appropriately use promise programs as a policy lever to meet the needs of individuals within their communities.

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APPENDICES

APPENDIX A

Data Preparation Stata Code

```
*******************************************************************************
*******************************************************************************

*  Title:   Completions Data Prep
*  Author:  Zachary Hyder
*  Date:    04/09/2024
* Purpose: Set up cross-section+panel data of <=2 year academic programs at
                                *public  <=2 year colleges from 2009-2010 through 2021-2022. Merges
                                *treatment data from collegepromise.org and fips codes

*******************************************************************************
*******************************************************************************
*******************************************************************************
*******************************************************************************

* Do File Prep and File Management

        * remove current data
                clear
        *closes any log you have open
                capture log close
        * allocate more memory to Stata: allocate 200 megabytes of memory
                set mem 500m
                set matsize 800
                *not needed in "modern stata according to ouput"
        *lets your program run to the end, it tells Stata not to pause or display the more message.
                set more off
                capture estimates drop *

        * Set File Path Management Globals
                global projectpath = "/Users/zachyder/Library/CloudStorage/OneDrive-UniversityofTennessee/EventStudyStaggerDiDPilot"
                *Don't forget to change file path if on a different device e.g,PC Work Station:
```

```
		*cd "C:\Users\zhyder\OneDrive - University of Tennessee\EventStudyStaggerDiDPilot"
		global dir     = "$projectpath"
		global data    = "$projectpath/data"
		global rawdata = "$projectpath/rawdata"
		global dofile  = "$projectpath/do"
		global figures = "$projectpath/figures"
		global logs    = "$projectpath/logs"
		global output  = "$projectpath/output"
		global tables  = "$projectpath/tables"
		global ipeds   = "$projectpath/ipeds"

	* Set log path with time stamp
		local logname "CompletionsDataPrepPilot"
		local logfilename "$logs/CompletionsDataPrepPilot`c(current_date)'_`c(current_time)'.log"
		log using "`logfilename'"

********************************************************************************
********************************************************************************
********************************************************************************
********************************************************************************

*Summon Completions Data
	foreach x in 2022 2021 2020 2019 2018 2017 2016 2015 2014 2013 2012 2011 2010 2009 2008 2007 2006 2005 2004 2003 2002 2001 2000    {
	quietly do $ipeds/c`x'_a.do
	keep unitid cipcode awlevel majornum awlevel ctotalt
	tostring cipcode, force replace
	destring cipcode, force replace
		rename cipcode cipthousands
			gen cipcode = cip*0.0001
			drop if majornum!=1
		recode awlevel (20=1) (21=1)
			drop if awlevel>=4
		gen cip= floor(cipcode)
			drop if cip==95
			*drops cases that are "95.9500 - Imputed, undesignated field of study"
			*do the necessary 6-digit crosswalks for areas that changed at the two digit level
		replace cipcode=40.1001 if cipcode==14.3101
		replace cipcode=26.1501 if cipcode==30.2401
		replace cipcode=38.0207 if cipcode==39.0606
		replace cipcode=01.8301 if cipcode==51.0808
```

```
replace cipcode=01.1302 if cipcode==51.1104
replace cipcode=01.8101 if cipcode==51.2501
replace cipcode=01.8105 if cipcode==51.2502
replace cipcode=01.8109 if cipcode==51.2503
replace cipcode=01.8107 if cipcode==51.2504
replace cipcode=01.8108 if cipcode==51.2505
replace cipcode=01.8111 if cipcode==51.2506
replace cipcode=01.8103 if cipcode==51.2507
replace cipcode=01.8104 if cipcode==51.2508
replace cipcode=01.8102 if cipcode==51.2509
replace cipcode=1.81100 if cipcode==51.2510
replace cipcode=01.8106 if cipcode==51.2511
replace cipcode=01.8199 if cipcode==51.2599
replace cipcode=61.2302 if cipcode==60.0401
replace cipcode=61.0301 if cipcode==60.0402
replace cipcode=61.0401 if cipcode==60.0403
replace cipcode=61.1102 if cipcode==60.0404
replace cipcode=61.0901 if cipcode==60.0405
replace cipcode=61.0902 if cipcode==60.0407
replace cipcode=61.2702 if cipcode==60.0409
replace cipcode=61.0501 if cipcode==60.0410
replace cipcode=61.2601 if cipcode==60.0411
replace cipcode=61.0601 if cipcode==60.0412
replace cipcode=61.0701 if cipcode==60.0413
replace cipcode=61.2701 if cipcode==60.0414
replace cipcode=61.0801 if cipcode==60.0415
replace cipcode=61.1001 if cipcode==60.0416
replace cipcode=61.1101 if cipcode==60.0417
replace cipcode=61.1201 if cipcode==60.0418
replace cipcode=61.1301 if cipcode==60.0419
replace cipcode=61.2303 if cipcode==60.0420
replace cipcode=61.1401 if cipcode==60.0421
replace cipcode=61.1501 if cipcode==60.0422
replace cipcode=61.1701 if cipcode==60.0423
replace cipcode=61.1801 if cipcode==60.0424
replace cipcode=61.1901 if cipcode==60.0425
replace cipcode=61.2001 if cipcode==60.0426
replace cipcode=61.2101 if cipcode==60.0427
replace cipcode=61.2401 if cipcode==60.0428
replace cipcode=61.2301 if cipcode==60.0429
```

```
replace cipcode=61.2501 if cipcode==60.0430
replace cipcode=61.2610 if cipcode==60.0431
replace cipcode=61.2707 if cipcode==60.0432
replace cipcode=61.2801 if cipcode==60.0433
replace cipcode=61.2708 if cipcode==60.0434
replace cipcode=61.2402 if cipcode==60.0501
replace cipcode=61.1902 if cipcode==60.0502
replace cipcode=61.1804 if cipcode==60.0503
replace cipcode=61.0804 if cipcode==60.0504
replace cipcode=61.1805 if cipcode==60.0505
replace cipcode=61.1903 if cipcode==60.0506
replace cipcode=61.2403 if cipcode==60.0507
replace cipcode=61.0805 if cipcode==60.0508
replace cipcode=61.1103 if cipcode==60.0509
replace cipcode=61.2704 if cipcode==60.0510
replace cipcode=61.0202 if cipcode==60.0511
replace cipcode=61.1806 if cipcode==60.0512
replace cipcode=61.0502 if cipcode==60.0513
replace cipcode=61.1904 if cipcode==60.0514
replace cipcode=61.2604 if cipcode==60.0515
replace cipcode=61.0806 if cipcode==60.0516
replace cipcode=61.1807 if cipcode==60.0517
replace cipcode=61.2405 if cipcode==60.0518
replace cipcode=61.0807 if cipcode==60.0519
replace cipcode=61.0203 if cipcode==60.0520
replace cipcode=61.2406 if cipcode==60.0521
replace cipcode=61.1302 if cipcode==60.0522
replace cipcode=61.1808 if cipcode==60.0523
replace cipcode=61.0808 if cipcode==60.0524
replace cipcode=61.0205 if cipcode==60.0525
replace cipcode=61.1809 if cipcode==60.0526
replace cipcode=61.0810 if cipcode==60.0527
replace cipcode=61.0811 if cipcode==60.0528
replace cipcode=61.1810 if cipcode==60.0529
replace cipcode=61.1303 if cipcode==60.0530
replace cipcode=61.0904 if cipcode==60.0531
replace cipcode=61.1811 if cipcode==60.0532
replace cipcode=61.2605 if cipcode==60.0533
replace cipcode=61.0813 if cipcode==60.0534
replace cipcode=61.0208 if cipcode==60.0535
```

```
replace cipcode=61.1812 if cipcode==60.0536
replace cipcode=61.1502 if cipcode==60.0537
replace cipcode=61.1905 if cipcode==60.0538
replace cipcode=61.0812 if cipcode==60.0539
replace cipcode=61.1106 if cipcode==60.0540
replace cipcode=61.0209 if cipcode==60.0541
replace cipcode=61.1813 if cipcode==60.0542
replace cipcode=61.2607 if cipcode==60.0543
replace cipcode=61.1702 if cipcode==60.0544
replace cipcode=61.2608 if cipcode==60.0545
replace cipcode=61.1503 if cipcode==60.0546
replace cipcode=61.1504 if cipcode==60.0547
replace cipcode=61.0210 if cipcode==60.0548
replace cipcode=61.1906 if cipcode==60.0549
replace cipcode=61.1907 if cipcode==60.0550
replace cipcode=61.0503 if cipcode==60.0551
replace cipcode=61.1908 if cipcode==60.0552
replace cipcode=61.1909 if cipcode==60.0553
replace cipcode=61.1910 if cipcode==60.0554
replace cipcode=61.1911 if cipcode==60.0555
replace cipcode=61.1912 if cipcode==60.0556
replace cipcode=61.1913 if cipcode==60.0557
replace cipcode=61.1505 if cipcode==60.0558
replace cipcode=61.1703 if cipcode==60.0559
replace cipcode=61.1814 if cipcode==60.0560
replace cipcode=61.1914 if cipcode==60.0561
replace cipcode=61.2609 if cipcode==60.0562
replace cipcode=61.2003 if cipcode==60.0563
replace cipcode=61.1915 if cipcode==60.0564
replace cipcode=61.2705 if cipcode==60.0565
replace cipcode=61.1917 if cipcode==60.0566
replace cipcode=61.2802 if cipcode==60.0567
replace cipcode=61.2103 if cipcode==60.0569
replace cipcode=61.2404 if cipcode==60.0570
replace cipcode=61.0814 if cipcode==60.0571
replace cipcode=61.1815 if cipcode==60.0572
replace cipcode=61.1304 if cipcode==60.0573
replace cipcode=61.0816 if cipcode==60.0574
replace cipcode=61.0212 if cipcode==60.0575
replace cipcode=61.2002 if cipcode==60.0576
```

```
replace cipcode=61.0213 if cipcode==60.0577
replace cipcode=61.0214 if cipcode==60.0578
replace cipcode=61.2706 if cipcode==60.0579
replace cipcode=61.2611 if cipcode==60.0580
replace cipcode=61.0818 if cipcode==60.0581
replace cipcode=61.0216 if cipcode==60.0582
replace cipcode=61.2612 if cipcode==60.0583
replace cipcode=61.1107 if cipcode==60.0584


gen supercip=.
                        foreach y in 1 2 3 11 14 15 27 26 40 41 {
                                replace supercip=1 if cip==`y'
                                }
                        foreach y in 46 47 48 49 {
                                replace supercip=2 if cip==`y'
                                }
                        foreach y in 5 16 23 24 30 38 39 42 45 50 54  {
                                replace supercip=3 if cip==`y'
                                }
                        foreach y in 12 19 20 31 32 33 34 36 37 53  {
                                replace supercip=4 if cip==`y'
                                }
                        foreach y in 4 8 13 22 29 30 43 44 51 52 60 9 10 25{
                                replace supercip=5 if cip==`y'
                                }
                        replace supercip=9 if cip==0
                        replace supercip=9 if cip==99
sort unitid awlevel supercip
collapse (sum) ctotalt, by(unitid awlevel supercip)
        gen gradyear =`x'

reshape wide ctotalt, i(unitid supercip) j(awlevel)
                                egen ctotalt4= rowtotal(ctotalt1 ctotalt2)
                                egen ctotalt5= rowtotal(ctotalt1 ctotalt2 ctotalt3)
rename ctotalt1 shortcred_
rename ctotalt2 longcred_
rename ctotalt3 degree_
rename ctotalt4 allcred_
rename ctotalt5 subbach_
```

```
                reshape wide shortcred_ longcred_ allcred_ degree_ subbach_, i(unitid) j(supercip)


        save $projectpath/data/Completions`x'.dta, replace
        }



*********************************************************************************
*********************************************************************************
*********************************************************************************
*********************************************************************************

*Preform Append
        clear
        use $projectpath/data/Completions2022.dta

foreach x in 2021 2020 2019 2018 2017 2016 2015 2014 2013 2012 2011 2010 2009 2008 2007 2006 2005 2004 2003 2002 2001 2000  {
        append using $projectpath/data/Completions`x'.dta
}
        *drops closed/new campsues by only keeping those with 12 observations
        sort unitid gradyear
        egen yearcount=count(gradyear), by(unitid)
        drop if yearcount!=23
        tostring unitid, force replace


        save $projectpath/data/Completions.dta, replace


*********************************************************************************
*********************************************************************************
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*********************************************************************************

*Summon instiutional describers
        clear
        do $ipeds/f2122_f1a.do
```

```
		keep unitid f1b11
		tostring unitid, force replace
		save "/Users/zachyder/Library/CloudStorage/OneDrive-UniversityofTennessee/EventStudyStaggerDiDPilot/data/finance.dta", replace
	clear
	do $ipeds/hd2022.do
		tostring unitid, force replace
	keep unitid instnm stabbr fips obereg zipcode sector iclevel control hloffer  hdegofr1 deggrant hbcu   tribal locale  act newid deathyr closedat cyactive postsec
pseflag pset4flg  instcat c21basic c21ipug  c21ugprf c21enprf c21szset c18basic c15basic ccbasic carnegie  instsize f1systyp cbsatype

*Merge in institutional describers
	merge 1:m unitid using $projectpath/data/Completions.dta
		*wonder why so many unmatched
		drop if _merge!=3
		drop _merge
	merge m:1 unitid  using $projectpath/data/finance.dta
		rename _merge finance_merge
			drop if finance_merge==2
	*drop system offices
		drop if sector==0
	*drop no-assocaites focused schools
		*drop if carnegie==15
		*drop if carnegie==16
		*drop if carnegie==21
		*drop if carnegie==22
		*drop if carnegie==31
		*drop if carnegie==32
		*drop if carnegie==52
	drop if stabbr=="PW" | stabbr=="PR"
	save $projectpath/data/DescribedCompletions.dta, replace
	clear

*Merge in funding data to filter out locally funded publics

*******************************************************************************
*******************************************************************************
*******************************************************************************
*******************************************************************************

* Now import the treatment data from PromiseRecord
clear
```

```
import excel $projectpath/rawdata/PromiseRecord.xlsx, firstrow
* keep the unitid, program, adoption year, serviced units, intistutional range type, program id, and dqreason
keep ID Level Program AdoptFallYear Service InstitutionRange UnitID dqreason RemoveFromFinal
* check for duplicates in treatment, you can only have one treatment start year per college (eg Pellissippi's start date is not 2015 with TN Prom but rather 2009 with K.Achieves)
rename UnitID unitid
sort unitid AdoptFallYear
drop if Level!= "A,CTE" &  Level!= "B,A" &  Level!= "B,A,CTE"
unique Program
unique unitid

* remove programs flagged for disqualificaiton, this data needs to change per project but is fine for now. Only make copies for Promise Record excel to not mess up this recrod in the future.
unique Program if dqreason!=""
unique Program if dqreason=="broad"
unique Program if dqreason=="Scattering"
unique Program if dqreason=="scattering"
drop if dqreason=="Scattering"
drop if dqreason=="broad"
drop if dqreason=="scattering"
unique Program if dqreason!=""

unique Program if dqreason=="adults only"
drop if dqreason=="adults only"

unique Program if dqreason=="bachelors"
drop if dqreason=="bachelors"

unique Program if dqreason=="complicated partial adoptions"
drop if dqreason=="complicated partial adoptions"

unique Program if dqreason=="epso"
drop if dqreason=="epso"

unique Program if dqreason=="merit"
drop if dqreason=="merit"

unique Program if dqreason=="scholarship"
drop if dqreason=="scholarship"
```

```
unique Program if dqreason=="cannot_find_year/no adopt date"
drop if dqreason=="cannot_find_year/no adopt date"

unique Program if dqreason=="expansion/dup"
drop if dqreason=="expansion/dup"

unique Program if dqreason=="pre-event"
drop if dqreason=="pre-event"

unique Program if dqreason=="non ipeds"
drop if dqreason=="non ipeds"

unique Program if dqreason=="workready NH"
drop if dqreason=="workready NH"

unique Program if dqreason==""

drop if dqreason!=""
* drops the 4-year colleges which zac did not collect serviced unitids for
drop if unitid==""
*drops some complicated examples from zac's primary reserach
drop if unitid=="drop"
drop if AdoptFallYear=="drop"
* drop untreated units with confusing circumstances
drop if RemoveFromFinal!=""

*sort unitid AdoptFallYear dup
duplicates report unitid
* quietly by unitid:  gen dup = cond(_N==1,0,_n)
* drops 2 programs/colleges zac missed in duplicate screening pre-import (Alabama's dumb 4-year programs)
* drops 196 programs from 4-years
* drops
* drop if dup>=2
* drop dup
* drops where capital idea serves mutliple counties with a dallas community college
* drops moorpark without a start data
* drops PikesPeak which had a state and local program take affect at the same time
save $projectpath/data/PromiseRecord.dta, replace
```

```
********************************************************************************
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********************************************************************************



* Now merge the indepdent (promise record treatment) into the depedent variable data (completions)

merge 1:m unitid using $projectpath/data/DescribedCompletions.dta
sort _merge
*unmatched from _merge=1 are four year granting instiutions which means I did not pull data for them. This can be pulled later by taking the unitids for this group and pulling them for ipeds like I did for the completion data earlier in the code

*merges from _merge=2 are my untreated group
*merges that are matched (_merge=3) are the treated group
drop if _merge==1
drop _merge
unique Program
unique unitid


*Now I am ready to assign treatment!
save $projectpath/data/pretreatdata.dta, replace
clear
use  $projectpath/data/pretreatdata.dta
        *My grad year is spring and my treat is fall. Therefore by subutracting 1 from grad year, I get a year variable that mataches the treatment though the graduation is for one year ahead (jump into the spring following Jan)
        gen year = gradyear

rename AdoptFallYear event
destring event, force replace
gen ToE= year - event if ID!=.
gen postevent=1
        replace postevent=0 if ToE==.
        replace postevent=0 if ToE<=-1
order unitid year event postevent ToE
sort unitid
by unitid: egen prom_firstyr=min(event)
```

```
gen prom_within_5 = (ToE==. | inrange(ToE, -5, 5))
gen init=1 if prom_firstyr<=2000
recode init (.=0)
tab year init

*gen never treated flag
gen never_prom=1 if prom_firstyr==.
	replace never_prom=0 if event!=.
	*treatements in fall 2022 and fall 2023 do not affect our data and mess up my descritpvies
		replace never_prom=1 if event==2022
		replace never_prom=1 if event==2023
recode never_prom (.=0)
tab year never_prom

*gen ever treated flag
gen evertreated = 0
	replace evertreated = 1 if event!=.
	*treatements in fall 2022 and fall 2023 do not affect our data and mess up my descritpvies
		replace evertreated=0 if event==2022
		replace evertreated=0 if event==2023
	tab year evertreated

*******************************************************************************
*******************************************************************************
*******************************************************************************
*******************************************************************************

* Create Lead and Lags for Analysis

	* first create leads / pre-delta
	forvalues i=1(1)23{
		gen predelta_`i'=(ToE==-`i')
		label var predelta_`i' "-`i'"
}

* next create lags / post-delta
	foreach v in 0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 {
	gen postdelta_`v'=(ToE==`v')
```

```
                label var postdelta_`v' "`v'"
                }

*generate the codes for the deggree lengths
rename shortcred_9 cip1991
rename longcred_9 cip2991
rename allcred_9 cip0991
rename degree_9 cip3991
rename subbach_9 cip4991

gen lncip4991= ln(cip4991)
gen lncip3991= ln(cip3991)
gen lncip2991= ln(cip2991)
gen lncip1991= ln(cip1991)
gen lncip0991= ln(cip0991)


*generate the codes for the academic types lengths
rename subbach_1 subbach_STEM
rename subbach_2 subbach_Trade
rename subbach_3 subbach_LibArt
rename subbach_4 subbach_HumServ
rename subbach_5 subbach_ProServ

gen lnsubbach_STEM              = ln(subbach_STEM)
gen lnsubbach_Trade             = ln(subbach_Trade)
gen lnsubbach_LibArt    = ln(subbach_LibArt)
gen lnsubbach_HumServ   = ln(subbach_HumServ)
gen lnsubbach_ProServ   = ln(subbach_ProServ)


*******************************************************************************
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*******************************************************************************

*replace blanks with zeros
foreach y in cip1991 cip2991 cip3991 cip4991 lncip1991 lncip2991 lncip3991 lncip4991 subbach_STEM subbach_Trade subbach_LibArt subbach_HumServ
subbach_ProServ  lnsubbach_STEM lnsubbach_Trade lnsubbach_LibArt lnsubbach_HumServ lnsubbach_ProServ  {
        replace `y'=0 if `y'==.
```

```
        }
********************************************************************************
********************************************************************************
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********************************************************************************



* use the csdid code to create a overall ATT for program length
gen interact = event * postevent
gen treatgroup = 0
        replace treatgroup=1 if event!=.
replace event=0 if event==.
egen fintrtgrp = group(unitid treatgroup)

save $projectpath/data/analysisready.dta, replace
*gen trend value for unit-specific time trends
gen trend = year - 2000

*get rid of always treated units
drop if event<=2004 & event!=0
unique Program
unique unitid

*get rid of four year schools
*drop if sector<=3
*drop if sector==99
*drop if sector==-3*

save $projectpath/data/finaleventstudy.dta, replace
export excel using analysisexcel.xlsx, first(var) replace

clear
use $projectpath/data/finaleventstudy.dta



save $projectpath/data/All_Sub-Baccalaurate.dta, replace
fre control if year==2000
```

```
fre sector if year==2000
fre carnegie if year==2000
*sucessful data run means analysis data set is ready

*CREATE THE RUN FILES uncut, non-profit, publics, and state-funded
	*note that the base file is uncut save $projectpath/data/treatdatawithfips.dta
	*non-profits
		drop if control==3
		unique Program
		unique unitid
		fre control if year==2000
		fre sector if year==2000
		fre carnegie if year==2000

		save $projectpath/data/Non-profits.dta, replace
	*publics
		drop if control==2
		unique Program
		unique unitid
		fre control if year==2000
		fre sector if year==2000
		fre carnegie if year==2000

		save $projectpath/data/Publics.dta, replace
	*state-funded
		*drop the locally controlled groups by taking out the zero dollar awardees for state appropirations
		drop if f1b11==0
		unique Program
		unique unitid
		fre control if year==2000
		fre sector if year==2000
		fre carnegie if year==2000
		save $projectpath/data/State-Funded.dta, replace


*close the log
log close
```

APPENDIX B

Data Analysis Stata Code

```
*******************************************************************************
*******************************************************************************

*  Title:   Staggered DiD Completions Data Analysis
*  Author:  Zachary Hyder
*  Date:    09/07/2024
*  Purpose: Run OLS ES and DID Impute estimators for a Stag. DiD analysis for cross-section+panel data of college completions
                        *       year academic programs at public  <=2 year colleges from 2009-2010
                        *       through 2021-2022. Merges treatment data from collegepromise.org
                        *       and fips codes*/

*Memos to self
*can run a varient of this using the K.Butts package of Gardner (2021)
*did2s lncip4991, first_stage(i.unitid i.year i.unitid#c.trend) second_stage(pre* tau*) *treatment(postevent) cluster(unitid)
*see:
*- https://friosavila.github.io/playingwithstata/main_did_union.html
*- https://jrgcmu.github.io/2sdd_current.pdf
*- https://github.com/kylebutts/did2s_stata
*- https://asjadnaqvi.github.io/DiD/docs/code_r/07_did2s_r/

*did_imputation     `y'  unitid year i.unitid#c.trend prom_firstyr, horizons(0/5) autosample pretrends(5) fe(unitid year) cluster(unitid) /*controls(i.unitid#c.trend)*/ tol(.01)
maxit(200)


*******************************************************************************
*******************************************************************************
*******************************************************************************
*******************************************************************************

* Do File Prep and File Management

        * remove current data
                clear
        *closes any log you have open
                capture log close
```

```
	* allocate more memory to Stata: allocate 200 megabytes of memory
		set mem 500m
		set matsize 800
		*not needed in "modern stata according to ouput"
	* lets your program run to the end, it tells Stata not to pause or
	*display the more message.*/
		set more off
		capture estimates drop *

	* Set File Path Management Globals
		global projectpath = "/Users/zachyder/Library/CloudStorage/OneDrive-UniversityofTennessee/EventStudyStaggerDiDPilot"
		*Don't forget to change file path if on a different device e.g,PC Work Station: cd "C:\Users\zhyder\OneDrive - University of
Tennessee\CompletionsByCIP"
		global dir     = "$projectpath"
		global data    = "$projectpath/data"
		global rawdata = "$projectpath/rawdata"
		global dofile  = "$projectpath/do"
		global figures = "$projectpath/figures"
		global logs    = "$projectpath/logs"
		global output  = "$projectpath/output"
		global tables  = "$projectpath/tables"

	* set log path with time stamp
		*local logtime di "$S_DATE $S_TIME"
		*local logfilename "$logs/CAP.`y'.`logtime'.log"

********************************************************************************
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********************************************************************************

*Create visual outputs using did_imputation, csdid, eventstudyinteract, and TWFE then compile them together into one chart
	foreach x in State-Funded Publics Non-Profits All_Sub-Baccalaurate {
	clear
		log using "$projectpath/logs/`x'`c(current_date)'_`c(current_time)'_consolidated.log", replace

	use $projectpath/data/`x'.dta
		destring unitid, force replace
		xtset unitid year
```

```
rename lncip4991 All
rename lncip3991 Assoicates
rename lncip2991 Credential
rename lncip1991 Year_Plus_Credential
rename lncip0991 Shorter_Credential
rename lnsubbach_STEM STEM
rename lnsubbach_Trade Trades
rename lnsubbach_LibArt Liberal_Arts
rename lnsubbach_HumServ Human_Services
rename lnsubbach_ProServ Professional_Services

                foreach y in All Assoicates Credential Year_Plus_Credential Shorter_Credential STEM Trades Liberal_Arts Human_Services Professional_Services{

*******************************************************************************
* Set Visual Scheme
set scheme s2color


*******************************************************************************
* Standard TWFE OLS estimation
display "`x'_`y' Standard TWFE OLS"
reghdfe `y' predelta_5 predelta_4  predelta_3 predelta_2 predelta_1 postdelta_0 postdelta_1 postdelta_2 postdelta_3 postdelta_4, a(unitid  year  i.unitid#c.trend)
cluster(unitid) noconstant
        estimates store ols_`y'
                pretrends power 0.5, pre(1/5) post(6/10)
                        return list
                                *matrix beta = e(b)
                                *matrix sigma = e(V)
                                *matrix beta  = beta[1..11]
                                *matrix sigma = sigma[1..11, 1..11]
                                *pretrends, numpre(5) b(beta) v(sigma) slope(`r(slope)')
                                        *return list
                                        *matlist r(results)
                                        *graph save "$figures/`x'_`y'_OLS_ptrends_power.gph", replace

display "`x'_`y' OLS ptrend joint test"
                test predelta_1 predelta_2  predelta_3 predelta_4 predelta_5
                *not accounting for lag to degree
```

```
		test postdelta_0 postdelta_1 postdelta_2 postdelta_3 postdelta_4
		*accounting for lag to degree
		test postdelta_2 postdelta_3 postdelta_4
		test postdelta_3 postdelta_4


	local plotopts xtitle("Periods Since Promise Adoption") ytitle("Headcount Effect (ln)") title("OLS Estimates Of `y' Awards") subtitle("For `x' Sample")
	coefplot, vertical yline(0) ciopts(recast(rcap)) xlabel(,angle(45)) `plotopts' keep(predelta_* postdelta_*)
				graph save "$figures/`x'_`y'OLS.gph", replace

display "`x'_`y' OLS HonestDID"
	honestdid, pre(1/5) post(6/10) mvec(0.5(0.5)4)
	honestdid, pre(1/5) post(6/10) mvec(0.0(0.01)0.1) delta(sd) omit coefplot `plotopts'  xtitle("M") ytitle("95% Robust CI (Rambachan & Roth, 2022)")
title("Sensitivity Analysis Using Smoothness Restirctions") subtitle("for `y' Awards in the `x' Sample")
					graph save "$figures/honest_OLS_`x'_`y'.gph", replace

*******************************************************************************
* Run Borusyak et al.
display "`x'_`y' did_imputation using did2s"
		did_imputation `y' unitid year event, fe(unitid year) horizons(0/5)  autosample  pretrends(5) cluster(unitid) tol(.01) maxit(200)
			estimates store `y'bjs
		pretrends power 0.5, pre(11 10 9 8) post(1/5)
			return list
				*matrix beta = e(b)
				*matrix sigma = e(V)
				*matrix beta  = beta[., 1..11]
				*matrix sigma = sigma[1..11, 1..11]
				*pretrends, numpre(5) b(beta) v(sigma) slope(`r(slope)')
					*return list
					*matlist r(results)
					*graph save "$figures/`x'_`y'_Impute_ptrends_power.gph", replace

display "`x'_`y' did_imputation ptrend joint test"
		test pre1 pre2  pre3 pre4 pre5
		*not accounting for lag to degree
		test tau0 tau1 tau2 tau3 tau4
		*accounting for lag to degree
		test tau2 tau3 tau4
		test tau3 tau4
```

```
display "`x'_`y' Impute HonestDID"
	honestdid, pre(10 9 8 7 6) post(1/5) mvec(0.5(0.5)4)
	honestdid, pre(10 9 8 7 6) post(1/5) mvec(0.0(0.01)0.1) delta(sd) omit coefplot `plotopts' xtitle("M") ytitle("95% Robust CI (Rambachan & Roth, 2022)")
title("Sensitivity Analysis Using Smoothness Restirctions") subtitle("for `y'  Awards in the `x' Sample")
                                        graph save "$figures/honest_imputation_`x'_`y'.gph", replace

event_plot, default_look graph_opt(xtitle("Periods Since Promise Adoption") ytitle("Headcount Effect (ln)") title("Borusyak et al. (2021) Estimates Of `y' Awards")
subtitle("For `x' Sample") xlabel(-5(1)5))
		graph save "$figures/`x'_`y'imputation.gph", replace

*******************************************************************************
* Estimation with eventstudyinteract of Sun and Abraham (2020)
display "`x'_`y' event study"
	eventstudyinteract `y'  predelta_5 predelta_4 predelta_3 predelta_2 postdelta_0 postdelta_1 postdelta_2 postdelta_3 postdelta_4, vce(cluster unitid) absorb(unitid
year i.unitid#c.trend) cohort(event) control_cohort(never_prom)
	*might be able to get an ATT with staggered package
	*staggered lncip4991, i(unitid) t(year) g(event) estimand(simple) sa
		pretrends power 0.5, pre(1/4) post(5/9)
			return list
				*matrix beta = e(b)
				*matrix sigma = e(V)
				*matrix beta  = beta[., 1..10]
				*matrix sigma = sigma[1..10, 1..10]
				*pretrends, numpre(5) b(beta) v(sigma) slope(`r(slope)')
					*return list
					*matlist r(results)
					*graph save "$figures/`x'_`y'_EventStudy_ptrends_power.gph", replace

display "`x'_`y' Event Study ptrend joint test"
		matrix b = e(b_iw)
		matrix V = e(V_iw)
		ereturn post b V
test predelta_2  predelta_3 predelta_4 predelta_5
		*not accounting for lag to degree
		test postdelta_0 postdelta_1 postdelta_2 postdelta_3 postdelta_4
		*accounting for lag to degree
		test postdelta_1 postdelta_2 postdelta_3 postdelta_4
		test postdelta_2 postdelta_3 postdelta_4

display "`x'_`y' Event Study HonestDID"
```

```
        honestdid, pre(1/4) post(5/9) mvec(0.5(0.5)4)
        honestdid, pre(1/4) post(5/9) mvec(0.0(0.01)0.1) delta(sd) omit coefplot `plotopts' xtitle("M") ytitle("95% Robust CI (Rambachan & Roth, 2022)")
title("Sensitivity Analysis Using Smoothness Restirctions") subtitle("for `y' Awards  in the `x' Sample")
                                graph save "$figures/honest_ES_`x'_`y'.gph", replace

        *code needs to rerun cause joint test wipes it
qui: eventstudyinteract `y'  predelta_5 predelta_4 predelta_3 predelta_2 postdelta_0 postdelta_1 postdelta_2 postdelta_3 postdelta_4, vce(cluster unitid) absorb(unitid year
i.unitid#c.trend) cohort(event) control_cohort(never_prom)


        event_plot e(b_iw)#e(V_iw), default_look graph_opt(xtitle("Periods Since Promise Adoption") ytitle("Headcount Effect (ln)") title("Sun & Abraham (2020)
Estimates Of `y' Awards") subtitle("For `x' Sample") xlabel(-5(1)5)) stub_lag(postdelta_#) stub_lead(predelta_#)  together
                graph save "$figures/`x'_`y'eventstudy.gph", replace
                        matrix esi_`y'_e = e(b_iw)
                        matrix esi_`y'_v = e(V_iw)


*******************************************************************************
*Joint Plots Code
                event_plot `y'bjs esi_`y'_e#esi_`y'_v ols_`y', stub_lag(tau# postdelta_# postdelta_#) stub_lead(pre# predelta_# predelta_#) plottype(scatter)
ciplottype(rcap) together trimlead(5) noautolegend graph_opt(title("`y' Headcount Effect (ln)") subtitle("For `x' Sample") xtitle("Years relative to promise adoption")
ytitle("Percent effect") xlabel(-5(1)5) ylabel(-0.2(0.05)0.2) xline(0) yline(0) graphregion(color(white) fcolor(white)) legend(position(5) order(1 "Borusyak et al. (2021)" 3
"Sun & Abraham (2020)" 5 "Ordinary Least Squares"))) lag_opt1(msymbol(Dh) color(forest_green)) lag_ci_opt1(color(forest_green)) lag_opt2(msymbol(Oh) color(navy))
lag_ci_opt2(color(navy))       lag_opt3(msymbol(Sh) color(dkorange))                lag_ci_opt3(color(dkorange))
graph save "$figures/`x'_`y'combined.gph", replace
estimates drop _all


*******************************************************************************
*Goodman-Bacon Decomposition from https://doi.org/10.1016/j.jeconom
*bacondecomp `y' postevent
        *           graph save "$figures/`x'_`y'baconDECOMP.gph", replace

*de Chaisemar & D'Haultfœuille from https://github.com/chaisemartinPackages/twowayfeweights
twowayfeweights `y' event year postevent, type(feTR) summary_measures
                graph save "$figures/`x'_`y'cdhDECOMP.gph", replace


}
*******************************************************************************
```

```
*******************************************************************************
*******************************************************************************
*******************************************************************************
*close the log
log close

}
```

APPENDIX C

Table 12: A Review of Staggered Difference-in-Difference Estimators

Estimator	Scholar & Relevant Stata Command	Strength	Weakness
Classic TWFE	n/a	A widely used and understood estimator by researchers.	Can bias estimates because of contamination from treatment effects at a different relative time.
Interaction Weighted	Sun & Abraham (2021) *Eventstudyinteract*	Exploits individual cohort-time-specific treatment effects	Does not allow for Not Yet Treated as a control
Doubly Robust	Callaway & Sant'Anna (2021) *Csdid*	Exploits individual cohort-time-specific treatment effects	Because csdid is flexible and conservative it also losses a lot of data that other approaches use for ATT estimation.
Decomposition (weighted average of all possible 2x2 estimators)	Goodman-Bacon (2021) *bacondecomp*	Exploits potential to consider already early treated units as a comparison group for later yet-to-be treated units	While this estimator does produce an ATT, producing an ATT is not its preferred use. Instead, this model should be utilized as a diagnostic tool to understand to what extent a model has a negative weighting problem. It also only works with strongly balanced panel data.

Table 12 continued

Decomposition (weighted average of all ATEs across group-time combinations)	de Chaisemartin & D'Haultfœuille (2020) *twowayfeweights*	Similar to bacondecomp, this command can work with unbalanced panel data.	Similar to bacondecomp, this estimator's preferred use is understanding to what degree a model has a negative weighting problem.
Imputation	Boursyak et al (2023) *didimputation*	Makes comparisons relative to the average of the pre-treatment periods rather than only the last pre-treatment period prior to treatment.	Gains precision at the cost of efficiency under real world conditions (heteroskedastic) and requires stronger pre-treatment trends assumptions.
Regression Adjustment	Heckman (1997) (or the RA estimator of *xthdidregress* in Stata 18)	Uses a two-step approach to estimate treatment effects by first fitting separate regressions of an outcome on each treatment cohort and then computing averages of the predicted outcomes for each unit and treatment cohort.	While this approach can account for covariate-specific time trends, it is not able to account for unit-specific time trends. An issue shared with several other models. Also, the estimates from this approach are usually not the ATT because of the treatment-variance weighting of the OLS.
Extended TWFE	Wooldridge (2021) *Jwdid* (or the TWFE estimator of *xthdidregress* in Stata 18)	Utilizes a TFWE approach that can be supplemented with additional regression specifications such as unit-level time trends.	Current packages do not provide support for these advantages, limiting utility of the model. This is because the model's complexity which requires many interactions and coefficients to be computed.

APPENDIX D

Output Logs from Stata Code

Panel A: All Sub-Baccalaureate Granting Institutions Sample	**Panel B: Non-Profit Sub-Baccalaureate Granting Institutions Sample**
https://drive.google.com/file/d/18djgZxwWpx-zc0M5ToEKu7gYHo2DTo-S/view?usp=sharing	https://drive.google.com/file/d/1qmVZv-qmDVyu5GmmUaA4_o1ZVrUBd6O7/view?usp=sharing
Panel C: Public Sub-Baccalaureate Granting Institutions Sample	**Panel D: State-Funded Sub-Baccalaureate Granting Institutions Sample**
https://drive.google.com/file/d/1q7TKV1mJOlbwj37zHggo2_8t_xydCuM-/view?usp=sharing	https://drive.google.com/file/d/1JkPRNYULpNSEoO4Bm-Lrq8QIYX4KzYIx/view?usp=sharing

Figure 26

Output Logs from Stata Code

Presented as both QR codes and links to an unrestricted access Google Drive repository, I provide the output logs for my research project. Note that Panel C contains the results specifically discussed by this manuscript while Panels A, B, and D link to the three other samples that were considered for final analysis in this dissertation.

APPENDIX E

Pre-treatment Trends Coefficient Tests of Significance

Table 13

Significance Pre-treatment Parallel Trends – Individual Period Test at State-Funded Institutions Only

	Ordinary Least Squares	Sun & Abraham (Event Study Interact)	Borusyak et al (DID Imputation)
All Awards	0.194 0.823 0.739 0.211 0.305	0.825 0.284 0.101 0.657	0.523 0.342 0.480 0.409 0.077
Associates Only	0.007** 0.091 0.067 0.056 0.054	0.057 0.228 0.762 0.267	0.195 0.124 0.111 0.074 0.004**
All Credentials	0.114 0.012* 0.035* 0.128 0.234	0.099 0.006** 0.004** 0.059	0.651 0.999 0.562 0.496 0.914
One-to-Two Year Credentials	0.142 0.011* 0.007** 0.060 0.069	0.067 0.001** 0.000*** 0.001**	0.452 0.365 0.322 0.306 0.781
< 1 Year Credentials	0.491 0.155 0.055 0.335 0.063	0.351 0.082 0.005** 0.057	0.680 0.926 0.721 0.944 0.620
STEM	0.603 0.457 0.771 0.715 0.486	0.611 0.028* 0.043* 0.193	0.424 0.507 0.356 0.302 0.826
Trades	0.693 0.394 0.323 0.926 0.612	0.439 0.188 0.195 0.829	0.088 0.031* 0.122 0.110 0.031
Lib Arts	0.008** 0.009** 0.009** 0.002** 0.000***	0.036* 0.032* 0.129 0.018*	0.000*** 0.000*** 0.000*** 0.000*** 0.000**
Pro. Services	0.889 0.708 0.299 0.468 0.283	0.922 0.869 0.213 0.259	0.793 0.884 0.974 0.703 0.376
Human Services	0.104 0.008** 0.016* 0.104 0.202	0.074 0.000*** 0.000*** 0.006**	0.071 0.098 0.045 0.071 0.308

* p<0.05, **p<0.01, ***p<0.001

Table 14

Significance Pre-treatment Parallel Trends – Individual Period Test at Non-Profit Institutions Only

	Ordinary Least Squares	Sun & Abraham (Event Study Interact)	Borusyak et al (DID Imputation)
All Awards	0.396 0.983 0.945 0.606 0.970	0.719 0.288 0.059 0.224	0.537 0.327 0.372 0.339 0.088
Associates Only	0.055 0.223 0.269 0.255 0.358	0.073 0.318 0.975 0.511	0.289 0.186 0.127 0.080 0.006**
All Credentials	0.231 0.041* 0.092 0.248 0.359	0.201 0.021* 0.011* 0.094	0.526 0.923 0.549 0.491 0.982
One-to-Two Year Credentials	0.044* 0.004** 0.003** 0.031* 0.045*	0.027 0.000*** 0.000*** 0.000***	0.468 0.388 0.374 0.349 0.811
< 1 Year Credentials	0.269 0.110 0.029* 0.175 0.024*	0.271 0.064 0.001** 0.011*	0.682 0.873 0.837 0.954 0.701
STEM	0.876 0.316 0.595 0.978 0.782	0.340 0.014* 0.025* 0.075	0.527 0.643 0.568 0.532 0.761
Trades	0.180 0.089 0.051 0.462 0.289	0.143 0.049* 0.024* 0.247	0.042* 0.013* 0.073 0.061 0.025*
Lib Arts	0.004** 0.004** 0.008** 0.004** 0.001**	0.010* 0.012* 0.097 0.039*	0.000*** 0.000*** 0.000*** 0.000*** 0.000***
Pro. Services	0.557 0.207 0.112 0.202 0.224	0.644 0.378 0.088 0.124	0.639 0.534 0.514 0.714 0.909
Human Services	0.006** 0.001** 0.002** 0.021* 0.047*	0.014* 0.000*** 0.000*** 0.001**	0.091 0.125 0.059 0.072 0.195

* p<0.05, **p<0.01, ***p<0.001

Table 15

Significance Pre-treatment Parallel Trends – Individual Period Test at All Institutions Only

	Ordinary Least Squares	Sun & Abraham (Event Study Interact)	Borusyak et al (DID Imputation)
All Awards	0.800 0.164 0.070 0.253 0.095	0.837 0.029 0.000*** 0.001**	0.534 0.328 0.366 0.329 0.096
Associates Only	0.021* 0.328 0.529 0.726 0.766	0.030* 0.487 0.608 0.690	0.266 0.194 0.125 0.077 0.004**
All Credentials	0.320 0.058 0.089 0.221 0.252	0.345 0.044* 0.017* 0.080	0.526 0.924 0.556 0.499 0.962
One-to-Two Year Credentials	0.003** 0.000*** 0.000*** 0.009** 0.021*	0.003** 0.000*** 0.000*** 0.000***	0.464 0.385 0.365 0.344 0.797
< 1 Year Credentials	0.021* 0.008* 0.001** 0.023* 0.002**	0.076 0.008** 0.000*** 0.001**	0.690 0.860 0.854 0.933 0.694
STEM	0.351 0.077 0.203 0.475 0.826	0.117 0.002** 0.003** 0.009**	0.537 0.653 0.583 0.552 0.755
Trades	0.046* 0.023* 0.015* 0.250 0.187	0.045* 0.014* 0.007** 0.104	0.043* 0.013* 0.074 0.061 0.028*
Lib Arts	0.047* 0.042* 0.065 0.023* 0.004**	0.063 0.087 0.337 0.128	0.000*** 0.000*** 0.000*** 0.000*** 0.000***
Pro. Services	0.278 0.078 0.039* 0.087 0.121	0.431 0.200 0.033* 0.049*	0.660 0.546 0.540 0.740 0.889
Human Services	0.004** 0.000*** 0.000*** 0.005** 0.012*	0.010* 0.000*** 0.000*** 0.000***	0.090 0.125 0.058 0.072 0.192

* p<0.05, **p<0.01, ***p<0.001

Table 16

Significance Pre-treatment Parallel Trends – Joint Period Test at State Funded Institutions Only

	Ordinary Least Squares	Sun & Abraham (Event Study Interact)	Borusyak et al (DID Imputation)
All Awards	0.1315	0.2842	0.0140*
Associates Only	0.0942	0.2248	0.0078**
All Credentials	0.1270	0.0265*	0.0527
One-to-Two Year Credentials	0.0992	0.0028**	0.7263
< 1 Year Credentials	0.1391	0.0959	0.0793
STEM	0.1455	0.1566	0.1163
Trades	0.3106	0.5414	0.0221
Lib Arts	0.0079**	0.0683	0.0000***
Pro. Services	0.6909	0.5980	0.6709
Human Services	0.0646	0.0033**	0.0987

* p<0.05, **p<0.01, ***p<0.001

Table 17

Significance Pre-treatment Parallel Trends – Joint Period Test at Non-Profit Institutions Only

	Ordinary Least Squares	Sun & Abraham (Event Study Interact)	Borusyak et al (DID Imputation)
All Awards	0.5657	0.2501	0.0682
Associates Only	0.5358	0.2644	0.0112*
All Credentials	0.3131	0.0936	0.0712
One-to-Two Year Credentials	0.0548	0.0007**	0.8113
< 1 Year Credentials	0.1303	0.0317*	0.1903
STEM	0.4669	0.1317	0.4703
Trades	0.1980	0.2120	0.0269
Lib Arts	0.0112*	0.0538	0.0000***
Pro. Services	0.7019	0.4800	0.7534
Human Services	0.0228*	0.0003**	0.2580

* p<0.05, **p<0.01, ***p<0.001

Table 18

Significance Pre-treatment Parallel Trends – Joint Period Test at All Institutions Only

	Ordinary Least Squares	Sun & Abraham (Event Study Interact)	Borusyak et al (DID Imputation)
All Awards	0.2295	0.0034**	0.0974
Associates Only	0.2422	0.1227	0.0073**
All Credentials	0.4556	0.1719	0.0776
One-to-Two Year Credentials	0.0039**	0.0000***	0.8105
< 1 Year Credentials	0.0095**	0.0013**	0.1886
STEM	0.2483	0.0208*	0.5008
Trades	0.0882	0.0734	0.0306*
Lib Arts	0.0352*	0.2470	0.0000***
Pro. Services	0.4759	0.2593	0.7586
Human Services	0.0091**	0.0000***	0.2552

* p<0.05, **p<0.01, ***p<0.001

APPENDIX F

Evidence of Pre-treatment Parallel Trends – Event Study Model, Public Sample

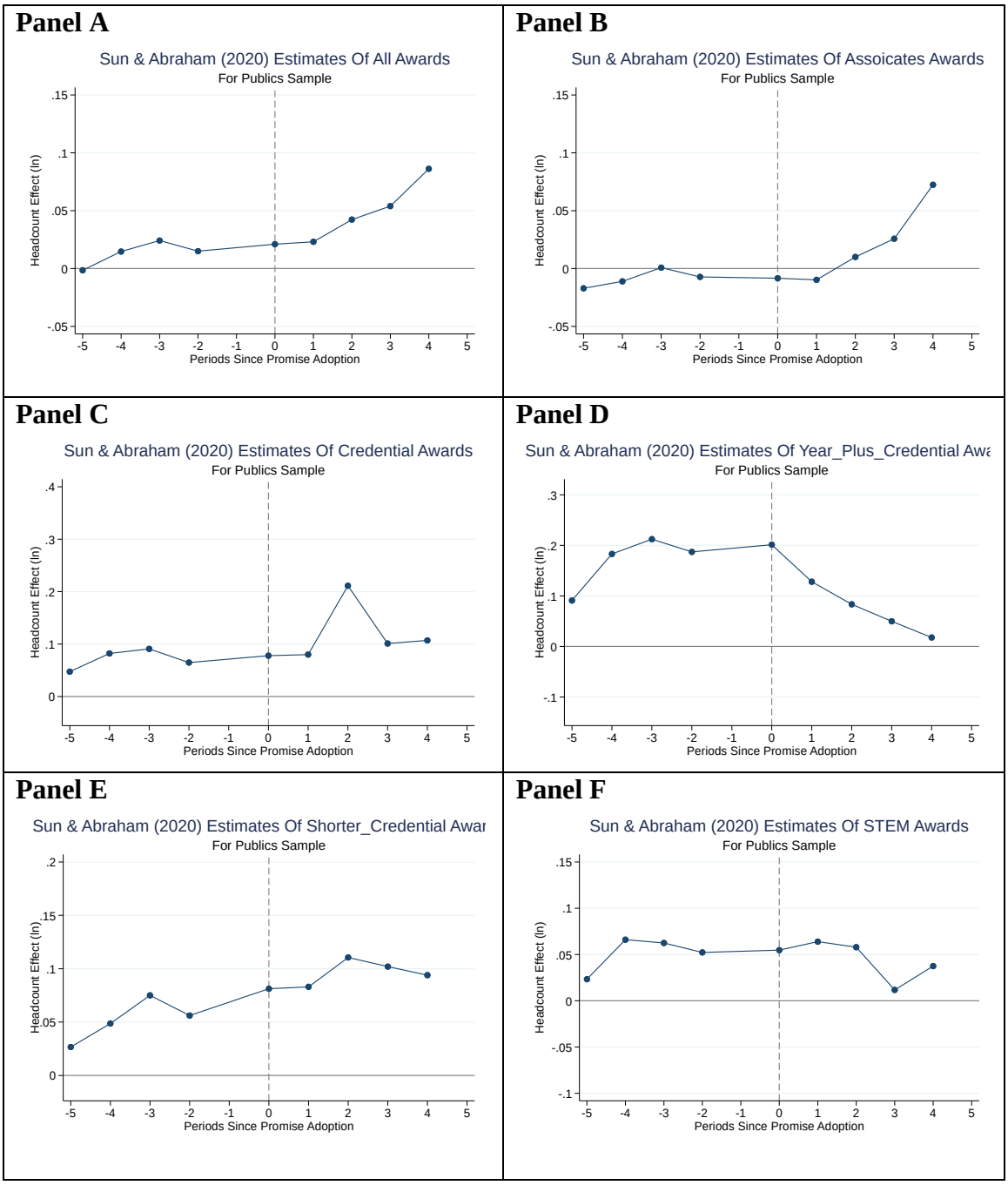

Figure 27

Visualization of Sun & Abraham's (2020) Trends for Public Institutions Only

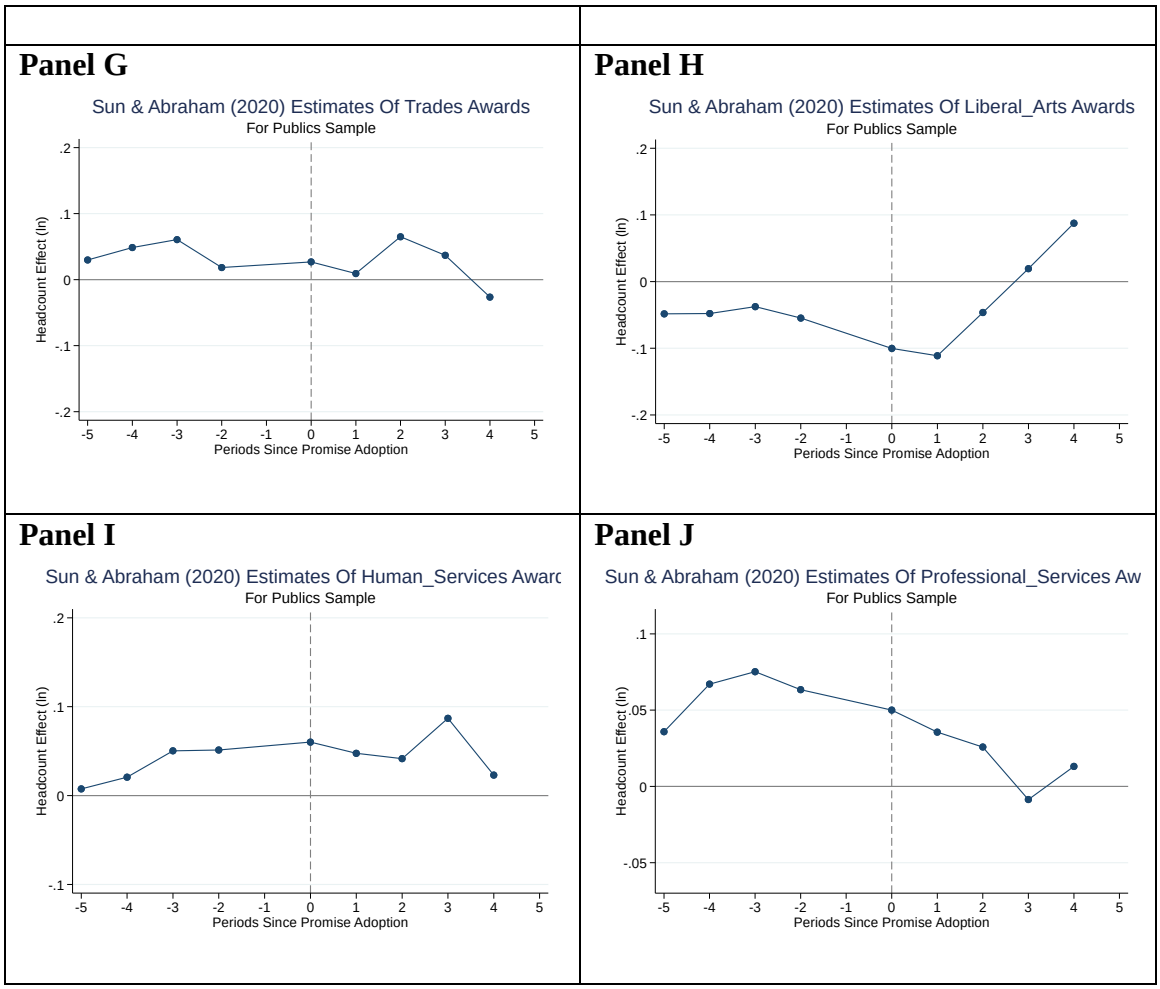

Figure 27 continued

Figure 27 contains outputs for the Sun & Abraham estimator in Panels A through J. In each panel, the y-axis represents the treatment coefficient's effect on the number of awards produced at institutions at that period (x-axis) in natural log units for each outcome variable of interest in this study.

APPENDIX G

Evidence of Pre-treatment Parallel Trends – OLS Model, Public Sample

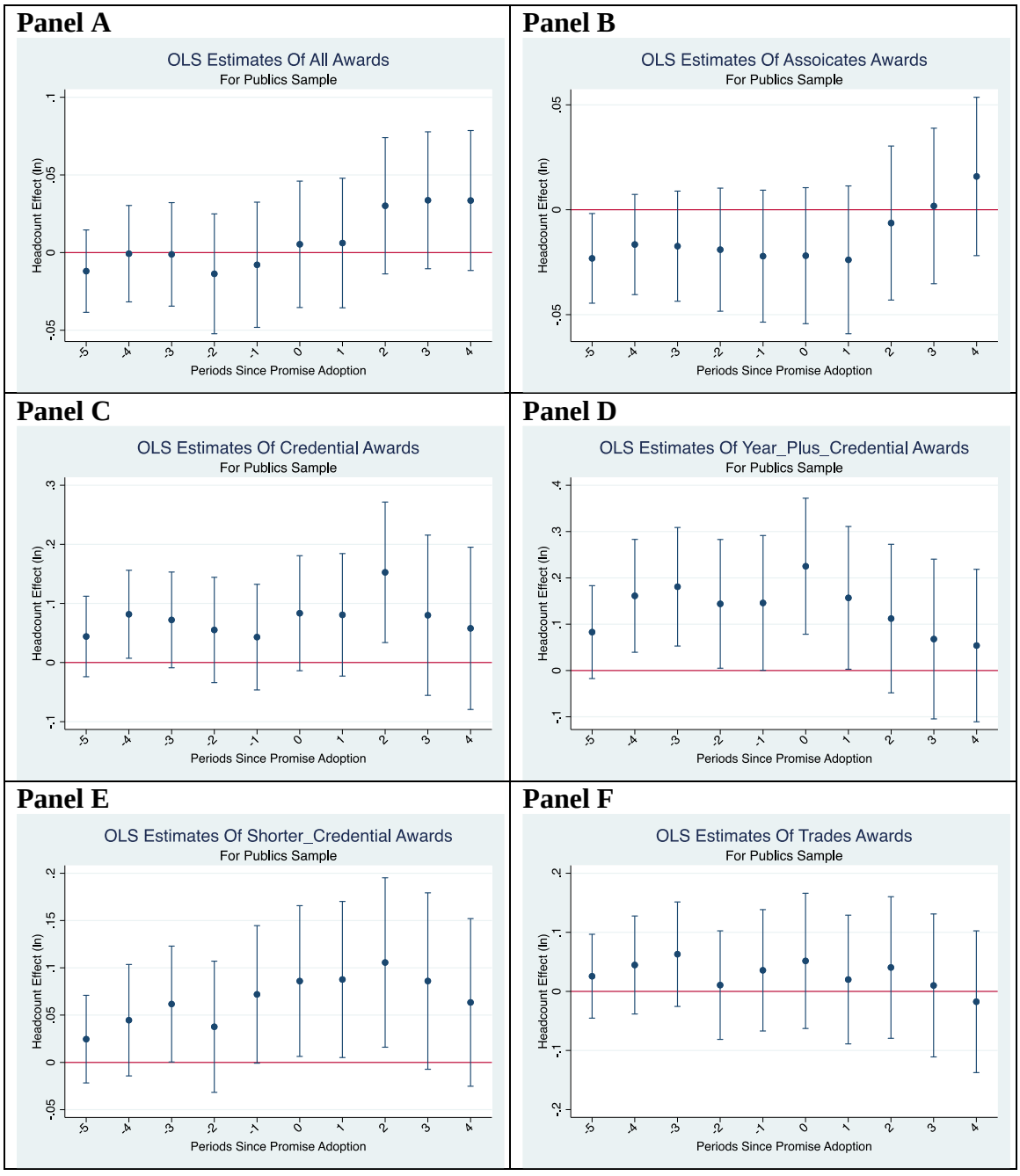

Figure 28
Visualization of OLS Trends for Public Institutions Only

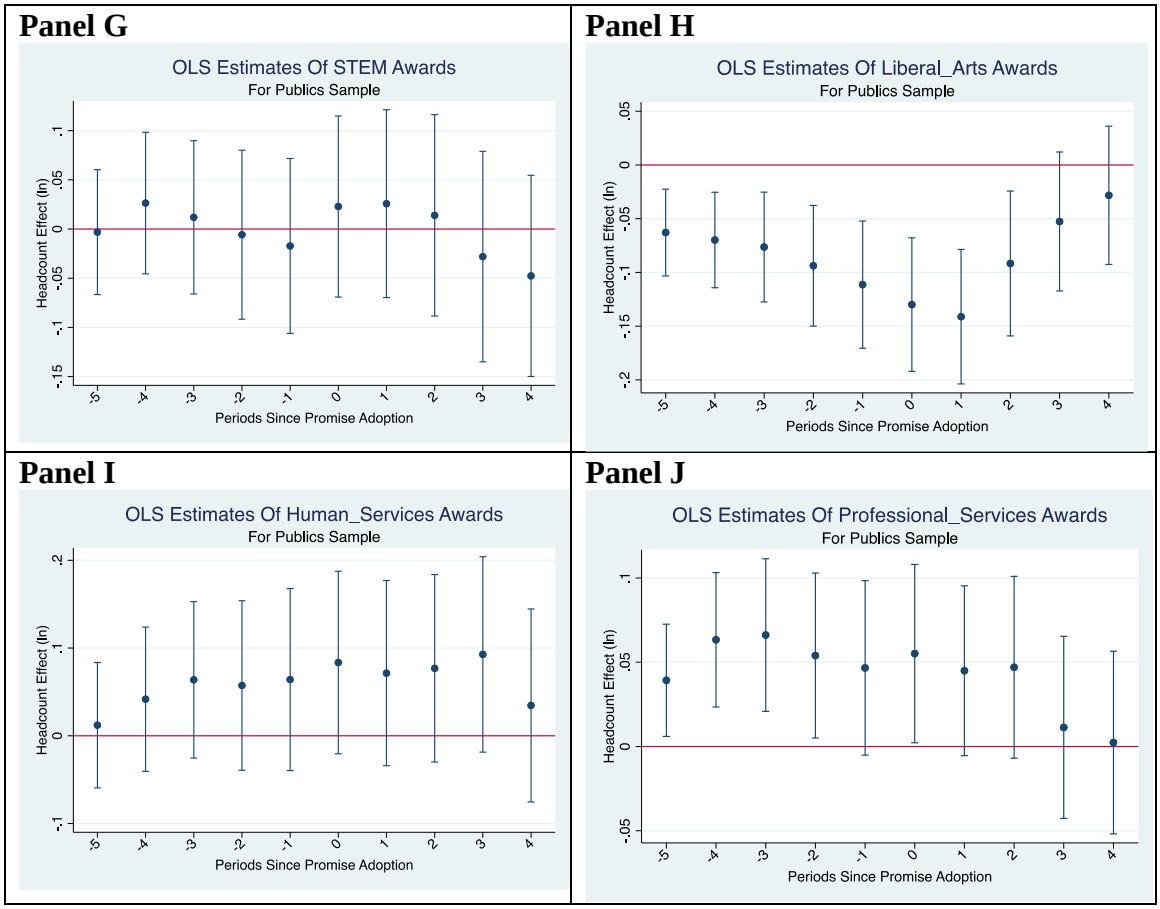

Figure 28 continued
Figure 28 contains outputs for the Ordinary Least Squares estimator in Panels A through J. In each panel, the y-axis represents the treatment coefficient's effect on the number of awards produced at institutions at that period (x-axis) in natural log units for each outcome variable of interest in this study.

APPENDIX I

Evidence of Pre-treatment Parallel Trends – Imputation Model, Public Sample

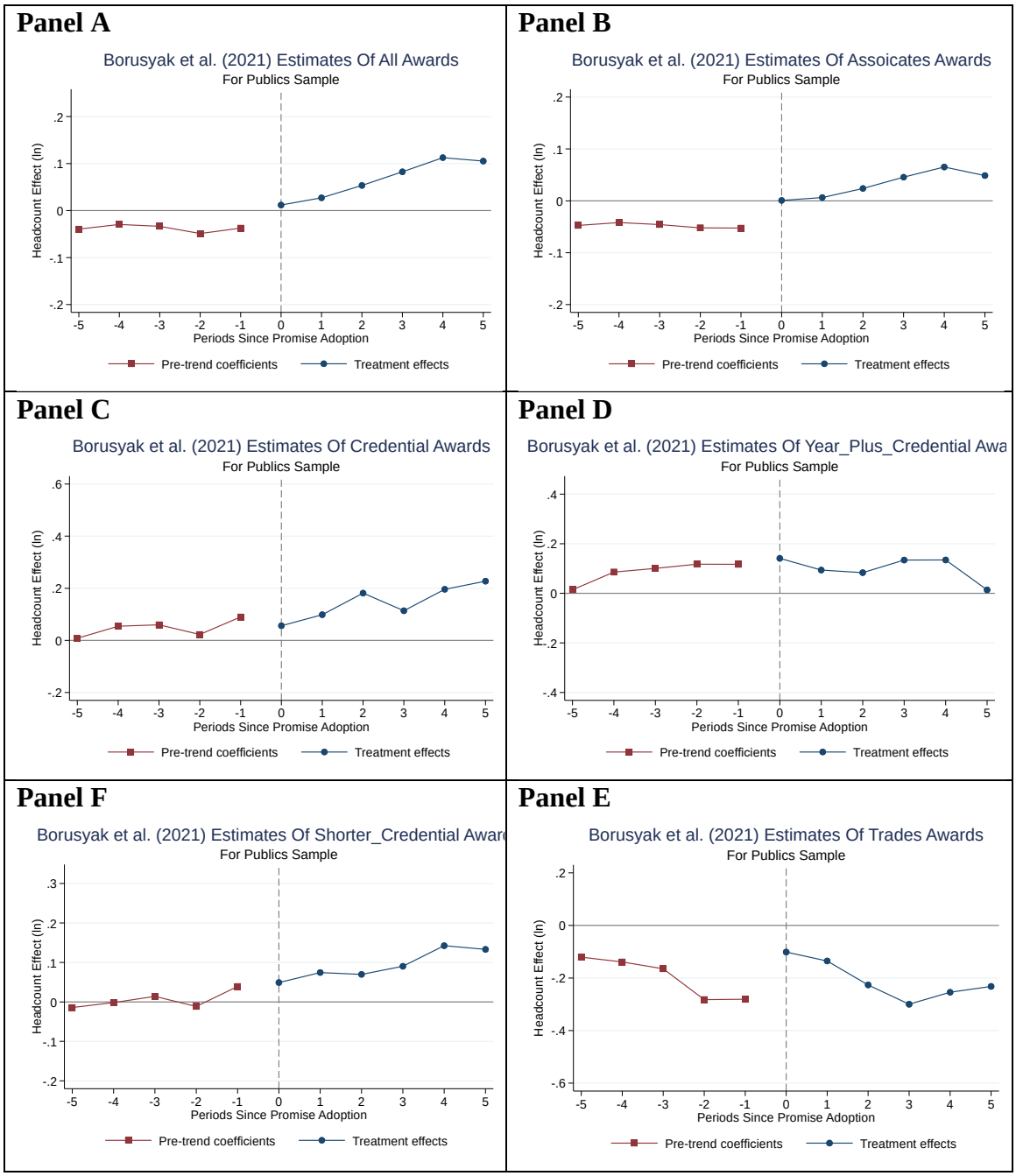

Figure 29
Visualization of Borusyak et al.'s (2021) Trends for Public Institutions Only

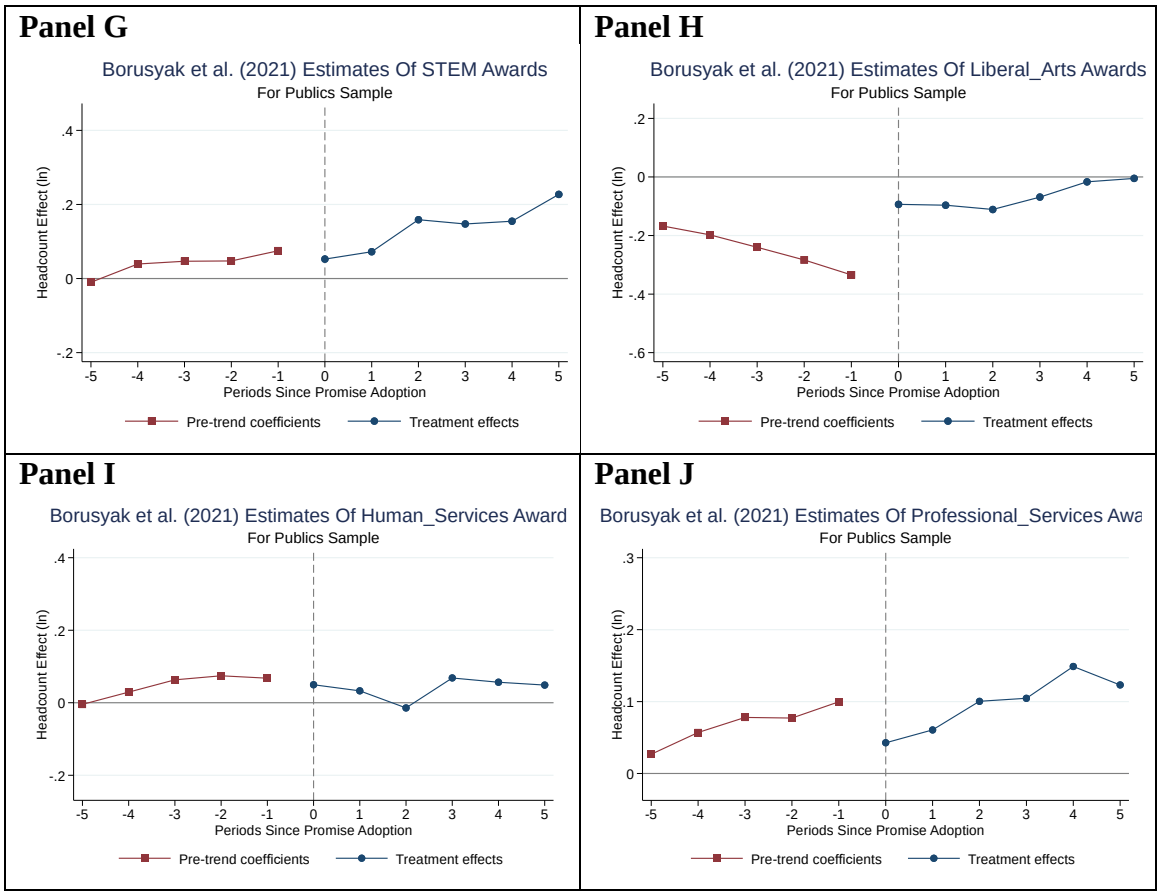

Figure 29 continued

Figure 29 contains outputs for the Borusyak et al. (2021) estimator in Panels A through J. In each panel, the y-axis represents the treatment coefficient's effect on the number of awards produced at institutions at that period (x-axis) in natural log units for each outcome variable of interest in this study.

APPENDIX J

Combined Event Study Coefficient Plot for Public Institutions Only

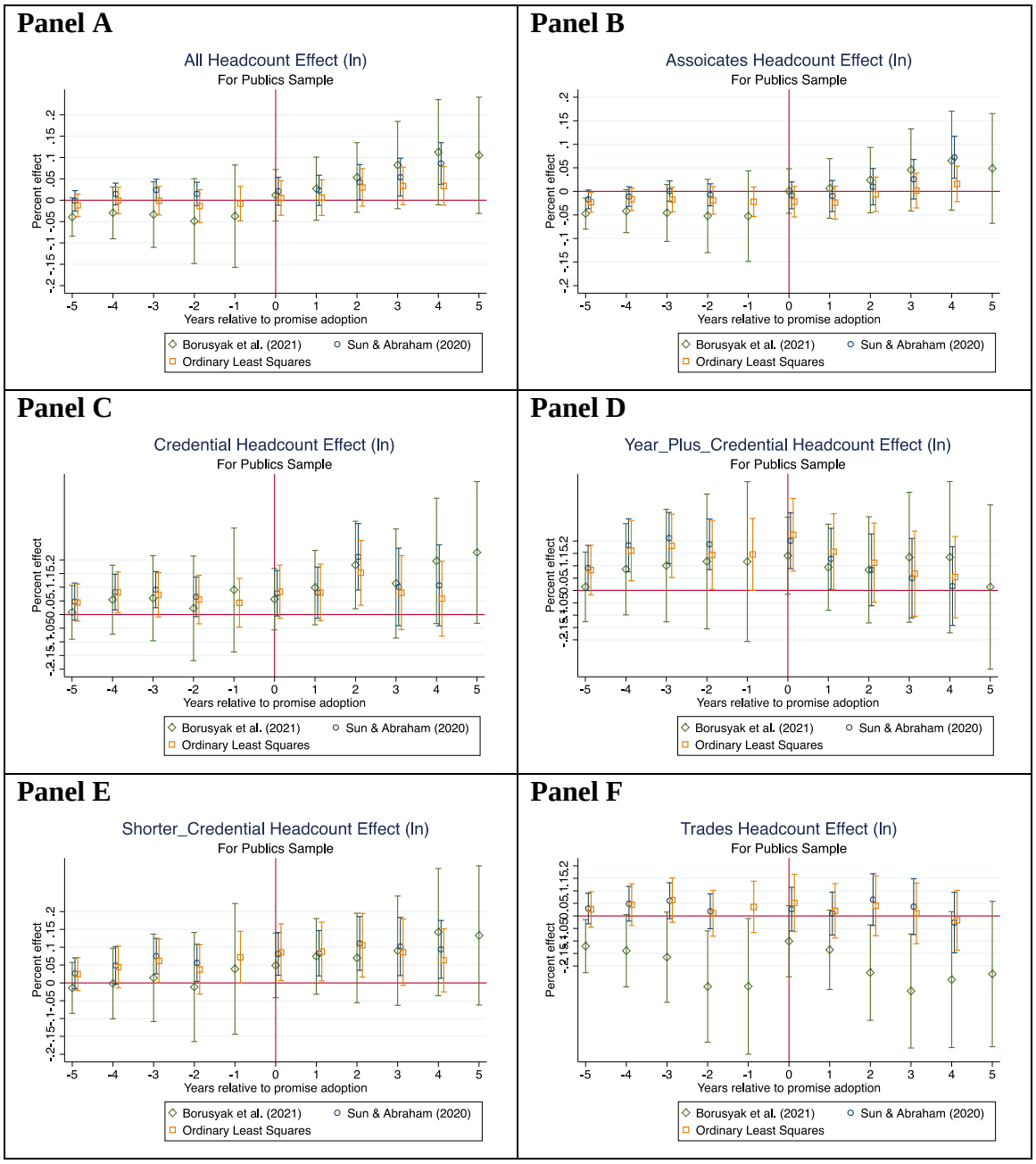

Figure 30
Visualization of Event Study, Imputation, and OLS Estimators Public Institutions Only

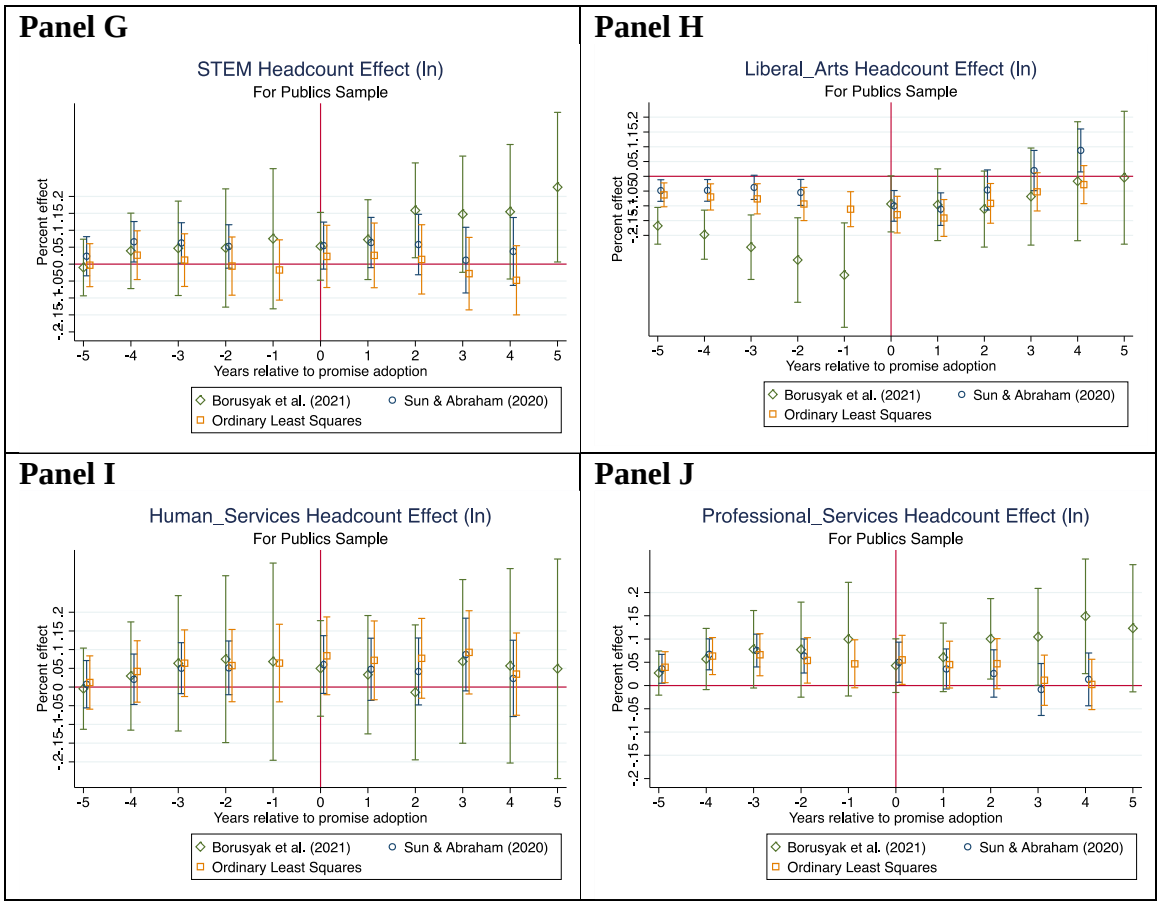

Figure 30 continued

Figure 30 contains a joint plot of the outputs for the Sun & Abraham (2020), Ordinary Least Squares, and Borusyak et al. (2021) estimators in Panels A through J. In each panel, the y-axis represents the treatment coefficient's effect on the number of awards produced at institutions at that period (x-axis) in natural log units for each outcome variable of interest in this study.

APPENDIX K

Combined Event Study Coefficient Plot for State-funded Institutions Only

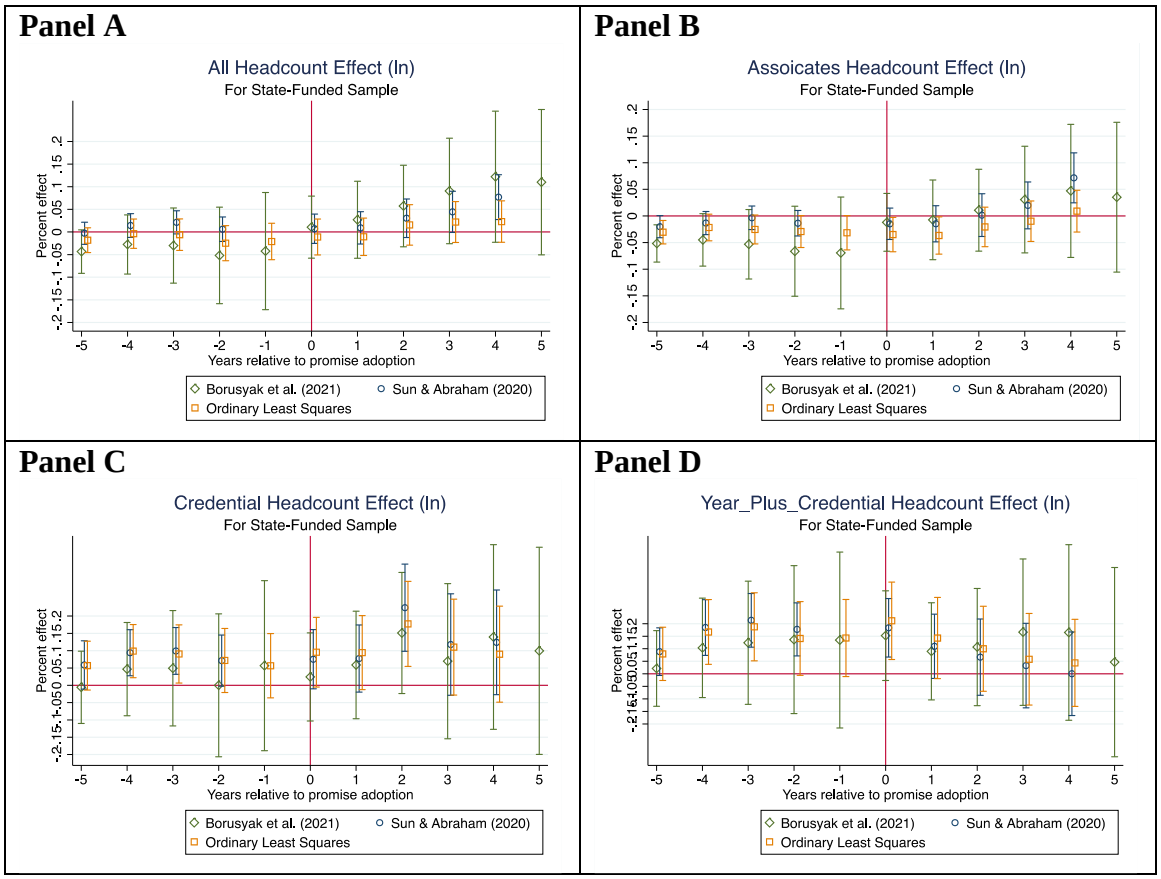

Figure 31

Visualization of Event Study, Imputation, and OLS Estimators State-funded Institutions Only

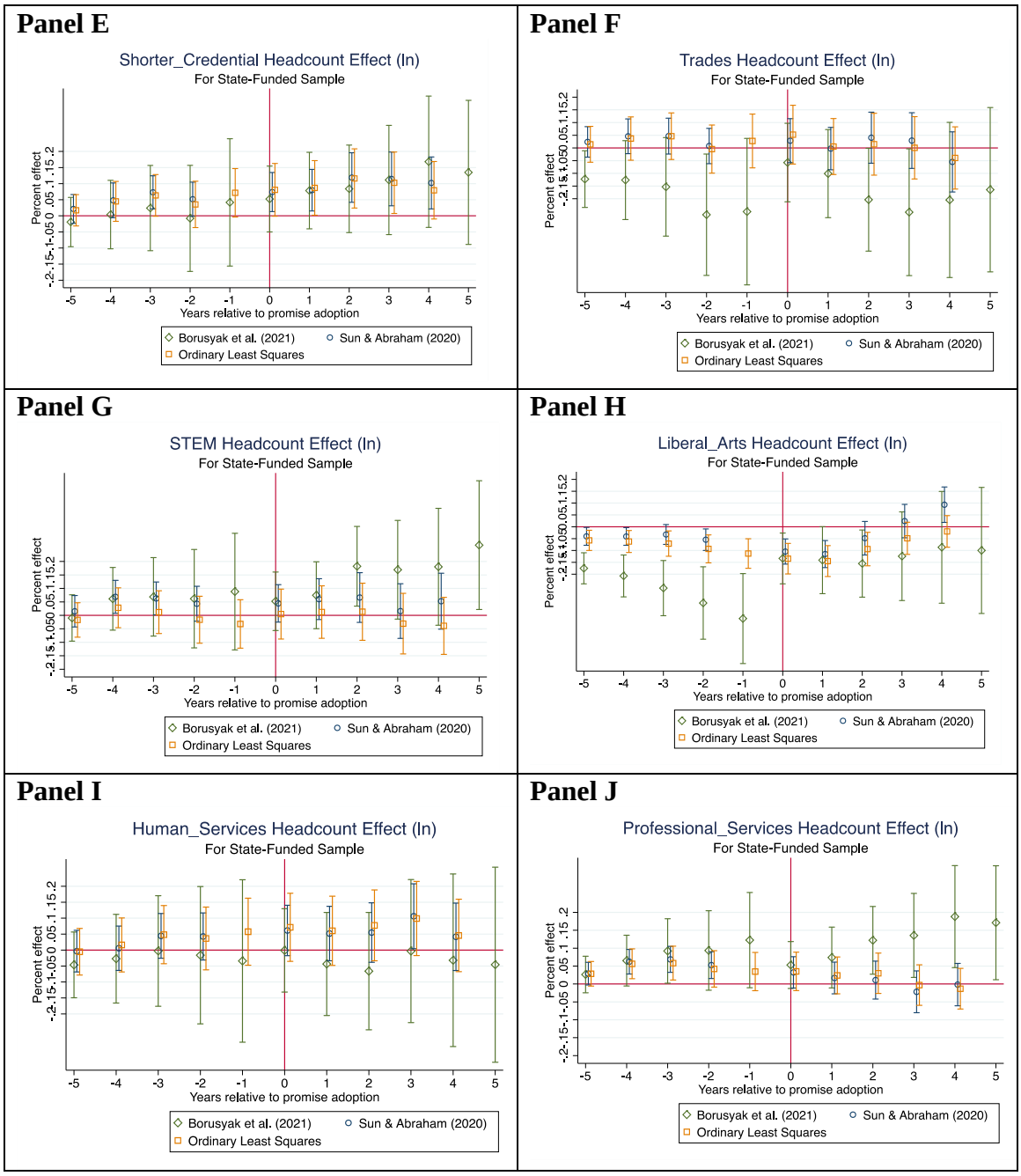

Figure 31 continued

Figure 31 contains a joint plot of the outputs for the Sun & Abraham (2020), Ordinary Least Squares, and Borusyak et al. (2021) estimators in Panels A through J. In each panel, the y-axis represents the treatment coefficient's effect on the number of awards produced at institutions at that period (x-axis) in natural log units for each outcome variable of interest in this study.

APPENDIX L

Combined Event Study Coefficient Plot for Nonprofit Institutions Only

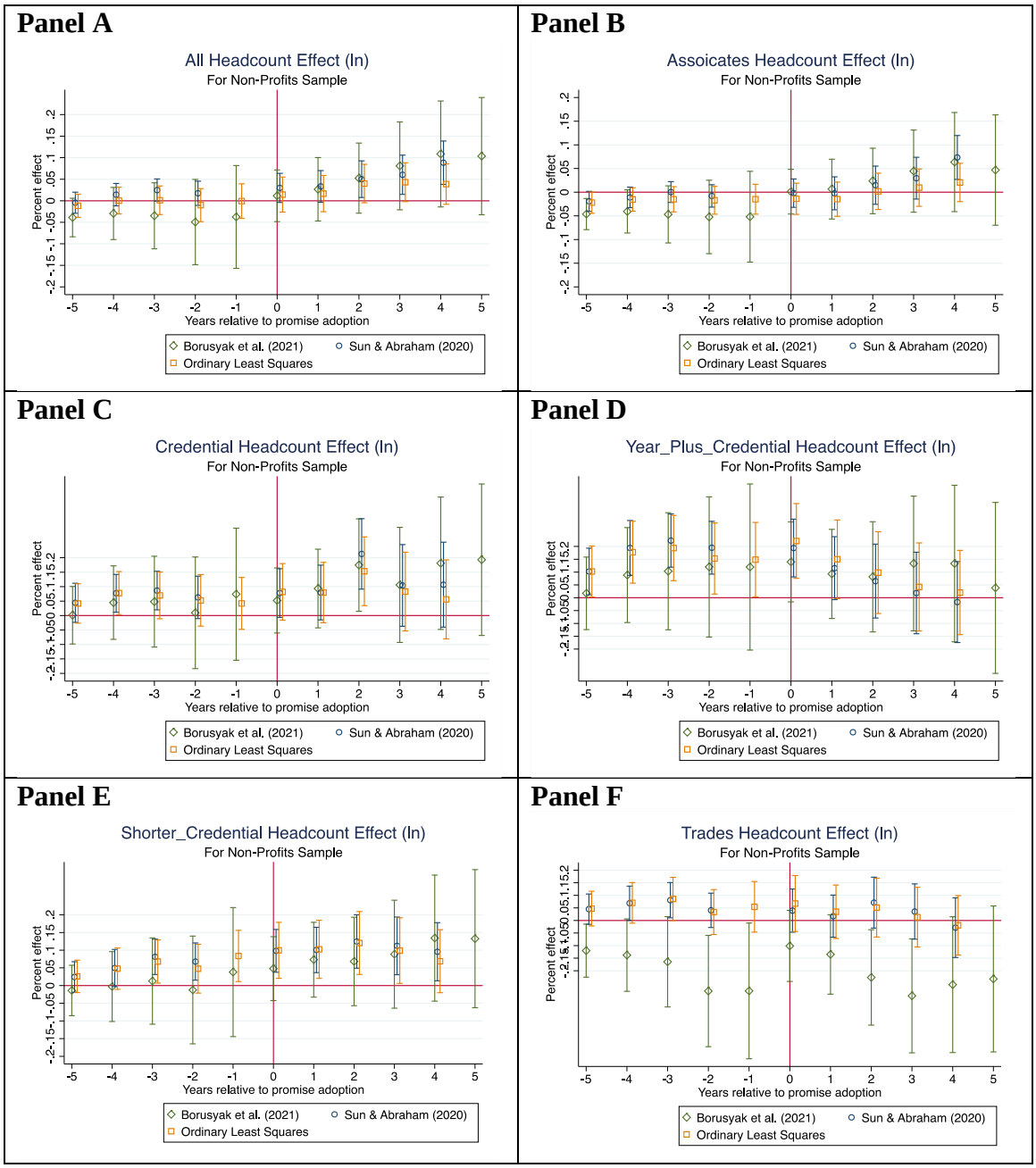

Figure 32
Visualization of Event Study, Imputation, and OLS Estimators Nonprofit Institutions Only

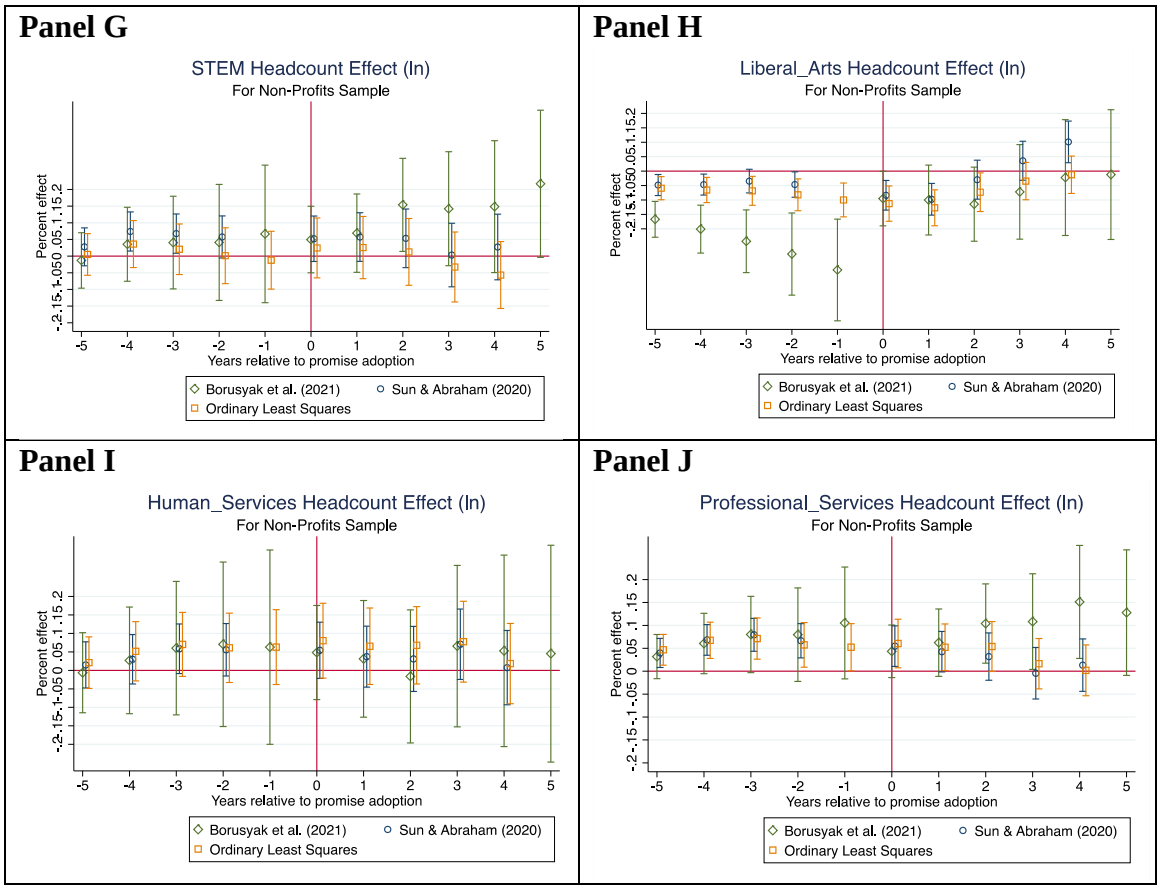

Figure 32 continued

Figure 32 contains a joint plot of the outputs for the Sun & Abraham (2020), Ordinary Least Squares, and Borusyak et al. (2021) estimators in Panels A through J. In each panel, the y-axis represents the treatment coefficient's effect on the number of awards produced at institutions at that period (x-axis) in natural log units for each outcome variable of interest in this study.

APPENDIX M

Combined Event Study Coefficient Plot for All Institutions

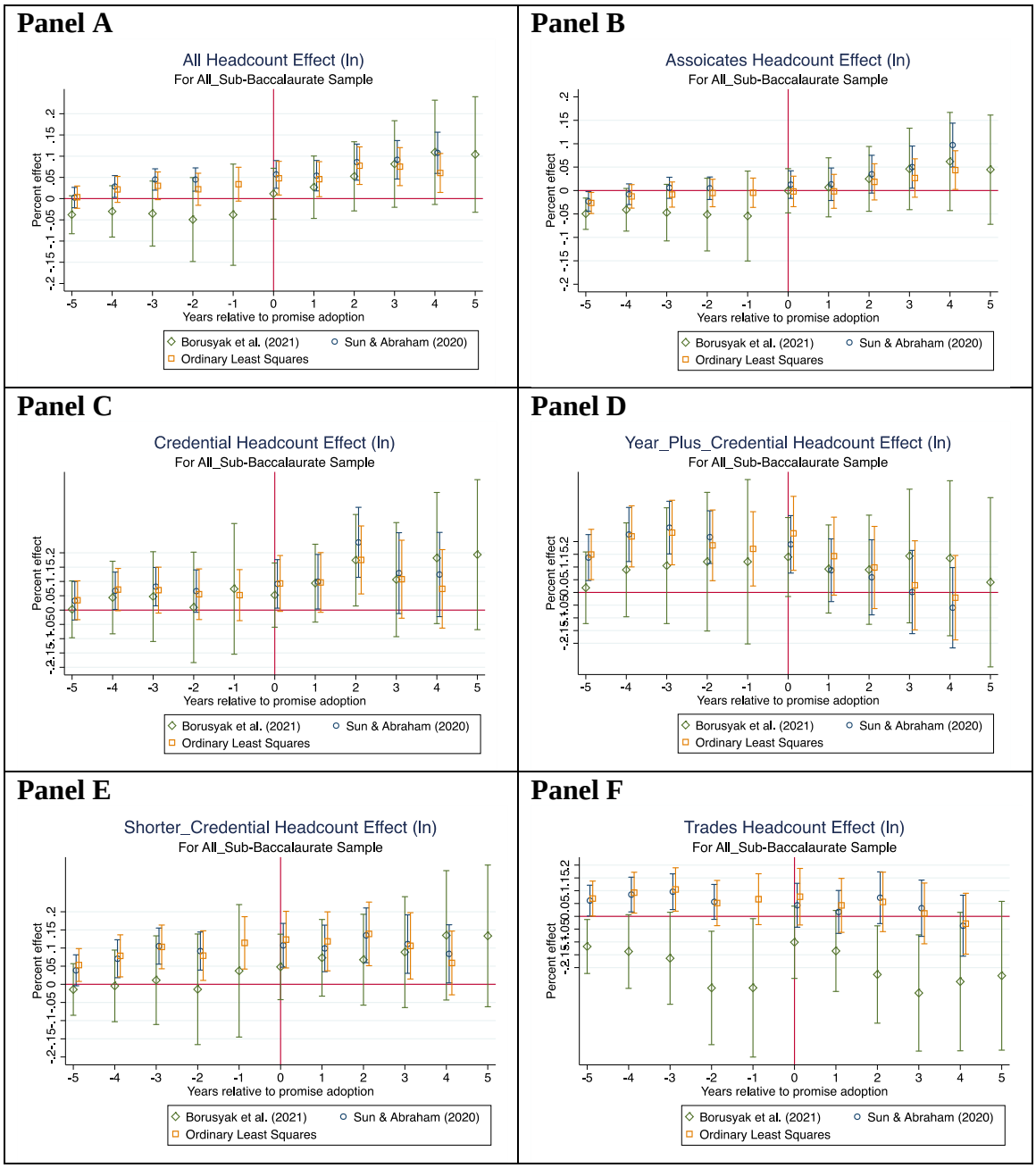

Figure 33
Visualization of Event Study, Imputation, and OLS Estimators All Institutions

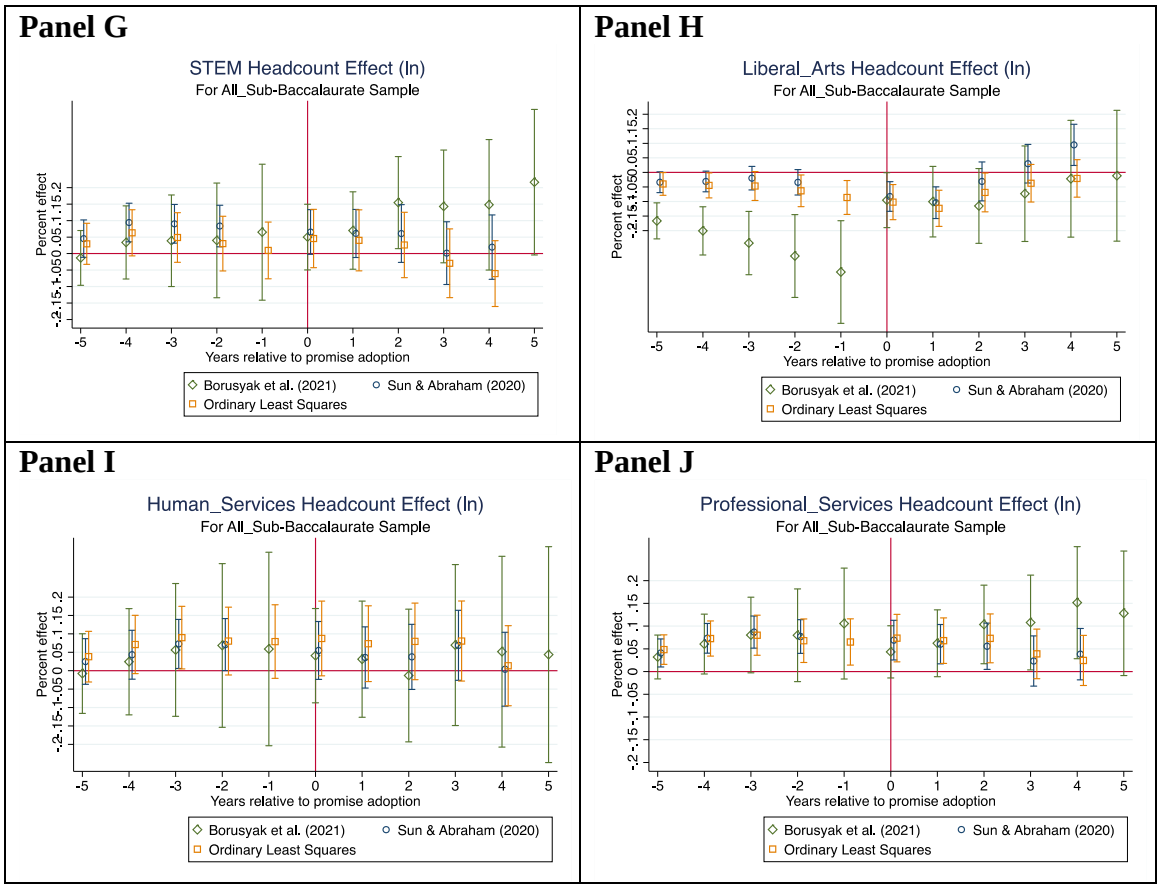

Figure 33 continued

Figure 33 contains a joint plot of the outputs for the Sun & Abraham (2020), Ordinary Least Squares, and Borusyak et al. (2021) estimators in Panels A through J. In each panel, the y-axis represents the treatment coefficient's effect on the number of awards produced at institutions at that period (x-axis) in natural log units for each outcome variable of interest in this study.

VITA

Zachary “Zac” John Hyder grew up on a family farm in the Appalachian Mountains of East Tennessee. While he was the first person in his family to attend a four-year postsecondary institution, Zac maintains that he is not the first well-learned person of his family with resilient grandparents who had a wealth wisdom and industrious parents who role-modeled the importance of education through their sub-baccalaureate postsecondary experiences. Zac attended the University of Tennessee, Knoxville and graduated with a B.S. in Biological Sciences with Chancellor’s Honors in 2015. During this time, Zac was heavily involved in student life activities such as fraternity/sorority life, student orientation and extended orientation, and the university honors office. Meeting and sharing college experiences with many, Zac became interested in why some students persist to graduation and find success in postsecondary education while others do not.

This motivated Zac to become a student affairs professional. Following graduation, he attended the University of Maryland, College Park where he completed a M.Ed. in Higher Education, Student Affairs, and International Education Policy (Student Affairs Concentration) in 2017. Following this, Zac worked professionally in metric-driven, student success focused universities. Working with and supporting students through the Covid-19 pandemic energized Zac to return to the University of Tennessee, Knoxville to pursue a Ph.D. in Higher Education Administration.

Today, Zac is still deeply concerned with issues of postsecondary student success as he researches how the lever of college affordability affects student outcomes such as

enrollment, graduation, and labor market returns. He is currently a recipient of an ECMC Foundation Post-Doctoral Grant where he works in a cost share agreement with the Tennessee State Collaborative on Reforming Education. In this position following graduation, he supports the organization in the realm of education to workforce policy, research, and data analysis.