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The waning prestige of the teaching profession has fueled a decline in the number of aspiring teachers entering preparation programs, compounding staffing challenges already inherent to many schools. In this paper, I show that state-level loan forgiveness policies modestly increase the number of first-year students enrolled in education programs by about 12% but have no overall effect on total enrollment or the number of bachelor's degrees awarded. Further analyses show that the effect for bachelor's degrees is masked by heterogeneity in the amount of debt forgiven. This suggests that these policies may be an option to consider, but should be adopted with caution, as effect sizes are small relative to staffing challenges and require a significant investment from the state.

Keywords: teacher pipeline; loan forgiveness; difference-in-difference

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The Effects of State-Level Loan Forgiveness Policies on Teacher Supply

Many schools across the country, particularly those with high proportions of students of color and economically disadvantaged students, struggle to fill teaching slots due to persistent teacher shortages (Edwards et al., 2025; Ingersoll et al., 2019; Nguyen et al., 2022). These challenges received considerable attention due to the pandemic, but in reality, the United States has for decades been experiencing a waning prestige of the teaching profession and a decline in the number of individuals entering the teacher preparation pipeline (Ellison et al., 2025; Kraft & Lyon, 2024). The resulting decline of aspiring teachers has been amplified by individuals dropping out at each of several checkpoints in the pathway to teaching (e.g., enrollment in a prep program, graduation from a prep program, certification tests, and employment as a K-12 teacher), which evidence shows disproportionately filters out teachers of color (Bartanen & Kwok, 2023; Carter Andrews et al., 2019). Thus, one way to shore up the teacher labor pool may be to better target potential candidates earlier in the pipeline and attenuate the "filtering" dynamics at key checkpoints.

One possible policy lever to accomplish this aim is targeted student loan debt forgiveness for teachers in return for their teaching service (Aragon, 2018; Feng & Sass, 2018; Glazerman et al., 2013; Jacob et al., 2024; Steele et al., 2010). While the federal Teacher Loan Forgiveness and Public Service Loan Forgiveness programs have been in place since 1998 and 2007, respectively, recent research shows limitations to these policies due to administrative burdens, confusion over eligibility requirements, and an overwhelming denial of applications (e.g., 99% denial rates for PSLF in 2019) (Jacob et al., 2024; *Public Service Loan Forgiveness: Opportunities for Education to Improve Both the Program and Its Temporary Expanded Process*, 2019). Additionally, the Teacher Loan Forgiveness program is fairly broad in scope, requiring only that teachers work for at least five years in schools serving at least 30% low-income students, and may not incentivize the intended individuals to enter a given state's teacher workforce, especially considering different states' varying needs. While these limitations may have been attenuated during the Biden administration through increased funding (U.S. Department of Education, 2023, 2024), non-federal programs may continue to play a role in shoring up the teacher pipeline at subnational levels – especially given any rollbacks of U.S. Department of Education roles.

Compounding the administrative limitations and the broad definition of a hard-to-staff school, the federal Teacher Loan Forgiveness policy forgives the same amount of student loan debt for every recipient (i.e., $5,000 for teachers in hard-to-staff schools and $17,500 for high-needs subject teachers in hard-to-staff schools). This relatively low amount of forgiveness might play a role in the potential impact of – or lack thereof (Jacob et al., 2024) – this policy for two reasons. First, the cost of attendance varies widely across institutions (Kelchen et al., 2017). For the 2019-20 school year, the national average for in-state tuition, fees, room, and board at a public four-year university was $21,035. Utah was the lowest at $14,619, and Vermont was the highest at $29,665 (National Center for Education Statistics, 2022). For those graduating from an institution in Utah, the federal policy forgives, on average, 34.2% of a single year of cost of attendance, and, for those in Vermont, only 16.9% of a single year of cost of attendance, on average. Second, the cost of living also varies widely across states. On a cost-of-living index where the national average is centered at 100 (with a standard deviation of 21.1), states range from 84.5 (i.e., Mississippi) to 198.6 (i.e., Hawaii) (Missouri Economic Research and Information Center, 2020).[1] Those students considering teaching as a profession in a high-cost-of-living state might not view $5,000 of student loan debt forgiveness the same way students in a low-cost-of-living state would, so that amount of forgiveness does not carry the same weight nationwide and across contexts.

A body of evidence shows that state-level loan forgiveness programs have the capacity to

[1] Data for 2020 was accessed through the Internet Archive Wayback Machine via https://web.archive.org/web/20210324204005/https://meric.mo.gov/data/cost-living-data-series

increase teacher retention in hard-to-staff schools and in high-needs subjects, and to incentivize teachers to transfer from their current schools to these hard-to-staff schools (Feng & Sass, 2018; Glazerman et al., 2013; Morgan et al., 2023). However, less is known about how targeted loan forgiveness might affect initial teacher supply (Steele et al., 2010). In particular, there is a dearth of research examining whether and to what extent these policies contribute to the supply of new potential teacher candidates early in the teacher pipeline, when they are deciding which undergraduate degree to pursue. Further, as teacher shortages are not monolithic and different states experience different patterns of teacher needs (Edwards et al., 2025; Nguyen et al., 2022), the ability of a state to construct and implement its own policy that specifically targets schools and positions that are more heavily experiencing shortages is likely more effective at addressing the need for teachers. Therefore, I ask the following three research questions:

1. To what extent do state-level targeted teacher loan forgiveness policies impact the number of students enrolling in an education program?
2. To what extent do state-level teacher loan forgiveness policies impact the number of teacher preparation program bachelor's degrees awarded?
3. To what extent do these effects of teacher loan forgiveness policies vary by policy design feature?

Drawing from an original dataset of state loan forgiveness policy adoption and education program enrollment and completion data from the Integrated Postsecondary Education Data System (IPEDS), I use event study and difference-in-differences models that leverage the variation in timing of policy adoption to identify a plausibly causal effect of the adoption of state-level teacher loan forgiveness policies on new teacher supply. I find a 12% increase in the number of education program first-year enrollees and no overall effect on total education program enrollment or teacher preparation program bachelor's degrees awarded. However, I find that the overall effects on bachelor's degrees appear to be masked by heterogeneity in policy design features. Policies that forgive greater shares of student loan debt appear to increase the number of bachelor's degrees awarded (i.e., up to about a 10% increase). In contrast, policies that forgive lower shares of student loan debt have no effect. Further, I find that the adoption of loan forgiveness policies has a greater effect for institutions in areas with higher unemployment rates, aligning with recent literature on the influence of unemployment rates on teacher supply (Goldhaber & Theobald, 2022; Rucinski, 2023).

The remainder of this paper proceeds as follows. First, I provide a brief overview of the teacher pipeline and how individuals may enter the pipeline through different entry points. Then, I detail the research on the extent to which teacher loan forgiveness policies have impacted teacher supply. Next, because there is limited research on targeted loan forgiveness and its impacts on the early teacher pipeline as opposed to transfers and turnover (for an exception, see Steele et al., 2010), I draw on research on financial aid programs more broadly. In the next section, I describe my data, sample, and identification strategy for isolating the effects of state-level loan forgiveness policies on first-year education program enrollment, total education program enrollment, and teacher preparation program bachelor's degrees. I then conclude with a discussion and policy implications.

Literature Review

The Teacher Pipeline

Despite the growing popularity of alternative preparation pathways (Edwards & Kraft, 2025), a vast majority of new teachers still gain certification through traditional means (Blazar et al., 2024), and these traditional teacher candidates must pass through multiple checkpoints en route to becoming teachers. Beginning with the individual's interest in the profession, they must enroll in, persist through, and complete a teacher preparation program; acquire a teaching certification in their state; and eventually gain employment as a teacher. Each of these checkpoints presents a barrier to entry into the profession and therefore acts as a "filter," removing potential teachers from the pipeline (Carter Andrews et al., 2019; Goldhaber et al., 2022; Goldhaber & Mizrav, 2021; Rucinski, 2023). Figure 1 below provides a model of the early teacher pipeline, showing how an aspiring teacher may progress through it. Boxes A and B of Figure 1 represent the portions of the pipeline addressed by my first research question, with Box A describing first-year enrollment and Box B describing total enrollment. Box C of Figure 1 represents the portion of the pipeline addressed by my second research question: bachelor's degrees awarded by teacher preparation programs.

[Figure 1]

Initial interest in the teaching profession serves as the first such filter. Over the past few decades, initial interest has been waning (Kraft & Lyon, 2024), with evidence showing a sharp decline in interest more recently (Bartanen & Kwok, 2023; Ellison et al., 2025). These declines in interest harm the pipeline by reducing the number of individuals actually entering it. Once the initial interest filter has been passed, there are three “inflows” of potential teachers that drive teacher preparation programs (Bartanen & Kwok, 2023; Goldhaber et al., 2014). Students who show initial early interest in the teaching profession are admitted to and enroll in college with the immediate intention of entering a teacher preparation program, as shown with green arrows in Box A of Figure 1. Students may transfer into an institution with a teacher preparation program, for example, from a community college or institution without a teacher preparation program. In this scenario, students may or may not have initially been interested in teaching. Finally, students may enroll at an institution under a different program of study or undeclared, but switch majors to teaching along the way. Transfers and major switchers are shown in Box B of Figure 1, along with the relationship encompassed by Box A. It is worth noting that, despite there being three sources of individuals entering a college teacher preparation program, students can transfer out of the college or switch majors away from teacher preparation, as shown with red arrows in Figure 1. Additionally, the supply of teachers from this traditional pathway is only part of the picture, as only 65% to 80% of teacher candidates become fully employed teachers (Bartanen & Kwok, 2023; Goldhaber et al., 2023). On the one hand, a graduate from a teacher preparation program may actively choose not to pursue a teaching job, but on the other, a graduate may not be able to find an available job opening (Goldhaber et al., 2023). This further supports the case for state-level loan forgiveness so that states may focus the target of such policies on specific schools and positions that experience greater shortages.

Financial Incentives and Teacher Supply

Many states and districts have implemented financial incentive policies (e.g., bonuses, scholarships/grants, loan forgiveness) in an attempt to compensate for lesser working conditions, and therefore increase initial supply or decrease turnover (Aragon, 2018). There is mixed evidence of such programs, with some successfully attracting teachers to schools and subsequently retaining them (Clotfelter et al., 2008; Feng & Sass, 2018; Glazerman et al., 2013; Morgan et al., 2023; Steele et al., 2010), but with others presenting null effects (Jacob et al., 2024; Pham et al., 2021; Springer et al., 2012). In work most closely related to the present study, Steele and colleagues (2010) drew on data on teacher-licensure candidates to find that a combination of a loan forgiveness and scholarship program increased the likelihood of teacher candidates working in hard-to-staff schools.

Under the broader umbrella of financial incentive policies, targeted loan forgiveness policies – those that aim to recruit or retain teachers to hard-to-staff schools and/or high-needs subjects in exchange for forgiveness of student loan debt – have been implemented at the district, state, and federal levels with teacher retention effects ranging from null to positive (Feng & Sass, 2018; Fulbeck, 2014; Glazerman et al., 2013; Jacob et al., 2024; Morgan et al., 2023). At the district level, these types of programs appear to lower turnover rates (Fulbeck, 2014) and increase the likelihood of high-quality teachers transferring to hard-to-staff schools for as long as the policies are still in place (Morgan et al., 2023). At the state level, Feng and Sass (2018) found that a Florida loan forgiveness program decreased the likelihood of exit for math, science, foreign language, and ESOL teachers, respectively. However, at the federal level, Jacob and colleagues (2024) found precisely estimated null effects of the federal Teacher Loan Forgiveness program on retention for teachers in Title I schools in Michigan, in contrast to the findings found at the district and state levels (Feng & Sass, 2018; Glazerman et al., 2013; Morgan et al., 2023). The authors of the Michigan study suggested that program complexity and administrative barriers may have inhibited the implementation of the federal policy, aligning with suggestions from prior work on the design features of teacher incentive policies (Clotfelter et al., 2008; Fryer, 2013).

Except for the study by Steele and colleagues (2010), teacher loan forgiveness research thus far has focused on teachers already in the profession and examined their propensity to transfer to different schools. Little is known, then, about how early pipeline candidates might respond to such financial incentive policies. However, research outside of the teacher context may provide some clues. Research in the law and medical school contexts, for example, suggests that financial incentives have the potential to induce students to work in public service and in understaffed remote areas (Field, 2009; Pathman et al., 2004). However, it is unclear whether findings from the law and medical professions, where graduates' eventual earnings are substantially higher than those of teachers, may generalize to the teaching context.

Financial Aid and Post-Secondary Outcomes

While little is known about the causal effects of loan forgiveness on the pipeline of teacher candidates, research on financial aid programs more broadly can provide context for the potential impacts of loan forgiveness on student postsecondary enrollment and completion. A large body of evidence documents the effects of lowering the cost of higher education through financial aid to increase enrollment and completions (Bettinger, 2015; Carruthers & Özek, 2016; Castleman et al., 2018; Cohodes & Goodman, 2014; Santos & Haycock, 2016). Financial aid policies appear to increase enrollment for first-time degree seekers, with the largest effects being observed on middle-class students and students of color (Leeds & DesJardins, 2015; Rosinger et al., 2019; Upton, 2016).

Financial aid policies also appear to increase both persistence and completion rates for students, conditional on enrollment (Nguyen et al., 2019). With respect to persistence, both eligibility for and receipt of financial aid increase students' retention between their first and second years of college (Doyle et al., 2017), while also reducing drop-out and transfer rates throughout all of college (Bettinger, 2015). With respect to degree completion, a randomized controlled trial of a performance-based scholarship program in Ohio found that a $900-per-semester award increased the number of degrees by 3.3 percentage points for the treatment group relative to the control group in the second year of the study and by 3.8 percentage points in the third year of the study (Mayer et al., 2016). In addition to financial aid policies' ability to increase completion rates, they may also incentivize students to change majors. Castleman and colleagues (2018) found that the Florida Student Assistance Grant, a need-based financial aid program for Florida students to receive aid above and beyond any federal Pell grant money, increased the likelihood of acquiring a STEM BA/BS by 2.7 percentage points, which the authors find to be driven by students switching into a STEM field to qualify for the grant. This latter finding suggests that the promise of loan forgiveness has the potential to induce students to select into a particular major—though there is little evidence thus far on whether effects would be similar for incentivizing students to select into education majors on a national scale.

In sum, financial aid policies aimed at increasing student enrollment, persistence, and completion appear to have positive effects on their intended aims. Targeted teacher loan forgiveness policies are implemented for the same purpose, though with a narrower scope in that they focus exclusively on teacher preparation degrees. It is possible that, similar to Castleman and colleagues (2018) findings, these policies could be successful in attracting new enrollments and incentivizing current students to switch programs.

Loan Forgiveness Policy Background

Teachers have multiple options when considering programs that forgive their student loans. At the federal level, teachers may consider Teacher Loan Forgiveness or Public Service Loan Forgiveness, among others (see Table 1 for a brief overview of these policies). Beginning with the adoption of the Teacher Loan Forgiveness program as a part of the Higher Education Amendments of 1998, teachers who worked in low-income schools (i.e., those with greater than or equal to 30% low-income students) for more than five years were eligible to receive $5,000 in student loan forgiveness. This policy was expanded in 2004 by the Taxpayer-Teacher Protection Act, which added a component for special education teachers and secondary math and science teachers to receive up to $17,500 in student loan forgiveness. The most well-known update to the federal student loan forgiveness policy landscape occurred in 2007 with the passage of the Public Service Loan Forgiveness program. In this update, all public sector employees – including, for example, teachers, nurses, or lawyers who work in the public sector – were eligible for total forgiveness of remaining student loan debt after 120 months of payments toward their existing loan debt. However, few people have been able to take full advantage of this loan forgiveness program, due to confusion about the application process and low acceptance rates from the department (Jacob et al., 2024; *Public Service Loan Forgiveness: Opportunities for Education to Improve Both the Program and Its Temporary Expanded Process*, 2019).

Both before and throughout the span of federal policy evolution outlined in Table 1, states have also passed their own teacher loan forgiveness policies. In 1977, Connecticut passed the first state-level teacher loan forgiveness policy that forgave up to $10,000 (in 1977$; $42,702 in 2020$) for high-needs subject teachers after two years of teaching service (CT. Stat. 10a-163, 1977). Since this first policy was adopted, 37 additional states have adopted teacher loan forgiveness policies through 2022 (see Appendix A for details). These state-level policies serve to attract and incentivize potential teachers to particular schools and contexts that may traditionally struggle to recruit teachers in a given state setting. As the federal policy designates a teacher eligible if they work in a school with at least 30% low-income students, states may use their own definition of "hard-to-staff" schools and their own definition of "eligible teacher." For example, North Dakota forgives student loan debt for teachers who work at least one year in a rural school district (N.D. Century Code 15.1-02-22, 2019), and Virginia's loan forgiveness policy contains a minority teacher component, where ethnoracial minority teachers are eligible for one year of tuition forgiveness for each year of teaching service (Va. Code Ann. § 22.1-290.01, 2001).

[Table 1]

Methods

Data

To answer my research questions surrounding state-level loan forgiveness policies, I primarily use data from the Integrated Postsecondary Education Data System (IPEDS), maintained and made available by the National Center for Education Statistics (NCES) at the U.S. Department of Education. IPEDS is a series of 13 surveys conducted annually for every college, university, and technical and vocational institution that participates in federal student financial aid programs. This repository contains longitudinal data from 1984-85 to 2023-24[2] on all relevant higher education institutions across all 50 states and the District of Columbia. I use data from five of the 13 surveys, including institutional directory information, enrollment and graduation counts, tuition rates, and financial data.[3] I draw from 22 years of data beginning with 1998-99 school year, the first year of

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[2] At the time of writing, the most recent year available was the 2022-2023 school year

[3] Formal sub-datasets used are the Directory, Institutional Characteristics, Fall Enrollment, Completions, and Finance files. I acquire these sub-datasets through the Urban Institute's Education Data Portal, which compiles large-scale public data, such as IPEDS, into more accessible formats and harmonizes variables across sub-datasets and years to simplify longitudinal analysis (Education Data Portal, Version 0.21.0, n.d.)

the federal Teacher Loan Forgiveness program that remains in effect today, through the 2019-20 school year, which contains the last fall semester before the COVID-19 pandemic.

I then supplement the IPEDS dataset with state-level demographic, economic, and educational characteristics acquired from publicly available data over the same time period from the Bureau of Labor Statistics, Current Population Survey, American Community Survey,[4] NCES Common Core of Data, and College Scorecard (see Table 2 and Table 3 for a breakdown of variables and sources).

Sample

I begin with an initial IPEDS universe of 166,172 institution-years representing 13,520 unique institutions over the 22-year study period. First, I follow Kelchen's (2025) use of Carnegie classifications[5] to identify baccalaureate, master's, and research universities for observations between the 1998-99 and 2019-20 school years. A slight change from Kelchen (2025) is that I use the 2018 classification codes instead of the 2021 codes. Using these criteria, I remove 129,019 observations. Next, I remove 5,168 observations with missing covariates and 3,445 observations from institutions that never granted a teacher preparation degree at any point in the sample window. Because my identification strategy requires observation of institutions before and after implementing teacher loan forgiveness, I exclude institutions in states that adopted a loan forgiveness policy before or during the 1998-99 school year (i.e., always-treated observations). To clarify, I only drop those observations that are treated in this school year. I maintain all never-treated and all not-yet-treated observations for the 1998-99 school year. After excluding 8,651 observations from the 12 early-adopter[6] states (Bleiberg et al., 2024; Roth et al., 2023), my sample includes 19,911 institution-year

[4] Teacher salaries only go back to 2000-01 in ACS so I acquired the 1998-99 and 1999-00 school year salaries from https://nces.ed.gov/programs/digest/d01/dt079.asp

[5] I include codes 15 through 23 for the CC Basic 2018 coding system

[6] Alaska, California, Connecticut, Delaware, Florida, Louisiana, Massachusetts, Maine, Pennsylvania, South Carolina, Texas, and Vermont adopted a targeted teacher loan forgiveness policy during or before 1998-99

observations. I finally trim the sample to include only those within the relative time range of *k-16* to *k+16,* given the small and stylized sample at the extremes of the relative years. My final analytic sample includes 18,864 institution-year observations, representing 917 unique institutions across 39 states.

Measures

Outcome Variables

My three outcome variables, as displayed in Figure 1, are the institution-by-year measures of 1) the count of undergraduate first-year enrollees (i.e., non-transfer freshmen) in an education program, 2) the count of undergraduate enrollees in an education program, and 3) the count of teacher preparation program (TPP) bachelor's degrees awarded. The IPEDS Fall Enrollment sub-dataset, from which I draw the first two measures, provides only two-digit Classification of Instructional Programs (CIP) codes, so both first-year and total enrollment are measures of education programs rather than teacher preparation programs. Despite a slightly coarser measure of enrollment, these variables serve as proxies for teacher preparation programs because about 94% of the total undergraduate education program Bachelor's degrees are awarded to students in teacher preparation programs.[7] Additionally, these enrollment data are only provided for even-numbered calendar years (e.g., 1999-00, 2001-02, 2003-04; as measured in spring), so I interpolate the values for odd-numbered calendar years (e.g., 2000-01, 2002-03, 2004-05). For example, the value interpolated for the 2002-03 school year is the average of the 2001-02 and 2003-04 school years. While I include these interpolated values for my main analyses, I run supplemental analyses that do not include them and provide qualitatively similar results in Appendix B. For my TPP bachelor's degree outcome (which is provided every year), I focus on programs defined as teacher preparation degrees in prior

[7] Author's calculations. The average institution awards 84.2 teacher preparation program Bachelors degrees and awards 89.6 education Bachelors degrees. This indicates that about 93.9% of all education program Bachelors degrees are designated as teacher preparation program Bachelors degrees. Master's level TPP programs consist of about 75% of total education program degrees awarded

research using IPEDS to examine teacher supply (Kraft et al., 2020). I used the following CIP codes to identify teacher preparation degrees: 130101, 130201-130299, 130301, 131001-131099, 131201-131299, 131301-131399, 131401-131499, and 139999. According to the 2017-18 National Teacher and Principal Survey, 39.3% of the teacher workforce holds a Bachelor’s degree and 49.2% holds a Master’s degree, with the lowest percentage of Bachelor's degree holders in New York at 4.7% and the highest in Texas at 67.5% (National Center for Education Statistics, 2017). Despite a plurality of teachers holding graduate degrees, a significant proportion still hold only undergraduate degrees, underscoring the importance of examining policies that primarily focus on this four-year teacher-preparation pathway. Descriptive statistics for these outcome variables are presented in Table 2 below. Since all three outcome variables are count variables, I run supplemental Poisson models and find qualitatively similar findings. I present these estimates in Appendix C.

[Table 2]

Treatment Status

I supplement the publicly available data with an original dataset of loan forgiveness policy information gathered from all 50 states and D.C. beginning in 1977, the first year of any state-level policy adoption. This includes the year each state adopted and subsequently implemented a targeted teacher loan forgiveness policy, any eligibility requirements for teachers to receive forgiveness (e.g., work in a hard-to-staff school, certified in a high-needs subject area), how much of the original loan can be forgiven under the policy, and the date of any policy repeal.

To gather this information, I began with the Education Commission of the States (ECS) State Legislation Tracker for Teaching Quality: Recruitment and Retention (Education Commission of the States, 2022). This repository of state legislation provides information on teacher recruitment and retention policies adopted in all 50 states and D.C. between 1992 and 2022. I searched the provided legislation titles and descriptions on this tracker to identify any legislation with the term “loan.” I then read the descriptions to determine if the policy either created or amended policies related to teacher loan forgiveness programs. I isolated teacher loan forgiveness policies from other financial incentive policies by examining the cited policy documents and ultimately coded a state as adopting a loan forgiveness policy in a given year if it had any policy providing post-hoc forgiveness of accrued student loan debt in return for teaching service. In particular, this is different from scholarships and grants that a student might receive before accruing any student loan debt.

Because the ECS tracker only extends back to 1992, I next used the legislative search on LexisNexis to search for policies with the search phrase *((“loan" AND “forgive*") OR (“loan forgive*")) AND teacher* to identify any potentially relevant state codes and public laws not identified in my search of the ECS legislation tracker, and to validate the policies that were identified from ECS. As part of the LexisNexis legislation search, I also focused on two additional pieces of information: (1) whether or not any states adopted and repealed a policy before 1992, which would not have been shown in the ECS legislation tracker; and (2) four key policy design features (i.e., requirement to work in a hard-to-staff school, requirement to work in a high-needs subject area, total possible amount forgiven, and the number of years of teaching service required).

From the ECS tracker, I identified 28 states that adopted a teacher loan forgiveness policy between 1992 and 2022. I then confirmed these findings for the 28 states with the LexisNexis legislation search and identified an additional 10 states that adopted a teacher loan forgiveness policy before 1992. I did not find any states that both adopted and repealed a targeted teacher loan forgiveness policy before 1992, despite finding five states (i.e., Connecticut in 2013, Florida in 2011, Indiana in 2019, Missouri in 2012, and Wyoming in 2011) that repealed their loan forgiveness policies at some point between 1992 and 2022.

Information on the staggered rollout of policy adoption years can be found in Figure 2 below, and detailed policy design information can be found in Appendix A. It is worth noting that all states adopted these policies with the intention to implement them immediately (i.e., adopted in spring and implemented for next upcoming school year in the fall), though three states were delayed in their implementation. Louisiana adopted their policy in 1992 but implemented in the 1993-94 school year, New Jersey adopted in 2021 but implemented in the 2022-23 school year, and Pennsylvania adopted in 1988 but implemented in the 1989-90 school year. Louisiana and Pennsylvania are dropped from the analysis as they adopted before 1998 and New Jersey is considered "Never Treated", as they adopted after the end of my analytic panel, so none of these adoption/implementation misalignments should bias my findings. Drawing on my codes, my independent variable of interest is a binary indicator variable that takes a value of 1 in each adoption year and every year after, 0 for years before the adoption year, and 0 in all years for a state that never adopted. Ultimately, my process yielded 38 states adopting between 1977 and 2022, with 23 of those states adopting in my study period of 1998-99 through 2019-20 and 13 never having adopted. As mentioned above, I dropped 12 states because they adopted a policy before or during 1998.

[Figure 2]

Policy Feature Subgroups

To answer my third research question about differences by design feature, I draw on data on the amount of student loan debt that can be forgiven under a state policy and county-level unemployment rates. To construct the subgroups for the amount of student loan debt that can be forgiven, I draw from my state loan forgiveness policy dataset described above and from the College Scorecard, a dataset maintained by the U.S. Department of Education that contains institution-level information beyond what is captured in IPEDS (e.g., student financial aid information). I construct a proportion variable representing the maximum amount of loan debt that may be forgiven, divided by the median student loan debt for each state. For both the median student loan debt and the maximum amount of loan debt forgiven, I first convert all dollar amounts to 2020 dollars using the Annualized

Consumer Price Index for All Urban Consumers (CPI-U) provided by BLS. There is variation in how states report this maximum amount forgiven, so, for states that provide a single dollar amount (regardless of the number of years of teaching required), I use that value as the maximum amount forgiven. For states that provide a dollar amount for each year of teaching (e.g., $1,000 after the first year of teaching, $1,500 after the second year, and $2,000 after the third year), I use the summation of the individual years' dollar amounts. Finally, some states provide a percentage of student loan debt that may be forgiven. In these cases, I directly use that value for my proportion variable. Figure 3 below provides a histogram of the distribution of the proportion of loan debt forgiven, and, as shown, a large majority of observations forgive 100% of the median loan debt (i.e., 76.1%). Therefore, I create a binary variable that takes a value of 1 when an institution is in a state that forgives 100% of student loan debt, and 0 otherwise.

[Figure 3]

To construct the subgroups for the county-level unemployment rate, I first draw from the BLS Local Area Unemployment Statistics (LAUS) and assign the annual average county-level unemployment rate to each institution based on the fall year of the school year and the county in which that institution resides in. For example, Florida State University (FSU) resides in Leon County, so for the 2018-19 school year, FSU was assigned the county-level unemployment for Leon County in 2018. Then, I lag the unemployment rate values so they represent year *t-1*. Drawing on this unemployment rate variable, I create within-year terciles of the lagged unemployment rates where the first tercile represents the lowest third of unemployment rates, the second tercile represents the middle third of unemployment rates, and the third tercile represents the highest third of unemployment rates.

Covariates

To help control for factors that might bias the estimated effects of loan forgiveness on teacher supply, I also include institution-level covariates from IPEDS that might be associated with the likelihood of policy adoption and my outcomes. These covariates include a logged function of enrollment, proportions of students enrolled by race/ethnicity and gender, respectively, lagged county-level unemployment rates, and average in-state undergraduate tuition. To create these variables, I calculate the proportion of students in each subgroup as the IPEDS-provided count of students within each subgroup divided by the IPEDS-provided total enrollment. IPEDS provides the average in-state undergraduate tuition for a given year, so I use those values as provided. I then use the BLS LAUS-provided annual average county-level unemployment rates for year *t-1*.

Table 3 summarizes the covariates overall and then separately for states that adopted and never adopted. As shown in Table 3, the treated and never-treated groups are slightly different in terms of ethnoracial identities of all enrolled students, as the proportion of Black students in treated observations is about 12% and about 16% for never-treated observations. Additionally, states that adopted appear to have larger institutions. Never-treated institutions appear to be located in counties with slightly higher unemployment rates (i.e., 6%) compared with treated institutions (i.e., 5.4%).

[Table 3]

Finally, I draw on time-varying state-level economic and demographic characteristics that might contribute to the number of TPP degrees awarded, including total state population, the proportion of families with at least one bachelor's degree holder, race/ethnicity proportions, and the median private sector hourly wage. Comparing treated versus never-treated states (see Table 3), it appears that treated states have about 4 percentage points fewer Black children than never-treated states, which potentially explains the discrepancy in overall enrollment of Black students and the discrepancy in share of TPP degrees awarded to Black students. Further, treated states have, on average, larger populations than never-treated states and also have slightly lower median hourly wages for the private sector.

Analytic Strategy

To identify a plausibly causal effect of targeted teacher loan forgiveness policies, I use an event study and difference-in-differences framework leveraging the variation in timing of state policy adoption. I begin with event study models predicting each of the three outcomes (i.e., first-year enrollment, total enrollment, and TPP bachelor's degrees awarded), respectively. These models take the form:

$$Y_{ist} = \alpha_0 + \sum_{k=-16,\neq -1}^{16} \tau_k(t = t_s^* + k) + \theta \boldsymbol{X}'_{ist} + \sigma_i + \pi_t + \varepsilon_{ist} \tag{1}$$

where Y_{ist} represents the outcome for institution *i* in state *s* in year *t*. The term $\tau_k(t = t_s^* + k)$ represents a set of pre- and post-policy reform relative year indicators, centered at the year prior to policy adoption (i.e., *t-1*), with t_s^* representing the year in which state *s* adopted a loan forgiveness policy. $\boldsymbol{X}'_{ist}$ is a vector of institution-level covariates (i.e., a logged function of enrollment, proportions of Black, Hispanic, and American Indian, Asian, multi-racial, or Native Hawaiian students, proportion of male students, unemployment rates for the county in which the institution resides, and the mean in-state undergraduate tuition) and state-level covariates (i.e., state population, proportion of families with at least one college degree, proportion of children aged 5-17 by racial identity, and the median private-sector hourly wage). Each model also includes institution fixed effects (σ_i), year fixed effects (π_s), and an idiosyncratic error term clustered at the state level (ε_{ist}).

The coefficients of primary interest in Equation 1 are the τ_0 through τ_{16} estimates, which represent the estimated effects of a loan forgiveness policy in a given adoption year. These estimates can be interpreted as the difference in outcomes at universities in states that adopted a loan forgiveness policy relative to the omitted reference year, compared with outcomes at universities in states that did not adopt a policy. Thus, positive estimates would point to an average increase in the respective outcome for adopter states relative to non-adopter states in relative year *k*, while negative estimates would reflect an average decrease in adopter relative to non-adopter states in year *k*.

There are two core assumptions for this model to identify a causal effect of loan forgiveness on each of my outcomes. The first is that there are parallel trends prior to treatment, conditional on covariates. This assumption means, in this context, that the respective outcome pre-trend for eventually treated institutions is similar to the pre-trend in the not treated institutions, conditional on covariates. I include both never-treated and not-yet-treated observations in the comparison group. I test this assumption visually through the pre-reform estimates, which individually should not be significantly different from zero, and by calculating Wald tests of the linear combination of pre-reform years, where a statistically significant finding (i.e., statistically distinguishable from zero) would point toward a violation of the parallel trends assumption. The second assumption states that there is no anticipatory effect of the policy reform. For example, in my case, if institutions anticipate the adoption of a loan forgiveness policy, they may ramp up recruiting of students before the policy is adopted under the presumption that those students could receive the loan forgiveness benefit after it is passed. I will test this assumption by examining the event study pre-reform trends to determine if states may have anticipated a policy change, which would appear as a positive slope to the coefficients as time passes. Additionally, I will run Wald tests for the treated versus comparison groups for relative year $k=-1$, where a statistically significant result (i.e., distinguishable from zero) will indicate a potential violation of the no anticipation assumption.

Recent work examining the two-way fixed effects (TWFE) specification demonstrates that such models yield biased estimates when treatment timing is staggered and effects differ across adoption cohorts (Goodman-Bacon, 2018; Roth et al., 2023). Specifically, my estimates could be biased because the canonical TWFE specification includes already-treated “earlier adopters” in the comparison group for the “later adopters”. This is problematic in my context if an “earlier adopter” increases the effectiveness of their policy over time, for example, through more efficient messaging or increased funding in response to initial success. Therefore, Equation 1 could produce biased estimates and inferences. Recent literature has provided a litany of specifications that address this effect heterogeneity (Callaway & Sant'Anna, 2021; de Chaisemartin & D'Haultfoeuille, 2022; Gardner et al., 2024; Sun & Abraham, 2020; Wooldridge, 2021), so I will follow recent teacher research (e.g., Camp et al., 2025) and use the two-stage estimator due to its efficiency with large numbers of small cohorts (Gardner et al., 2024).[8]

The two-stage estimator, as the name suggests, splits Equation 1 into two separate stages. In the first stage, I regress each respective outcome on institution fixed effects, year fixed effects, and covariates for the untreated subsample. This stage takes the following form:

$$Y_{ist} = \theta \boldsymbol{X}'_{ist} + \sigma_i + \pi_t + \varepsilon_{ist} \tag{2}$$

To clarify, Equation 2 differs from Equation 1 in two important ways. First, I remove the relative-year treatment indicators from Equation 2, and second, I include *only* observations that are part of the comparison group (i.e., never-treated and not-yet-treated). Further, since there are no always-treated units in my sample, every institution has at least one year that is untreated and, therefore, at least one observation included in the first stage. Separating this first stage addresses potential contamination due to effect heterogeneity by preventing the "already treated" observations from being included in the comparison group.

The second stage begins by including all observations (i.e., treated and untreated) and constructing residuals from the estimates provided by Equation 2. Specifically, I subtract the average institution fixed effects ($\hat{\sigma}_i$) and average year fixed effects ($\hat{\pi}_t$) that were calculated in Equation 2 from *all* observations, as shown below:

$$Y^*_{ist} = Y_{ist} - (\theta \boldsymbol{X}'_{ist} + \hat{\sigma}_i + \hat{\pi}_t) \tag{3}$$

Finally, I regress these residualized outcomes on the treatment, using the following model:

[8] I use the Stata "did2s" package to perform this analysis

$$Y_{ist}^{*} = \sum_{k=-16,\neq -1}^{16} \tau_k (t = t_s^{*} + k) + \varepsilon_{ist} \tag{4}$$

As in Equation 1, the coefficients of primary interest in Equation 4 are the τ_0 through τ_{16} estimates, which still represent the estimated effects of a loan forgiveness policy in a given adoption year. Though my main analysis uses the two-stage specification, I also run supplemental models with the Sun and Abraham (2020) specification and find qualitatively similar results, which can be found in Appendix D.

I supplement my event study specification with a difference-in-differences model to increase power by pooling my pre- and post-reform estimates, respectively. In the two-stage specification, I only adapt Equation 4 to shift to a difference-in-differences framework. Specifically, I replace the relative year treatment indicators with a dichotomous treatment indicator. This model takes the following form:

$$Y_{ist}^{*} = \beta_1 LF\ Policy_{ist} + \varepsilon_{ist} \tag{5}$$

where LF Policy represents a binary indicator that takes a value of 1 for all treated observations in post-reform years and zero otherwise. The coefficient of interest here is β_1, the average effect of the teacher loan forgiveness policy across all post-period years in my panel.

Influence from Federal Loan Forgiveness Policies

While not a formal research question, I also examine the possible influence of changes in federal policy throughout my panel. As the federal policy landscape is evolving in the background (see Table 1 for an overview), it may be that these changes to the federal landscape influence the effects of the state-level policies. Specifically, the federal policies impact all states in my sample, including the never-treated comparison group, so there is reason to believe that this may cause an increase in enrollment in and graduation from teacher preparation programs for those never-treated states and therefore attenuate the impact of state-level policies, leading to a deflated estimate.

Alternatively, the combination of the federal policies and the state policies may have an additive effect and accentuate the impact of state-level policies, leading to an inflated estimate. Either way, it is necessary to identify any possible influences that the federal policy landscape may have on the effects of the state-level loan forgiveness policies. Therefore, I run multiple models for each outcome where I allow for flexible effects based on adoption timing; specifically, I estimate separate effects for early adopters (i.e., states that adopt before a respective federal policy change) vs. late adopters (i.e., state that adopt after a respective federal policy change). I adapt Equation 6 and replace the *Less than 100%* and *100%* indicators with an indicator for Early Adopters and an indicator for Late Adopters. I do this for two time period cutoffs: the Taxpayer-Teacher Protection Act in 2004, where those states that adopted a state-level teacher loan forgiveness policy before 2004 are considered early adopters, and those states that adopted during or after 2004 are considered late adopters; and the Public Service Loan Forgiveness program in 2007.

Heterogeneity Analyses

To answer my final research question about system design features, I will extend my difference-in-differences models (i.e., Equation 5) to allow for variation in effects by policy design and by local economic conditions (Bleiberg et al., 2024). Here, I examine the heterogeneous effects across the amount of student loan debt forgiven, operationalized as either 100% of debt forgiven or less than 100% of debt forgiven, as described above. Additionally, I examine the variation of effects across county-level unemployment rates,[9] operationalized as terciles within a given year. Table 4 below displays descriptive statistics for all covariates across values of the amount forgiven indicators and the unemployment rate terciles, respectively.

[Table 4]

To evaluate the heterogeneous effects, I run two different models for each outcome – one for

[9] Unemployment rates are measured in year *t-1*

the levels of amount of student loan debt forgiven (i.e., 100% or not) and one for terciles of unemployment rates. Below, I present Equation 6, which describes the model for the amount of student loan debt forgiven.

$$Y^*_{ist} = \beta_1(LF\ Policy_{ist} \times Less\ than\ Full_{ist-1}) + \beta_2(LF\ Policy_{ist} \times Full_{ist-1}) + \varepsilon_{ist} \qquad (6)$$

Here, β_1 represents the effect for institutions in states that grant less than 100% debt forgiveness (i.e., the maximum dollar amount forgiven by the state divided by the median student loan debt for institution i) and β_2 represents the effect for institutions in states that grant 100% of debt forgiveness. Both of these indicators are measured in the baseline year (i.e., t-1). As the dataset is at the institution-year level and I include institution fixed effects in my model, there is no main effect coefficient so these estimates should be interpreted as relative to non-adopters. I then repeat Equation 6 for unemployment rate but replace the *Less than Full* and *Full* indicators with unemployment rate tercile indicators and therefore have three coefficients of interest (i.e., β_3).

Robustness Checks

After my main analysis, I run multiple robustness checks to test the sensitivity of my analysis to federal policy changes, omitted variable bias, unemployment changes over time, and states repealing loan forgiveness policies. I detail all of these checks below.

As states seek to improve their teacher workforce and implement policies and programs aimed at the teacher pipeline, some states are likely to adopt additional policies, in addition to loan forgiveness, that might influence the number of individuals entering and persisting in the teacher pipeline. Examples of such policies include scholarships and grants, teacher evaluation reform, elimination of teacher tenure, weakening of collective bargaining protections, and increasing teacher probationary periods. In such cases, my estimates might be confounded by the effects of these other policies (Bleiberg et al., 2024; Howell & Magazinnik, 2017; Kraft et al., 2020). Specifically, if a state adopts both a loan forgiveness policy and, for example, a teacher scholarship program and both are related to an increase in teacher preparation program enrollment and completion, my estimates would be biased upward, and it may become difficult to disentangle the effects of the separate policies. Therefore, I will draw from Kraft and colleagues (2020) to run additional models that aim to control for these other policies that may have affected education program enrollment and teacher preparation program degrees at the same time. Findings from these analyses are consistent with my main analyses, and results can be found in Appendix E. It is worth noting that this robustness check is not a comprehensive analysis of all possible teacher policies, but a grouping of policies that have been shown to be related to teacher supply (Bleiberg et al., 2024; Howell & Magazinnik, 2017; Kraft et al., 2020).

I also run a placebo analysis with alternative outcomes to check for evidence of omitted variable bias. I run this analysis on two separate variables: 1) in-state undergraduate tuition and 2) the count of nursing bachelor's degrees awarded. Findings from these analyses show that treatment timing for state-level teacher loan forgiveness policy adoption is not related to changes in in-state undergraduate tuition or to the number of nursing bachelor's degrees awarded. Estimates from these analyses can be found in Appendix F.

I then run a sensitivity check of my policy feature subgroup analyses on unemployment rates. In my main models, I measure unemployment rates in year *t-1*, so, in these sensitivity analyses, I run additional models across all three outcomes where I measure unemployment rates in years *t-4*, *t-3*, *t-2*, and *t*, respectively, in order to rule out any short term shocks in a given year that may have prompted states to adopt a policy and also increase the number of individuals entering the teacher pipeline. Findings from these analyses are consistent with my main analyses, and results can be found in Appendix G.

Finally, I check the sensitivity of my analysis to the inclusion of states that repeal their loan forgiveness policy within the timeline of my panel. In total, there are three states included in my sample that repealed their teacher loan forgiveness policy (i.e., Indiana, Missouri, and Wyoming). In my main analyses, I code the institutions in these states as treated throughout the duration of the panel, despite the policies having been repealed. Therefore, I run a final robustness check where I remove all observations from these three states. I present estimates from this check in Appendix H and show that findings are very similar to my main analysis.

Findings

Enrollment

Figure 4 presents the event study estimates for my first research question where I examine the effect of teacher loan forgiveness policies on first-year education program enrollment (Panel A) and total education program enrollment (Panel B).[10] In each case, the estimates should be interpreted as the estimated change in the number of enrollees within an institution for a given year associated with state-level LF adoption. Concerning the parallel trends and no anticipation assumptions for an event study and difference-in-difference, I find no evidence that the analysis of first-year enrollment or total enrollment violates either assumption.[11]

[Figure 4]

The event study estimates indicate a moderate increase in the number of first-year undergraduate education program enrollees, but, on average, no effect on total undergraduate education program enrollees. As shown in Panel A of Figure 4, despite a descriptive increase beginning in year *t+2*, statistically significant effects on first-year enrollees are delayed until year *t+7*. These effects steadily climb to a maximum of 15.1 in year *t+15*.

Table 5 provides estimates from my difference-in-differences models. Here, estimates should also be interpreted as the change in enrollees, but on average across all post-treatment time

[10] Tables with event study estimates can be found in Appendix I

[11] Wald tests show that the pretreatment estimates are not jointly different from zero, providing additional evidence in support of the CPT assumption

periods. My findings in Table 5 confirm a positive overall effect of loan forgiveness policies on first-year undergraduate education program enrollment, showing an average increase of about 6.3 enrollees for institution *i* in year *t* — equivalent to about a 12% increase in first-year enrollment, on average.

[Table 5]

Delay of Effects in First-Year Enrollment

As mentioned above, I observe a delay in effects for first year enrollment. A possible explanation for this delayed effect is that the changing federal landscape (see Table 1) led to differential effects before and after key federal loan-forgiveness policies. Federal policies and initiatives were adopted and modified throughout the duration of my panel. Specifically, the Taxpayer-Teacher Protection Act (TTPA) in 2004 modified the existing Teacher Loan Forgiveness program, and in 2007, the federal government implemented the Public Service Loan Forgiveness (PSLF) program. As discussed above, these federal policy changes may have influenced the effects of state-level policy adoption by either increasing the number of individuals entering the teacher pipeline in never-treated states, thereby attenuating state-level effects, or having an additive effect in combination with state-level policies, thereby inflating their effects. Specifically, if either federal policy had an attenuating effect, we would expect immediate effects among early adopters but not among late adopters. If there were an additive effect, we would expect immediate effects for late adopters but not for early adopters.

In order to disentangle any influences of federal policy changes – whether they be positive or negative – from state-level policy adoption effects, I provide event study estimates that separate early-adopters from late-adopters for both the adoption of the TTPA (i.e., early adopters adopt prior to 2004 and late adopters adopt in 2004 or beyond) and of the PSLF program (i.e., early adopters adopt prior to 2007 and late adopters adopt in 2007 or beyond), respectively. Findings for TTPA models are shown in Figure 6 and PSLF models are shown in Figure 7.

[Figure 6]

[Figure 7]

For both the TTPA and PSLF analyses, I find no differential trends or effects in first-year enrollment or in total enrollment between early and late adopters (see Panels A and B in Figure 6 and Figure 7). Given that the findings show a delay in both early and late adopters, these findings suggest that there appears to be no influence of the TTPA or PSLF adoption, respectively, on the effects of state-level teacher loan forgiveness policies on education program enrollment. However, I find that in both TTPA and PSLF analyses, late adopters experience a larger effect than early adopters for the number of Bachelor's degrees awarded (see Panel C of Figure 6 and Figure 7). Therefore, these findings suggest a possible additive effect of state-level and federal-level loan forgiveness policies, with respect to the number of teacher preparation Bachelor's degrees awarded.

Since the changing federal policy background does not appear to explain the delay in effects, I now briefly turn to a more theoretical explanation of this finding. While I am unable to empirically test these mechanisms because my dataset is at the institution level and not the individual level, the delay in effects could potentially stem from two possible mechanisms. First, there could be a lack of initial awareness of the policy due to administrative limitations. The barriers to loan forgiveness are well established in the literature (Clotfelter et al., 2008; Jacob et al., 2024), so it is likely that these barriers could be limiting the initial uptake. However, the longer the policy exists, the more likely it becomes that individuals become aware of and are influenced by it. Second, individuals may be initially aware of the policy but feel a sense of risk or ambiguity about how those policies may benefit them (Fox & Tversky, 1995; Kahneman & Tversky, 1979), and therefore they may avoid the "riskier" choice of a teaching profession. However, once the policy becomes more established, the risk and ambiguity decrease, leading to the delayed effects.

Bachelor's Degrees

Figure 5 presents the event study estimates for my second research question, where I examine the effect of teacher loan forgiveness policies on the number of teacher preparation program (TPP) bachelor's degrees awarded. The estimate should be interpreted as the estimated change in TPP bachelor's degrees within an institution for a given year associated with state-level LF adoption. Regarding the parallel trends and no anticipation assumptions for an event study and a difference-in-differences analysis, I find no evidence that the analysis of TPP bachelor's degrees violates either assumption.[12] Event study estimates in Figure 5 show no effect of loan forgiveness policies on the number of bachelor's degrees awarded. Further, when the relative year indicators are pooled together in the difference-in-difference analysis, I find no overall average effect of loan forgiveness policies on bachelor's degrees awarded, as shown in Table 5.

[Figure 5]

Design Heterogeneity

As discussed above, I find an overall positive effect of student loan forgiveness policies on first-year enrollment, and I find overall null effects for total enrollment and for TPP bachelor's degrees. That said, many of these state policies differ in their design, which may reveal heterogeneous effects. In particular, the amount of student loan debt that can be forgiven varies across states and may differentially impact teacher supply (see Table 4 and Figure 3), so I allow for heterogeneous effects based on the amount of student loan debt forgiven (i.e., less than 100% forgiveness compared with 100% forgiveness). I also test for heterogeneity across county-level unemployment rates,[13] operationalized as terciles representing the lowest, middle, and highest unemployment rates.

[12] Wald tests show that the pretreatment estimates are not jointly different from zero, providing additional evidence in support of the CPT assumption

[13] County-level unemployment rates are measured in year *t-1*

Amount of Student Loan Debt Forgiven

Table 6 presents the difference-in-differences results from Equation 6, allowing for flexible effects depending on whether the state allows 100% student loan debt forgiveness or not. Column 1 displays the estimates for first-year enrollment, Column 2 displays the estimates for total enrollment, and Column 3 displays the estimates for TPP Bachelor's degrees. I find that loan forgiveness policies that forgive less than 100% of student loan debt increase first-year enrollment by about 6.4 students for institution *i* in year *t*, on average. However, this estimate is not statistically different from zero at any conventional level. I find that loan forgiveness policies that forgive 100% of student loan debt increase first-year enrollment by about 6.2 students for institution *i* in year *t*, on average (i.e., equivalent to a 12% increase in first-year enrollment). Although the estimate for institutions in states that forgive 100% of student loan debt is significantly different from zero, it is not significantly different from the less than 100% forgiveness estimate ($\chi^2=0.001$), suggesting that the amount of student loan debt forgiven does not differentially impact first-year enrollment.

[Table 6]

Column 2 of Table 6 shows the results for the subgroup analysis of total enrollment in education programs. I find that loan forgiveness policies that forgive less than 100% of student loan debt descriptively decrease total enrollment by about 16.7 students for institution *i* in year *t*, on average. I find that loan forgiveness policies that forgive 100% of student loan debt increase total enrollment by about 15.6 students for institution *i* in year *t*, on average (i.e., equivalent to a 4.4% increase in total enrollment).

Lastly, column 3 of Table 6 displays the estimates for the effect on TPP bachelor's degrees. Here, I find that loan forgiveness policy adoption that forgives less than 100% of student loan debt does not affect the number of TPP bachelor's degrees awarded, but these policies do increase the number of TPP bachelor's degrees awarded when 100% of student debt may be forgiven.

Specifically, when a state adopts a loan forgiveness policy that allows for 100% forgiveness, institutions in that state produce 8.2 additional bachelor's degrees (i.e., equivalent to about a 9.6% increase). Not only does this estimate significantly differ from zero, but it also significantly differs from the less than 100% estimate ($\chi^2=8.294$), suggesting that the amount of loan debt that can be forgiven has a meaningful influence on the effectiveness of these policies.

Unemployment Rates

Table 7 presents the difference-in-differences results from Equation 6, allowing for flexible effects by tercile of lagged county-level unemployment rates. Column 1 displays the estimates for first-year enrollment, Column 2 displays the estimates for total enrollment, and Column 3 displays the estimates for TPP Bachelor's degrees.

[Table 7]

Column 1 in Table 7 shows an increase in effect size as the lagged unemployment rates increase. In particular, I find an increase of about 4.9 first-year enrollees for the lower tercile of unemployment rates and a descriptive increase of about 4.7 first-year enrollees for the middle tercile. Finally, I find a larger effect for those institutions in the highest tercile for unemployment rates, in that the adoption of loan forgiveness policies increases the number of first-year enrollees by about 10.0 students per institution per year, on average. Wald tests on these estimates show that none of these estimates are significantly different from each other (i.e., low-mid: $\chi^2=0.001$; mid-high: $\chi^2=1.043$; low-high: $\chi^2=0.742$), despite the positive trend with increasing unemployment rates, and that the highest terciles is significantly different from zero. Columns 2 and 3 show that there is no differential effect across unemployment rate terciles of loan forgiveness policies on total enrollment but that there is a marginally significant positive effect on bachelor's degrees.

Discussion

Using publicly available IPEDS data and an event study and difference-in-differences analysis, I show that the adoption of student loan forgiveness policies targeted at teachers has small to moderate effects on teacher supply. Specifically, I find that the adoption of such policies increases the average number of first-year education program enrollees by about 6.3 students per institution per year, which is equivalent to about a 12% increase in first-year enrollment, on average. I also find that such policies have no overall effect on either total enrollment or on the number of teacher preparation program bachelor's degrees awarded. However, I do find evidence of an interaction between these state loan forgiveness policies and federal loan forgiveness policies that occur contemporaneously, specifically for the number of bachelor's degrees awarded, which suggests a possible additive effect of the two policies.

To contextualize the effects on the teacher pipeline, I conduct a quick "back-of-the-envelope" calculation for four states (i.e., Florida, Kansas, Maryland, and Pennsylvania). To start, I use a rough estimate of the national average for teacher attrition (i.e., 8%) (Carver-Thomas & Darling-Hammond, 2017) to find the number of teachers (on average) who leave the teaching profession for each state. In 2019-20, Florida had 166,002 teachers, Kansas had 36,603 teachers, Maryland had 61,484 teachers, and Pennsylvania had 124,294 (National Center for Education Statistics, 2020). Using the national average attrition to calculate the number of teachers who left each state by the end of the 2019-20 school year, Florida lost 13,280 teachers, Kansas lost 2,928 teachers, Maryland lost 4,919 teachers, and Pennsylvania lost 9,943 teachers. Then, using the first-year enrollment estimate[14] and the number of institutions that grant teacher preparation degrees, I calculate how many new individuals, on average, will enter the teacher pipeline each year *due to the state-level loan forgiveness policy adoption*. Florida would add 490 new first-year enrollees, Kansas would add 183 new first-year enrollees, Maryland would add 141 new first-year enrollees, and

[14] I choose first-year enrollment for this calculation due to the interaction of federal policies on the number of bachelor's degrees awarded

Pennsylvania would add 714 new first-year enrollees. Finally, I calculate the ratio of the number of new first-year enrollees added by the adoption of the loan forgiveness policy to the number of teachers who left the profession. This ratio should be interpreted as the percentage-point reduction in the gap between individuals entering the teacher pipeline and those leaving the profession caused by the adoption of the loan forgiveness policy. Florida would see a reduction in the supply/attrition gap of 3.7%; Kansas, 6.3%; Maryland, 2.9%; and Pennsylvania, 7.2%. Of course, for these programs to fill in even these modest gaps, institutions and states would need to retain students in teacher preparation programs from initial enrollment through graduation, so these rough calculations should be viewed as the ceiling for supply/attrition gap reduction.

I also find that the overall effects on TPP Bachelor's degrees awarded may be masked by differences in the amount of total student loan debt that can be forgiven. When states forgive 100% of a student's loan debt, the number of TPP bachelor's degrees awarded increases by about 8.2 students per institution per year (i.e. equivalent to about a 10% increase), but, when a state does not forgive 100% of student loan debt, there is no overall effect on the number of TPP bachelor's degrees awarded. The amount of debt forgiven does not appear to differentially impact the number of first-year enrollees, suggesting that those students making decisions so early in the teacher pipeline might be influenced by the idea of loan forgiveness without considering the actual amount of such forgiveness.

Regarding the influence of unemployment rates on the impact of loan forgiveness policies and teacher supply, I find that as unemployment rates increase, the number of first-year education program enrollees also increases (however, only descriptively). This finding aligns with prior work on the research between selection into teaching and unemployment rates (Goldhaber & Theobald, 2022; Rucinski, 2023) in that, as unemployment rates increase, individuals tend to select into teaching at greater rates.

There are some limitations to this study. First, this study uses institution-level data instead of student-level data. Future research would benefit from student-level data in three main ways: 1) ability to track students throughout their post-secondary schooling and identify entry-points to and exit-points from the teacher pipeline, 2) a more granular measure of the amount of student loan debt that each student had and the amount of loan debt that could be forgiven, and 3) matching student post-secondary records to teacher workforce outcomes. A particular caveat here is that my outcome measures are institution-level counts, while the policy operates through individual decisions about whether to enter teaching, which institution to attend, and how much debt to take on. Unfortunately, this analysis is not able to fully evaluate each of these areas comprehensively so these findings about the policy effects should be interpreted with this in mind. An increase in enrollment or degrees awarded due to the loan forgiveness policy can be attributed to either a true new entry to the teacher pipeline or to a student switching programs or institutions. To the extent that these increases reflect student switching majors or relocating institutions rather than a true new entrant, my estimates would overstate the loan forgiveness policy's true effect on state teacher supply. Related, because higher levels of forgiveness reduces the cost of more expensive teacher preparation programs, part of the larger effect I observe for institutions in states that forgive 100% of debt may reflect students shifting toward costlier programs rather than a larger behavioral response to the incentive itself. This raises the cost of these policies without a proportional gain in teacher supply. To truly disentangle these mechanisms, future work using student-level data is needed. Second, IPEDS only provides two-digit CIP codes for their enrollment data, so this study uses a proxy measure of enrollment in an Education program instead of enrollment in a teacher preparation program. That said, this should be a fairly decent proxy as the number of education program Bachelor's degrees awarded is made up almost entirely of teacher preparation program Bachelor's degrees.[15]

[15] Author's calculation based on IPEDS degree completions data

Conclusion

While this is not the first study to examine the effects of loan forgiveness on teacher mobility (Feng & Sass, 2018; Fulbeck, 2014; Jacob et al., 2024; Steele et al., 2010), this study uniquely contributes the first plausibly causal effects of state-level loan forgiveness policies on teacher supply. In particular, this study attempts to contribute to our understanding of the efficacy of financial incentive policies originating from different levels of government. As Jacob and colleagues (2024) find, the federal loan forgiveness program appears to have severe administrative burdens that limit the potential for this program to address teacher staffing challenges successfully. Conversely, locally driven policies, such as those investigated by Fulbeck (2014) and Morgan and colleagues (2023), appear to have meaningful impacts on teacher staffing challenges. This study adds to the existing work on how state-level policies might positively impact teacher staffing challenges by showing that they have the potential to bolster teacher supply. However, these are not necessarily "silver bullet" solutions, as there is a reliance on large amounts of funding to forgive student loan debt for teachers. Additionally, even when a state forgives 100% of a teacher's student loan debt, the impact on teacher supply is modest. The impact of these policies is further attenuated by who is actually receiving the loan forgiveness. While no longitudinal systematic dataset exists on individuals who receive this forgiveness, there are two groups who would be receiving these funds: 1) individuals induced by the loan forgiveness to enter the teaching profession who wouldn't have otherwise and 2) individuals who were always going to enter the teaching profession but are taking advantage of this loan forgiveness policy. Therefore, a question remains about the *cost per induced teacher* to the state.

School districts across the country are struggling to recruit and retain effective teachers, so they are implementing a litany of policies and programs to attenuate these struggles (Aragon, 2018). Targeted loan forgiveness should be considered one component of a state's policy infrastructure aimed at strengthening the teacher pipeline, alongside other policies that address multiple checkpoints. However, loan forgiveness policies should be designed purposefully not only to meet the state's needs but also to weigh the potential benefits of expanding loan forgiveness to more individuals against the risk of subsidizing recipients who would have entered the teaching profession regardless.

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Tables

Table 1. Federal loan forgiveness policy landscape

Policy	Adoption Year	Eligibility Criteria	Amount Forgiven
Teacher Loan Forgiveness (TLF)	1998	Work for five years in a school with greater than 30% low-income students	$5,000
Taxpayer-Teacher Protection Act	2004	Extension of TLF to include teachers in high-needs subjects (math, science, special education)	$17,500
Public Service Loan Forgiveness (PSLF)	2007	Any public sector employee who has made 120 payments toward loan debt	100%

Table 2. Outcome variables

	Analytic Sample			Early adopters (excluded)	Source
	Overall	Never treated	Ever Treated		
First-Year Education Program Enrollment	52.8 (81.6)	56.9 (91.6)	50.3 (74.7)	43.2 (64.6)	IPEDS: Fall Enrollment
Total Education Program Enrollment	355.7 (581.0)	388.0 (700.8)	335.8 (492.5)	253.9 (401.9)	IPEDS: Fall Enrollment
Teacher Preparation Program Bachelor's Degrees Awarded	85.4 (130.2)	91.8 (140.7)	81.5 (123.1)	62.2 (107.4)	IPEDS: Completions
Observations	18,864	7,169	11,695	8,593	

Note: Column 1 displays estimates for the entire analytic sample, Column 2 displays estimates for all observations in the Never Treated group, Column 3 displays estimates for all observations in the Ever Treated group, and Column 4 displays estimates for always treated units that have been dropped from the analysis. The analytic sample includes 917 unique institutions across 39 states. I used the following CIP codes to identify teacher preparation degrees (Kraft et al., 2020): 130101, 130201-130299, 130301, 131001-131099, 131201-131299, 131301-131399, 131401-131499, and 139999.

Table 3. Covariate descriptive statistics

	Analytic Sample			Early adopters (excluded)	Source
	Overall	Never Treated	Ever Treated		
Panel A. Institution Covariates					
Enrollment	5501.0	5787.0	5325.8	6168.8	IPEDS: Fall
	(7302.4)	(7638.0)	(7083.5)	(8043.4)	Enrollment
Black Students	0.139	0.165	0.124	0.139	IPEDS: Fall
	(0.209)	(0.235)	(0.190)	(0.209)	Enrollment
Hispanic Students	0.057	0.049	0.062	0.112	IPEDS: Fall
	(0.079)	(0.060)	(0.088)	(0.143)	Enrollment
American Indian, Asian, Native	0.138	0.136	0.140	0.158	IPEDS: Fall
Hawaiian, or Multi-Racial Students	(0.112)	(0.104)	(0.116)	(0.126)	Enrollment
White Students	0.665	0.650	0.675	0.589	IPEDS: Fall
	(0.233)	(0.240)	(0.229)	(0.250)	Enrollment
Male Students	0.414	0.413	0.414	0.406	IPEDS:
	(0.103)	(0.105)	(0.102)	(0.114)	Enrollment
In-State Tuition (2020$)	18895.28	18646.76	19047.62	20509.46	IPEDS:
	(12025.03)	(11834.67)	(12138.23)	(13727.50)	Finance
Unemployment Rate	5.583	5.889	5.395	5.673	BLS: LAUS
	(2.276)	(2.438)	(2.149)	(2.247)	
Observations	18,864	7,169	11,695	8,593	
Panel B. State Covariates					
State Population (millions)	4.806	4.715	4.873	9.587	Census
	(3.892)	(3.725)	(4.015)	(11.072)	
Families with at least a Bachelor's	0.364	0.379	0.353	0.380	CPS
	(0.081)	(0.086)	(0.075)	(0.069)	
White Children Between 5 and 17	0.655	0.645	0.663	0.619	CPS
	(0.190)	(0.185)	(0.193)	(0.186)	
Black Children Between 5 and 17	0.142	0.173	0.119	0.157	CPS
	(0.145)	(0.188)	(0.095)	(0.119)	
Hispanic Children Between 5 and 17	0.125	0.122	0.127	0.156	CPS
	(0.113)	(0.086)	(0.130)	(0.155)	
Children of American Indian, Asian,	0.078	0.060	0.091	0.068	CPS
Native Hawaiian, or Multi-Racial	(0.111)	(0.038)	(0.142)	(0.079)	
Identities Between 5 and 17					
Median Hourly Wage (2020 Dollars)	17.49	17.87	17.20	18.13	CPS
	(1.94)	(2.24)	(1.61)	(1.85)	
Median Student Loan Debt	9528.55	9821.00	9309.53	10107.84	College
	(1981.39)	(2010.51)	(1932.62)	(2008.11)	Scorecard
100% Forgiveness of Student Loan	0.555	0.188	0.830	0.640	Author's
Debt	(0.497)	(0.391)	(0.376)	(0.481)	Calculations
Observations	822	352	470	264	

Note: Column 1 displays estimates for the entire analytic sample, Column 2 displays estimates for all observations in the Never Treated group, Column 3 displays estimates for all observations in the Treated group, and Column 4 displays estimates for always treated units that have been dropped from the analysis. The analytic sample includes 917 unique institutions across 39 states. Average Teacher Salary was gathered primarily from the ACS survey, but was gathered from the NCES Digest of Education Statistics for school years 1998-99 and 1999-00. BLS: LAUS = Bureau of Labor Statistics Local Area Unemployment Statistics; CPS= Current Population Survey; NCES CCD= Common Core of Data; ACS=American Community Survey.

Table 4. Descriptive statistics across policy feature

	Ever Treated	Amount Forgiven		Unemployment Rates		
		Less Than Full	Full	Low	Mid	High
Panel A. Institution-Level Covariates						
Enrollment	5403.3	4811.7	5585.0	5016.3	6812.3	4413.6
	(7719.2)	(5554.0)	(8268.2)	(6109.3)	(10749.0)	(5217.7)
Black Students	0.123	0.087	0.134	0.088	0.127	0.167
	(0.191)	(0.114)	(0.207)	(0.160)	(0.182)	(0.227)
Hispanic Students	0.063	0.037	0.071	0.044	0.088	0.063
	(0.093)	(0.037)	(0.103)	(0.074)	(0.115)	(0.081)
Other Students of Color	0.133	0.125	0.136	0.115	0.152	0.138
	(0.115)	(0.100)	(0.119)	(0.087)	(0.130)	(0.128)
Male Students	0.415	0.415	0.416	0.419	0.422	0.403
	(0.101)	(0.110)	(0.099)	(0.105)	(0.095)	(0.102)
In-State Tuition (2020$)	18717.77	17485.19	19096.38	17041.23	19704.42	19974.09
	(12686.350	(11385.32)	(13048.57)	(11574.34)	(13790.50)	(12731.61)
Median Student Loan Debt (2020$)	10074.73	9291.83	10315.22	9769.70	10447.20	10094.28
	(2131.96)	(2027.15)	(2107.62)	(1877.59)	(2098.50)	(2426.65)
Unemployment Rate (*t-1*)	5.442	4.685	5.629	4.074	5.359	6.874
	(1.961)	(1.032)	(2.088)	(1.558)	(1.501)	(1.756)
Full Student Loan Debt Forgiveness	0.765	0.000	1.000	0.665	0.868	0.792
	(0.424)	(0.000)	(0.000)	(0.473)	(0.339)	(0.407)
Observations						
Panel B. State-Level Covariates						
State Population (millions)	7.51	7.66	7.46	5.27	9.90	8.02
	(5.73)	(3.71)	(6.22)	(3.85)	(6.93)	(5.32)
Families with at least a Bachelor's	0.360	0.350	0.363	0.339	0.387	0.359
	(0.081)	(0.052)	(0.088)	(0.076)	(0.087)	(0.072)
Black Children Between 5 and 17	0.149	0.148	0.150	0.131	0.147	0.177
	(0.082)	(0.054)	(0.089)	(0.082)	(0.074)	(0.083)
Hispanic Children Between 5 and 17	0.111	0.076	0.122	0.073	0.157	0.114
	(0.107)	(0.054)	(0.116)	(0.095)	(0.120)	(0.086)
AI/AS/NH/MR Children Between 5 and 17	0.063	0.056	0.066	0.053	0.067	0.073
	(0.081)	(0.042)	(0.090)	(0.044)	(0.039)	(0.136)
Median Hourly Wage of Private Sector	17.408	17.703	17.317	16.932	17.893	17.543
	(1.696)	(1.729)	(1.677)	(1.703)	(1.643)	(1.574)
Avg. Teacher Salary	49,268.48	49,116.46	49,313.49	47,587.84	50,157.77	49,401.60
	(7,212.39)	(3,094.65)	(8,039.56)	(6,855.70)	(8,306.26)	(6,239.92)
Observations	583	137	446	233	182	168

Note: Estimates are presented as means with standard deviations in parentheses. All columns are measured in the baseline year (i.e., one year prior to policy adoption).

Table 5. Difference-in-differences estimates for the effects of loan forgiveness policy adoption

	First Year Enrollment	Total Enrollment	Bachelor's Degrees
LF Policy	6.280*	4.636	2.011
	(2.644)	(12.771)	(3.763)
Observations	18,864	18,864	18,864

Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. All models include institution and year fixed effects.

*+ p<.10, * p<.05, ** p<.01, *** p<.001*

Table 6. Heterogeneity analysis across amount of student loan debt forgiven

	First Year Enrollment	Total Enrollment	Bachelor's Degrees
LF Policy x Less than 100% Forgiveness	6.382	-16.668	-9.917+
	(5.542)	(24.901)	(5.918)
LF Policy x 100% Forgiveness	6.228**	15.603	8.150***
	(2.373)	(17.945)	(2.305)
Observations	18,864	18,864	18,864
Wald test of difference between coefficients	0.001	0.802	8.294 **

Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. The amount of forgiveness level was measured at year *t-1*. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. All models include institution and year fixed effects.
*+ p<.10, * p<.05, ** p<.01, *** p<.001*

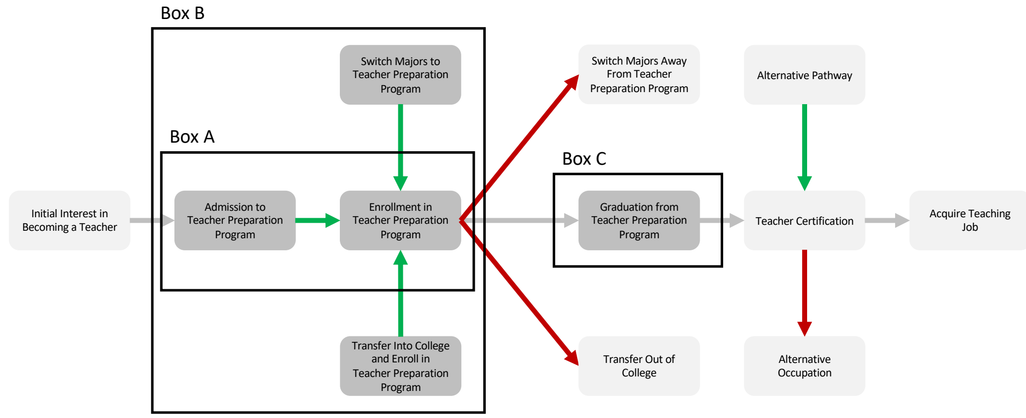
Table 7. Heterogeneity analysis across county-level unemployment rates (*t-1*)

	First Year Enrollment	Total Enrollment	Bachelor's Degrees
LF Policy x Low UR	4.924*	-0.525	1.604
	(2.157)	(14.225)	(3.086)
LF Policy x Mid UR	4.739	4.340	-1.256
	(5.739)	(32.784)	(10.077)
LF Policy x High UR	10.019*	13.986	5.587+
	(4.667)	(14.572)	(3.176)
Observations	18,864	18,864	18,864
Wald test of difference between coefficients			
Low vs. Mid	0.001	0.027	0.096
Mid vs. High	1.043	0.522	1.410
Low vs. High	0.742	0.073	0.590

Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. The unemployment rates were measured at year *t-1*. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. All models include institution and year fixed effects.
*+ p<.10, * p<.05, ** p<.01, *** p<.001*

Figures

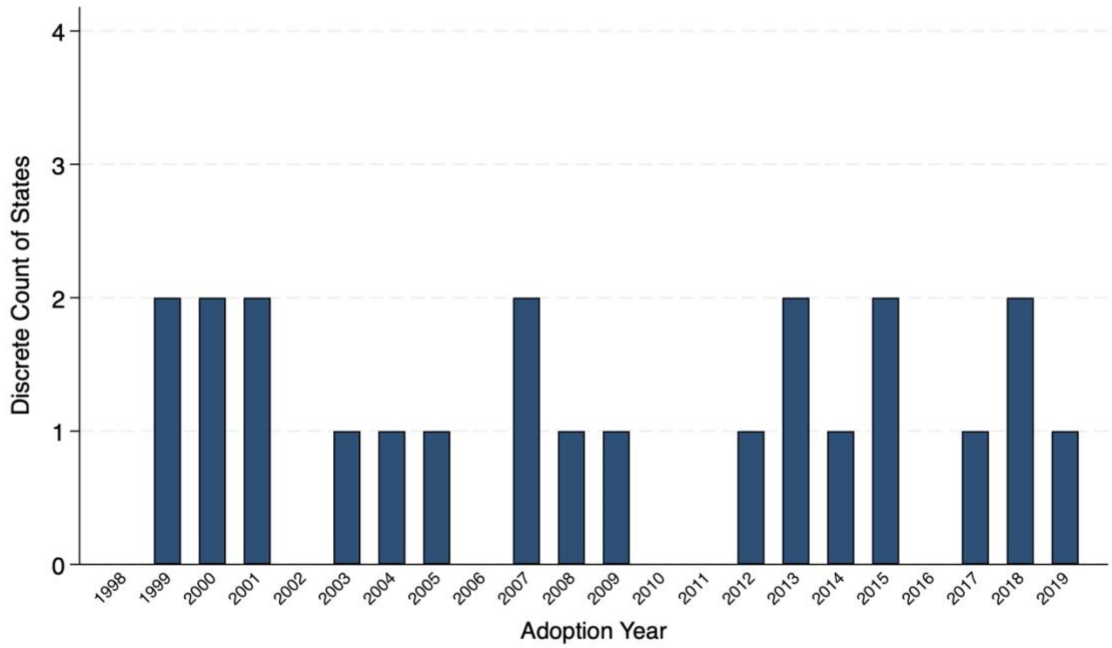
Figure 1. Pathway for Traditional Teacher Pipeline



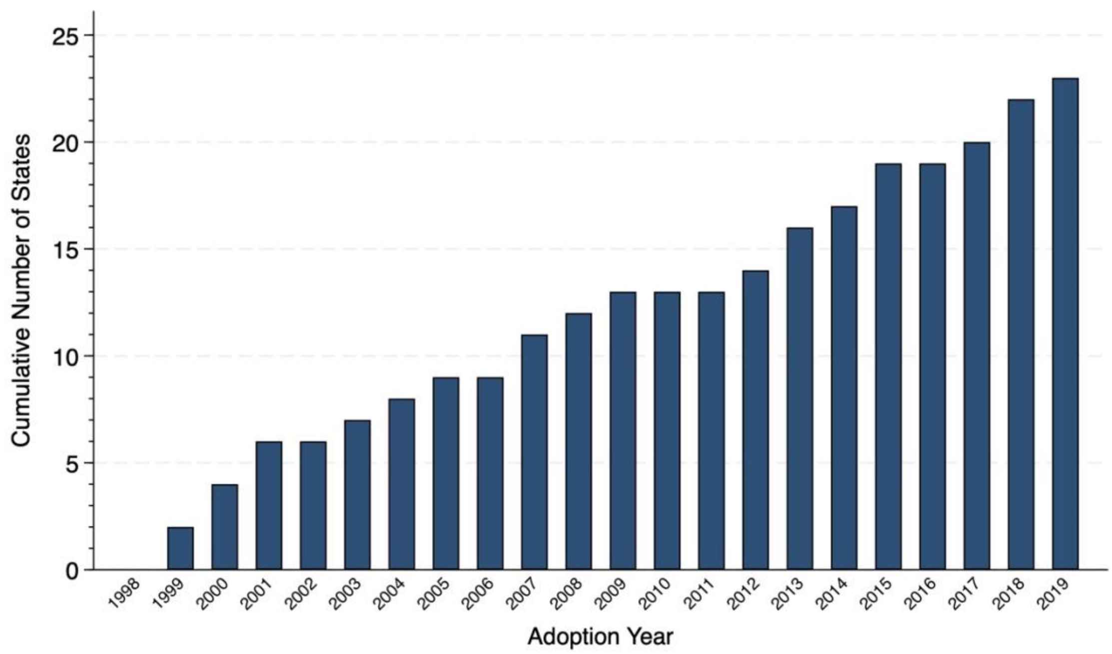
Note: This figure provides a model of the traditional teacher pipeline, beginning with initial interest in becoming a teacher and ending with acquiring a teaching job. Dark gray boxes represent checkpoints along the pipeline that I directly consider in this paper and light gray boxes represent checkpoints along the pipeline that, while still vital to the teacher pipeline, I do not consider in this paper. Box A and Box B represent the relationship that I examine in research question 1, and Box C represents the relationship that I examine in research question 2. Green arrows are inflows and red arrows are outflows.

Figure 2. State-level teacher loan forgiveness policy adoption timeline

Panel A. Discrete count of state adoption

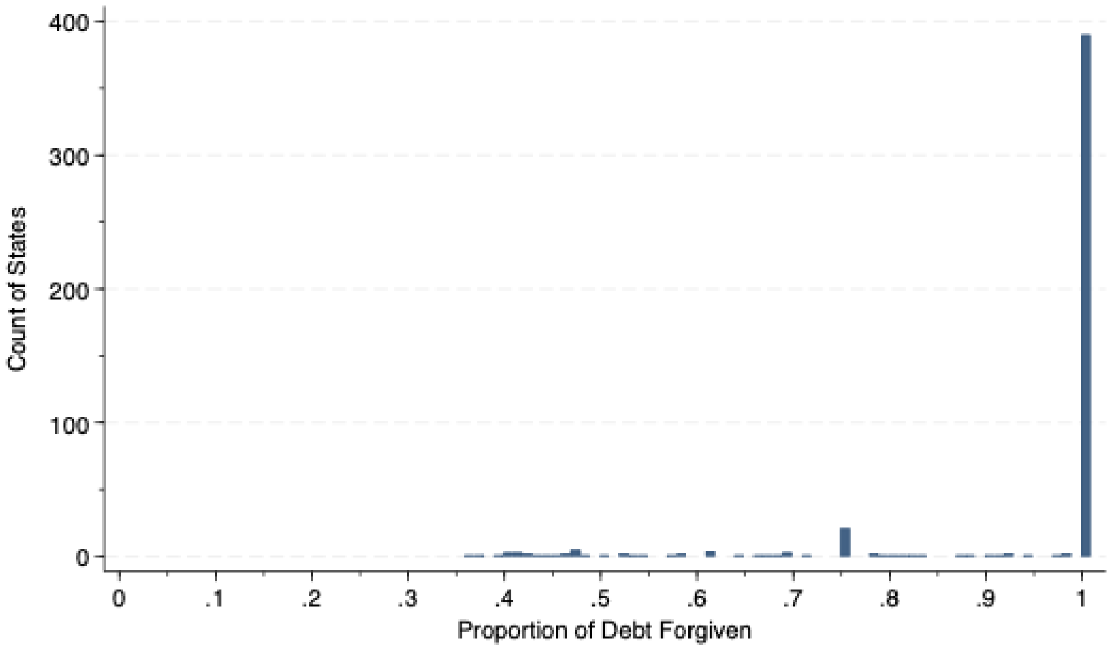


Panel B. Cumulative count of state adoption



Note: Alaska, California, Connecticut, Delaware, Florida, Louisiana, Massachusetts, Maine, Pennsylvania, South Carolina, Texas, and Vermont adopted a targeted teacher loan forgiveness policy during or before 1998, so I dropped observations in these states. Idaho, Mississippi, and New Jersey adopted after 2019 so they are considered "Never Treated"

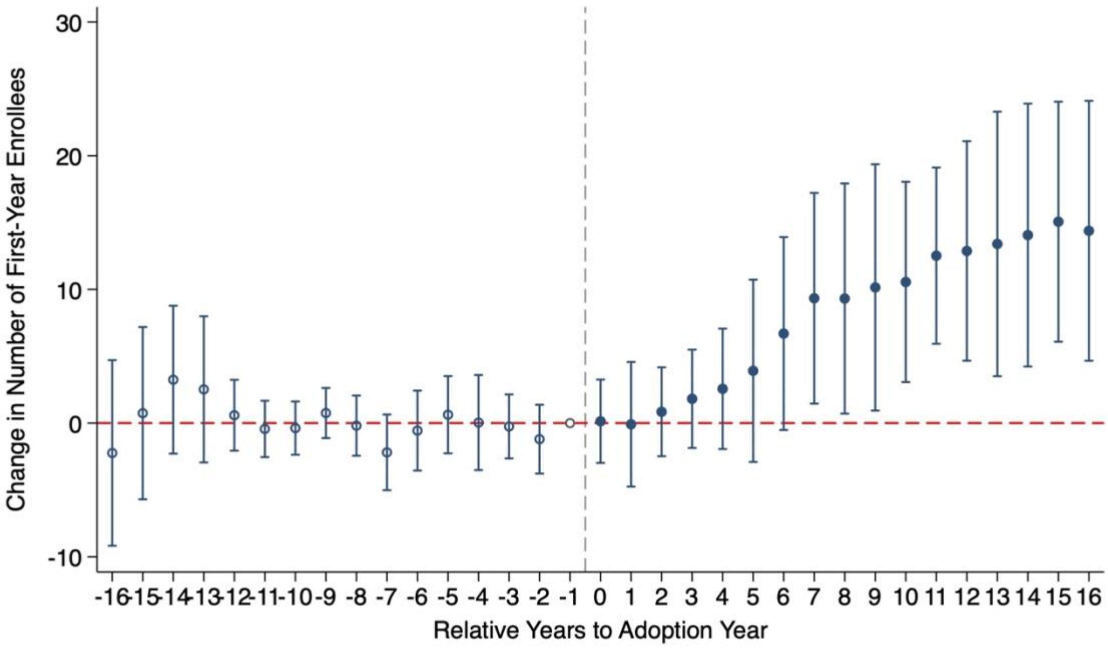
Figure 3. Histogram of the maximum proportion of student loan debt able to be forgiven, state-by-year level



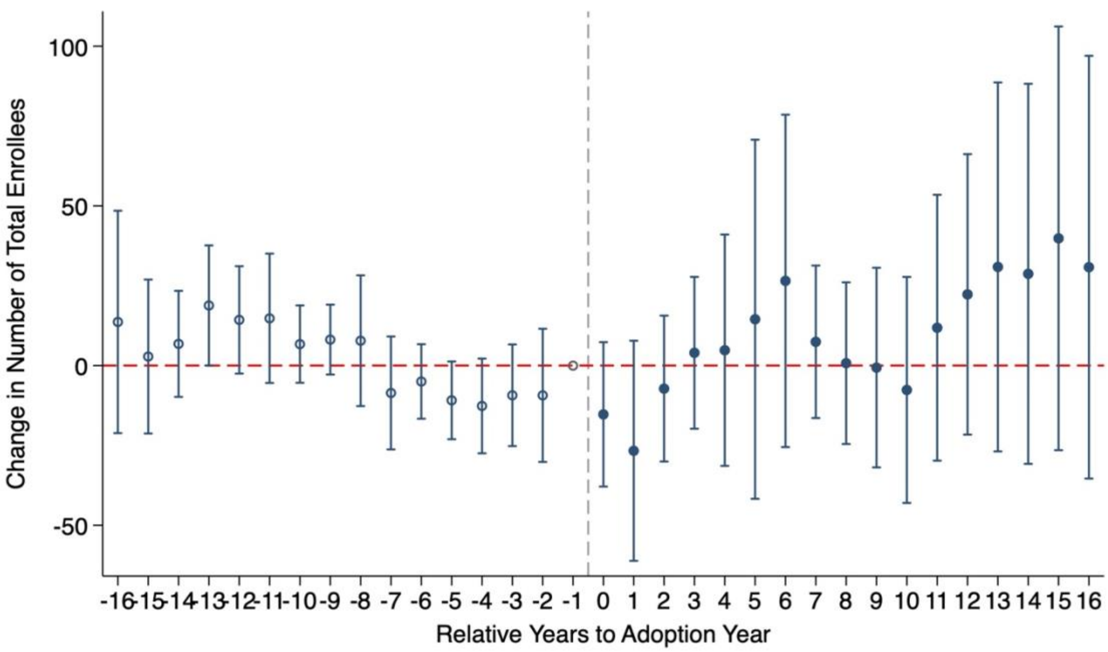
Note: Heights of bars represent the count of institutions. Proportion of debt forgiven was calculated by dividing the maximum amount of debt forgiven indicated by the state policy by the median student loan debt of each state in a given year. All monetary figures are recalculated to 2020 dollars before the proportion of debt forgiven is calculated.

Figure 4. Event study estimates for first year enrollment and total enrollment

Panel A. First Year Undergraduate Education Program Enrollment

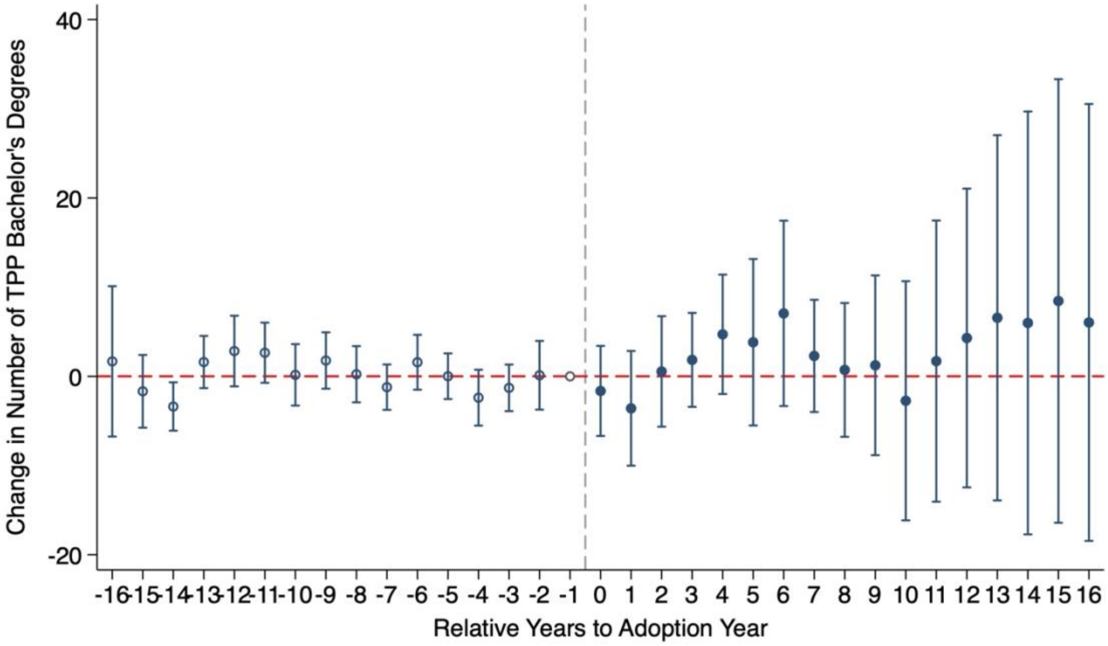


Panel B. Total Undergraduate Education Program Enrollment



Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. The marker caps represent the 95% confidence interval. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage.

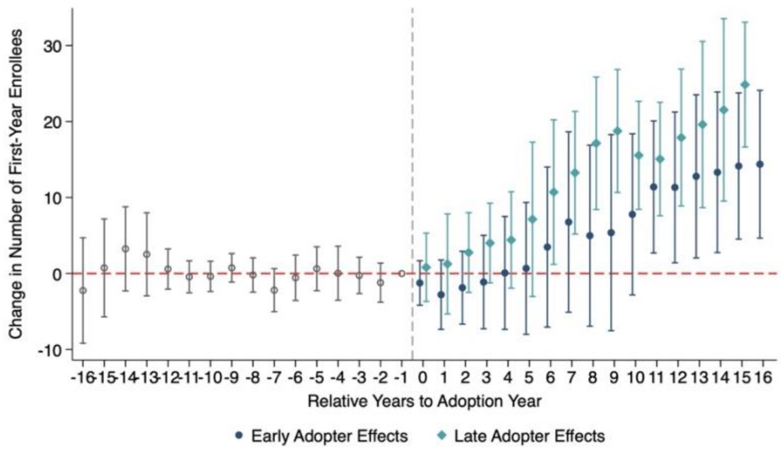
Figure 5. Event study estimates for TPP Bachelor’s degrees awarded



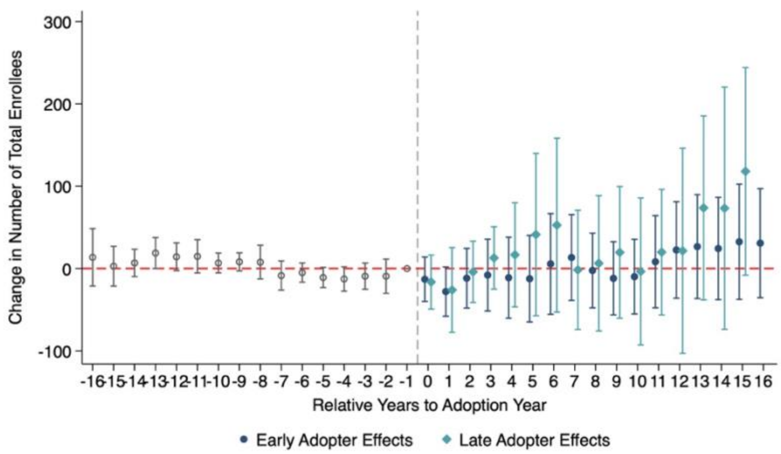
Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. The marker caps represent the 95% confidence interval. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage.

Figure 6. Comparing event study estimates for early vs late adopters, Teacher-Taxpayer Protection Act (2004)

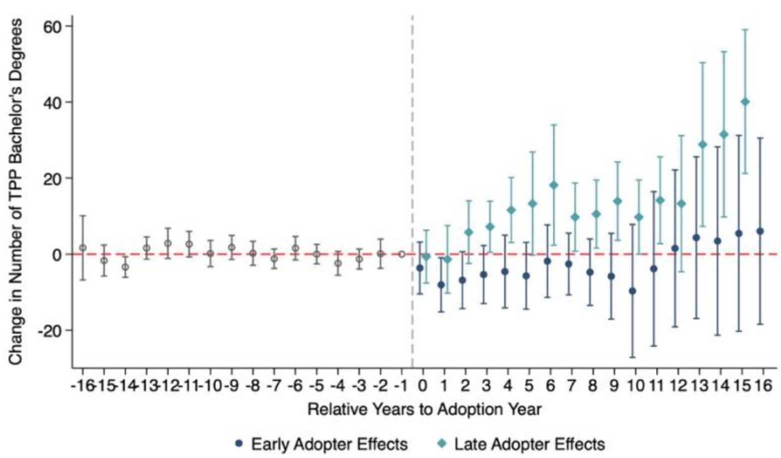
Panel A. First Year Undergraduate Education Program Enrollment



Panel B. Total Undergraduate Education Program Enrollment



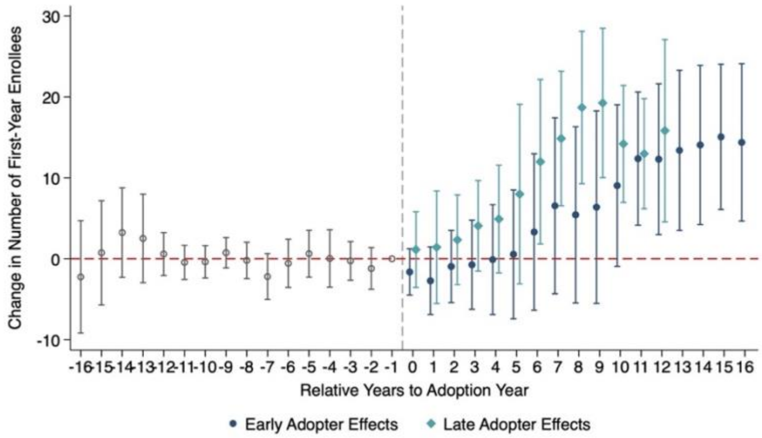
Panel C. Event study estimates for TPP Bachelor's degrees awarded



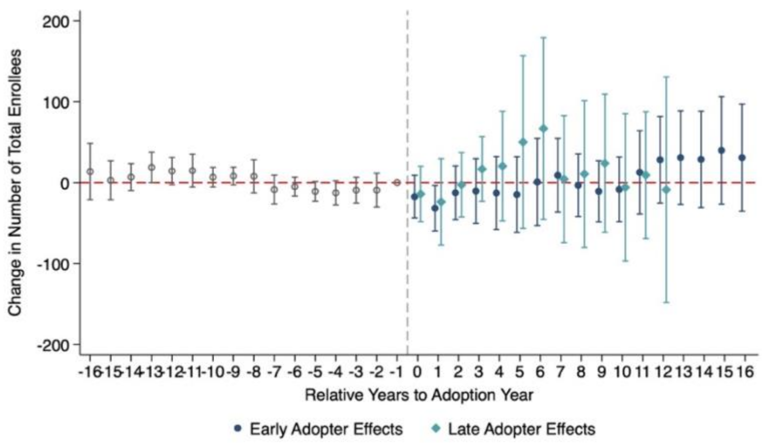
Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. The marker caps represent the 95% confidence interval. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage.

Figure 7. Comparing event study estimates for early vs late adopters, Public Service Loan Forgiveness (2007)

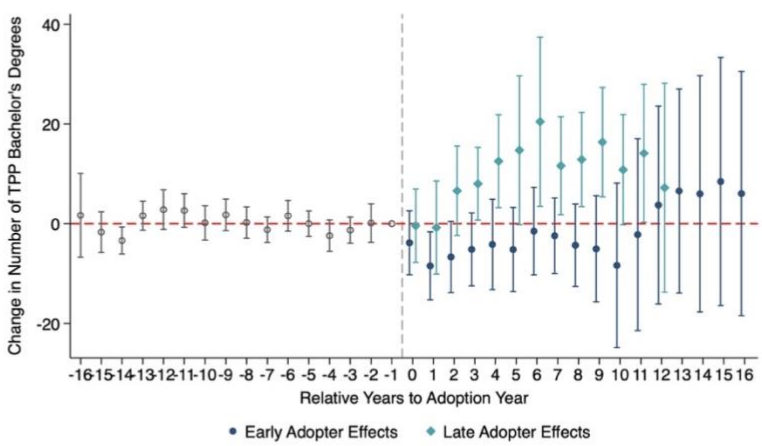
Panel A. First Year Undergraduate Education Program Enrollment



Panel B. Total Undergraduate Education Program Enrollment



Panel C. Event study estimates for TPP Bachelor's degrees awarded



Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. The marker caps represent the 95% confidence interval. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage.

Appendix A. Policy Adoption Table

Table A1. Loan forgiveness policy information

State	Adoption Year	Max Amount Forgiven	Repayment Details	Source	Notes
Alabama	2018	100%		§ 16-5-50	
Alaska	1984	50%	10% per year for five years	§ 14.43.600	
Arizona	2017	100%	One year of repayment for each year teaching	2017 AZ SB 1040	
Arkansas	2009	$18,000	6,000 per year for 3 years	§ 6-81-1601	
California	1985	$13,000	2,000 first year; 3,000 second year; 3,000 third year; 3,000 fourth year; additional 1,000 per year if shortage subject; additional 1,000 per year if HTS school	§ 69613	
Colorado	2004	100%		§ 23-3.9-102	
Connecticut	1977	$10,000	5,000 per year, corresponding to junior and senior year	§ 10a-163	Repealed in 2013
Delaware	1986	100%	One year of repayment for each year teaching	§ 14.3419	
Florida	1984	$8,000/ $16,000	4000 per year for two years of upper level undergrad or for three years if a 5 year program; 8000 per year for 2 graduate years	§ 1009.57	Repealed in 2011
Hawaii	2001	100%	10% per year for five years; 25% per year for 6th and 7th years	§ 304A-701	
Idaho	2022	$12,000	$1,500 first year, $2,500 2nd year, $3,500 third year, $4,500 fourth year	§ 33-6501	
Illinois	2003	$5,000	one time	§ 110-947/65.56	
Indiana	2014	100%		§ 21-13-10-3	Repealed in 2019
Iowa	2007	100%	20% per year for five years	§ 261.112	
Kentucky	2008	100%		§ 161.032	
Louisiana	1992	100%	2 years forgiven for each year teaching in HTS; 1 year forgiven for each year teaching generally	1992 LA SB 1006	
Maine	1983	100%	25% per year for four years of teaching; 50% per year for two years in hard-to-staff school	§ 428.12502	
Maryland	2012	100%	100% after 2 years in hard-to-staff school or high-needs subject	§ 18-1502	
Massachusetts	1993	$2,400	$150 per month for four years	1993 Mass HB 1000	Must be in top 25% of class
Minnesota	2015	$5,000	1,000 per year for five years	§ 136A.1791	
Mississippi	2021	$15,000	1,500 for first year, 2500 for second, 3500 for third year; 4000 for first year in HTS, 5000 for second HTS year, 6000 for third HTS year	§ 37-106-36	Expired in 2024
Missouri	1999	$4,000/ $8,000	2000 per year for undergrad for 2 years; 4000 per year for grad for 2 years	§ 168.600 R.S.Mo.	Repealed in 2012
Montana	2007	$17,000	$3,000 first year, $4,000 second year, $5,000 third year, up to $5,000 for fourth year	§ 20-4-501	4th year payment is the district's responsibility
Nebraska	2000	100%	One year forgiven for each year of service; two years forgiven for each year in hard-to-staff school	§ 79-8,137	Repealed in 2024
New Jersey	2021	$20,000	$5,000 max per year for four years	§ 18A:71C-84	
New Mexico	2013	100%	One year repayment for each year of service in hard-to-staff school	§ 21-22H-4	
New York	2018	$25,000	$5,000 per year for five years	§ 679-j	
North Dakota	2019	100%		2019 ND HB 1429	Expired in 2022
Oklahoma	2000	75%	3 years tuition repaid for 5 years of service in a secondary school	§ 70-698.3	
Pennsylvania	1988	$10,000	$2,500 per year for four year	§ 24-5194	
South Carolina	1979	100%	20% per year of service in critical need area	§ 59-26-20	
Tennessee	1999	100%	3 years teaching for full repayment; one year tuition repaid for each year teaching	§ 49-4-212	
Texas	1989	$5,000	$1,000 per year for max 5 years	§ 61.705	
Vermont	1983	100%	25% per year of teaching	§ 16-2869	
Virginia	2001	100%	One year of tuition repayment for each year teaching	§ 22.1-290.01	
West Virginia	2013	100%	No less than 3,000 per year	§ 18C-4A-1	
Wisconsin	2015	$30,000	$10,000 per year for max 3 years	§ 39.399	
Wyoming	2005	100%	One year tuition repaid for each year teaching	§ 21-7-601	Expired in 2011

Note: Policy information gathered using *Education Commission of the States (ECS) State Policy Database* (2024) and the author's own search. Policy information pertains to only the year of the policy adoption and does not account for later-year policy amendments and changes. No loan forgiveness policy information was found for Georgia, Kansas, Michigan, Nevada, New Hampshire, North Carolina, Ohio, Oregon, Rhode Island, South Dakota, Utah, Washington, and Washington, D.C. in any year between 1977 and 2022.

Appendix B. Remove Interpolated Values for Enrollment Outcomes

Table B1. Difference-in-differences estimates for the effects of loan forgiveness policy adoption

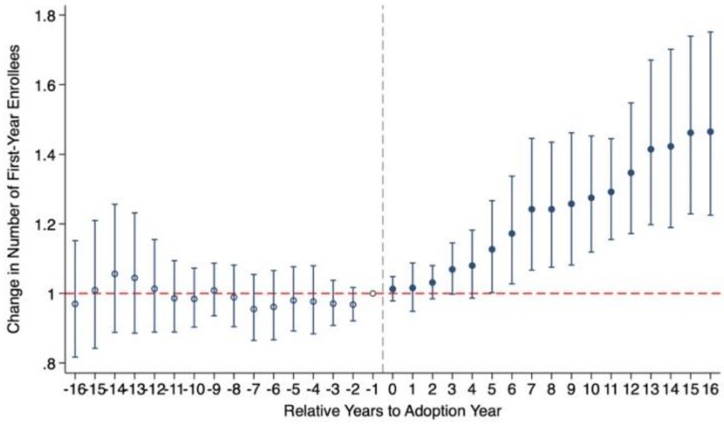
	First Year Enrollment	Total Enrollment
LF Policy	6.343*	-0.919
	(2.870)	(15.312)
Observations	9,409	9,409

Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all k between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Table 5.

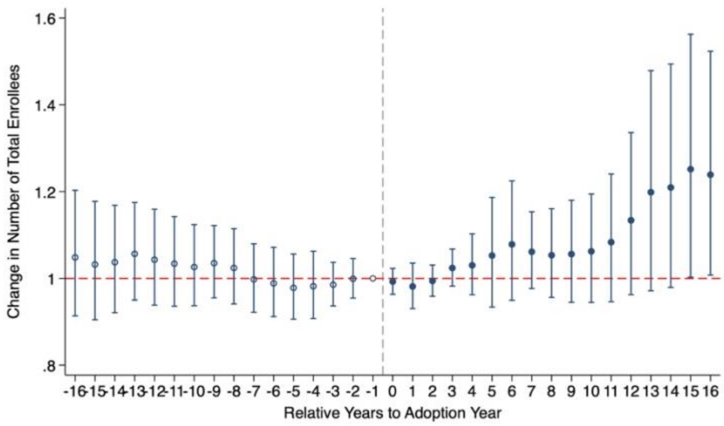
Appendix C. Alternative Poisson Models

Figure C1. Event study estimates for the effects of loan forgiveness policy adoption

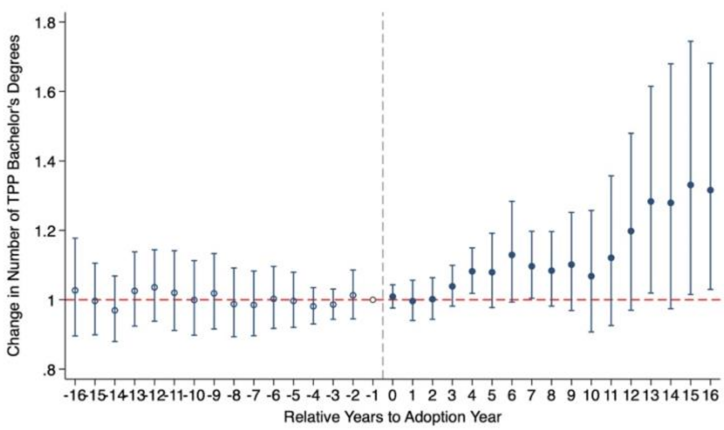
Panel A. First year undergraduate education program enrollment



Panel B. Event study estimates for total enrollment



Panel C. Event study estimates for TPP bachelor's degrees awarded



Note: Estimates from two-way fixed effect Poisson models with institution and year fixed effects. Estimates are presented in exponentiated form. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Figures 4 and 5.

Table C1. Difference-in-differences estimates for the effects of loan forgiveness policy adoption

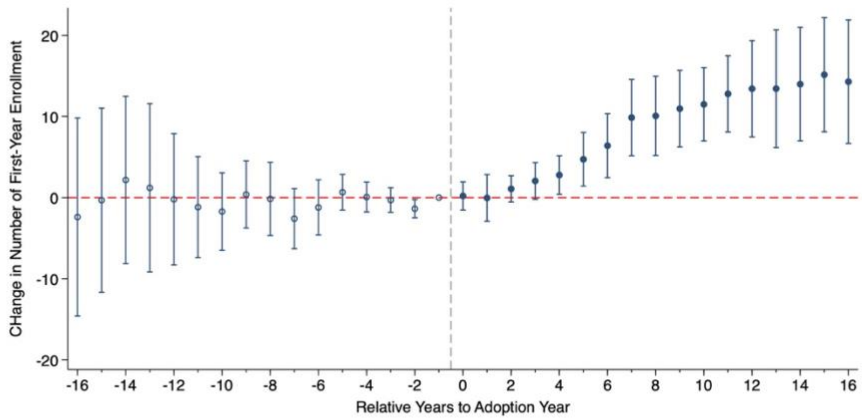
	First Year Enrollment	Total Enrollment	Bachelor's Degrees
LF Policy	1.075+	1.010	1.030
	(0.042)	(0.030)	(0.029)
Observations	17,532	18,662	18,772

Note: Estimates from two-way fixed effect Poisson models with institution and year fixed effects. Estimates are presented in exponentiated form. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Table 5.

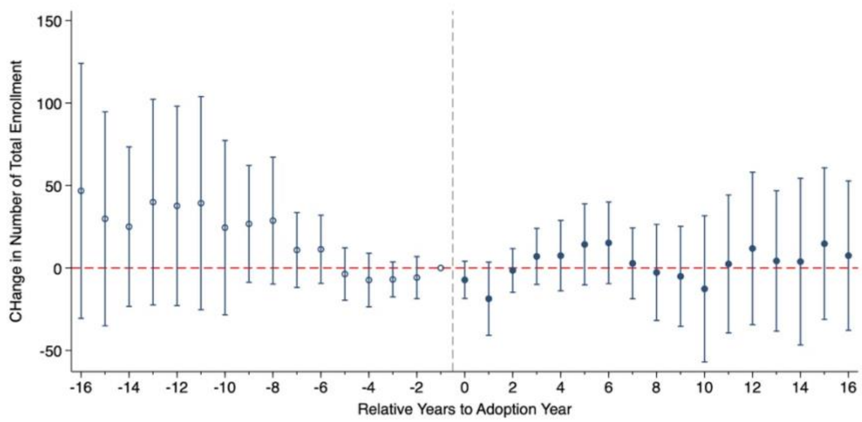
Appendix D. Sun & Abraham Specification

Figure D1. Event study estimates for the effects of loan forgiveness policy adoption

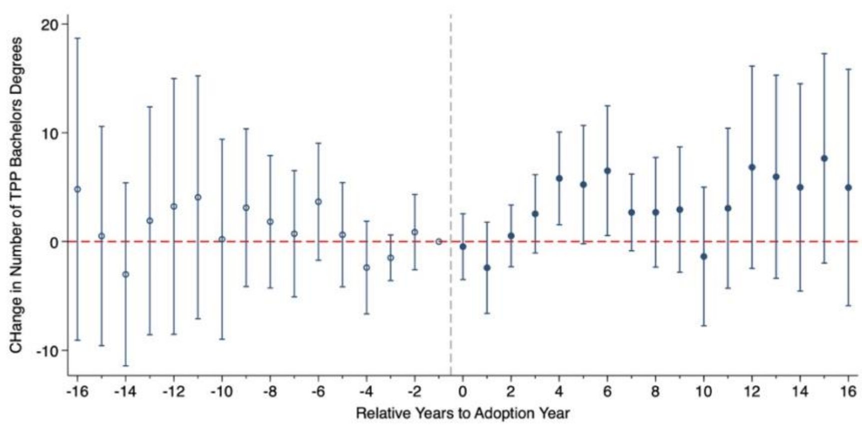
Panel A. First year undergraduate education program enrollment



Panel B. Event study estimates for total enrollment



Panel C. Event study estimates for TPP bachelor's degrees awarded



Note: Estimates from Sun & Abraham (2020) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Figures 4 and 5.

Table D1. Difference-in-differences estimates for the effects of loan forgiveness policy adoption

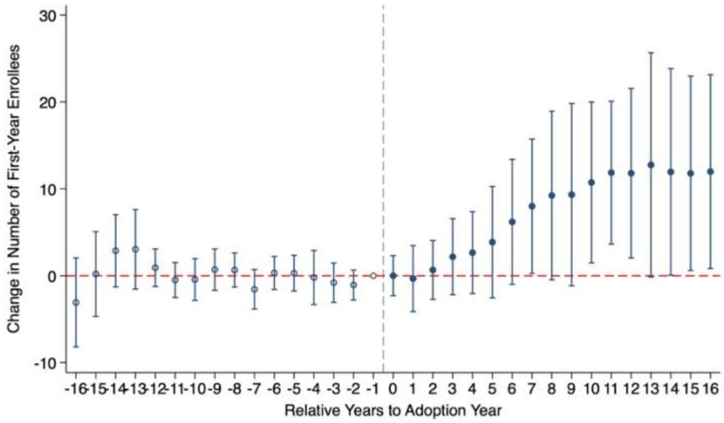
	First Year Enrollment	Total Enrollment	Bachelor's Degrees
LF Policy	8.387***	2.549	3.426
	(1.789)	(13.126)	(2.259)
Observations	18,864	18,864	18,864

Note: Estimates from Sun & Abraham (2020) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Table 5

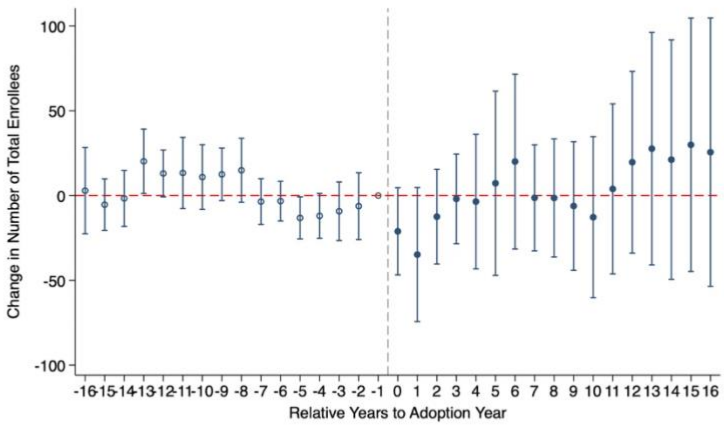
Appendix E. Contemporaneous Policies Analysis

Figure E1. Event study estimates for first year education program enrollment

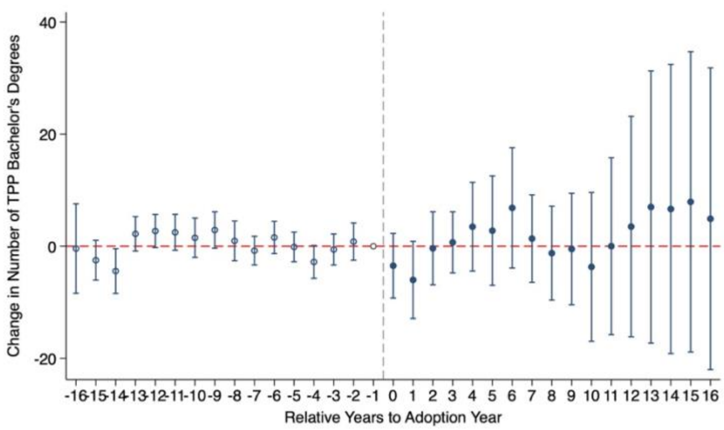
Panel A. First year undergraduate education program enrollment



Panel B. Event study estimates for total enrollment



Panel C. Event study estimates for TPP bachelor's degrees awarded



Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Figures 4 and 5.

Table E1. Difference-in-differences estimates for the effects of loan forgiveness policy adoption

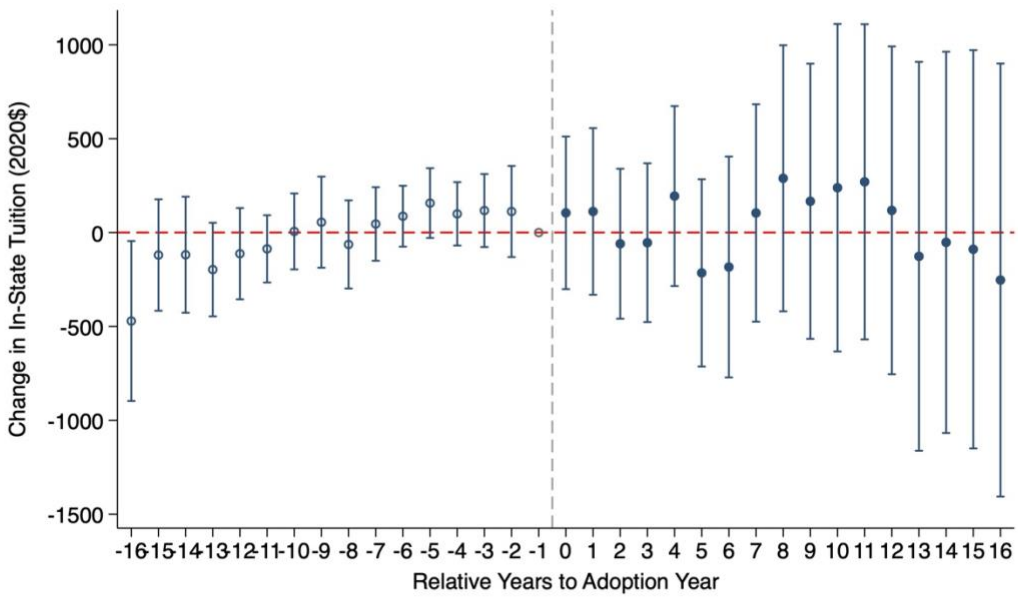
	First Year Enrollment	Total Enrollment	Bachelor's Degrees
LF Policy	5.745+	-1.680	0.825
	(3.008)	(16.049)	(4.066)
Observations	18,864	18,864	18,864

Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Table 5

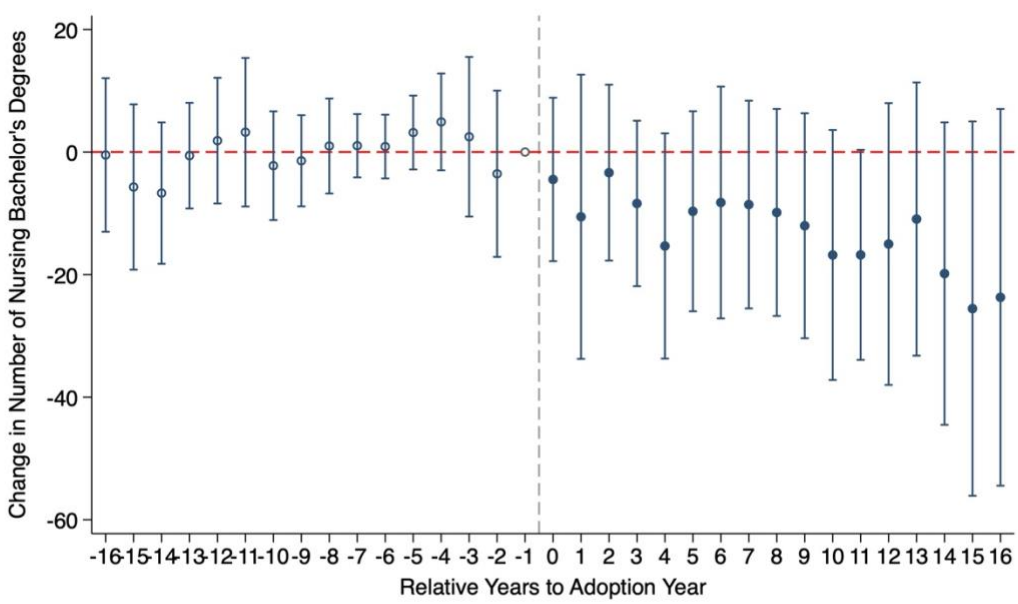
Appendix F. Placebo Test of Alternative Outcomes

Figure F1. Event study estimates for alternative operationalization of outcomes

Panel A. In-state undergraduate tuition (in 2020$)



Panel B. Number of nursing Bachelor's degrees



Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage.

Table F1. Difference-in-differences estimates for the effects of loan forgiveness policy adoption

	In-State Undergraduate Tuition	Nursing Bachelor's Degree
LF Policy	43.981	-11.442
	(243.391)	(7.052)
Observations	18,664	18,664

Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage.

Appendix G. Alternative Measurement Timing for Unemployment Rates

Table G1. Unemployment Rate Sensitivity Analysis

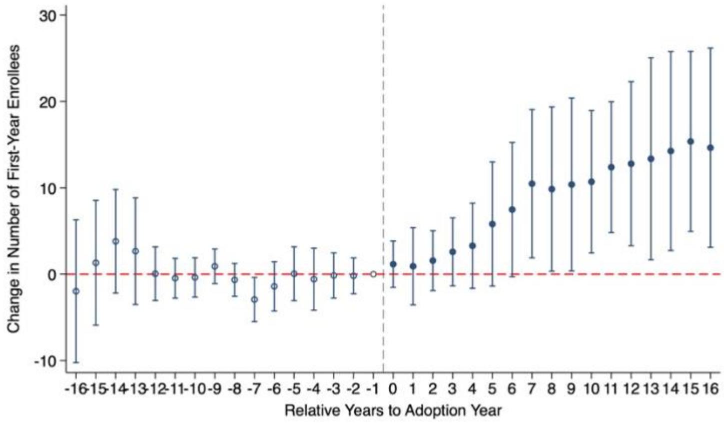
	First Year Enrollment	Total Enrollment	Bachelor's Degrees
Panel A. Unemployment rates measured in year t			
LF Policy x Low UR	3.386	-1.045	1.820
	(3.439)	(21.622)	(4.871)
LF Policy x Mid UR	7.644**	10.353	2.264
	(2.831)	(15.567)	(5.065)
LF Policy x High UR	9.692**	7.394	2.024
	(3.404)	(11.850)	(4.677)
Observations	18,864	18,864	18,864
Wald test - Low Mid	1.426	0.241	0.006
Wald test - Mid High	0.557	0.055	0.006
Wald test - Low High	1.830	0.120	0.001
Panel B. Unemployment rates measured in year t-2			
LF Policy x Low UR	3.501	-0.858	1.498
	(3.510)	(22.082)	(4.909)
LF Policy x Mid UR	7.693**	10.064	2.791
	(2.770)	(16.923)	(5.457)
LF Policy x High UR	9.377**	7.295	1.897
	(3.433)	(9.733)	(3.764)
Observations	18,864	18,864	18,864
Wald test - Low Mid	1.263	0.193	0.046
Wald test - Mid High	0.527	0.035	0.063
Wald test - Low High	1.590	0.122	0.005
Panel C. Unemployment rates measured in year t-3			
LF Policy x Low UR	3.183	-2.720	1.108
	(3.396)	(22.208)	(4.897)
LF Policy x Mid UR	8.408**	11.899	3.287
	(2.831)	(16.984)	(5.214)
LF Policy x High UR	9.060**	8.424	1.985
	(3.262)	(9.604)	(3.996)
Observations	18,864	18,864	18,864
Wald test - Low Mid	2.160	0.330	0.140
Wald test - Mid High	0.089	0.056	0.163
Wald test - Low High	1.830	0.239	0.026
Panel D. Unemployment rates measured in year t-4			
LF Policy x Low UR	3.065	-4.839	0.609
	(3.207)	(20.577)	(4.773)
LF Policy x Mid UR	8.668**	15.072	3.372
	(2.876)	(17.062)	(6.002)
LF Policy x High UR	9.424**	9.634	2.978
	(3.180)	(8.985)	(3.448)
Observations	18,864	18,864	18,864
Wald test - Low Mid	2.828+	0.777	0.194
Wald test - Mid High	0.135	0.140	0.009
Wald test - Low High	2.456	0.501	0.231

Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Table 7.

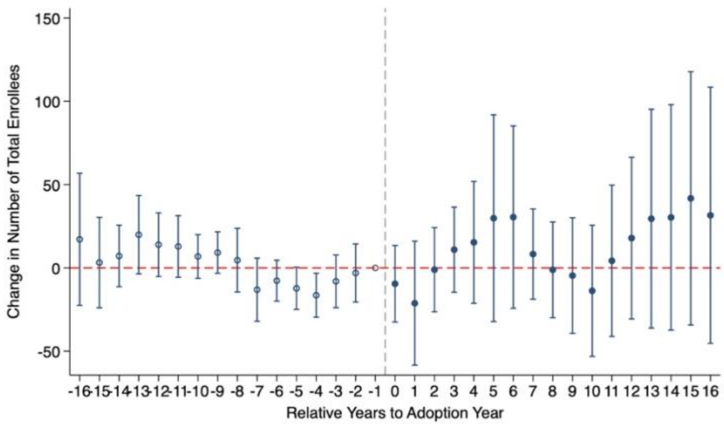
Appendix H. Removal of observations in states that repealed policy

Figure H1. Event study estimates for first year education program enrollment

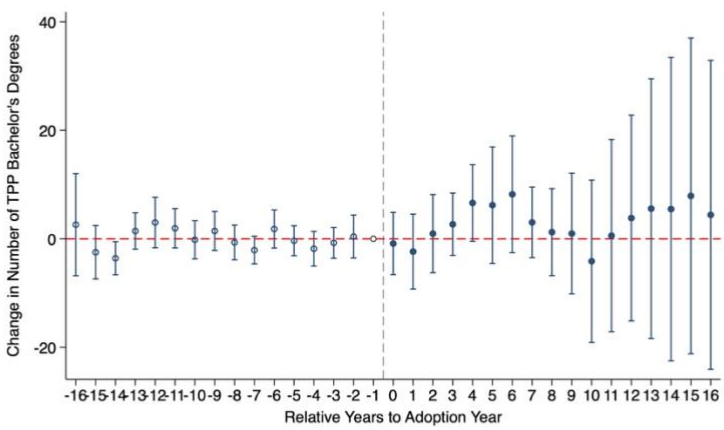
Panel A. First year undergraduate education program enrollment



Panel B. Event study estimates for total enrollment



Panel C. Event study estimates for TPP bachelor's degrees awarded



Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Figures 4 and 5.

Table H1. Difference-in-differences estimates for the effects of loan forgiveness policy adoption

	First Year Enrollment	Total Enrollment	Bachelor's Degrees
LF Policy	6.962*	7.551	2.413
	(2.898)	(14.330)	(4.329)
Observations	17,322	17,322	17,322

Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all k between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Table 5

Appendix I. Event Study Tables

Table I1. Event study estimates

	First Year Enrollment	Total Enrollment	TPP Bachelor's Degrees
-16	-2.236	13.676	1.676
	(3.541)	(17.768)	(4.298)
-15	0.740	2.845	-1.679
	(3.287)	(12.297)	(2.081)
-14	3.247	6.812	-3.385*
	(2.821)	(8.474)	(1.383)
-13	2.524	18.836*	1.604
	(2.789)	(9.597)	(1.492)
-12	0.587	14.323+	2.833
	(1.351)	(8.577)	(2.020)
-11	-0.438	14.810	2.645
	(1.074)	(10.328)	(1.720)
-10	-0.374	6.739	0.162
	(1.016)	(6.179)	(1.758)
-9	0.750	8.153	1.773
	(0.957)	(5.580)	(1.613)
-8	-0.191	7.797	0.232
	(1.148)	(10.443)	(1.607)
-7	-2.189	-8.563	-1.217
	(1.443)	(9.020)	(1.297)
-6	-0.561	-4.970	1.578
	(1.526)	(5.952)	(1.570)
-5	0.627	-10.869+	0.011
	(1.474)	(6.213)	(1.307)
-4	0.039	-12.628+	-2.393
	(1.813)	(7.565)	(1.598)
-3	-0.252	-9.295	-1.291
	(1.221)	(8.116)	(1.332)
-2	-1.199	-9.317	0.119
	(1.313)	(10.636)	(1.965)
-1 (reference category)	---	---	---
0	0.136	-15.260	-1.639
	(1.590)	(11.536)	(2.577)
1	-0.089	-26.665	-3.591
	(2.377)	(17.586)	(3.281)
2	0.845	-7.193	0.545
	(1.696)	(11.651)	(3.161)
3	1.818	4.011	1.843
	(1.876)	(12.126)	(2.687)
4	2.563	4.820	4.709
	(2.299)	(18.479)	(3.414)
5	3.911	14.527	3.820
	(3.477)	(28.688)	(4.762)
6	6.697+	26.539	7.056
	(3.680)	(26.553)	(5.303)

Table I1 (continued below)

Table I1 (continued)

7	9.338*	7.456	2.289
	(4.020)	(12.182)	(3.209)
8	9.317*	0.759	0.719
	(4.393)	(12.908)	(3.825)
9	10.151*	-0.619	1.235
	(4.700)	(15.951)	(5.140)
10	10.555**	-7.611	-2.741
	(3.822)	(18.054)	(6.841)
11	12.522***	11.863	1.705
	(3.362)	(21.246)	(8.042)
12	12.876**	22.310	4.296
	(4.189)	(22.406)	(8.546)
13	13.401**	30.899	6.564
	(5.048)	(29.469)	(10.447)
14	14.065**	28.748	5.988
	(5.017)	(30.362)	(12.096)
15	15.064**	39.851	8.449
	(4.580)	(33.845)	(12.689)
16	14.384**	30.821	6.041
	(4.960)	(33.771)	(12.495)
Observations	18,864	18,864	18,864

Note: Estimates from Gardner and colleagues (2024) models with institution and year fixed effects. Models include all *k* between -16 and 16. Institutional covariates include logged enrollment, and proportions of Black, Hispanic, American Indian/ Asian/ multi-racial/ Native Hawaiian students. State covariates include total population, unemployment rates, proportions of families with at least one bachelor's degree, proportions of White, Black, Hispanic, and American Indian/Asian/multi-racial/Native Hawaiian children, and median private sector wage. Main estimates found in Figures 4 and 5.