Localized Teacher Recruitment through "Grow-Your-Own": Impacts of the High School Teacher Academy of Maryland Program

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Abstract

Recruiting teachers via "grow-your-own" (GYO) programs is a popular, yet rarely evaluated, strategy for addressing local workforce shortages and ensuring that incoming teachers resemble, understand, and have strong connections to their communities. We provide novel evidence on the impacts of one such GYO program by exploiting the staggered rollout of the Teacher Academy of Maryland Career and Technical Education (CTE) program across public high schools. Exposed students were more likely to become teachers a decade later by 0.6 percentage points (pp), or 45%. Effects were concentrated among White girls (1.4pp/39%) and Black girls (0.7pp/82%), though boys benefitted too (0.2pp/59%). While White girls induced by the program to become teachers often did so in the same district they attended as students (0.9pp/43%)—a key goal of GYO and localized teacher recruitment programs—this was less common for Black girls. Rather, Black girls induced by the program to become teachers did so in districts with more Black teachers than their home district (0.4pp/143%) and in districts with higher starting salaries (0.5pp/239%). Access to the program also increased wages (5% on average/18% for Black girls), challenging the narrative that such programs cause students to forego more lucrative professions.

Keywords: Teaching, High School Curricula, Career and Technical Education College Major Choice, Occupational Choice, Earnings

JEL Classifications: I20, J24, H52

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1. Introduction

In the United States (U.S.), interest in teaching as a career has ebbed and flowed over the past 50 years but today is at a historical low. Fewer than 5% of high school seniors and first-year college students identify teaching as their expected career, down from roughly 20% in the 1970s (Kraft & Lyon, 2024). These trends challenge school systems, as teachers are the most important educational resource to support students' short- and long-run outcomes (Chetty et al., 2014).

The decline in interest in teaching has been most precipitous for Black individuals (Kraft & Lyon, 2024), raising additional concerns since Black teachers substantially improve the test-score, socio-emotional, and educational attainment outcomes not just of Black students (Dee, 2004; Gershenson et al., 2021, 2022) but of all students (Blazar, 2024). Accordingly, many state and local education agencies propose addressing teacher shortages generally—and the shortage of Black and other teachers of color more specifically—through what are often referred to as "grow-your-own" or "GYO" programs (Garcia, 2020). In broad terms, GYO policies and programs recruit non-teachers (e.g., high school students, instructional aides, community members) in the vicinity of a school district into the teaching profession, while implicitly assuming (or explicitly requiring) that successful recruits will eventually teach in the local system (Valenzuela, 2017).

This approach aligns with evidence that teachers are more likely than other professionals to work near their childhood home (Boyd et al., 2005; Reininger, 2012) such that successful GYO programs may help create cohorts of new teachers who look like their students and have strong connections to their communities. In practice, GYO has morphed into an umbrella term claimed by many flavors of localized teacher recruitment efforts (Edwards & Kraft, 2024). Many self-described GYO programs resemble strategies that have been around for decades (Gelber, 2022)—e.g., building networks of high school students interested in teaching, providing financial incentives and scholarships to college students to major in teaching, recruiting prospective teachers at HBCUs, and designing pathways for instructional aides and other career-changers to become full-time teachers of record (for one historical example, see Maryland State Department of Education, 1993).

Despite the popularity of GYO programs (Garcia, 2020) and their predecessors, there exists remarkably little credible evidence of their effectiveness (Edwards & Kraft, 2024; Gist et al., 2019). This is consistent with the idea that teacher recruitment more broadly is an understudied policy area with limited causal evaluations of policy and program impacts (Fleck et al., 2025), despite the known importance of teachers in the education production function. We contribute to this gap in the literature by examining the effects of the Teacher Academy of Maryland (TAM), a localized teacher recruitment and GYO program that—like other similar programs across the country—targets high school students (Edwards & Kraft, 2024).

TAM is one of many Career and Technical Education (CTE) programs of study offered by the state, which supplement students' core academic subjects (i.e., math, English, science, social studies) with career-focused training. TAM's four-course sequence is designed to develop specific teaching skills and competencies (e.g., knowledge of child development and lesson planning), as well as opportunities for hands-on practice through a student-teaching experience. Unlike some other CTE program areas, teaching requires a college degree and, as such, TAM course credits accrued in high school can transfer into two- and four-year colleges, making the process of becoming a teacher easier and less expensive. More broadly, TAM seeks to build interest in teaching at a time when students and their families are starting to think seriously about potential careers (Cooc & Kim, 2023).

To estimate program impacts, we exploit the staggered rollout of the TAM program across Maryland public high schools in a generalized difference-in-differences (DD) framework. Since its inception in 2004, the program has been implemented in all school districts in the state. However, there is variation across districts, schools, and time in terms of which students were exposed to the program. As such, we compare exposed students to similar students in the same school who graduated before TAM began, and between schools that did and did not offer the program. Intent-to-treat (ITT) estimates suggest that attending a high school that offered TAM significantly increased the likelihood of becoming a teacher and subsequent wages, driven in part by intermediary effects on educational attainment (e.g., high school graduation and college enrollment). These results are robust to a variety of modeling and variable-construction decisions, and to using estimators that accommodate parallel-trends violations and heterogeneous treatment effects suggested by the modern DD literature (e.g., Callaway & Sant'Anna, 2021; Roth et al., 2023).

Overall, exposure to TAM increased the likelihood that individuals went on to become teachers by 0.6 percentage points (pp). Given that entering teaching is a rare event observed for roughly 1.3% of public high school students in our control-group sample, this ITT effect represents a large increase of 45%. Importantly, the average ITT estimates mask significant, and nuanced, heterogeneity across demographic groups. The ITT effect of TAM is largest for girls (0.9pp/40%). In absolute terms, the effect is larger for White girls (1.4pp) than for Black girls (0.7pp). The reverse is true when effects are captured in percent changes (39% for White girls and 82% for Black girls), as White girls in our control-group sample were roughly four times as likely to become teachers as Black girls. Similarly, the teaching effect is larger for White boys (0.5pp/72%) than for Black boys (0.1pp/25%) in both absolute and relative terms, though our preferred estimates pool across all boys (0.2pp/59%) given the smaller number of boys who participated in the program.

Differential effects by race are notable given that an expectation of many GYO programs and their advocates is that they will increase racial diversity in the teaching profession (Edwards & Kraft, 2024; Gist et al., 2019; Valenzuela, 2017). White females are overrepresented in teaching nationally, as well as in Maryland where the teaching workforce is roughly 70% White compared to a student body that is 43% White (see Table 1). Another nuance in our results is that White girls induced into teaching by TAM entered via traditional routes and certifications, while Black girls entered via alternative pathways that bypassed undergraduate teacher education programs. This is all to say that, while TAM increased consideration of and entry into the teaching profession, it did so without necessarily increasing the representativeness or diversity of the teacher workforce.

TAM also increased students' educational attainment, even among those who did not enter teaching. Black girls primarily benefited in this domain, as their high school graduation rate increased by 2.2pp, or 2%. Estimates for four-year college enrollment are similar in magnitude but not statistically significant. Similarly, we observe that, for Black students, increases in the likelihood of becoming a teacher—which requires a four-year degree—were driven in part by decreases in the likelihood of becoming an instructional assistant/teaching aide—which is another prospective career choice for students interested in education that does not require a four-year degree. These patterns suggest that TAM created new teachers through at least two distinct channels: increasing attainment (extensive margin) and changing "always-college going" students' choice of major/occupation (intensive margin).

Finally, average wages increased with exposure to TAM, with the largest gains accruing to Black girls. TAM did not significantly reduce the average earnings of any group, which is important given concerns that GYO programs may cause students to leave more lucrative majors and career pathways for teaching (Bergey et al., 2019; Gershenson et al., 2022; Murnane et al., 1989). Specifically, wages increased by about 5% on average and 18% for Black girls. The wage effects for Black girls are particularly large at the lower end of the wage distribution, which is consistent with the finding that TAM shifted some Black girls' career choices from instructional aide to teacher. These wage gains are similar for girls overall and for the subset of girls who entered teaching, which again counters the prevailing narrative that teaching leaves one worse off financially relative to other labor market opportunities. More broadly, this suggests that TAM increased earnings generally, even for those students who did not enter teaching, potentially driven by increased educational attainment.

Our analysis of TAM contributes to several strands of literature, which we discuss from broadest to narrowest. First, despite workforce development being a frequent topic of policy discussions, there is a surprising dearth of evidence on what actually works (Escobari et al., 2021). Bloom (2010) identified just 11 rigorous evaluations of job training programs for students who had or were close to dropping out of high school, which yielded mixed results. This may explain the lack of continued investment in workforce development programs targeted to young people and the more recent shift to career academies and CTE programs offered in high school (Bonilla, 2020; Dougherty, 2018; Hemelt et al., 2019; Kemple & Willner, 2008; Page, 2012).

Within the CTE literature, our study is most similar to Brunner et al. (2023), who also link high school records to employment data. They find that CTE high schools that include many different programs of study and prepare students for a variety of industries increased high school graduation by roughly 5pp and boosted wages by over 30%—both slightly larger than our estimates. However, their estimates were driven by males who entered the workforce shortly after high school and whose average high school graduation rate was lower than in our sample. Our analysis of TAM extends this literature by identifying educational, employment, and wage effects of an occupation-specific CTE program for a female-dominated profession that requires a college degree.

Second, our study contributes to the related but distinct literature on teacher labor supply with what is, to our knowledge, the first causal evidence of a large-scale GYO program's impacts on long-run educational and employment outcomes. While questions of "who will teach?" go back many decades (e.g., Hanushek & Pace, 1995; Murnane et al., 1991), most extant work on teacher recruitment to this day is descriptive in nature—highlighting, for example, changes in interest in teaching over time (Kraft & Lyon, 2024), large racial/ethnic gaps in who pursues a career in teaching (e.g., Chen et al., 2000; Dilworth & Coleman, 2014), and where along the pathway from high school to career these gaps emerge (e.g., Bartanen & Kwok, 2023; Blazar et al., 2024; Cooc & Kim, 2023; Redding & Baker, 2019). A recent systematic review and meta-analysis on the effectiveness of interventions and policies to reduce shortages in education (and in healthcare) identify just a handful of studies that estimate causal effects on teacher employment; most of the reviewed literature instead focused on policies to increase teacher retention (Fleck et al., 2025). In contrast, TAM focuses on early recruitment stages before students self-select into a teaching major, or even decide to enroll in college.

A notable exception relevant to the GYO discussion is a recent study by Redding (2022), who estimates the effects of state alternative certification policies that allow individuals to work full time as a teacher of record while they complete final requirements for full licensure (e.g., coursework, testing). These alternative routes are situated prominently in the GYO literature, in part because they recruit and train prospective teachers locally (Carver-Thomas, 2018; Valenzuela, 2017). Alternative routes to certification also can fast-track the time it takes to earn a license and, thus, more readily support individuals to balance employment with course obligations, student teaching, and other opportunity costs of teacher preparation programs—barriers that disproportionately affect Black and other individuals of color (Bergey et al., 2019; Dinkins & Thomas, 2016). DD-style analyses that exploit changes across states and over time indicate that alternative certification increased the share of Black teachers by 1.8pp (from roughly 7%, or a roughly 25% increase) (Redding, 2022). These findings align with ours, which show that TAM's effect on the likelihood that Black females became teachers was driven almost entirely by alternative rather than traditional routes into the profession.

Third, in addition to providing novel evidence on a recruitment program that works to increase teacher supply, we document some of the revealed preferences of individuals in their career pursuits, and how these vary across race. Researchers have long studied the wage and non-wage factors that influence teacher labor supply on the extensive margin (i.e., entry into the profession) (Dolton, 2006; Guarino et al., 2006; Hanushek & Pace, 1995). This literature generally finds that, while relative wages and non-teaching job prospects influence entry into teaching (Bacolod, 2007), so too do myriad non-wage job characteristics such as the stress provided by consequential accountability policies (Kraft et al., 2020) and flexibility to exit and return to the profession (Flyer & Rosen, 1997). Parental influences may be particularly relevant in the case of teaching, a female-dominated occupation that is transmitted from parents to children at higher rates than other similar professions, due to some combination of information and a sense of altruism being passed from parents to children (Jacinto & Gershenson, 2021). Similarly, while altruism is a critical factor in career decisions for prospective teachers of all backgrounds, experiences with adversity and giving back to one's own community is particularly important for Black individuals—revealed in both quantitative analyses (Bartanen et al., 2025) and interview studies (Goings, 2015; Johnson, 2014; Lynn, 2002; Warren, 2014). Complementing this descriptive work, our analyses show that White females induced by TAM to become teachers often stayed local (i.e., in the same district they went to high school), while Black females chose a district with more Black teachers. We also find that starting salary likely is a stronger driver for Black females compared to White females—at least about *where* to teach.

The paper proceeds as follows: Section 2 describes GYO programs generally and the specifics of Maryland's TAM program. Section 3 describes the administrative data and provides summary statistics, while section 4 describes the identification strategy including event-study analyses. Section 5 presents the main results regarding program uptake and impacts on becoming a teacher, using our preferred two-way fixed effects DD approach. Section 6 examines potential mechanisms and unintended consequences by examining effects on educational attainment, college major choice, and wages. Section 7 presents an array of sensitivity analyses and robustness checks, including use of an alternative DD estimator (Callaway & Sant'Anna, 2021) and probing of potential confounders. Section 8 discusses the findings and concludes.

2. Background

2.1. "Grow-Your-Own" Programs

GYO is as much a call to action and service as it is a description of any specific policy or program. The core idea of GYO—i.e., to recruit new teachers locally—has longstanding roots dating back at least to the 1940s, when leaders of the National Education Association (the largest teachers' union in the U.S.) and a national alliance of high school clubs called Future Teachers of America suggested that organizing young people within the local community could help bolster the status of the teaching profession (Faust, 1950). While the concept of "growing your own" teachers later broadened to include racially diverse groups of community college students, instructional aides and paraprofessionals, and others, high school organizations remained the most prevalent form of these efforts throughout the late 20th Century (Gelber, 2022).

Today, the majority of self-described GYO programs still target high school students, sometimes through clubs and affinity groups (e.g., Future Teachers of America, Educators Rising) but more often through set course sequences with curricula aligned to state-specific teaching standards (Edwards & Kraft, 2024). One of the first course-based high school programs is the South Carolina Center for Educator Recruitment, Retention, and Advancement's Teacher Cadet program, which aimed to address the impending teacher shortage in the early 1980s and is still in operation today (Valenzuela, 2017). In addition to earning college-level credits towards a teaching degree, students can gain licensure as an instructional aide or paraprofessional. Since then, pre-collegiate programs have grown to include additional state-sponsored programs (e.g., TAM in our study), community-based programs such as one started by the Logan Square Neighborhood Association (LSNA) in Chicago, and university-school partnership programs such as Pathways2Teaching in Denver.

Because localness is central to GYO efforts, it is perhaps unsurprising that GYO often is mentioned in discussions on diversifying the teacher workforce (Bireda & Chait, 2011; Carver-Thomas, 2018; Gist et al., 2019). Local recruitment can help ensure that new teachers look like and have strong connections to the students in their community. Notably, though, the reverse is not necessarily true, with just about half of self-described GYO programs nationally—and 52% of high school GYO programs—describing teacher workforce diversity as central to their mission. The more "local" and community-focused the program, the more intentional the focus on race and diversity. For example, the original, community-led LSNA program in Chicago first developed high school coursework with an emphasis on critical race theory and critical pedagogy (Skinner, 2011; Valenzuela, 2017). When the program expanded statewide, under the Illinois Grow Your Own Teacher Act, IL P.A. 93-802 of 2005, legislation outlined criteria for recruiting potential candidates (e.g., commitment to pursuing postsecondary coursework) but did not mention race or ethnicity specifically. Without this focus, participant demographics may mirror those of the existing teacher workforce rather than those of the student population (Gist et al., 2019).

Despite the growing interest in and prevalence of GYO programs generally and high school curricular GYO programs more specifically, there is little credible evidence on their effectiveness (Edwards & Kraft, 2024; Gist et al., 2019). To date, the evidence base is largely limited to suggestive self-studies of the early returns to pre-collegiate GYO programs. For example, Villagómez et al. (2016) discuss parameters and first-year engagements of the Oregon Teacher Pathway (OTP), a state-funded GYO initiative in rural Oregon designed by the study's authors. Similarly, Hill and Gillette (2005) discuss the design and results of the foundation-funded Paterson Teachers for Tomorrow (PT4T) GYO initiative in Paterson, New Jersey. Through two years of PT4T, about 90 high school students participated in Future Teachers of America clubs and 19 students had received college scholarships on the condition that they major in education and return to teach in Paterson's public schools.

The current study aims to fill this gap in the literature by providing credible, arguably causal estimates of the impact of a statewide pre-collegiate GYO program, the Teacher Academy of Maryland, on high school students' educational attainment, entry into teaching, and earnings.

2.2. Teacher Academy of Maryland

The state-sponsored Teacher Academy of Maryland (TAM) program that we study targets high school students with course offerings whose credits can transfer into college and provide initial steps towards full teacher certification.[1] TAM was designed and implemented in 2004 with support from a grant from the University System of Maryland (USM), the governing body of the State's public higher education system, with the goal of addressing teacher shortages within the state. Key collaborators and stakeholders include the Maryland State Department of Education, the Maryland Higher Education Commission, county-based local education agencies (LEAs) or districts, and two-year community colleges and four-year institutions of higher education governed by USM.[2]

Importantly, while TAM has the same features of high school GYO programs—and today uses the term GYO in state and local documentation[3]—that was not the case when it was initially implemented. Instead, early documentation focused on TAM's placement with the state CTE sequence. Similarly, early documentation of the program mentioned but did not focus on teacher diversity as central to the program's mission. However, our own policy tracking of more recent documentation indicates that roughly two-thirds of school districts identify TAM as critical to a statewide goal of diversifying the profession. As such, we view our analyses as an evaluation of the TAM program, with clear connections to—and implications for—modern-day GYO and other localized teacher recruitment efforts, as well as efforts to diversify the teacher workforce.

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1 Maryland State Department of Education. (n.d.). Human Resource Services. Retrieved January 5, 2024, from https://www.marylandpublicschools.org/programs/Pages/CTE-Programs-of-Study/Clusters/HRS.aspx

2 To start a TAM program, each district is responsible for working with institutions of higher education to establish articulation agreements that detail the postsecondary education benefits available to students who complete the TAM program. Local governance of TAM generally lies in CTE offices and with district CTE directors.

3 In a 2022 competitive grant competition drawing down Covid-relief funds, the state asked school districts to describe expansion of existing or development of new GYO programs—almost all applicants mentioned TAM. See: https://marylandpublicschools.org/about/Pages/MDLeads/index.aspx. Similarly, the state department of education and local education agencies prominently reference TAM in their written plans for implementation of the Blueprint for Maryland's Future, a 2021 law requiring substantial state and local investment to "strengthen and diversify the teacher workforce." See https://aib.maryland.gov/Pages/local-school-systems.aspx

Today, all 24 county-based school systems offer the program, though there is variation within districts and across schools in exposure to TAM—as well as the timing of adoption—facts we exploit in our analyses. During the period we focus on, TAM was available in 16 out of 24 districts. While districts liaise with the state and with higher education institutions regarding memoranda of understanding and articulation agreements for credit transfer, the access point for students is through their home high school. School-cohorts are the primary level at which treatment varies and the source of identifying variation in our analysis.

Within participating schools and cohorts, TAM students enroll in a structured sequence of four college-level courses that are aligned to the Maryland Associate of Arts in Teaching (A.A.T.) degree: Human Growth and Development, Teaching as a Profession, Foundations of Curriculum and Instruction, and an Education Academy Internship (e.g., fieldwork in classrooms). Because of the set course sequence, TAM generally takes three years to complete with course-taking beginning in tenth grade. Administrative data confirm that most TAM participants begin in tenth grade and that TAM completers generally do so in three years. Accordingly, we identify school-cohorts as treated if they were exposed to TAM for at least three years (see online Appendix Table 1).

TAM participation (and completion) can lead to several career trajectories. Upon completing all four courses, students are encouraged to take a ParaPro assessment to earn an industry-recognized certificate for immediate employment as an instructional assistant or paraprofessional. Because the state also offers a CTE program on early childhood education—where training and credentialing overlap with instructional support positions—a more desirable outcome of TAM is that students transition from high school to college in pursuit of a teaching degree. Students can transfer TAM credits earned in high school to two- or four-year degree programs by submitting a course/program completion verification form signed by their high school principal and guidance counselor. If students start in a two-year program, they must then transfer these credits to and eventually earn a bachelor's degree from a four-year institution to become a fully licensed teacher in a Maryland public school. High school course credits can transfer into college regardless of whether TAM students completed all four courses in the sequence and whether they passed the ParaPro licensure exam.[4]

3. Data

3.1. Data Construction

We integrate publicly available data on TAM rollouts at the school-year level[5] with student/person-level administrative data from the Maryland Longitudinal Data System (MLDS) Center. MLDS is a state agency and data repository that links person-level data from several other state agencies including: (i) Maryland public primary and secondary schools (provided by the Maryland State Department of Education); (ii) all public and private higher education institutions in the state (provided by the Maryland Higher Education Commission) and out-of-state college enrollment data for students who graduated from a Maryland public high school (from the National Student Clearinghouse); (iii) employment in K-12 public schools in the state (also supplied by the State Department of Education); and (iv) quarterly wages (in 2023 real dollars) collected from Unemployment Insurance (UI) records (provided by the Maryland Department of Labor).

Our analytic sample contains five cohorts of entering ninth graders who began high school between the 2008-09 and 2012-13 school years, which is the last cohort of entering high school students for whom we can observe meaningful labor market outcomes. There are about 320,000 unique students in these five cohorts, though the main analytic sample excludes students in always-

[4] Under some district-higher education institution agreements, students may be eligible to apply for a modest scholarship of about $500 per semester. While there is some variation in the incentives available to students based on their districts' articulation agreements and memoranda of understanding with higher-education institutions, our conversations with program leaders and coordinators indicate that colleges and universities have little to no involvement in TAM implementation. To understand TAM rollout and implementation, the research team undertook a quasi-qualitative inquiry by reviewing program documentation and contacting TAM program coordinators in all LEAs. These data did not identify any systemic reason why some schools implement TAM and others do not, beyond local resource constraints (e.g., availability of teachers to teach TAM courses, space to offer the courses, etc.).

[5] Maryland Public Schools CTE Enrollment Dashboard. (n.d.). Retrieved January 5, 2024, from https://www.mdctedata.org/dashboards/schoolprogram.php?p=130150&l=25&y=2010&pl=25

treated schools, yielding about 226,000 unique students in the analytic sample. We also exclude incoming transfers (i.e., students whose first observed grade of enrollment is tenth grade or later) because of concerns of self-selection and the fact that we cannot observe their full high-school history.

Students are observed for ten or 11 years (up to 2023) after starting ninth grade, which includes the following steps on the pathway to a career: During high school, we observe enrollment in a TAM course, completing all four courses in the sequence, earning a TAM certificate, and high school graduation, all within six years. In college, we examine effects on enrollment in either two- or four-year degree-seeking programs within seven years; completion of an associate's (AA) degree within eight years or a bachelor's degree (BA) within ten years; and receipt of an AA in education within eight years or a BA degree in teaching in ten years. Finally, labor market outcomes include employment as an instructional aide or full-time teacher of record in Maryland K12 public schools; characteristics of teachers' first school district relative to their high school district (i.e., "local" or same district and, if not local, whether or not the district employs 10pp more Black teachers or has a higher starting salary than their high school district[6]), and license type (i.e., traditional versus alternative[7]); and wages either in teaching or in any other profession where employers must submit unemployment insurance (UI) data to the state. Most labor market outcomes are captured 10 years after ninth grade, while wages are measured 11 years after ninth grade, at approximately age 25, which requires excluding one cohort

[6] To compare characteristics of the district that teachers start working in and their high school district, we examine the racial characteristics and baseline teacher salary for both in the same year (i.e., the year they started teaching). With regard to racial characteristics of districts' teachers, we focus on a 10pp increase due to the distribution of teacher race across the state. Districts in Maryland tend to fall into three broad categories of racial demographics of their teacher workforces: 17 districts have teacher workforces with zero to 10% Black teachers, five districts have 10% to 20% Black teachers, and two districts (i.e., Baltimore City, Prince George's County) employ 40% or more Black teachers. A 10pp increase is roughly equivalent to a 1 SD increase in the share of Black teachers. Other thresholds lead to similar conclusions. Share of Black teachers is positively correlated with starting salary, though the relationship is not strong (r = 0.22).

[7] We define an alternative teaching certificate as including two types: resident teacher certificate for individuals who went through a state-approved alternative-route teacher certification program (e.g., Teach for America, Baltimore Teaching Residency), and conditional certificates for individuals not in a specific program who need to complete steps in the certification process (i.e., completing exams and/or completing coursework). From the state's perspective, there are substantive differences between an alternative pathway that is aligned versus not aligned with a state-approved program. However, in practice, the program-aligned resident teacher certificates are very rare at roughly 15% of the alternative pathway certificates in our sample. Therefore, we do not disaggregate the two.

from the analytic sample. We choose expanded time horizons for each outcome rather than "on-time" measures, as time-to-event distributions indicate that a narrower time frame would differentially censor students by race/ethnicity. For example, Black and Hispanic students who become teachers do so in roughly ten years, on average, compared to roughly nine years for White students. Specific data construction decisions are outlined in the online appendix.

3.2. Characteristics of Treated Schools

Table 1 provides characteristics of Marylnd high schools, split into three groups: (i) never-treated schools, where none of the cohorts in our analysis sample were exposed to TAM (*n* = 137 high schools across 17 out of 24 county-based school districts in the state); (ii) always-treated schools, where all cohorts in our sample were exposed to TAM (*n* = 50 schools across 12 districts); and (iii) sometimes-treated schools that first adopted TAM in the timeframe of our analyses (*n* = 20 schools across nine districts) and contribute the primary identifying variation for our analyses. We link students to these schools based on the first high school they enrolled in for ninth grade.

[INSERT TABLE 1 ROUGHLY HERE]

The set of sometimes-treated schools come from a demographically and geographically diverse set of districts across the state. The largest share (30%) come from Prince George's County Public Schools (PGCPS)—a middle-class, predominantly Black region adjacent to Washington D.C.—with additional sometimes-treated schools in other large school systems along the D.C/Baltimore corridor (e.g., Howard, Montgomery) and in more rural counties—some of which primarily serve White students (e.g., Carroll, Queen Anne's) and others that have larger shares of Black students (e.g., Calvert, Kent). Never-treated schools, which also are included in our analytic sample, come from the eight largest school systems in the state (e.g., PGCPS, Montgomery, Baltimore City) that together make up 75% of the student population, and from 10 of the remaining 16 smaller districts.

Within districts, the set of TAM schools—including sometimes and always treated schools—enrolled fewer low-income students eligible for free and reduced-price meals (FARMS) (31%, compared to 40% for never-TAM schools and 36% for the state as a whole; see Table 1). These patterns are driven, in part, by the small share of TAM schools in Baltimore City, which has the highest FARMS rate in the state (77%). Early adopters of the program (i.e., always-TAM schools) had higher baseline test scores in math and English language arts (ELA) than the state as a whole (0.15 and 0.16 SDs, respectively), as well as fewer Black students (29%) than sometimes- and never-treated schools (both at 37% Black).

In Table 1, we also summarize the characteristics of students who started and completed the program (across all three school groups). Students who started TAM, meaning that they took at least one of the four courses, were more likely to be White (51%) and female (85%) than the statewide population. This comports with characteristics of the teacher workforce in Maryland and nationally. Roughly one-third of students who started TAM finished all four courses and a slightly smaller share earned their certificate, meaning that they also passed the ParaPro licensure assessment. TAM completers were more likely to be White and had higher ELA achievement at baseline.

In never-treated schools, a small share of students started TAM (0.2%) and finished the program (0.1%) due to two reasons. First, some students transferred schools after ninth grade and may have self-selected into a TAM school. Because of this, our analyses exploit access to TAM based on students' initial ninth grade school. Students in never-treated schools may also have accessed TAM by taking TAM courses outside of their home school. This is also why the number of schools represented in column 5 (TAM starters) and column 6 (TAM completers) in Table 1 is higher than the sum of always-treated and sometimes-treated schools. That said, the rate of "non-compliance" is exceedingly small and, if anything, such contamination suggests our estimates are lower bounds.

3.2. Summary Statistics for Full Analytic Sample and Subgroups

Table 2 summarizes the main analytic sample of students in sometimes- and never-treated schools, overall and for distinct demographic groups. Panel A of Table 2 summarizes access to and engagement with TAM. About 10% of students were exposed to TAM, with equal shares of girls and boys—an expected pattern given that girls and boys attend the same schools. Notably, exposure to TAM also was similar for Black and White students.

[INSERT TABLE 2 ROUGHLY HERE]

Overall, 0.7% of the analytic sample engaged with TAM, including 5.6% of students with direct exposure to the program (i.e., treated cohorts in sometimes-treated schools). However, consistent with prior literature and national trends on interest in teaching and its proxies (e.g., Cooc & Kim, 2023; Ellison et al., 2025; Kraft & Lyon, 2022). Girls were about five times as likely as boys to participate in TAM. In Figure 1, we show that, amongst treated cohorts, roughly 12% of White females took at least one TAM course, compared to 9% of Black females. While roughly one-third of White females who started the program earned a certificate, only about 4% of Black females did so. Amongst boys, Black boys took up the offer at the highest rate (3%), but—like Black females—were less likely than White boys to complete the program conditional on starting (3% versus 24%). Given these trends, in our main analyses, we document the effects of TAM on average across all students, separately for Black and White girls, and for boys as a broad group given more limited sample sizes and statistical power for boys disaggregated by race/ethnicity. We explore takeup and outcomes for additional subgroups (e.g., Black versus White boys, Hispanic students, Asian American and Pacific Islander [AAPI] students) following the main results.

[INSERT FIGURE 1 ROUGHLY HERE]

Panel B of Table 2 summarizes students' long-run educational and labor market outcomes. The six-year high school graduation rate for these cohorts was about 90%, with slightly higher graduation rates for girls (92%) relative to boys (88%) and for White girls (95%) relative to Black girls

(90%). These numbers are slightly higher than national data, largely driven by reporting of four- or five-year graduation rates (Atwell et al., 2019). Qualitatively similar patterns exist for college enrollment and graduation, which, again, resemble national figures.

Teaching is a rare outcome, which is unsurprising given the multitude of other college majors and occupations available to students: slightly more than 1% of students earn a BA in teaching and/or become a teacher. Consistent with national data on the demographic composition of the teaching force (Gershenson et al., 2021; Putman et al., 2016), girls and specifically White girls were significantly more likely than their Black and male counterparts to major in education and/or become a teacher. For example, girls were about six times more likely than boys, and White girls were four times more likely than Black girls, to major in education and/or become a teacher.

Two-thirds of Black girls who became teachers worked locally in the same school district in which they attended high school. The remaining one-third of Black girls who became teachers worked in a district that employed 10pp more Black teachers than their high school district. These other districts also appear to have higher baseline teacher salaries, on average, than students' home districts. Local teaching also was common for White girls (57%) but slightly less common compared to Black girls. Conditional on becoming a teacher, Black girls were substantially more likely than White girls to enter the profession through an alternative route that bypassed traditional undergraduate teacher education (51% versus 10%), with the vast majority hired under a "conditional" license that provided teachers three years to complete the required steps (e.g., coursework, testing) to transition to a full, standard license. On average, boys were more likely than girls to enter the profession through an alternative route (31% versus 18%).

Unsurprisingly, rates of becoming a teacher in a Maryland public school were substantially higher amongst TAM starters (9.8%) and TAM completers (20%; see Table 1) than among the full student population (1.3% in sometimes- or never-treated schools, and 1.4% when also including always-treated schools). Our identification strategy aims to tease out how much of this difference is driven by students who would not have pursued teaching without TAM versus self-selection into the program amongst students with a predisposition to teach. The fact that far fewer than 100% of TAM students became teachers also suggests that the program could benefit students who ultimately chose a different career.

Finally, the wage data show that Black girls were about 10% more likely to show positive earnings than White girls, but earned significantly less, on average, to the tune of almost 40%. The wage differential between Black and White boys was even larger (over 70%). Individuals missing wage data were unemployed, worked out of state, or worked in state for an employer not required to submit UI insurance records (e.g., federal agencies, self-employed individuals).

4. Identification Strategy

Our goal is to estimate causal effects of TAM on long-run student outcomes. We do so by exploiting conditionally random variation in TAM adoption between cohorts within the same school and between treated and untreated schools. Specifically, we estimate generalized difference-in-differences (DD) models of the form

$$y_{ist} = \beta TAM_{st} + \theta_s + \delta_t + \varepsilon_{ist} \tag{1}$$

where *y* is a long-run outcome of interest for student *i*, who entered high school *s* in year *t*; θ and δ are school and cohort fixed effects (FE), respectively; ε is an idiosyncratic error term; and *TAM* is the variable of interest: a school-cohort specific indicator of whether a particular cohort was exposed to TAM for three or more years. Because equation (1) focuses on exposure to TAM, we interpret β as the intent-to-treat (ITT) effect.

Our primary estimates cluster standard errors at the school level. In sensitivity analyses, we show that inference is robust to instead clustering at the school-year or district-year levels (Abadie et al., 2023). Equation (1) can be augmented to include a vector of student and time-varying school characteristics, including gender, race/ethnicity, eligibility for FARMS, multilingual learners, special education (SPED), lagged standardized math and ELA test scores from 8th grade, total high school enrollment, and indicators for imputed values of these baseline controls.

In a generalized DD framework with two-way fixed effects (TWFE), the school and cohort FE are central to our identification strategy: school FE control for selection into schools, school quality, and school support for TAM, while the cohort FE control for time-varying aggregate (statewide) shocks common to all students in each cohort. The main identifying assumption is that the timing of a school's TAM adoption is independent of the quality and teaching interest of a particular cohort and of other school-specific initiatives being undertaken. We first probe the plausibility of this assumption by estimating variants of equation (1) that replace y with pre-determined student characteristics such as gender, race/ethnicity, and socioeconomic status to test whether TAM "affects" these things; it does not (see online Appendix Table 2). This regression-based balance test indicates that within a school, treated and non-treated cohorts are observably similar. Similarly, TAM adoption is unrelated to several school-level factors: cohort size, principal turnover within the two years prior to TAM adoption, the number of full-time equivalent (FTE) staff/teaching positions, and student-staff/teacher ratios. Estimates of equation (1) are robust to the inclusion of the aforementioned set of controls, school linear time trends, and observable principal and teacher characteristics.

We can also state the identifying assumption in terms of a parallel trends assumption: had a school not adopted TAM, trends in their students' average outcomes would have evolved in parallel fashion to corresponding trends in their non-TAM counterparts (Roth et al., 2023). We probe the plausibility of this assumption by examining event-study models that estimate "effects" of TAM for cohorts who were not exposed to TAM. The presence of systematic and significant differences in average outcomes between TAM and non-TAM schools just before TAM is introduced would suggest the assumption is violated.

Specifically, in Figure 2, we replace the binary treatment in equation (1) with a full set of leads and lags (Roth et al., 2023), focusing on the first stage—started TAM—and for our main outcome—became a teacher. (See online Appendix Table 1 for coding of event-time indicators.) For the first stage, point estimates for the pre-trend coefficients are fairly precise zeros, which is somewhat mechanical since TAM was unavailable in the school, but is nonetheless reassuring given the possibility that students could, in theory, enroll in TAM programs offered at neighboring schools. After adoption of TAM at the school-cohort level, there is a sharp increase in the likelihood that students took at least one TAM course, driven by Black and White girls. The slight increase in takeup in the second year of adoption may be due to program maturity or to the fact that the first year of adoption captures effects for students exposed for three or four years (depending on if they were in ninth or tenth grade at the time of adoption) while the second year of adoption captures effects for four years of exposure. In the bottom panel of Figure 2, focused on becoming a teacher, pre-period trends are flat and indistinguishable from zero in the full sample, for Black and White girls separately, and for boys. Post-treatment estimates resemble the average effect reported below in the main results and are consistent in magnitude in the first year of adoption and in later cohorts.

[INSERT FIGURE 2 ROUGHLY HERE]

From equation (1), our ITT estimates provide evidence on the effects of exposure to TAM, which is policy relevant given that the state and school districts only can offer the program. It is unlikely that TAM ever will be required for high school graduation. Treatment-on-the-treated can be inferred by scaling the ITT estimates by the first stage (roughly a factor of 10; see Table 3 below). However, we interpret these estimates with caution as they may be biased by the failure of the exclusion restriction. Specifically, there may be spillover effects on non-participants if participants discuss TAM with their peers.

5. Main Results

5.1. First Stage and TAM Program Takeup

Table 3 reports TWFE estimates of equation (1), in which the outcomes are different measures of TAM involvement (i.e., first-stage regressions). We conduct this exercise to understand program takeup and to cross validate subsequent ITT estimates on long-run outcomes. Column 1 shows that TAM exposure significantly increased takeup, as defined by enrolling in at least one TAM class, particularly among girls. Takeup was similar and statistically indistinguishable between Black girls (7pp) and White girls (8pp).[8] Takeup for boys (across all race/ethnicity groups) was far lower (1pp, $p < 0.001$ on difference in takeup between boys and girls), but our estimates are still statistically significantly different from zero. (See online Appendix Table 3 for effects for girls, on average across race/ethnicity groups.)

[INSERT TABLE 3 ROUGHLY HERE]

Of course, beginning TAM does not guarantee completion, and any multi-year high school program is likely to experience some attrition. This may be particularly true in a program like TAM, where the final course is a field/student teaching experience that requires students to travel to another public school in the same district. Columns 2 through 3 of Table 3 report additional variations of the first-stage estimates for two measures of TAM completion: finishing all required TAM courses, including the field experience, and earning a TAM certificate, which also requires passing a standardized knowledge assessment. The ParaPro assessment is a licensure exam required for instructional assistants. On average across the full analytic sample, about 1% of students finished their

[8] We test for statistically significantly different effects between Black and White girls by combining the estimation results from the two samples into one parameter vector and simultaneous covariance matrix, and then conducting a Wald test of coefficient equivalence.

TAM coursework and earned a certificate when their cohort was exposed to TAM. However, unlike in the case of takeup, there is a notable racial disparity in the effect of TAM exposure on girls' TAM completion rates (3.4pp for White girls and 0.3pp for Black girls, $p < 0.001$).

Notably, though, within each subgroup, the point estimates are similar between the two measures of completion, suggesting that most of the attrition is happening during the coursework phase. Prior research points out that teacher licensure exams may discriminate against Black women who otherwise would be successful in the classroom (Cowan et al., 2023; Goldhaber & Hansen, 2010). In our sample, though, it appears that most Black girls did not take the assessment in the first place.

These patterns in takeup and completion foreshadow possible racial disparities in program effects on educational and labor market outcomes. That said, it is worth keeping in mind that exposure to teaching via TAM—even absent completion of the four courses and a TAM certificate—might influence students' eventual educational and occupational choices due to exposure to the career and to information transmission. Further, while the TAM certificate is directly transferable to an instructional aide position (vis-à-vis credits and a passing score on the ParaPro licensure exam), it requires several additional steps to become a full-time teacher of record in a public school (i.e., enrolling in college, earning a BA). These steps are required for TAM completers, as well as students who started but did not complete the program.

5.2. TAM Effects on Becoming a Teacher

Next, we investigate the program's impact on long-run career outcomes. The first column of Table 4 shows effects on becoming an instructional assistant or aide, which is one possible outcome of TAM. On average across the sample, we find no effect. However, effects are negative and statistically significant for boys (-2pp/-108%) and Black girls (-3pp/-75%). Across the next set of columns, which focus on effects on becoming a full-time teacher, estimates are consistently positive, suggesting that exposure to TAM likely shifted some boys' and Black girls' career decisions from aide to teacher.

Column 2 uses the broadest possible definition of becoming a teacher, which simply indicates whether the student was ever observed as a teacher of record in a Maryland public-school classroom within ten years after ninth grade. Overall, exposure to TAM increased the likelihood of students becoming a teacher by 0.6pp, or 45%. In absolute terms, the effect is largest for White girls (1.4pp), followed by Black girls (0.7pp), and then boys (0.2pp)—which is entirely consistent with patterns of takeup. The ordering is different when effects are captured in percent changes (82% for Black girls, 59% for boys, and 39% for White girls) because, in the absence of TAM, Black girls and boys were substantially less likely to become teachers than White girls. That said, the difference between Black and White girls is not statistically significant ($p = 0.149$), while the difference between boys and girls is ($p = 0.018$).

[INSERT TABLE 4 ROUGHLY HERE]

GYO programs, like other localized teacher recruitment efforts, have an acute interest not just in supporting high school students to enter teaching but to do so locally. Therefore, in column 3 of Table 4, we estimate the effects of TAM on the likelihood that individuals become teachers in the same school district where they attended high school. For White girls, the estimate in column 3 is roughly two-thirds of the estimate in column 2 (0.9pp versus 1.4pp), which is similar to descriptive statistics on the share of White girls who teach overall versus teach locally (see Table 2). However, for Black girls (and boys), we find no effect of TAM on local teaching ($p = 0.078$ on difference between Black and White girls), which is different from descriptive patterns showing that Black girls are slightly more likely than White girls to teach locally.

We probe possible explanations for this pattern by examining the characteristics of districts that teachers moved to (if they did not stay local). We find that TAM induced Black girls to become teachers in districts that employed 10pp or more Black teachers compared to their home district (0.4pp/143%, $p = 0.037$ on difference from null effect for White girls) and to districts that had higher starting teacher salaries than their home district (0.5pp/239%, not statistically significantly different from White girls). These characteristics are overlapping: of the Black girls who taught in a district with 10pp more Black teachers, 58% also earned a higher salary. A 10pp or larger increase in the share of Black teachers means that Black girls moved from relatively rural districts (that tend to employ no more than 10% Black teachers) to more suburban districts (that tend to employ between 10% and 20% Black teachers), or to Baltimore City or Prince George's County which both employ 40% or more Black teachers. The findings also suggest that TAM's impacts on Black girls may be driven more by the rural and suburban districts, where there is room for a 10pp increase on the share of Black teachers. These findings shed some light on some of working conditions that may make teaching an appealing profession for Black girls, and are consistent with the primarily descriptive literature on occupational choice and retention decisions amongst Black teacher candidates and teachers (Bartanen et al., 2025; Boyd et al., 2005; Edwards et al., 2024; Goings, 2015; Reininger, 2012).

Subsequent columns of Table 4 examine the types of teaching certificates and licenses held by TAM-induced entrants into Maryland's teaching force. This exercise is important for two general reasons. First, license type may influence both teacher effectiveness and a teacher's tenure in the profession (Glazerman et al., 2006; Kane et al., 2008). Second, these results help us to understand the channels through which TAM works to increase the number of Maryland public-school students who go on to become teachers in the state. The last two columns in Table 4 estimate TAM effects on becoming a traditionally and alternatively licensed teacher in Maryland, respectively. These estimates should approximately sum to the estimates reported in column 2, as these categories are mutually exclusive. In absolute terms, the impact on entering teaching with a traditional license in the full sample (0.4pp) is about twice as large as the impact on entering with an alternative license (0.2pp). Because traditional licenses are substantially more common than alternative licenses, the relative differences are reversed in magnitude (47% versus 78%).

However, like with teaching location, when we split the sample by gender and race, interesting differences appear. TAM's impact on Black girls' entry into teaching is almost exclusively due to alternative licenses (0.7pp/156%), while White girls' entry is driven primarily by traditional licenses (1.2pp/40%, $p = 0.003$ and 0.045 for between-group differences in effects for traditional and alternative licenses, respectively). This finding is consistent with extant evidence that traditional teacher preparation programs discriminate against Black individuals (Carter Andrews et al., 2021; Sleeter, 2017) and that, at least descriptively, Black individuals make up a much larger share of teachers who went through alternative preparation routes compared to traditional ones, both in Maryland and elsewhere (Blazar et al., 2024; Redding, 2022). For boys, effects on becoming a teacher are driven by traditional routes into the profession (2pp/87%, no statistically significant difference relative to the effect for girls)—despite the fact that boys had an even higher rate of entering the profession through an alternative route than Black girls (Table 2).

This result also suggests that TAM is operating in different ways for different demographic groups—particularly for Black girls. A key feature of the program is offering dual credit classes that count towards a teaching degree, which should make obtaining a traditional license easier. However, TAM appears to induce Black girls to eventually enter the teaching force through alternative pathways in which the dual credit is irrelevant (from the perspective of earning a degree). This suggests that the TAM coursework and other programming influenced Black girls' occupational choice, at least in part, via information transmission as opposed to subsidizing the cost of a degree, which is consistent with the finding that program exposure increased TAM completion for White girls but not Black girls. Future iterations of TAM and GYO programs like it should cover all avenues through which they may bolster educational attainment, interest in the teaching profession, and eventual entry into the teaching profession. Event-study figures for additional teaching outcomes are shown in online Appendix Figure 1.

6. Possible Mechanisms and Externalities

6.1. TAM Effects on Educational Attainment and College Major Choice

We examine the potential channels through which TAM may impact students' occupational choice in Table 5, by examining the programs' effects on educational attainment milestones that are prerequisites for becoming a teacher (i.e., high school graduation, college enrollment, bachelor's degree) and college major choice. These analyses also provide useful information on possible unintended consequences of TAM: even students who do not complete TAM and/or choose not to pursue a teaching career may nevertheless be encouraged to think about their career plans in ways that increase their effort and motivation in high school. Sample sizes vary slightly across outcomes due to data decision rules we describe in the online appendix. For example, some students are missing high school graduation data if they moved out of a Maryland public school after ninth grade, whereas we can observe if anyone in the sample became a public-school teacher in Maryland.

[INSERT TABLE 5 ROUGHLY HERE]

Column 1 of Table 5 shows that, on average, high school graduation rates increase by 0.8pp, or 1%, for cohorts exposed to TAM. This effect is statistically significant and largely driven by higher graduation rates among girls and Black students. The effect for Black girls (2.2pp/2.5%) is roughly double but not statistically distinguishable from the effect for White girls (1pp/1%). These results are interesting, as teacher pathway programs are sometimes thought to work on the margin of changing college students' choice of major and/or career, and not on the high school completion margin (Carver-Thomas, 2018). That said, the TAM program is part of a CTE sequence specifically focused on students' transition from K12 into college or the workforce. Studies of CTE high schools—which provide career training in any number of fields—often find effects on high school graduation for boys (roughly 6-8pp/10%) but smaller effects for girls (1-4pp/1%-5%) that are similar to our estimates on TAM (Brunner et al., 2023; Hemelt et al., 2019).

Columns 2 and 3 of Table 5 report estimates for enrollment in two- and four-year higher education institutions, respectively. Overall, exposure to TAM increased the likelihood of enrolling in a four-year college by 1.8pp, or 6%, with no effects on two-year college enrollment. As with high school graduation, the effects on four-year college enrollment are driven by girls and Black students (2.6pp/8% and 2.2pp/8.6%, respectively; see online Appendix Table 3), though the specific subgroup effects we show in Table 6 are not statistically significant. Notably, the effect on two-year college enrollment for girls, on average across race/ethnicity groups, is negative (-1.5pp/-3%). Paired with the positive effects on four-year college enrollment, these patterns suggest that TAM shifted some always-going college students from two- to four-year institutions while also creating new post-secondary enrollees. For graduation, we do not find statistically significant effects for either two- or four-year degrees, except for a negative effect on two-year college graduation for Black girls (-1.2pp/-28%). The corresponding estimate for four-year college graduation for Black girls is positive but not statistically significant, aligning with patterns on enrollment.

Finally, given TAM's intent to encourage students to consider a career in teaching and its apparent impacts on postsecondary enrollments (if not graduation), we use transcript data to examine whether TAM influenced students' graduating majors. Specifically, columns 6 and 7 of Table 5 consider whether students earned an AA in education or BA in teaching, respectively. Credits accumulated from an AA in education could be transferred towards a BA in teaching—and provide a wage boost for instructional assistants—but an AA on its own is not sufficient to become a full-time classroom teacher. On average across the sample, effects are null. However, exposure to TAM increased the likelihood of Black girls graduating with an AA in education (0.1pp/105%, marginally statistically significant), and of graduating with a BA in teaching for White girls (0.9pp/23%) and boys

(0.3pp/68%). These findings are consistent with those from Table 4 on teacher licenses: White girls and boys induced by TAM to become teachers did so almost exclusively through a traditional license that requires a BA (or MA) in teaching, whereas Black girls entered the profession through alternative routes where a BA is required but not necessarily in teaching. However, unlike with licenses, the effects on BA in teaching between Black and White girls are not statistically distinguishable from one another.

In sum, the patterns observed in Table 5 suggest that TAM created new teachers through at least two distinct channels: increasing attainment (extensive margin)—particularly for Black girls—and changing "always-college going" students' degree type/major (intensive margin)—particularly for White girls and boys. In event-study analyses in online Appendix Figure 1, we show that effects on these educational attainment measures uphold the parallel trends assumption.

6.2. TAM Effects on Earnings

In Table 6, we estimate the impact of TAM on earnings. The motivation for this exercise is twofold. First, some of the results discussed thus far suggest that TAM increased the educational attainment of some students who did not go on to become teachers. It stands to reason, then, that the wage earnings of these students should increase as well. Second, in discussions of recruiting more individuals into the teaching profession and of diversifying the teacher workforce, it has been suggested that such efforts may actually exacerbate existing racial and gender wage gaps if teachers are recruited away from other, higher paying fields (Gershenson et al., 2022). Accordingly, we test whether, and in which direction, TAM exposure affected earnings using earnings data from the state unemployment insurance (UI) system.

[INSERT TABLE 6 ROUGHLY HERE]

While other outcome measures are captured anytime within a given interval (e.g., became a teacher within ten years of entering ninth grade), wages are captured exactly 11 years after first enrolling in ninth grade, which is typically at age 25. Because TAM boosts four-year college-going, earnings in earlier periods may be higher in the control group for individuals who transition into the labor market following high school or a two-year college program. We capture wages across four quarters in this 11th year, which we align to the academic calendar (i.e., quarters three and four from one calendar year, and quarters one and two from the next), and then average across those quarters.

Column 1 of Table 6 estimates the baseline model using an indicator for having earned positive wages in Maryland (i.e., appearing in the UI data with non-zero earnings). TAM effects here are precise zeros, indicating that exposure to TAM does not cause students to systematically exit the Maryland labor market (or to work in jobs that are not tracked in state UI records, such as federal employees, independent contractors, and those who work in another state). Accordingly, any potential impact on wages is on the intensive rather than extensive margin. Moreover, this result motivates our decision to drop individuals with missing earnings data from subsequent analyses without concern for differential (endogenous) attrition (Foote & Stange, 2022).

In columns 2 and 3 of Table 6, we estimate the impact of exposure to TAM on earnings in levels and logs, respectively, for the sample of individuals with positive earnings. The estimates in column 2 show positive and significant effects of TAM exposure on mean quarterly earnings, overall ($271 per quarter) and for Black girls ($643 per quarter, which is marginally significant). Column 3 reports estimates from log-wage regressions, which we prefer because they mitigate the impact of outliers and provide semi-elasticities. Once again, the impact of TAM on log wages is positive and statistically significant overall and for Black girls (now significant at standard thresholds). The point estimate of 0.16 indicates that TAM increased Black girl's average quarterly earnings by about 18%. This alone suggests there is value in the TAM program, regardless of its impacts on teacher labor supply. Moreover, this result should allay fears that GYO programs cause college-going students to re-sort into (potentially) lower-paying majors.

In columns 4 and 5 of Table 6, we present naive mediation analyses to examine the extent to which wage effects are driven by educational attainment versus occupational choice. Specifically, we estimate log-wage regressions that control for educational attainment indicators (i.e., high school graduation, college enrollment, college graduation) and becoming a teacher, respectively. The estimated TAM effects here are slightly smaller than that shown in column 3. Controlling for attainment attenuates the wage effect by roughly 20% overall and for Black girls, while controlling for becoming a teacher attenuates the effect by 8% for Black girls and 10% overall. Taken at face value, these results suggest that a small but non-zero share of the TAM effect on earnings is driven by changes in occupational choice and a slightly larger share is driven by students' educational attainment.

These mediation estimates are naive in the sense that they may be biased by the presence of intermediate confounders (Acharya et al., 2016) or the failure of sequential ignorability (Imai et al., 2010). The concern is that the observed mediators (attainment and occupational choice) are themselves influenced by other unobserved mediators (e.g., a change in work ethic or peer group). This is problematic because, even if our research design successfully identifies exogenous exposure to TAM, it does not do so for changes in work ethic, etc. However, the analyses pass a sensitivity analysis proposed by Imai et al. (2010) that correlates the errors across two models: the baseline equation that estimates the effect of TAM on the mediator, and a second equation that estimates the effect of TAM on wages conditional on the mediator. For all mediators, the correlations are zero to four decimal places, suggesting that sequential ignorability holds.

Finally, in Figure 3, we explore TAM's impact on the distribution of earnings by estimating fixed-effect quantile regressions using the estimator proposed by (Powell, 2022). This exercise is motivated by the heterogeneity by gender and race observed to this point and by evidence from Texas that the returns to college quality and college major vary across the earnings distribution (Andrews et al., 2016, 2022). Generally, TAM's effects on log wages for the 30th percentile and higher resemble the average (OLS) estimates reported in column 3 of Table 6. However, for both Black and White girls, the impact of TAM is notably larger at the bottom of the earnings distribution. For example, both Black and White girls' earnings at the 5th percentile was about 0.3 log points higher when they were exposed to TAM than their counterparts who were not. This is about twice as large as the mean effect for Black girls and eight times as large for White girls. This is consistent with the distributional returns to an education major (Andrews et al., 2022), as well as our own findings that TAM shifts some Black girls' course choices and options from instructional aide (a lower-paying job) to full-time teacher. This suggests that in addition to heterogeneity by gender and race, TAM's impact varied across the earnings distribution.

[INSERT FIGURE 3 ROUGHLY HERE]

In online Appendix Figure 1, the event-study analyses for wage outcomes show that pre-treatment trends generally are flat. For White girls, there is some evidence of a pre-trend in wages in both levels and logs. If anything, higher wages in the pre period would lead us to understate TAM's effects on wages for White girls. Indeed, when we condition our main estimates from equation (1) on school-specific linear time trends that account for the pre-trend, wage effects for White girls are larger (online Appendix Table 4).

6.3 Additional Subgroups

In online Appendix Tables 3a and 3b, we report the effects of TAM access on outcomes for additional race-by-gender subgroups not reported in the main results (i.e., girls, Black students, White students, Hispanic students, AAPI students, Black boys, and White boys; sample sizes and takeup rates for AAPI and Hispanic students by gender are too low to report separately). For girls, results fall somewhere between those already reported for Black and White girls, who make up 80% of the female population in the state.

For boys, there are some substantive differences in the magnitude of effects between Black and White boys. However, given limited sample sizes of boys who started the TAM program, we generally cannot detect statistically significant differences at conventional thresholds—hence our decision to pool the results for boys across race/ethnicity groups in the main analyses. For example, while the magnitudes of the coefficients on earning a BA in teaching are the same for both Black and White boys (0.3pp), effects on becoming a teacher in a Maryland public school are five times as large for White boys compared to Black boys (0.5pp versus 0.1pp, $p = 0.067$ on the difference). Further, we observe effects of TAM on several educational attainment outcomes of Black boys (1.3pp/1.6% for high school graduation, 1.9pp/9% for four-year college enrollment, and 1.6pp/14% for four-year college graduation), but not for White boys (though none of the between-group differences are statistically significant). Though exploratory, these patterns suggest that TAM likely created more Black male teaching majors/graduates through intermediary effects on educational attainment, but that these Black males ended up teaching elsewhere (out of state, private school) or not at all.

Hispanic and AAPI students enrolled in TAM at lower rates than Black and White students (Figure 1). For Hispanic students, we find null effects on all educational attainment and career outcomes. For AAPI students, we estimate decreases in two-year college enrollment and graduation (-5.8pp/-14% and -3pp/-28%) countered by increases in four-year enrollment (0.4pp/8%), as well as a meaningful effect on teaching locally in the same district they went to high school (0.6pp/139%). Event-study affirm these conclusions; results available upon request.

7. Robustness and Sensitivity Analyses

In this section, we probe the robustness of our results to possible threats to identification and inference, beyond the event-study estimates discussed above. In sum, the robustness checks and sensitivity analyses reported in this section reaffirm a causal interpretation of the results presented in Sections 5 and 6. Specifically, we show that the necessary parallel trends, no anticipation, and no confounding shock assumptions of DD estimators are plausible and that the TWFE estimates are unlikely to be biased by negative weighting or heterogeneous treatment effects. We also show that the statistical and economic significance of the results is quite robust to a variety of different, but reasonable, modeling decisions.

7.1. Alternative DD Estimator

In addition to the parallel trends assumption, the modern DD literature has identified an additional assumption necessary for OLS estimates of equation (1) to deliver causal estimates when treatment timing is staggered across units: homogenous treatment effects (Goodman-Bacon, 2021). This second assumption states that, when TAM is adopted, effects are similar in magnitude for subsequent cohorts. We address this concern by following the common practice (Roth et al., 2023) of estimating event-study versions of equation (1), shown above, as well as using the estimator proposed by Callaway and Sant'Anna (2021) (CSDID), which eliminates problematic comparisons by only using never-treated and not-yet-treated schools. In the same vein, we exclude always-treated schools from all analyses.

In Figure 4, we present coefficient plots for effects on starting TAM (first stage) and becoming a teacher (main outcome), by subgroup, for our preferred TWFE estimates alongside an alternative DD estimator proposed by Callaway and Sant'Anna (2021). Side-by-side comparisons for other outcomes are shown in online Appendix Figure 2. Overall, the CSDID estimates closely resemble the TWFE ones, for all outcomes and subgroups. Ninety-five percent confidence intervals for the CSDID estimates often are larger than for the TWFE estimates, though inference and qualitative patterns of results are not substantially different between the two estimators. Together, the patterns in Figure 4 and online Appendix Figure 2—in combination with the event-study analyses shown in Figure 2 and online Appendix Figure 1—suggest that our main TWFE estimates are not violating the main identifying assumptions of DD analyses.

7.2. Confounding Treatments

In online Appendix Figure 3, we probe another assumption of DD designs: no confounding treatments. To do so, we extend the event-study analyses to examine whether TAM's adoption coincides with large changes in staffing and staff-student ratios, which might indicate the presence of other interventions or policies. Schools that start a TAM program need to allocate some FTE to the program (generally one or two classes per semester), but they do not necessarily need to hire a new teacher given that all current teachers are equipped to teach TAM. In the statewide data, we observe that, in most implementing schools, one person taught the varied TAM courses, and that TAM courses made up roughly one-third of their overall courseload. If schools simultaneously implemented other new CTE or career-training programs, that likely would require new hires. If anything, though, we observe in online Appendix Figure 3 that the number of staff and teaching positions declined leading up to and immediately after TAM adoption. Trends in student-staff ratios are flat. These results imply that nothing "unusual" occurred in TAM schools around the time of TAM adoption that may confound our baseline estimates.

7.3. Heterogeneity Across Schools

Next, we ensure that the main results are not driven by any particular school. The motivation for this exercise is that the implementation and advertisement of TAM may vary across schools and we want to rule out the possibility that one particularly well run or enthusiastic TAM location drives the results. Online Appendix Figure 4 presents leave-one-out estimates of the effects of TAM on takeup, becoming a teacher, and log wages after iteratively dropping one (of 20) sometimes-treated schools at a time. For simplicity, we focus on average effects across the full sample. The estimates of the first stage and of becoming a teacher are remarkably robust to this exercise. As expected, the estimated wage effects are a bit more variable, yet here too each of the 20 estimates is positive and individually statistically significant. This reinforces the finding that throughout the state and across school locations, TAM was utilized, increased entry into the teaching profession, and increased wages.

7.4. *Specification Checks*

In online Appendix Tables 4 and 5, we present several additional sensitivity analyses of the main result that access to TAM increased the likelihood of becoming a teacher. Online Appendix Table 4 examines the robustness of the ITT estimate to conditioning on different sets of control variables and of its significance when clustering the standard errors at different levels. Column 1 reproduces the baseline estimate (with controls) from Table 4, which clusters standard errors at the school level, but also shows standard errors clustered at both higher (district-year) and lower (school-year) levels. For all subsamples, the standard errors and resulting statistical inferences are quite similar regardless of how they are clustered. We prefer to cluster at the school level because this is a conservative approach that is commonly used when students are nested in schools (Angrist & Pischke, 2009). However, because treatment varies not only across schools but also across cohorts within schools, it is also reasonable to cluster by school-cohort (Abadie et al., 2023). Similarly, because TAM adoptions are discussed at the district level, we investigate clustering by district-year. Ultimately, the choice is inconsequential.

In the next four columns of online Appendix Table 4, we consider different sets of controls. Once again, the magnitude and significance of the ITT estimate is robust to whether, and which, control variables are added to the model. Specifically, column 2 shows that the results are robust to excluding observed student- and school-level characteristics entirely. This is consistent with the balance test described in section 4 and online Appendix Table 2: TAM adoption is unrelated to within-school changes in the observable characteristics of students. Similarly, in columns 3 and 4 we augment the baseline model to condition on observable teacher characteristics (i.e., gender, race/ethnicity, experience, certification) or principal characteristics (i.e., gender, race/ethnicity, experience) and an indicator for a cohort experiencing a change in principal. The ITT estimates are robust to the inclusion of these controls, which provides additional support for a causal interpretation of the estimates representing the effect of exposure to TAM and not to a bundle of school-level changes enacted simultaneously. Principal turnover is the most obvious confounding shock that might occur at the same time as anticipation or adoption of TAM but adjusting for this does not change our findings.[9]

In column 5 of online Appendix Table 4, we add school-specific linear time trends to the baseline model (Angrist & Pischke, 2009; Goodman-Bacon, 2021). The estimates are qualitatively similar when doing so, which suggests both that the parallel trends assumption is plausible and that treatment effects are not varying over time, and is consistent with the event-study and CSDID estimates presented thus far. In column 6, we restrict the sample to exclude cohorts of students that entered ninth grade after the school had adopted TAM. The idea here is that students may seek out schools that have TAM, and this sort of selection is more likely to occur when a school is already known to offer TAM. It is reassuring, then, that this does not appear to be the case: the point estimates remain similar in size when making this sample restriction. In practice, we only drop 3% to 4% of the original sample because there are few later cohorts.

Finally, online Appendix Table 5 presents fixed-effect logit estimates (Chamberlain, 1984; Wooldridge, 2010), for models analogous to equation (1), for binary outcomes. These results mimic the baseline (linear) estimates, which is reassuring, particularly for the rare outcome of becoming a public-school teacher.

8. Discussion and Conclusion

Our findings on the Teacher Academy of Maryland provide novel evidence on the impact of a GYO program that has particular relevance to teacher labor markets, but also speaks to broader

[9] Notably, the racial/ethnic characteristics of teachers is not associated with the likelihood of becoming a teacher, contrasting other literature documenting race/ethnicity-matching effects on end-of-year outcomes, high-school graduation, and college enrollment (Blazar, 2024; Dee, 2004; Gershenson et al., 2022).

literatures on workforce development, occupational choice, and labor supply—and high-school interventions' capacities to influence those decisions.

Our findings are encouraging along several dimensions. TAM induced exposed high school students to become teachers in Maryland public schools at markedly higher rates than their counterparts: 47% in the full sample. Our ITT estimates are policy relevant and provide insight on how school-level adoption of the TAM program influences longer-run outcomes, recognizing that only a small subset of students participate. The magnitude of our main effect on becoming a teacher is quite similar to that found by Reback (2004), who examined the effect of introducing teacher preparation programs at elite colleges. When coursework is offered at the high school level, though, the pool of prospective teachers is substantially larger than that at four-year colleges and universities. Further, Reback's estimates may not generalize outside of selective four-year institutions, whereas TAM is offered across almost all county-based public-school systems in Maryland. Given perennial concerns about teacher shortages (Ingersoll, 2001; Sutcher et al., 2016) and more recent concerns that interest in teaching is rapidly declining (Kraft & Lyon, 2024), GYO programs like TAM can be a fruitful avenue for building pathways into the profession.

Approximately how many "extra" teachers did TAM produce, and at what cost? Combining our ITT effects with control-group means and the sample size of exposed students, we estimate that offering TAM increased the raw count of new teachers by 130 across our sometimes-treated school sample, or 2.3 new teachers per each of 57 implementing high school-cohorts. While this number may appear small at face value, it is meaningful relative to current teacher vacancies. Statewide, roughly 1,600 teaching positions are left unfilled after the start of each school year (higher during the height of Covid), equivalent to roughly 1.1 vacancies per each of roughly 1,400 public schools (Maryland State Department of Education, 2024). Our ITT estimates suggest that if TAM were scaled up to all public high schools the program would help to fill more than one-third of existing vacancies in the state. Naturally, implied TOT estimates indicate that enrolling in TAM creates many more new teachers, though we prefer the ITT effects for stronger identification and because *offering* TAM is most immediately in the control of state and local leaders.

On the cost side, the average per-year, per-school cost of implementing TAM is largely driven by instructors' time. Most schools that offered TAM had just one teacher who taught all four courses in the sequence (with more teachers in larger schools). These teachers generally already worked in the school before TAM began. With the start of TAM, instructors allocated an average of one-third FTE to the program, equivalent to $46,080 in average salary and benefits, or $32 per exposed student per year. Given the lack of causal evidence on teacher recruitment programs and policies writ large (Fleck et al., 2025)—let alone studies of cost-effectiveness—we are not aware of any reasonable benchmarks for comparing this cost, overall or per new teacher produced. In our view, this cost seems small relative to the large financial benefits of staffing schools and classrooms with high-quality teachers (Chetty et al., 2011).

GYO programs like TAM may also help diversify the profession, which is a central goal of many program designers and policymakers (Edwards & Kraft, 2024; Gist et al., 2019; Valenzuela, 2017). Unsurprisingly, TAM's effects often are driven by White females, who already are overrepresented in teaching. These findings are consistent with patterns of intergenerational transmission of teaching from parents to children (Jacinto & Gershenson, 2021), which tends to reproduce the existing demographic makeup of the teaching force. However, Black girls benefit too, with relative increases in becoming a teacher of 80%. TAM's effects on Black girls are not enough to outpace White girls, but gaps are reduced. In the absence of TAM, the rate at which White girls become teachers is four times the rate for Black girls (3.6% versus 0.9%). With TAM, rates increase to 5% and 1.6%, for White and Black girls respectively, shrinking the gap to a three-fold difference. In raw counts, we estimate that, for exposed cohorts, TAM increased the number of new White female teachers by 1 per school-cohort and new Black teachers by 0.5 per school-cohort—a two-fold difference. There also is substantial interest in supporting more Black males to become teachers (Goings & Bianco, 2016; Pabon et al., 2011), though TAM's effects are larger for White than Black boys in both absolute and relative terms.

For TAM, or any GYO program, to close this race gap entirely, effects for Black girls would need to be larger, or effects for White girls would need to be much smaller. It is unrealistic to expect a state-sponsored GYO program like TAM to solely benefit Black or other students of color. That said, if increasing diversity and representation is a policy goal—as public documentation on TAM indicates that it is in Maryland—program expansion efforts could target schools and districts with large populations of Black and other students of color not already offering TAM. Indeed, the set of "never-TAM" schools in our sample include larger shares of Black students (37%) compared to "always-TAM" schools (29%). Further, takeup of TAM by Black girls is concentrated in large urban school districts (e.g., Baltimore City, Prince George's County), where the program is implemented in a small subset of high schools and thus there is substantial room for expansion in those districts.

Another consideration regarding diversity in the teacher workforce is where teachers teach and how the characteristics of teachers compare to those of students. We find that White girls induced by TAM to become teachers tended to stay local (i.e., in their high school district), while Black girls induced by the program to teach frequently did so in districts with larger shares of Black teachers. Increasing the share of Black teachers in settings where there already is a moderate to large share of Black teachers would increase the likelihood that Black students in those settings have greater access to Black teachers. Black students also make up a large share of enrollments in districts that employ large shares of Black teachers. At the same time, this sort of movement—and Black teachers' desires to work in settings with large shares of other Black teachers—may decrease demographic representation and access to Black teachers in rural settings.

Policy efforts that seek either to expand or replicate programs like TAM must also consider potential mechanisms and how these differ between groups of students. White girls largely followed the path laid out by the program: we observe positive effects on take up and completion, receipt of a BA in teaching, and then on entry into teaching with a traditional license. For Black girls, program effects are positive but operate through a different channel: they took up the program at similar rates as White girls but were less likely to complete it. TAM has some effect on Black girls' receipt of an AA in education, suggesting that some TAM credits may be transferred from high school to college. Yet, coursework and degree do not translate into a four-year teaching degree. Instead, Black girls induced by TAM to become teachers did so almost exclusively with an alternative license that bypasses traditional undergraduate teacher education, which is consistent with descriptive patterns of teacher pathways in Maryland and in other states and settings (Bacher-Hicks et al., 2023; Backes & Goldhaber, 2023; Blazar et al., 2024; Redding, 2022). While TAM likely benefited White girls by making the process of becoming a teacher easier, vis-a-vis dual-enrollment credits, the effects for Black girls likely are driven by exposure to teaching as a realistic career and information transmission, as well as by increases in educational attainment.

Beyond effects on teaching, TAM's impacts on educational attainment and wages compare favorably with other CTE and job training programs provided in or shortly after high school (Bloom, 2010; Bonilla, 2020; Dougherty, 2018; Hemelt et al., 2019; Kemple & Willner, 2008; Page, 2012). We extend this literature, which often identifies positive effects for males who do not attend or do not graduate from college, by focusing on a career dominated by women and that requires an advanced degree. Wage effects are largest for Black women, echoing findings from Escobari et al. (2021) who argue that employment in education (and government) offer more equitable access to upward mobility for Black and Hispanic individuals than most other job sectors. That wage estimates are positive for most race and gender subgroups (though not always statistically significant) further addresses a common concern that recruiting individuals into teaching may pull them away from other higher-paying jobs (Gershenson et al., 2022)

Teaching is not amongst the highest-wage, highest-growth industries such as information technology and health care, where career training programs can have substantially larger effects than we observe (e.g., Stevens et al., 2019). At the same time, teaching as a profession taps into social and interpersonal skills that have increasing value in the labor market (Deming, 2017). Despite some efforts (Selwyn, 2019), teachers are unlikely to be replaced at scale by computer-assisted technology, and there are perennial teacher shortages that need filling (Maryland State Department of Education, 2024; Sutcher et al., 2016). Our results show that a GYO program in Maryland not only achieves its intended goal of producing more teachers for the state's public-school system, but also contributes to individuals' educational attainment and labor market success.

Data Availability Statement

The data that support the findings of this study are available from the Maryland Longitudinal Data System Center (MLDSC). Restrictions apply to the availability of these data, which were used under license for this study. Data are available at https://mldscenter.maryland.gov/ProjectApprovalandManagementProcedures.html with the permission of the MLDSC.

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Tables and Figures

Table 1. School and Student Analytic Sample by Treatment Participation

	All	Schools			Students	
	Schools/ Students	Never TAM	Always TAM	Sometimes TAM	TAM Starters	TAM Certificate
	(1)	(2)	(3)	(4)	(5)	(6)
AAPI	0.056	0.047	0.073	0.057	0.034	0.016
Black	0.345	0.367	0.292	0.368	0.334	0.250
Hispanic	0.100	0.099	0.104	0.090	0.051	0.024
White	0.435	0.427	0.455	0.424	0.511	0.645
Female	0.491	0.489	0.498	0.485	0.851	0.882
FARMS	0.361	0.396	0.307	0.314	0.298	0.257
Multilingual Learner	0.046	0.044	0.049	0.045	0.016	0.011
Special Education	0.135	0.142	0.119	0.139	0.090	0.088
Math Achievement	0.021	-0.032	0.147	-0.022	-0.031	0.092
ELA Achievement	0.058	0.008	0.159	0.058	0.077	0.153
Access to TAM	0.359	0.000	1.000	0.601	0.895	0.872
Started TAM	0.013	0.002	0.027	0.036	1.000	1.000
Finished TAM Courses	0.004	0.001	0.011	0.008	0.329	1.000
TAM Certificate	0.004	0.001	0.009	0.007	0.284	1.000
Become a Teacher	0.014	0.013	0.017	0.014	0.098	0.201
Schools	*210*	*137*	*53*	*20*	*156*	*94*
Observations	*318,753*	*189,783*	*92,910*	*36,060*	*8,428*	*4,792*

Notes: Sample restricted to 9th grade cohorts between the 2008-09 and 2012-2013 school years. Never TAM schools are schools in which no 9th grade cohort student was treated. Sometimes TAM schools are schools in which some cohorts participated in the program while others did not. Always TAM schools are schools in which all cohorts of students participated in TAM. This last set of schools are excluded from all analyses. AAPI = Asian American and Pacific Islander; FARMS = free and reduced-price meals; ELA = English language arts.

Table 2. Student Analytic Sample by Subgroups

	All	Boys	Girls	Black		White	
				Boys	Girls	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: TAM							
Access to TAM	0.096	0.096	0.096	0.093	0.092	0.090	0.090
Started TAM	0.007	0.003	0.012	0.004	0.011	0.002	0.015
Finished TAM Courses	0.002	0.000	0.003	0.000	0.001	0.001	0.006
TAM Certificate	0.002	0.000	0.003	0.000	0.001	0.001	0.006
Panel B: Outcomes							
High-School Graduation	0.895	0.873	0.918	0.823	0.890	0.923	0.947
2-Year College Enrollment	0.396	0.369	0.425	0.326	0.372	0.382	0.458
4-Year College Enrollment	0.292	0.258	0.328	0.218	0.303	0.296	0.358
2-Year College Graduation	0.082	0.071	0.093	0.032	0.042	0.096	0.126
4-Year College Graduation	0.257	0.207	0.308	0.109	0.203	0.280	0.397
Associate Degree Education	0.004	0.001	0.007	0.000	0.001	0.002	0.012
Bachelor Degree Teaching	0.014	0.004	0.024	0.002	0.009	0.006	0.039
Become a Teaching Aide	0.003	0.001	0.004	0.001	0.003	0.001	0.005
Became a Teacher	0.013	0.004	0.022	0.002	0.009	0.007	0.037
Locally	0.008	0.002	0.013	0.001	0.006	0.004	0.021
Dist. with 10% More Black Tchr.	0.002	0.001	0.003	0.001	0.003	0.001	0.004
District with Higher Salary	0.004	0.001	0.006	0.001	0.002	0.002	0.010
Traditional License	0.010	0.003	0.017	0.000	0.003	0.005	0.031
Alternative License	0.003	0.001	0.004	0.002	0.005	0.001	0.004
Positive Earnings	0.549	0.534	0.565	0.531	0.600	0.559	0.553
Mean Quart. Earnings (Non-Missing)	8,912	9,640	8,189	7,040	6,439	11,465	9,477
Log Earnings (Non-Missing)	8.719	8.790	8.649	8.428	8.377	9.038	8.843
Observations	*225,843*	*115,598*	*110,245*	*42,767*	*40,142*	*49,216*	*47,092*

Notes: In Panel A, each row indicates the proportion of the student subgroup in each TAM category. Access to TAM represents the proportion of students that enrolled in a school/cohort that participated in the TAM program in the years under analysis. Started TAM corresponds to the proportion of students that took at least one TAM course, and TAM completer the proportion of students that completed the program. In both Panel A and Panel B, the timeframe for each outcome varies. TAM participation and completion and high school graduation are tracked within six years of starting ninth grade. College enrollment (either in two- or four-year institutions) is captured within seven years. College graduation and degrees are captured within eight years for two-year institutions and ten years for four-year institutions. Employment outcomes are captured within ten years. Wages are captured within 11 years, at approximately at age 25, which requires reducing the number of cohorts by one. The number of observations between the different set of outcomes vary slightly based on the administrative data from which the outcome was retrieved.

Table 3. Effect of TAM Access on TAM Participation and Completion

	Started TAM	Finished TAM Courses	TAM Certificate
	(1)	(2)	(3)
All	0.041***	0.010***	0.009**
	(0.007)	(0.003)	(0.003)
Control Mean	*0.002*	*0.001*	*0.001*
F-Statistic	*9.508*	*2.095*	*1.829*
Observations	*225,843*	*225,843*	*225,843*
Boys	0.011**	0.001	0.001
	(0.004)	(0.001)	(0.001)
Control Mean	*0.001*	*0.000*	*0.000*
F-Statistic	*3.466*	*1.092*	*1.076*
Observations	*115,598*	*115,598*	*115,598*
Black Girls	0.068***	0.004	0.003
	(0.014)	(0.002)	(0.002)
Control Mean	*0.003*	*0.001*	*0.001*
F-Statistic	*13.60*	*1.396*	*1.286*
Observations	*40,142*	*40,142*	*40,142*
White Girls	0.083***	0.038***	0.034***
	(0.016)	(0.008)	(0.008)
Control Mean	*0.004*	*0.002*	*0.002*
F-Statistic	*5.610*	*4.538*	*4.333*
Observations	*47,092*	*47,092*	*47,092*
P-Values on Between-Group Differences:			
Boys v. Girls	*0.000*	*0.000*	*0.000*
Black v. White Girls	*0.425*	*0.000*	*0.000*

Notes: Estimates in each cell come from separate two-way fixed effect (TWFE) models of the effect of TAM access on TAM takeup/participation and two measures of completion. All models include school and cohort fixed effects, as well as student and school-year covariates. Standard errors are clustered at the school level. In the last set of rows, estimates are *p*-values comparing the magnitude of the treatment effect between boys and girls and between Black and White girls. The average effect for girls, across race/ethnicity groups, is reported in online Appendix Table 3. The control-group mean is calculated for all non-treated students, including non-exposed students in sometimes-treated schools and all students in never-treated schools. * p<0.10, ** p<0.05, *** p<0.001

Table 4. Effect of TAM Access on Employment, Characteristics of District, and Teacher License

	Aide	Teacher	Local	More Black Teachers	Higher Salary	Trad. License	Alt. License
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	-0.001	0.006***	0.003**	0.001*	0.001	0.004**	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Control Mean	*0.003*	*0.013*	*0.008*	*0.002*	*0.003*	*0.009*	*0.002*
Relative Difference	*-44.16*	*44.76*	*41.89*	*59.32*	*38.14*	*47.4*	*79.8*
Observations	*225,843*	*225,843*	*225,843*	*225,843*	*225,843*	*225,843*	*225,843*
Boys	-0.002**	0.002**	0.001	0.000	0.001	0.002**	0.001
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Control Mean	*0.001*	*0.004*	*0.003*	*0.001*	*0.001*	*0.002*	*0.001*
Relative Difference	*-108.10*	*59.22*	*21.15*	*52.82*	*87.40*	*86.61*	*74.06*
Observations	*115,598*	*115,598*	*115,598*	*115,598*	*115,598*	*115,598*	*115,598*
Black Girls	-0.003**	0.007**	0.002	0.004**	0.005**	0.001	0.007***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Control Mean	*0.003*	*0.009*	*0.005*	*0.003*	*0.002*	*0.003*	*0.004*
Relative Difference	*-74.65*	*82.23*	*37.49*	*142.7*	*238.7*	*32.96*	*155.8*
Observations	*40,142*	*40,142*	*40,142*	*40,142*	*40,142*	*40,142*	*40,142*
White Girls	0.000	0.014***	0.009**	-0.000	0.002	0.012**	0.001
	(0.002)	(0.004)	(0.003)	(0.001)	(0.003)	(0.004)	(0.002)
Control Mean	*0.005*	*0.036*	*0.022*	*0.004*	*0.01*	*0.031*	*0.004*
Relative Difference	*4.83*	*38.77*	*43.08*	*-3.33*	*16.34*	*39.76*	*29.76*
Observations	*47,092*	*47,092*	*47,092*	*47,092*	*47,092*	*47,092*	*47,092*
P-Values on Between-Group Differences:							
Boys v. Girls	*0.625*	*0.018*	*0.010*	*0.097*	*0.751*	*0.117*	*0.106*
Black v. White Girls	*0.188*	*0.149*	*0.078*	*0.037*	*0.232*	*0.003*	*0.045*

Notes: See notes to Table 3. Rounding control-group means and estimated impacts to three decimal places distorts some patterns when outcomes are very rare. For example, while Black and White girls have the same reported control-group mean for alternative certification even though there are differences (0.00423 versus 0.00362 for Black and White girls, respectively). However, the relative differences are calculated from unrounded control-group means and effect sizes. * p<0.10, ** p<0.05, *** p<0.001

Table 5. Effect of TAM Access on Educational Attainment and Degree

	HS Grad.	2-Year Coll. Enroll.	4-Year Coll. Enroll.	2-Year Coll. Grad.	4-Year Coll. Grad.	AA in Ed.	BA in Teaching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	0.008**	-0.008	0.018**	-0.002	0.010	-0.000	0.003
	(0.004)	(0.007)	(0.008)	(0.004)	(0.006)	(0.001)	(0.002)
Control Mean	*0.893*	*0.398*	*0.287*	*0.082*	*0.26*	*0.004*	*0.014*
Relative Difference	*0.95*	*-1.94*	*6.10*	*-2.54*	*3.66*	*-11.60*	*18.22*
Observations	*207,484*	*209,861*	*209,861*	*209,861*	*209,861*	*207,129*	*207,129*
Boys	0.005	-0.001	0.009	0.001	0.009	-0.000	0.003**
	(0.005)	(0.010)	(0.007)	(0.006)	(0.007)	(0.001)	(0.001)
Control Mean	*0.871*	*0.37*	*0.254*	*0.071*	*0.211*	*0.001*	*0.004*
Relative Difference	*0.53*	*-0.33*	*3.57*	*1.59*	*4.10*	*-3.57*	*67.79*
Observations	*105,368*	*106,388*	*106,388*	*106,388*	*106,388*	*104,954*	*104,954*
Black Girls	0.022**	-0.005	0.025	-0.012**	0.006	0.001*	0.001
	(0.009)	(0.013)	(0.017)	(0.006)	(0.018)	(0.001)	(0.003)
Control Mean	*0.888*	*0.376*	*0.295*	*0.043*	*0.207*	*0.001*	*0.008*
Relative Difference	*2.45*	*-1.28*	*8.51*	*-27.82*	*2.98*	*154.1*	*10.56*
Observations	*36,948*	*37,358*	*37,358*	*37,358*	*37,358*	*36,838*	*36,838*
White Girls	0.010*	-0.009	0.025	0.010	0.004	-0.002	0.009*
	(0.006)	(0.014)	(0.015)	(0.009)	(0.017)	(0.002)	(0.005)
Control Mean	*0.946*	*0.46*	*0.353*	*0.127*	*0.397*	*0.012*	*0.039*
Relative Difference	*1.06*	*-1.95*	*6.98*	*7.65*	*1.03*	*-16.72*	*22.50*
Observations	*43,895*	*44,560*	*44,560*	*44,560*	*44,560*	*43,995*	*43,995*
P-Values on Between-Group Differences:							
Boys v. Girls	*0.186*	*0.260*	*0.049*	*0.375*	*0.978*	*0.579*	*0.771*
Black v. White Girls	*0.293*	*0.845*	*0.981*	*0.036*	*0.933*	*0.197*	*0.172*

Notes: See notes to Table 3. * p<0.10, ** p<0.05, *** p<0.001

Table 6. Effect of TAM Access on Earnings

	Positive Earnings	Mean Quarterly Earnings	Log Earnings		
			No Mediation	Control for Ed. Attain.	Control for Tch.
	(1)	(2)	(3)	(4)	(5)
All	0.004	271.453**	0.051**	0.040*	0.046**
	(0.008)	(126.768)	(0.020)	(0.021)	(0.020)
Control Mean	*0.553*	*8838*	*8.711*	*8.71*	*8.71*
Relative Difference	*0.74*	*3.07*	*0.59*		
Prop. Mediated				*0.22*	*0.10*
Observations	*182,167*	*100,034*	*100,034*	*96,103*	*100,034*
Boys	0.005	176.051	0.019	0.020	0.018
	(0.012)	(171.656)	(0.020)	(0.024)	(0.081)
Control Mean	*0.537*	*9582*	*8.783*	*8.78*	*4.72*
Relative Difference	*0.90*	*1.84*	*0.21*		
Prop. Mediated				*-0.05*	*0.05*
Observations	*93,215*	*49,799*	*49,799*	*47,592*	*49,799*
Black Girls	-0.004	643.135*	0.163**	0.130	0.150**
	(0.016)	(333.551)	(0.074)	(0.081)	(0.074)
Control Mean	*0.608*	*6368*	*8.37*	*8.37*	*5.09*
Relative Difference	*-0.61*	*10.1*	*1.94*		
Prop. Mediated				*0.20*	*0.08*
Observations	*32,671*	*19,608*	*19,608*	*18,836*	*19,608*
White Girls	0.005	-18.882	0.041	0.029	0.031
	(0.014)	(243.800)	(0.033)	(0.030)	(0.032)
Control Mean	*0.556*	*9394*	*8.833*	8.833	8.833
Relative Difference	*0.85*	*-0.20*	*0.47*		
Prop. Mediated				*0.29*	*0.24*
Observations	*38,307*	*21,200*	*21,200*	*20,551*	*21,200*
P-Values on Between-Group Differences:					
Boys v. Girls	*0.919*	*0.393*	*0.115*		
Black v. White Girls	*0.675*	*0.125*	*0.146*		

Notes: See notes to Table 3. The last two columns examine the extent to which effects on wages are mediated by educational attainment or becoming a teacher, by controlling for these measures in models that predict wages. The proportion of the effect that is mediated is calculated as the difference in the effect on wages without the mediators minus the effect on wages with the mediators, divided by the effect without mediators. * p<0.10, ** p<0.05, *** p<0.001

Figures

Figure 1. TAM Takeup and Completion, by Race/Ethnicity and Gender

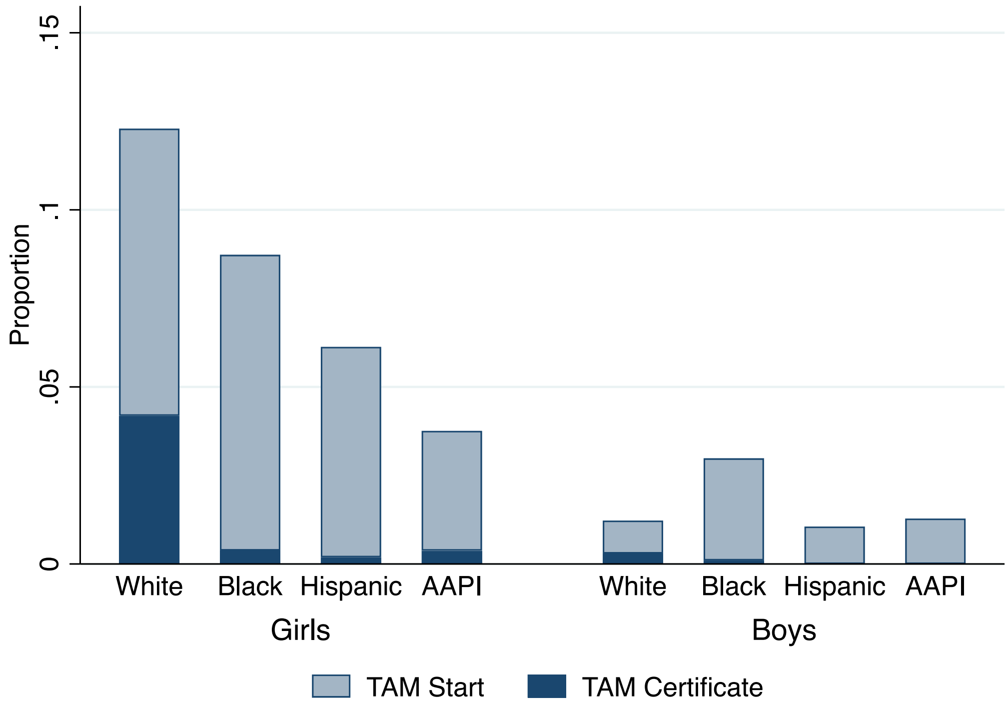
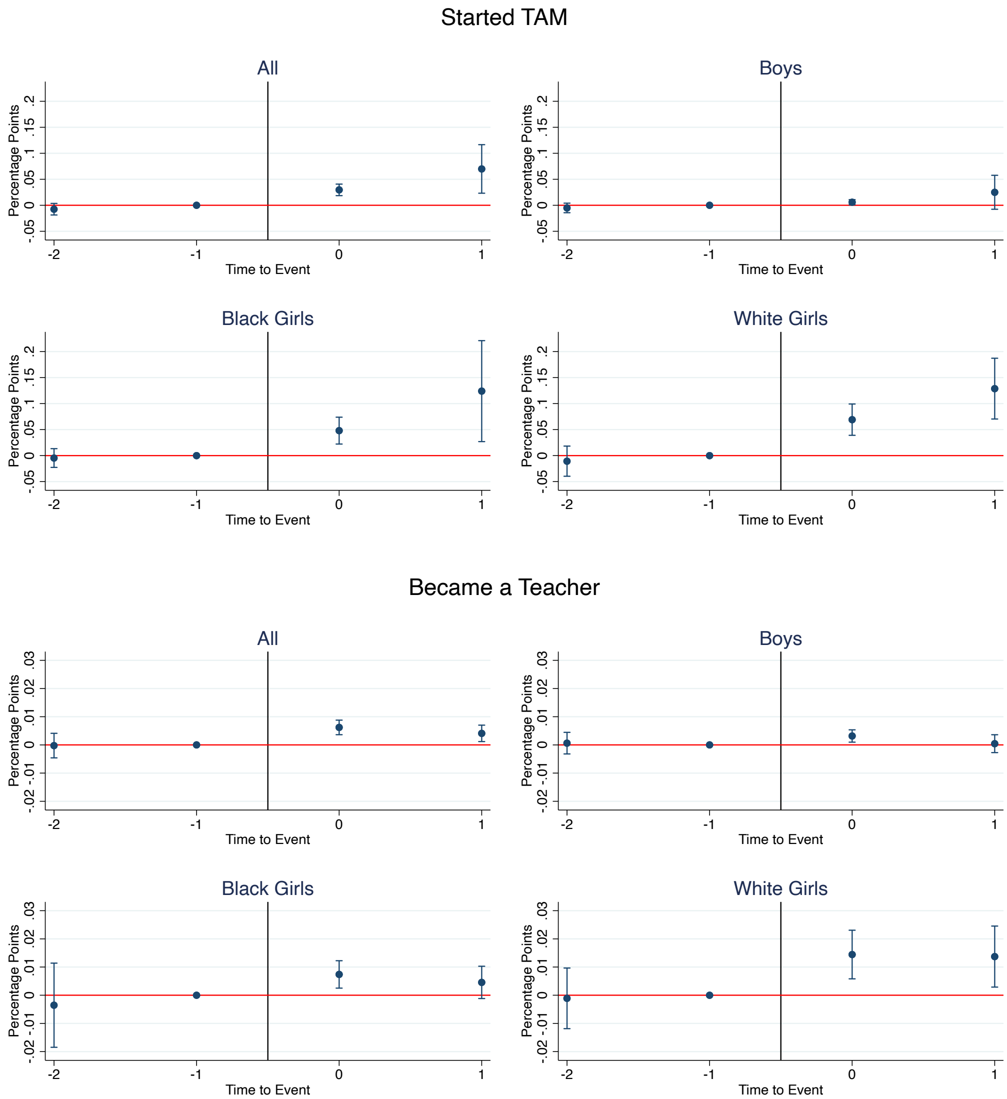
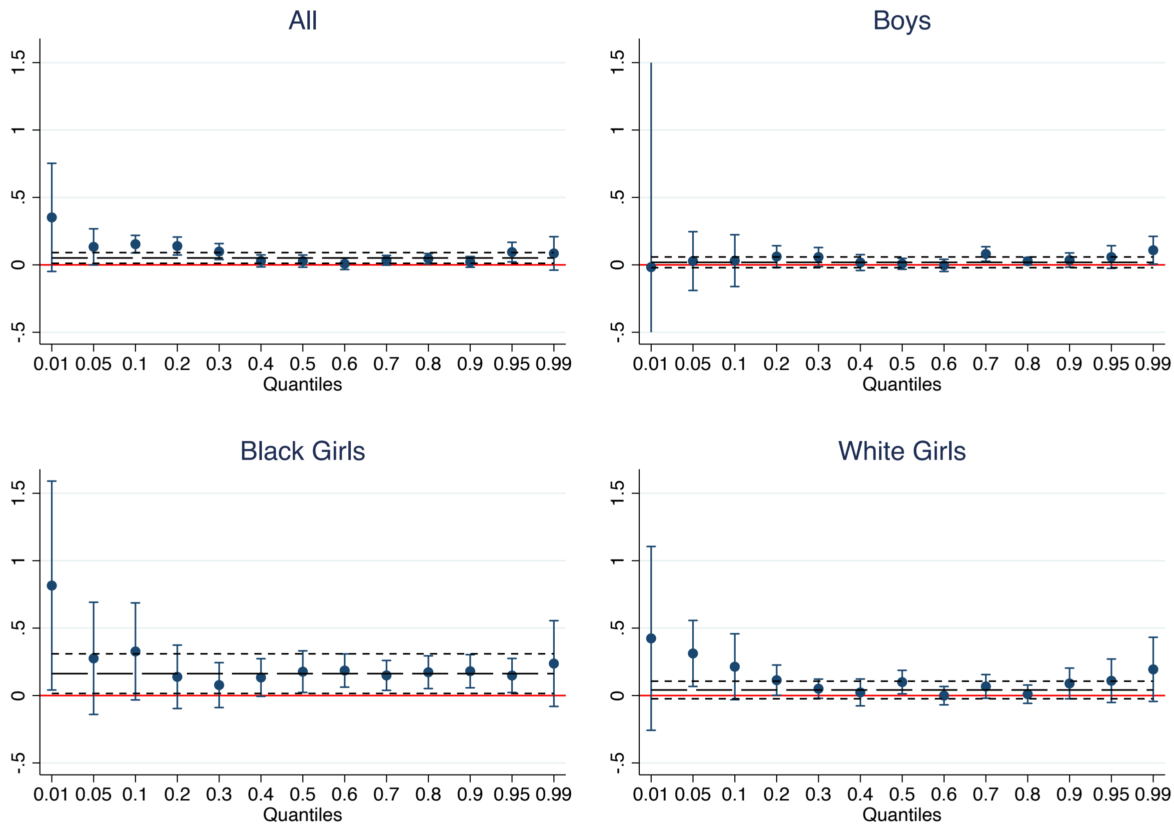


Figure 2. Event Study Analysis of the Effect of TAM Access



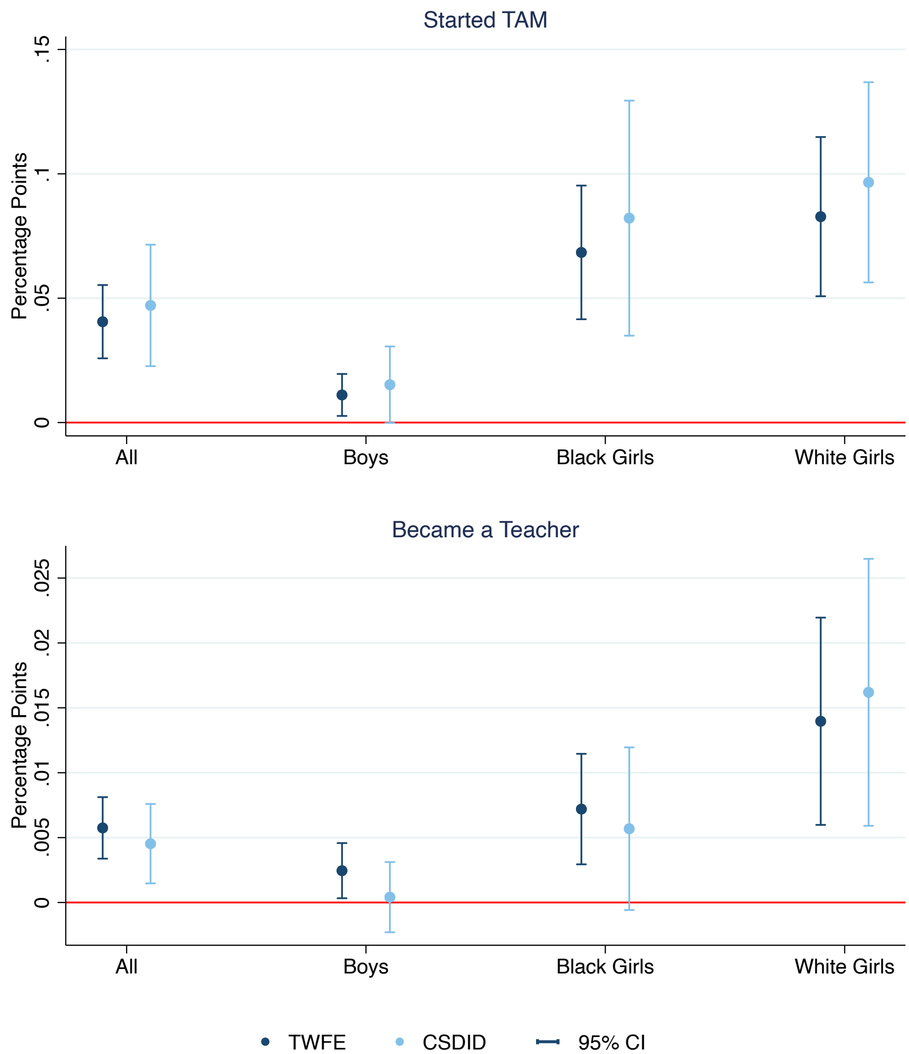
Notes: This figure reports event study point estimates and 95% confidence intervals from regression specifications that include lead and lag indicators for access to TAM as well as school and cohort fixed effects. Student and school-year covariates are excluded. The event time variable on the x-axis is a continuous variable, where zero identifies students who were in 9th or 10th grade/first or second year of high school when TAM was first adopted, meaning that they were exposed to TAM for three or four years. Positive values represent post-adoption cohorts, while negative values represent pre-adoption cohorts (i.e., students who were in 11th or 12th grade or post-graduation when TAM was first adopted). Due to limited sample size and precision, we pool event-time period two with period one, and event-time period negative three with negative two. Coding of event-time indicators is shown in online Appendix Table 1. Standard errors used to compute confidence intervals are clustered at the high school level.

Figure 3. Quantile Regression Estimates of the Effect of TAM Access on Log Earnings



Notes: This figure reports point estimates and 95% confidence intervals from quantile regression specifications that include school and cohort fixed effects. Point estimates correspond to the coefficient of treatment indicator of exposure to TAM for twelve quantiles of log earnings: 0.01, 0.05, 0.1, 0.2, 0.3, 0.4, 0.5, 0.6. 0.7, 0.8, 0.9, 0.95, and 0.99. Results are obtained using the quantile regression with panel data command, *qregpd,* in Stata. The reference lines in dashes correspond to average treatment effects of TAM on log earnings obtained with two-way-fixed effects (TWFE) models that include student and school-year covariates. The short dashes correspond to the 95% confidence interval of that average treatment effect estimate. For boys, the lower bound of the 95% confidence interval for the lowest reported quantile is larger than the y-axis range [-0.987, 0.953] and is truncated to not distort values for other groups and quantiles.

Figure 4. Estimates of Effect of TAM Access Using Alternative DID Estimators



Notes: TWFE estimates are the same as those reported in Table 3 (Started TAM) and Table 4 (Became a Teacher). CSDID estimates come from Callaway and Sant'Anna's (2021) estimator, implemented using the *csdid* command in Stata. Corresponding 95% confidence intervals are provided for both sets of estimates.

Supplemental Online Appendix

Data Construction

Several decision rules inform construction of the analytic sample and key variables. First, we identify relevant cohorts based on overlap between MLDS data and publicly available data on TAM rollout. We exclude the first year of MLDS data (2007-08) because we do not know if it is students' first time enrolling in ninth grade, nor do we have information (e.g., test scores) from the prior year to use as controls and to assess balance. The first year we can observe TAM exposure at the school-year level, through publicly available data, is 2009-10. We treat all schools that offered TAM in 2009-10 as "always treated" because we do not know if this is the first program year or later.

Second, we construct TAM participation indicators at the student level using course enrollment data and School Courses for the Exchange of Data (SCED) course codes that identify the four TAM courses. However, course data only become available in the 2012-13 school year, so this variable will systematically misclassify participants who only took the first course or two in the sequence as nonparticipants. Accordingly, we focus on ITT estimates of TAM exposure and by using indicators for TAM concentration (completing two or more TAM courses) and completion, which are observed for all cohorts, to complement first-stage estimates of TAM exposure on TAM takeup.

Third, we define high school graduation as earning a Maryland public high school diploma. For this variable, we exclude students who are censored from the graduation data if they transferred from a Maryland public high school to an in-state private school or out of state, neither of which is observed in our dataset. This is reasonable, given that there is no effect of TAM on missing high school data, and the background characteristics of students missing/not missing these data do not differ between treated/untreated students (see Appendix Table 2). Similarly, we are missing college data for students who transferred out of a Maryland public high school and enrolled in college out of state. NSC data track out-of-state college enrollments, but only for students who graduated from a

Maryland public high school. A small share of students missing high school graduation data re-emerge for other outcomes, if they later enroll in a Maryland college or enter the Maryland labor market.

Fourth, we define a college degree in teaching using Classification of Instructional Program (CIP) codes that categorize college majors in a consistent way throughout the state. For four-year degrees, we focus on teaching because that is the credential that provides direct entry to the teacher workforce. A two-year degree in teaching does not provide direct entry, and so we focus on a broader set of education degrees that include teaching as well as educational psychology, etc., which provide a signal of interest in the profession. Results are qualitatively similar if we expand our four-year degree measure to education and if we narrow our two-year degree measure to teaching.

Fifth, we identify eventual teachers somewhat narrowly as "observed as a teacher in a Maryland public school" for both practical and substantive reasons. We do not observe individuals who become teachers out of state or in private schools. That said, our definition has policy relevance because state policies and GYO programs generally are designed to fill teacher shortages within the state's public schools. Moreover, to the extent that our teacher indicator "misclassifies" some private school and out-of-state teachers as nonteachers, our estimates would likely understate the true effect of TAM. Teacher (and instructional aide) indicators come from human resource files that identify staff positions and full-time equivalency. Our teacher definition does not include teaching-adjacent positions (e.g., instructional coaches).

Finally, like any analysis of labor market outcomes that relies on state UI records, we are missing data for individuals who work out of state, independent contractors, and federal employees. As such, we first estimate effects on a binary indicator for positive earnings (and appearing in the UI data) to examine whether data limitations may systematically bias our estimates of the effect of TAM on wages due to out-of-state and out-of-sample migration (Foote & Stange, 2022). Because we find no relationship, we then estimate effects on mean quarterly earnings and log of mean quarterly earnings, excluding missing/zeroes. Wages are reported in 2023 real dollars.

Tables and Figures

Appendix Table 1. Coding of Event-Time Indicators to Define TAM Exposure

TAM Start Year	HS Enroll Year	Grade at TAM Start	(-3)	(-2)	(-1)	0	(+1)	(+2)
SY 2010-11	2008-09	11th	0	0	1	0	0	0
SY 2010-11	2009-10	10th	0	0	0	1	0	0
SY 2010-11	2010-11	9th	0	0	0	1	0	0
SY 2010-11	2011-12	8th	0	0	0	0	1	0
SY 2010-11	2012-13	7th	0	0	0	0	0	1
SY 2011-12	2008-09	12th	0	0	1	0	0	0
SY 2011-12	2009-10	11th	0	0	1	0	0	0
SY 2011-12	2010-11	10th	0	0	0	1	0	0
SY 2011-12	2011-12	9th	0	0	0	1	0	0
SY 2011-12	2012-13	8th	0	0	0	0	1	0
SY 2012-13	2008-09	13th	0	1	0	0	0	0
SY 2012-13	2009-10	12th	0	0	1	0	0	0
SY 2012-13	2010-11	11th	0	0	1	0	0	0
SY 2012-13	2011-12	10th	0	0	0	1	0	0
SY 2012-13	2012-13	9th	0	0	0	1	0	0
SY 2013-14	2008-09	14th	1	0	0	0	0	0
SY 2013-14	2009-10	13th	0	1	0	0	0	0
SY 2013-14	2010-11	12th	0	0	1	0	0	0
SY 2013-14	2011-12	11th	0	0	1	0	0	0
SY 2013-14	2012-13	10th	0	0	0	1	0	0

Notes: Event-time of 0 is the first year of exposure to TAM. Grade at the start of TAM is approximate and assumes on-time grade progression. In our event study analyses, we pool event-time period -3 with -2 and event-time period +2 with +1, as there is only one observed cohort for each. For analyses that estimate effects on wages, high school enrollment year of 2013 is excluded.

Appendix Table 2. Balance Tests on Observable Characteristics and Missingness

	Baseline Balance	Missing High School Grad.
	(1)	(2)
AAPI	0.004	-0.004
	(0.003)	(0.015)
Black	-0.004	0.003
	(0.006)	(0.022)
Hispanic	0.002	0.000
	(0.004)	(0.010)
White	-0.003	0.007
	(0.008)	(0.015)
Female	0.001	0.018
	(0.007)	(0.021)
FARMS	-0.008	-0.006
	(0.009)	(0.013)
Multilingual Leaner	0.002	0.015
	(0.003)	(0.024)
Special Education	-0.007	-0.009
	(0.005)	(0.013)
Math Achievement	-0.018	-0.056
	(0.029)	(0.036)
ELA Achievement	-0.003	-0.023
	(0.026)	(0.029)
P-Value on Joint Test	0.767	0.564
Observations	*225,843*	*225,843*

Notes: Estimates in each cell come from separate TWFE regression models. In column (1), we predict each student or school-year characteristic as a function of TAM adoption at the school-year level. In column (2), we predict each student characteristic as a function of TAM adoption, a dummy indicator for missing high school graduation data and their interaction. Here, we report the coefficients on the interactions, which provide evidence of whether the observable characteristics of attriters differ between treated/non-treated students. High school graduation is the primary source of missingness, as students who moved out of a Maryland public high school before graduation also often are missing college (i.e., enrollment, graduation, degree) because their records were not requested from the National Student Clearinghouse. Joint tests of significance come from models that predict TAM adoption as a function of all baseline student characteristics (column 1), as well as their interaction with the dummy variable for missing data. Standard errors are clustered at the school level. School-year characteristics also are uncorrelated with TAM adoption: cohort size (est. = -14.036, s.e. = 12.401), change in principal one or two years prior to adoption (est. = 0.022, s.e. = 0.062), number of instructors (est. = -3.111, s. e. = 3.501), and student-instructor ratio (est. = 0.014, s. e. = 0.295). * $p<0.10$, ** $p<0.05$, *** $p<0.001$

Appendix Table 3a. Effect of TAM on Selected Outcomes (Takeup and Educational Attainment) and Subgroups

	TAM Start	TAM Cert.	HS Grad.	2-Yr. Enroll.	4-Yr. Enroll.	2-Yr. Grad.	4-Yr. Grad.	AA in Ed.	BA in Tch.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Girls	0.071***	0.018***	0.013**	-0.015*	0.026**	-0.006	0.009	-0.001	0.002
	(0.012)	(0.005)	(0.005)	(0.008)	(0.010)	(0.005)	(0.011)	(0.001)	(0.003)
Control Mean	*0.004*	*0.001*	*0.916*	*0.428*	*0.321*	*0.094*	*0.310*	*0.007*	*0.024*
Relative Difference			*1.42*	*-3.42*	*8.19*	*-5.94*	*2.91*	*-12.11*	*7.77*
Observations	*110,245*	*110,245*	*102,116*	*103,473*	*103,473*	*103,473*	*103,473*	*102,175*	*102,175*
Black	0.042***	0.002	0.017**	-0.004	0.022**	-0.003	0.011	0.001**	0.002
	(0.010)	(0.001)	(0.007)	(0.011)	(0.011)	(0.005)	(0.009)	(0.000)	(0.001)
Control Mean	*0.002*	*0.000*	*0.853*	*0.351*	*0.254*	*0.037*	*0.160*	*0.001*	*0.005*
Relative Difference			*1.99*	*-1.00*	*8.64*	*-8.11*	*6.58*	*134.80*	*37.39*
Observations	*82,909*	*82,909*	*75,035*	*75,719*	*75,719*	*75,719*	*75,719*	*74,535*	*74,535*
Black Boys	0.018**	0.001	0.013*	-0.002	0.019*	0.005	0.016*	0.000	0.003**
	(0.007)	(0.000)	(0.008)	(0.018)	(0.011)	(0.009)	(0.009)	(0.000)	(0.001)
Control Mean	*0.001*	*0.000*	*0.820*	*0.326*	*0.213*	*0.031*	*0.114*	*0.000*	*0.002*
Relative Difference			*1.64*	*-0.62*	*9.01*	*16.81*	*14.19*	*38.35*	*140.50*
Observations	*42,767*	*42,767*	*38,087*	*38,361*	*38,361*	*38,361*	*38,361*	*37,697*	*37,697*
White	0.044***	0.017***	0.005	-0.007	0.016	0.002	0.007	-0.001	0.006*
	(0.009)	(0.005)	(0.004)	(0.009)	(0.010)	(0.006)	(0.009)	(0.001)	(0.003)
Control Mean	*0.002*	*0.001*	*0.933*	*0.421*	*0.322*	*0.111*	*0.340*	*0.007*	*0.022*
Relative Difference			*0.57*	*-1.58*	*4.89*	*1.58*	*1.92*	*-19.44*	*27.06*
Observations	*96,308*	*96,308*	*89,577*	*90,765*	*90,765*	*90,765*	*90,765*	*89,655*	*89,655*
White Boys	0.007**	0.001	0.002	-0.001	0.007	-0.005	0.006	-0.000	0.003
	(0.003)	(0.002)	(0.006)	(0.012)	(0.010)	(0.007)	(0.010)	(0.001)	(0.002)
Control Mean	*0.001*	*0.000*	*0.921*	*0.383*	*0.293*	*0.096*	*0.284*	*0.002*	*0.006*
Relative Difference			*0.18*	*-0.32*	*2.31*	*-4.86*	*2.28*	*-25.13*	*44.73*
Observations	*49,216*	*49,216*	*45,682*	*46,205*	*46,205*	*46,205*	*46,205*	*45,660*	*45,660*
AAPI	0.020**	0.002*	-0.008	-0.058**	0.040*	-0.030**	0.029	-0.000	-0.005
	(0.009)	(0.001)	(0.014)	(0.027)	(0.021)	(0.014)	(0.021)	(0.004)	(0.005)
Control Mean	*0.002*	*0.000*	*0.972*	*0.417*	*0.483*	*0.107*	*0.512*	*0.003*	*0.013*
Relative Difference			*-0.78*	*-13.92*	*8.35*	*-28.27*	*5.62*	*-7.75*	*-36.30*
Observations	*10,905*	*10,905*	*10,116*	*10,237*	*10,237*	*10,237*	*10,237*	*10,144*	*10,144*
Hispanic	0.028***	0.001	0.007	-0.005	-0.019	0.003	-0.003	-0.003	-0.004
	(0.008)	(0.001)	(0.016)	(0.019)	(0.021)	(0.012)	(0.016)	(0.002)	(0.003)
Control Mean	*0.001*	*0.000*	*0.788*	*0.361*	*0.154*	*0.065*	*0.133*	*0.002*	*0.007*
Relative Difference			*0.93*	*-1.28*	*-12.18*	*4.75*	*-2.11*	*-162.00*	*-61.78*
Observations	*22,038*	*22,038*	*19,861*	*19,991*	*19,991*	*19,991*	*19,991*	*19,813*	*19,813*

Notes: Estimates in each cell come from separate TWFE models that include student and school-year covariates. Standard errors are clustered at the school level. AAPI = Asian American and Pacific Islander. * p<0.10, ** p<0.05, *** p<0.001

Appendix Table 3b. Effect of TAM on Selected Outcomes (Labor Market) and Subgroups

	Aide	Teacher	Local	More Black Tch.	Higher Salary	Trad. License	Alt. License	Pos. Earn.	Log Earn.
	(1)	(2)	(2)	(4)	(5)	(7)	(8)	(9)	(1)
Girls	-0.001	0.009***	0.006**	0.002**	0.002	0.007**	0.003***	0.003	0.080**
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)	(0.010)	(0.035)
Control Mean	*0.004*	*0.022*	*0.013*	*0.003*	*0.006*	*0.017*	*0.004*	*0.570*	*8.640*
Relative Difference	*-21.21*	*40.11*	*45.56*	*59.73*	*25.94*	*39.17*	*81.87*	*0.55*	*0.93*
Observations	*110,245*	*110,245*	*110,245*	*110,245*	*110,245*	*110,245*	*110,245*	*88,952*	*50,235*
Black	-0.003**	0.004**	0.001	0.002**	0.003**	0.001	0.003**	0.007	0.113***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.014)	(0.034)
Control Mean	*0.002*	*0.005*	*0.003*	*0.002*	*0.001*	*0.002*	*0.003*	*0.571*	*8.396*
Relative Difference	*-108.10*	*71.56*	*33.53*	*111.10*	*209.70*	*57.97*	*110.90*	*1.23*	*1.35*
Observations	*82,909*	*82,909*	*82,909*	*82,909*	*82,909*	*82,909*	*82,909*	*67,425*	*38,052*
Black Boys	-0.003**	0.001	0.000	-0.000	0.000	0.001	-0.000	0.015	0.057
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.021)	(0.049)
Control Mean	*0.001*	*0.002*	*0.001*	*0.001*	*0.001*	*0.000*	*0.002*	*0.535*	*8.425*
Relative Difference	*-182.80*	*24.94*	*20.50*	*-22.15*	*89.94*	*198.20*	*-0.74*	*2.86*	*0.67*
Observations	*42,767*	*42,767*	*42,767*	*42,767*	*42,767*	*42,767*	*42,767*	*34,754*	*18,444*
White	-0.000	0.009***	0.005**	0.000	0.002	0.008***	0.002**	0.002	0.025
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.009)	(0.024)
Control Mean	*0.003*	*0.021*	*0.013*	*0.002*	*0.006*	*0.017*	*0.002*	*0.558*	*8.934*
Relative Difference	*-14.18*	*45.37*	*39.73*	*13.00*	*29.33*	*47.34*	*66.06*	*0.44*	*0.28*
Observations	*96,308*	*96,308*	*96,308*	*96,308*	*96,308*	*96,308*	*96,308*	*78,251*	*43,517*
White Boys	-0.001	0.005**	0.001	0.001	0.001	0.004**	0.002	0.000	0.002
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.016)	(0.031)
Control Mean	*0.001*	*0.006*	*0.004*	*0.001*	*0.002*	*0.005*	*0.001*	*0.560*	*9.031*
Relative Difference	*-73.49*	*72.06*	*17.87*	*84.60*	*83.65*	*85.61*	*154.40*	*0.06*	*0.02*
Observations	*49,216*	*49,216*	*49,216*	*49,216*	*49,216*	*49,216*	*49,216*	*39,944*	*22,317*
AAPI	-0.000	-0.005	0.006**	0.000	-0.007	-0.001	-0.000	-0.037	0.065
	(0.001)	(0.008)	(0.003)	(0.003)	(0.004)	(0.006)	(0.003)	(0.024)	(0.058)
Control Mean	*0.001*	*0.008*	*0.004*	*0.001*	*0.002*	*0.006*	*0.001*	*0.452*	*8.938*
Relative Difference	*-9.85*	*-66.76*	*139.40*	*15.95*	*-425.60*	*-23.60*	*-25.41*	*-8.22*	*0.72*
Observations	*10,905*	*10,905*	*10,905*	*10,905*	*10,905*	*10,905*	*10,905*	*8,651*	*3,855*
Hispanic	-0.001	0.003	0.004	0.000	-0.002	0.003	0.001	0.006	-0.024
	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.021)	(0.084)
Control Mean	*0.002*	*0.006*	*0.003*	*0.001*	*0.002*	*0.004*	*0.001*	*0.475*	*8.817*
Relative Difference	*-22.98*	*45.32*	*123.40*	*21.38*	*-114.70*	*81.41*	*97.46*	*1.23*	*-0.27*
Observations	*22,038*	*22,038*	*22,038*	*22,038*	*22,038*	*22,038*	*22,038*	*17,141*	*8,180*

Notes: See notes to Appendix Table 3a.

Appendix Table 4. Sensitivity of Main Estimates on Becoming a Teacher to Alternative Specifications

	Main/ Preferred	No controls	Control for Teacher Characteristics	Control for Principal Characteristics and Turnover	Control for School Time Trends	Exclude Post-Adoption Cohorts
	(1)	(2)	(3)	(4)	(5)	(6)
All	0.006***	0.006***	0.006***	0.006***	0.008***	0.006***
SE Clustered at School Level	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
SE Clustered at School-Year	(0.001)					
SE Clustered at District-Year	(0.002)					
Observations	*225,843*	*225,843*	*225,843*	*224,954*	*225,843*	*217,777*
Boys	0.002**	0.002**	0.003**	0.002**	0.002	0.003**
SE Clustered at School Level	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
SE Clustered at School-Year	(0.001)					
SE Clustered at District-Year	(0.001)					
Observations	*115,598*	*115,598*	*115,598*	*115,170*	*115,598*	*111,453*
Black Girls	0.007**	0.007**	0.008***	0.008**	0.011**	0.008**
SE Clustered at School Level	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)
SE Clustered at School-Year	(0.002)					
SE Clustered at District-Year	(0.001)					
Observations	*40,142*	*40,142*	*40,142*	*39,796*	*40,142*	*38,809*
White Girls	0.014***	0.014***	0.015***	0.012**	0.019**	0.014**
SE Clustered at School Level	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)	(0.004)
SE Clustered at School-Year	(0.004)					
SE Clustered at District-Year	(0.005)					
Observations	*47,092*	*47,092*	*47,092*	*47,019*	*47,092*	*45,670*

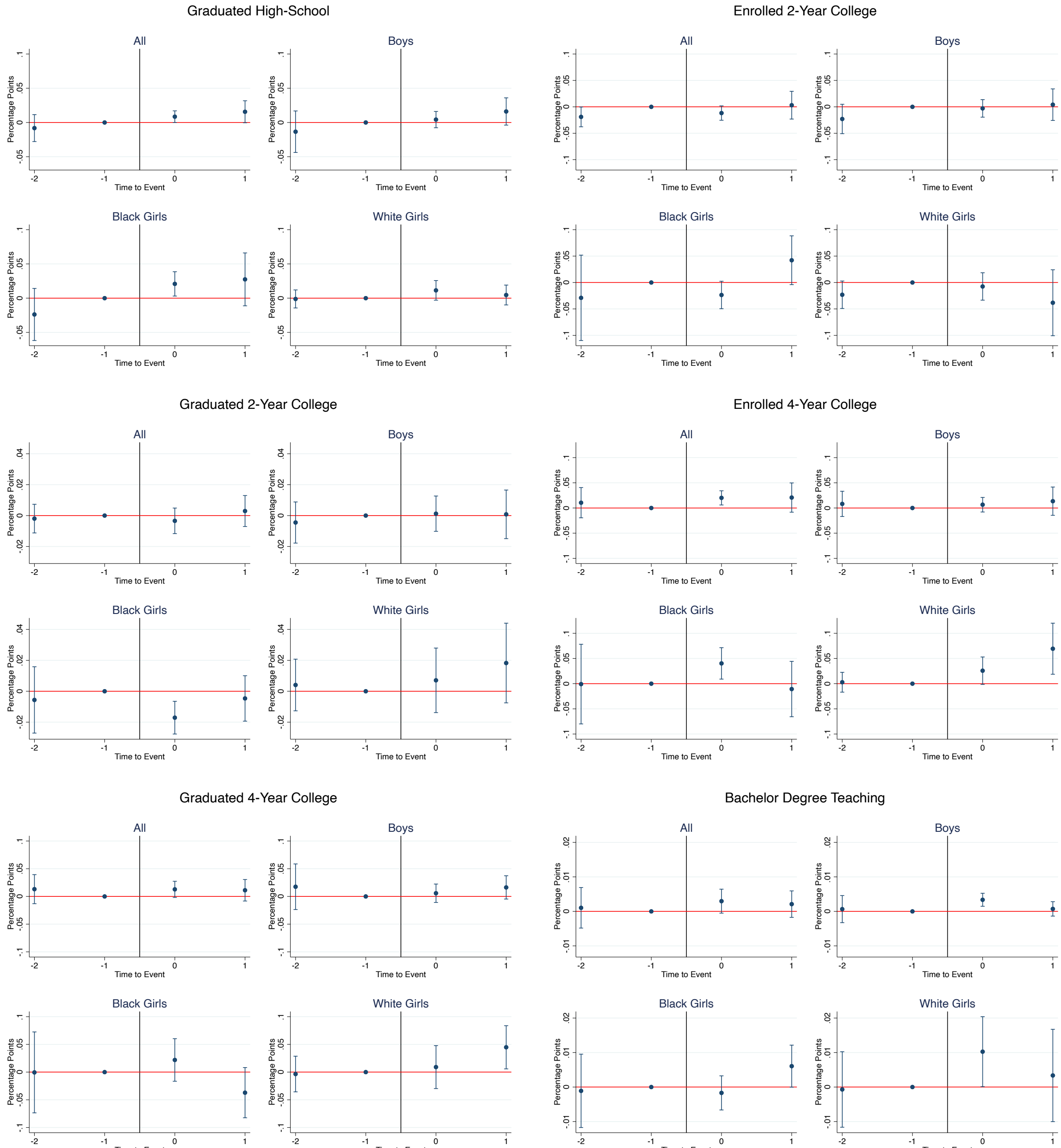
Notes: Estimates in each cell come from separate TWFE models of the effect of TAM on becoming a teacher (our main outcome). In column (1), we show the coefficients for this outcome using our preferred estimation approach, with standard errors clustered at three levels: school (our preferred method), school-year, and district-year. Column (2) reports the results when excluding covariates. Column (3) reports estimates after conditioning the main set of controls, plus average characteristics of teachers (i.e., gender, race/ethnicity, teaching experience, share alternative certified) in each school-cohort. Column (4) removes the teacher characteristics and adds an indicator for principal turnover and observable principal characteristics (i.e., gender, race/ethnicity, principal experience). Column (5) reports the results when including a school linear time trend. Column (6) excludes from the sample students that enrolled in schools after TAM was already adopted. * p<0.10, ** p<0.05, *** p<0.001

Appendix Table 5. Effect of TAM Access on Becoming a Teacher and Teacher License (Logits)

	Became a Teacher		Became a Teacher Locally		Traditional License		Alternative License	
	Logit	OLS	Logit	OLS	Logit	OLS	Logit	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All	0.406***	0.006***	0.528**	0.003**	0.447***	0.005**	0.553**	0.002***
	(0.112)	(0.001)	(0.169)	(0.001)	(0.133)	(0.001)	(0.235)	(0.000)
APE	*0.005*		*0.004*		*0.005*		*0.002*	
Control Mean	*0.013*		*0.008*		*0.010*		*0.003*	
Observations	*219,126*	*219,126*	*217,805*	*217,805*	*206,072*	*206,072*	*204,378*	*204,378*
Boys	0.548**	0.003**	0.415	0.001	0.909**	0.003**	0.452	0.001
	(0.273)	(0.001)	(0.423)	(0.001)	(0.379)	(0.001)	(0.441)	(0.001)
APE	*0.003*		*0.001*		*0.003*		*0.001*	
Control Mean	*0.005*		*0.003*		*0.003*		*0.002*	
Observations	*103,083*	*103,083*	*88,676*	*88,676*	*82,335*	*82,335*	*70,505*	*70,505*
Black Girls	0.495*	0.008**	0.092	0.003	0.187	0.001	0.874**	0.008***
	(0.293)	(0.002)	(0.349)	(0.002)	(0.469)	(0.002)	(0.397)	(0.002)
APE	*0.005*		*0.001*		*0.001*		*0.006*	
Control Mean	*0.010*		*0.007*		*0.005*		*0.006*	
Observations	*34,393*	*34,393*	*29,449*	*29,449*	*24,503*	*24,503*	*27,440*	*27,440*
White Girls	0.395**	0.014***	0.650**	0.009**	0.412**	0.012**	0.354	0.002
	(0.151)	(0.004)	(0.245)	(0.003)	(0.164)	(0.004)	(0.523)	(0.002)
APE	*0.014*		*0.014*		*0.013*		*0.002*	
Control Mean	*0.037*		*0.022*		*0.031*		*0.005*	
Observations	*46,395*	*46,395*	*46,106*	*46,106*	*46,305*	*46,305*	*37,453*	*37,453*

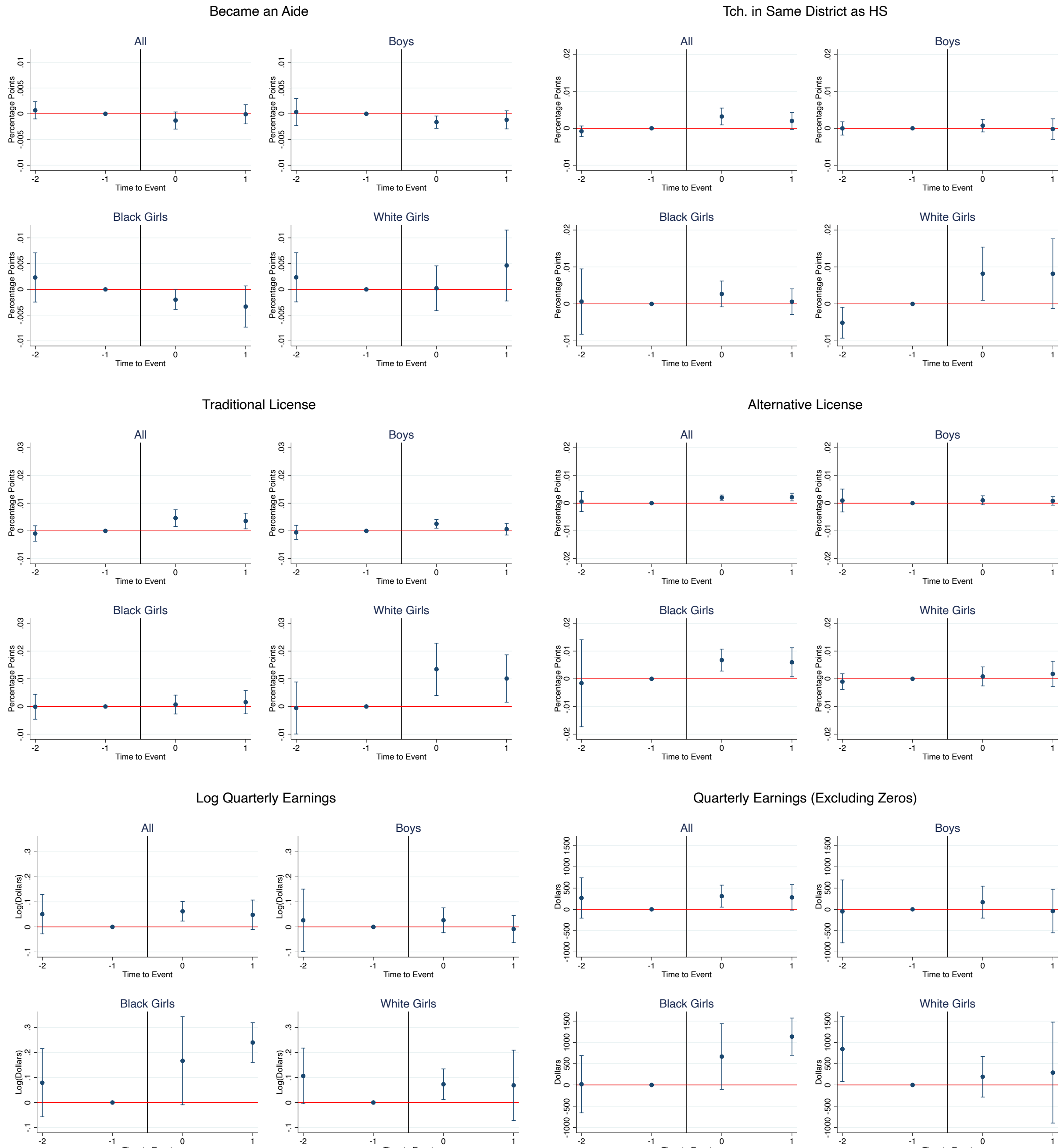
Notes: Logit estimates come from separate FE (conditional) logistic regression models that include school and year fixed effects, and student and school-year covariates. Standard errors are clustered at the school level for OLS estimates but cannot be clustered for FE-Logit models. The approximate average partial effect (APE) is estimated by multiplying the logit coefficient by p*(1-p), where p is the mean of the outcome. See Puhani (2012) for discussion of DD logit models. We also present OLS estimates are estimated from the same sample as the FE logit models, which drop units without identifying variation. * p<0.10, ** p<0.05, *** p<0.001

Appendix Figure 1a. Event Study Analyses of the Effect of TAM Access on Educational Attainment



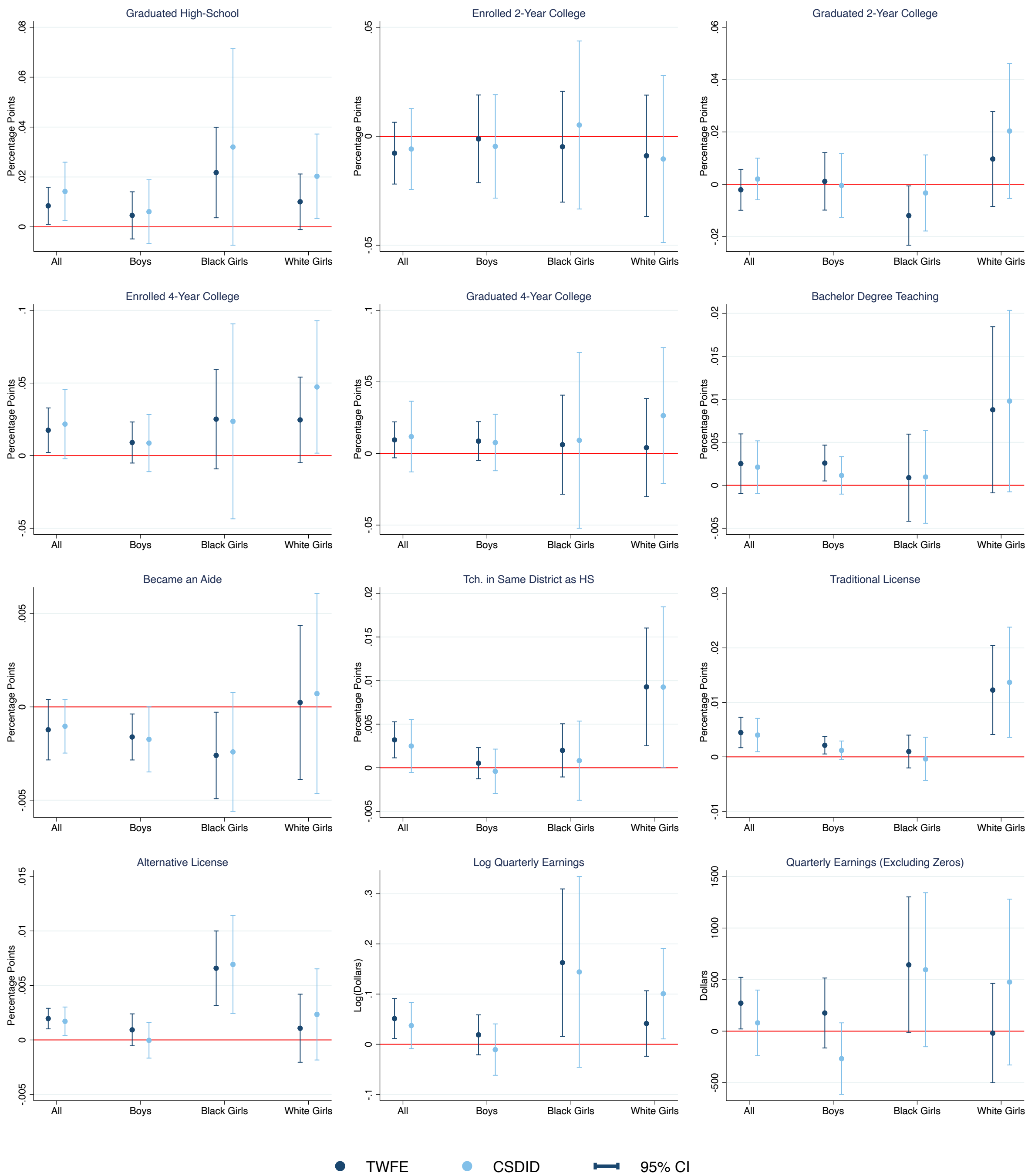
Notes: This figure reports event study point estimates and 95% confidence intervals from regression specifications that include lead and lag indicators for access to TAM as well as school and cohort fixed effects. Student and school-year covariates are excluded. The event time variable on the x-axis is a continuous variable, where zero identifies students who were in 9th or 10th grade/first or second year of high school when TAM was first adopted. Positive values represent post-adoption cohorts, while negative values represent pre-adoption cohorts (i.e., students who were in 11th or 12th grade or post-graduation when TAM was first adopted). Due to limited sample size and precision, we pool event-time period two with period one, and event-time period negative three with negative two. Coding of event-time indicators is shown in Appendix Table 1. Standard errors used to compute confidence intervals are clustered at the high school level.

Appendix Figure 1b. Event Study Analyses of the Effect of TAM Access on Labor Market Outcomes



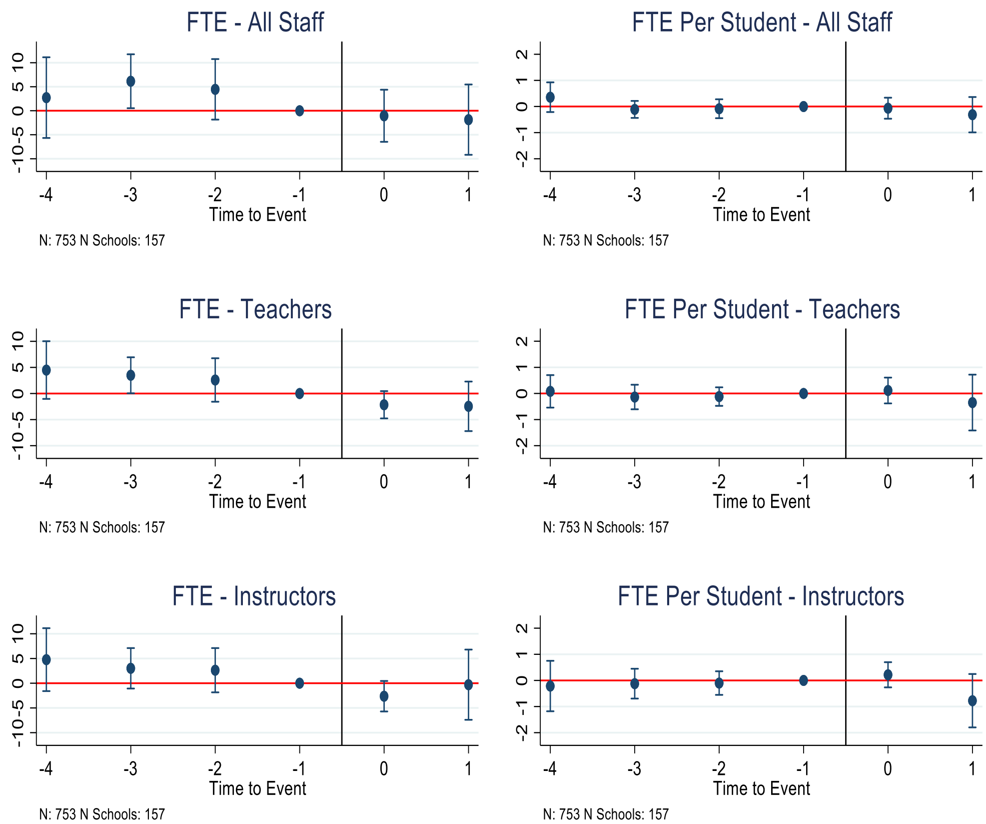
See notes to Appendix Figure 1a.

Appendix Figure 2. Estimates of Effect of TAM Access on Educational Attainment and Labor Market Outcomes Using Alternative DID Estimators



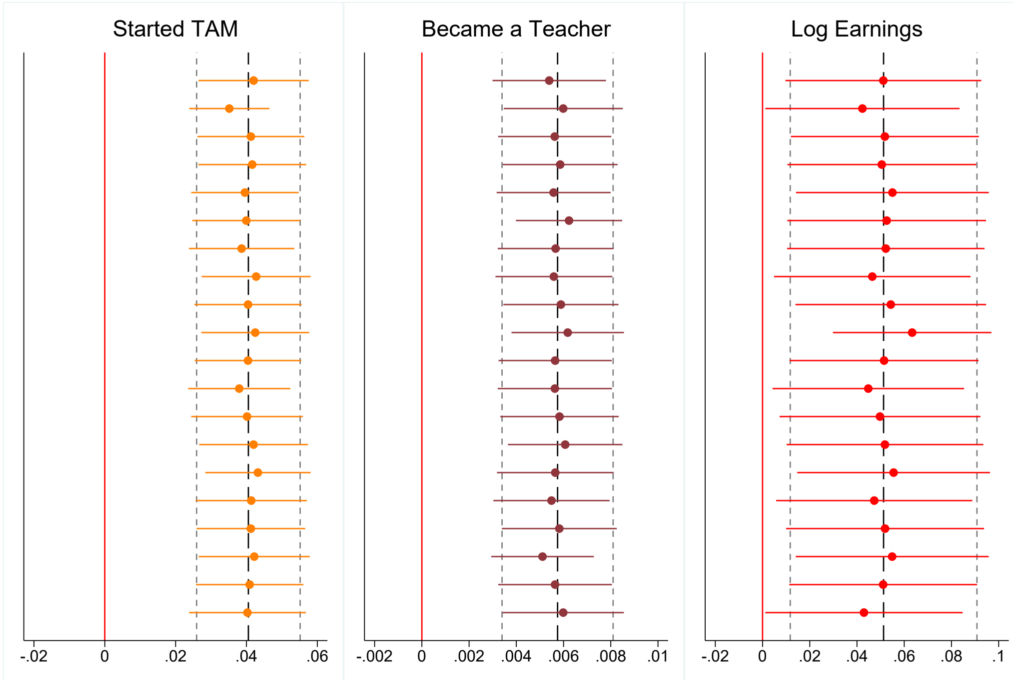
Notes: TWFE estimates are the same as those reported in Tables 4 through 6 in the main text. CSDID estimates come from Callaway and Sant'Anna's (2021) estimator, implemented using the *csdid* command in Stata. Corresponding 95% confidence intervals are provided for both sets of estimates.

Appendix Figure 3. Event Study Analyses of TAM Access on the Number of Teachers and Student-Teacher Ratios



Notes: FTE = full-time equivalent positions. All staff include all positions in the school, while teachers include only staff specifically flagged as full-time teachers in the human resources data. Instructors include teachers, as well as some other positions that include instruction in at least part of their job responsibilities.

Appendix Figure 4. Robustness of TAM Effects to Leaving Out One School at a Time



Notes: This figure reports two-way-fixed effects (TWFE) point estimates of TAM exposure on selected outcomes and 95% confidence intervals from regression specifications that include school and cohort fixed effects. Each point estimate excludes one school at a time out of the 20 sometimes-treated schools in our sample. Standard errors used to compute confidence intervals are clustered at the high school level. The reference line in dashes correspond to average treatment effects of TAM on each selected outcome, with short dashes showing the 95% confidence interval of each average treatment effect estimate.