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David Blazar
University of Maryland,
College Park

Wenjing Gao
University of Maryland,
College Park

Seth Gershenson
American University and IZA

Ramon Goings
University of Maryland,
Baltimore County

Francisco Lagos
University of Maryland,
College Park

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Do Grow-Your-Own Programs Work?
Evidence from the Teacher Academy of Maryland

David Blazar, *University of Maryland, College Park*
Wenjing Gao, *University of Maryland, College Park*
Seth Gershenson, *American University and IZA*
Ramon Goings, *University of Maryland, Baltimore County*
Francisco Lagos, *University of Maryland, College Park*

Abstract

Local teacher recruitment through "grow-your-own" programs is a prominent strategy to address workforce shortages and ensure that incoming teachers resemble, understand, and have strong connections to their communities. We exploit the staggered rollout of the Teacher Academy of Maryland career and technical education certificate program across public high schools, finding that exposed students were more likely to become teachers by 0.6 percentage points (pp), or 47%. Effects are concentrated among White girls (1.4pp/39%) and Black girls (0.7pp/80%). We also identify positive impacts on wages (5% on average/18% for Black girls), countering a prevailing narrative that teaching leaves one worse off financially relative to other labor market opportunities.

Keywords: Teaching, High School Curricula, College Major Choice, Occupational Choice, Earnings
JEL Classifications: I20, J24, H52

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1. Introduction

Teachers play a vital role in the functioning of schools and in students' cognitive and socioemotional development (Jackson 2018; Hanushek and Rivkin 2010). Accordingly, school leaders, policymakers, researchers, and parents have long fretted about potential teacher shortages and inequitable access to effective teachers (Clotfelter, Ladd, and Clifton 2023; Goldhaber, Lavery, and Theobald 2015; Ingersoll 2001). These concerns are related, as teacher shortages are fundamentally acute, or local, to specific schools, districts, and even subjects, many of which serve historically underserved communities (Edwards et al. 2024), and high levels of teacher turnover harm both students and the teachers who remain (Ronfeldt, Loeb, and Wyckoff 2013; Hanushek, Rivkin, and Schiman 2016). An obvious countermeasure is for schools to recruit teachers from the local community. The rationale for local recruitment is further strengthened by the fact that demographic representation is one dimension of quality that is lacking in a teaching force that is woefully unrepresentative of the diverse student population it serves (Gershenson, Hansen, and Lindsay 2021; Gist and Bristol 2022). Experimental evidence identifies large and sustained effects of teacher-student race matching on varied academic, socio-emotional, and attainment measures of their students (Dee 2004; Gershenson et al. 2022; Blazar 2024).

"Grow-your-own" (GYO) programs are amongst the most popular and prominent strategies through which states and school districts locally identify and recruit prospective teaching talent. The defining characteristic of GYO programs is that they 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. In practice, GYO has morphed into an umbrella term claimed by many flavors of localized teacher recruiting programs, though the majority of self-described GYO programs target high school students (Edwards and Kraft 2024). About half of GYO programs offer some level of financial support, but few fully cover the cost of becoming a certified teacher.

Despite the growing popularity of GYO programs (Garcia 2020), however, there exists remarkably little credible evidence of their effectiveness (Edwards and Kraft 2024; Gist, Bianco, and Lynn 2019). We contribute to this gap in the literature by examining the impacts of the Teacher Academy of Maryland (TAM), a GYO program that provides high school students with early exposure to teaching as a career through a four-course Career and Technical Education (CTE) sequence, as well as the opportunity to dually enroll in courses whose credits count towards high school graduation and a teaching degree in either two- or four-year postsecondary institutions. Our research design exploits the staggered rollout of the program across Maryland high schools in a generalized difference-in-differences (DD) framework. Intent-to-treat (ITT) estimates suggest that attending a high school that offers TAM significantly increases educational attainment (e.g., high school graduation, college enrollment), employment as a teacher, and wages. 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 and 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 47%. However, this average effect masks significant, and nuanced, heterogeneity across demographic groups. Specifically, the ITT effect of TAM is largest for girls (0.9pp). 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 80% for Black girls), as White girls in our sample are roughly four times as likely to become teachers as Black girls. These patterns are notable given that an emphasis, or expectation, of many GYO programs and their advocates is that they will increase diversity in the teaching profession (Valenzuela 2017; Edwards and Kraft 2024; Gist, Bianco, and Lynn 2019). White females already 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 these results is that White girls induced into teaching by TAM entered via traditional routes and certifications, while Black girls entered via nontraditional pathways that bypassed undergraduate teacher education programs.

TAM also affected students' educational attainment. Exposure to TAM increased high school graduation rates by 0.8 pp, or 1%. This effect was larger for girls than boys. Black girls particularly benefited in this domain, as their graduation rate increased by 2.2pp, or 3%. Four-year college enrollments increased as well (1.7pp/6% on average across the sample), attracting both students who likely would have attended two-year colleges and students who would not have attended college at all. These patterns suggest that programs like TAM likely create 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, Ranellucci, and Kaplan 2019; Murnane, Singer, and Willett 1989; Gershenson et al. 2022). Specifically, wages increased by about 5% on average and 18% for Black girls. 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 two broad but distinct literatures in labor and education economics. First, despite workforce development being a frequent topic of policy discussions, there is a surprising dearth of evidence on what actually works (Escobari, Seyal, and Contreras 2021). Surveying 30 years of evidence, 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. More recently, economists equipped with modern econometric tools have revisited data from these programs. For example, recent analyses of Job Corps find employment and earnings effects that are larger for males (Chen, Flores, and Flores-Lagunes 2018; Flores et al. 2012). An experimental analysis of a youth training program in Argentina similarly finds significant, persistent employment effects concentrated amongst males (Alzúa, Cruces, and Lopez 2016).

The somewhat mixed results and effects that faded out over time in the earlier literature reviewed in Bloom (2010) may explain the lack of continued investment in workforce development programs targeted to young people, as well as the more recent shift to career academies and career and technical education (CTE) programs offered in high school (e.g., Bonilla 2020; Kemple and Willner 2008; Hemelt, Lenard, and Paeplow 2019; Page 2012; Dougherty 2018). Career academies and programs cover a wide range of work sectors from information technology, health and biosciences, and construction and development. TAM is a CTE program situated within Maryland's human resources cluster. The extant CTE literature generally finds positive effects on high school graduation and college enrollment that are similar in magnitude to the effects of TAM documented in this study.

Within the job training, career academy, and CTE literature, our study is most similar to Brunner, Dougherty, and Ross (2023), who also link high school records to employment data. They find that CTE high schools that prepare students for a variety of industries boost wages by over 30%. However, these effects are driven by males who enter the workforce shortly after high school. The current 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. Moreover, the current study provides the first causal evidence of a large-scale GYO program's impacts on long-run educational and employment outcomes.

Second, our study contributes to the related but distinct literature on teacher labor supply and the determinants of occupational and college major choice. Regarding the former, economists and education 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) (e.g., Dolton 2006; Guarino, Santibañez, and Daley 2006; Hanushek and 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 and Rosen 1997).

Regarding the latter, economists have studied occupational choice at least since Roy (1951) posited that individuals consider their own standing in the ability distribution and choose to work in the profession that offers the highest expected earnings. Doepke and Zilibotti (2008) suggest that occupational choice is shaped by the preferences instilled by one's parents and their social class. Similarly, Wiswall and Zafar (2021) find that familial expectations play an outsized role in female college students' choice of major. 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 and Gershenson 2021). The logic model of GYO programs is similar: they provide logistical information about the teaching profession and the idea that it is an honorable, rewarding job to high school students.

Because teaching requires a college degree, and a traditional teaching license requires a degree in education, the literature on college major choice is relevant too (e.g., Altonji 1993; Beffy, Fougere, and Maurel 2012). Expected earnings do influence college students' choice of major, though not as strongly as innate preferences, and students' beliefs about earnings are frequently inaccurate (Wiswall and Zafar 2015). Teaching is a particularly interesting case study given the aforementioned multitude of non-wage benefits, the fact that educators and the general public believe that teachers are underpaid relative to similar professionals (Allegretto 2023; Steiner, Woo, and Doan 2023), and that there is a good deal of confusion about how much teachers actually earn (Henderson et al. 2020; West 2014).

Coursework can influence major choice as well. In addition to the literature on career academies and CTE programs, which are coursework based, additional studies document how math and science coursework in high school increases the likelihood of earning a college degree in an aligned field (Darolia et al. 2020; Görlitz and Gravert 2018; De Philippis 2023; Liu, Conrad, and Blazar 2024). In college, Carrell, Page, and West (2010) show that having a female professor in entry-level math and science courses improves female college students' class performance and increases the likelihood they take subsequent courses in the subject and major in a related field. Field experiments conducted in high school and introductory college courses similarly show that female students are more likely to major in the subject after even a brief exposure to a female role model in the field (Porter and Serra 2020; Breda et al. 2023). This suggests that TAM's effects on becoming a teacher are driven at least partly by information provision and changing students' preferences, which is consistent with the findings of Wiswall and Zafar (2015). More generally, the current study adds to the literature on occupational choice by providing a compelling example of a school-based program that achieved its intended goal of increasing labor supply into a particular profession.

The paper proceeds as follows: Section 2 describes what is known about GYO programs generally and the specifics of Maryland's TAM program. Sections 3 and 4 describe the administrative data and identification strategy, respectively. Section 5 presents the main results and section 6 presents an array of sensitivity analyses and robustness checks. Section 7 discusses the findings and concludes.

2. Background

2.1. "Grow-Your-Own" Programs

GYO programs are an increasingly popular type of pathway program that states and local education agencies use to both address local teacher shortages and to diversify the teacher workforce (Edwards and Kraft 2024; Garcia 2020; Toshalis 2013; Valenzuela 2017). Pathway programs can take many forms and can target different populations of potential teachers. For example, GYO programs might provide early exposure to teaching and opportunities to high school students to earn dual-enrollment credits toward a teaching credential (Gist, Bianco, and Lynn 2019), offer financial scholarships to college students who change to a teaching major (Hrabowski and Sanders 2015), or facilitate alternative-route teacher certification programs to mid-career professionals already working in or in close proximity to schools (Skinner, Garreton, and Schultz 2011). That said, high school students are the most common target of GYO programs (Edwards and Kraft 2024) and are the focus of the TAM program that we evaluate in the current study.

In addition to being convenient, GYO programs are appealing because they facilitate local recruitment of teaching candidates who already live nearby, are familiar with the local culture and community, and (presumably) reflect the demographic composition of the current student body. Recruiting such individuals would therefore address both local teacher shortages (Edwards et al. 2024) and longstanding concerns that, in many districts, the current teacher workforce does not reflect the demographics of the student body (Gershenson, Hansen, and Lindsay 2021; Putman et al. 2016). These appealing aspects of programs that seek to recruit local high school students into teaching careers have led to a flurry of policy activity in this space, with almost all U.S. states engaging in some form of GYO activity (Garcia 2020).

One of the first programs of this kind 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, 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), 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. While these programs are coursework-based, several additional GYO programs include mentorship programs and affiliation networks, such as Future Educators of America, Educators Rising, and Call Me Mister (Jones, Holton, and Joseph 2019; Valenzuela 2017).

Some but not all pre-collegiate GYO programs have a stated mission to diversify the teaching profession. In fact, Title VI of the 1964 Civil Rights Act prohibits state or federally-sponsored programs from targeting specific racial or ethnic groups, including underrepresented ones. Therefore, programs may have an unstated goal of diversifying the teacher workforce but are not able to develop recruitment materials and programming to targeted populations based on race or ethnicity. For example, the original, community-led LSNA program in Chicago first developed coursework with an emphasis on critical race theory and critical pedagogy (Valenzuela 2017). When the program expanded statewide, 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, Bianco, and Lynn 2019).

Despite the growing interest in and prevalence of these sorts of GYO programs, there is little credible evidence on their effectiveness (Edwards and Kraft 2024; Gist, Bianco, and Lynn 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 eastern 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 school-based "future teachers" 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 is both a CTE program that allows high school students to gain certification as an instructional aide and a dual-enrollment program that allows students to earn college-level credits towards an associate's or bachelor's degree in teaching.[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. Key collaborators and stakeholders include MSDE, the Maryland Higher Education Commission (MHEC), county-based local education agencies (LEAs) or districts, and two-year community colleges and four-year institutions of higher education governed by USM.[2]

While districts liaise with the state and with higher education institutions regarding memoranda of understanding and articulation agreements, 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

[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.

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: (i) Human Growth and Development, (ii) Teaching as a Profession, (iii) Foundations of Curriculum and Instruction, and (iv) Education Academy Internship (e.g., fieldwork in classrooms). These courses cover key pedagogical elements of development and learning theory, positive and effective classroom management and discipline, curriculum delivery models, and the creation of developmentally appropriate curriculum and learning environments. Because of the set sequence, TAM generally takes three years to complete, and program implementation guides lay out a course-taking trajectory that begins in tenth grade.[3] Administrative data confirms that the majority of TAM participants begin the program in tenth grade and that program completers generally earn their certificate in three years. Accordingly, we identify school-cohorts as treated if they were exposed to TAM for at least three years (see Appendix Table 1 for coding of event-time indicators based on TAM start year and high school enrollment year).

TAM participation (and completion) can lead to several career trajectories. Upon completing all four courses, students must also pass the ParaPro assessment in order to earn their industry-recognized certificate. At this point, TAM graduates are eligible for immediate employment in an educational support position. From the state's perspective, a more desirable outcome is that students transition from high school to college in pursuit of a teaching degree. Students can transfer TAM credits to two- or four-year degree programs by submitting a course/program completion verification form signed by their high school principal and guidance counselor. Additionally, under some district-higher education institution agreements, students may be eligible to apply for a modest scholarship of about $500 per semester.[4] If students start in a two-year program, they must then transfer these credits

[3] https://www.towson.edu/coe/centers/teacheracademy/teachers/documents/implementationguide.pdf

[4]

to and eventually earn a bachelor's degree from a four-year institution in order to become a fully licensed teacher in a Maryland public school.[5] 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.[6]

Our research design exploits the staggered rollout of TAM across schools and cohorts between 2008-09 and 2012-13, which is the last cohort of entering high school students for whom we can observe meaningful labor market outcomes. Table 1 provides characteristics of schools, split into three groups: (i) never-treated schools that had not implemented TAM as of 2013-14 (when the 2012-13 cohort was in 10th grade), (ii) sometimes-treated schools that first adopted TAM in the timeframe of our analyses, and (iii) always-treated schools that adopted TAM beforehand. We link students to these schools based on the first high school they enrolled in for ninth grade.

Compared to the state as a whole (column 1), TAM schools—including sometimes and always treated—enrolled slightly more White students (45% and 46%, respectively, compared to 43% across the state) and fewer low-income students eligible for free and reduced-price meals (FARMS) (30% and 31%, compared to 36%). However, the differences are not large. Earlier adopters (i.e., always treated) enrolled fewer Black students (29% compared to 35%), though the share of Hispanic students is similar across school types (roughly 10%).

https://resources.finalsite.net/images/v1662565187/carrollk12org/vsfsd4j5nqsfz9wr7ye9/TAMArticulationAgreementScholarshipChartApril2022.pdf

[5] Only one certification area (Professional and Technical Education within CTE) allows teachers to work in a Maryland public school without a bachelor's degree. Professional and Technical Education-certified teachers substitute years of experience for years of higher education. Teachers with this certification make up a tiny fraction of the overall workforce.

[6] 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.).

In never-treated schools, a small share of students started TAM (0.2%) and finished the program (0.1%) due to two reasons. 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. That said, the rate of "non-compliance" is exceedingly small and, if anything, such contamination suggests our estimates are lower bounds.

3. Data

3.1. Data Construction

We integrate publicly available data on TAM rollouts at the school-year level[7] 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 [MSDE]); (ii) all public and private higher education institutions in the state (provided by the Maryland Higher Education Commission [MHEC]) and out-of-state college enrollment data for students who graduated from a Maryland public high school (from the National Student Clearinghouse [NSC]); (iii) teacher workforce in K-12 public schools (also supplied by MSDE); and (iv) quarterly wages 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. There are about 320,000 unique students in these five cohorts, though the main analytic sample excludes students in always-treated schools, yielding about

[7] 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

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) from the time they entered ninth grade, which includes the following steps on the pathway to a career: (i) enrollment in a TAM course within six years of starting ninth grade; (ii) completing all four courses in the sequence and (iii) earning a TAM certificate within six years; (iii) high school graduation, also within six years; college enrollment, in either (iv) two- or (v) four-year degree-seeking programs within seven years; completion of (vi) an associate's (AA) or (vii) a bachelor's degree (BA) within eight and ten years, respectively; receipt of (vii) an AA in education or (iv) a BA degree in teaching within eight and ten years, respectively; (x) observed as a teacher of record in Maryland K12 public schools, as well as (xi) license type (i.e., traditional versus alternative[8]) within ten years; and (xii) wages either in teaching or in any other profession where employers must submit unemployment insurance (UI) data to the state. We capture wages 11 years after ninth grade, at approximately age 25, which requires excluding one cohort 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 more narrow 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.

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

[8] 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). Both types of certificates allow individuals to teach while earning their credential and, thus, aim to fast-track the licensure and certification process. 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.

rollout. We exclude the first year of MLDS data (2007-08) because we do not know if it is their first time enrolling in ninth grade, nor do we have information from the prior year to use as controls and to assess balance (e.g., test scores). 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 codes 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 identify first-stage effects of TAM exposure.

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. 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.

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 of positive earnings (and appearing in the UI data). 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.

3.2. Summary Statistics

Table 2 summarizes the main analytic sample, 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. Column 2 shows that about 10% of girls were exposed to TAM, which is unsurprising because boys and girls attend the same schools. Columns 3 and 4 show that about 9% of Black and White girls were exposed to TAM, respectively. Overall, 0.7% of students engaged with TAM, though column 2 shows that girls were significantly more likely to participate in TAM than boys. Columns 3 and 4 show that Black and White girls participated in TAM at roughly equal rates. However, White girls were about six times more likely to complete the TAM coursework and earn a TAM certificate than Black girls. Because takeup of TAM is concentrated amongst Black and White girls, we focus on these subgroups throughout the main analyses. We explore takeup and outcomes for additional subgroups (e.g., boys, Asian students, Hispanic students) following the main results.

Panel B of Table 2 summarizes students' long-run educational and labor market outcomes. The six-year high school graduation rate for these cohorts is 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, are similar to 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, Hansen, and Lindsay 2021; Putman et al. 2016), girls and specifically White girls are significantly more likely than their Black and male counterparts to major in education and/or become a teacher. For example, girls are about six times more likely than boys, and White girls are four times more likely than Black girls, to major in education and/or become a teacher. Black girls are slightly more likely to hold an alternative or nontraditional teaching license than White girls, though overall these are significantly rarer than traditional teaching licenses. Alternative or "conditional" licenses allow individuals to teach while completing steps necessary for full certification (i.e., coursework, testing), while traditional licenses imply that individuals completed all steps prior to entering the classroom as a full-time teacher. Because of this, alternative pathways into the profession often are described as decreasing barriers to entry to teaching.

Unsurprisingly, rates of becoming a teacher in a Maryland public school are substantially higher amongst TAM starters (9.8%) and TAM completers (20%; not shown in Table 2), relative to students as a whole (1.3%). 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 become teachers also suggests that the program could benefit students who ultimately choose a different career.

Finally, the wage data show that Black girls are about 10% more likely to show positive earnings than White girls, but earn significantly less, on average, to the tune of 40%.

4. Identification Strategy

Our goal is to estimate causal ITT 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. Our primary estimates cluster standard errors at the school level. In Appendix Table 5 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 free or reduced-price meals (FARMS), English language learner (ELL), special education (SPED), lagged standardized math and English language arts (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 a given 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 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. This regression-based balance test indicates that within a school, treated and non-treated cohorts are observably similar. These results are shown in Appendix Table 2. Similarly, TAM adoption is unrelated to two school-level factors: cohort size and principal turnover within the two years prior to TAM adoption. Estimates of equation (1) are robust to the inclusion of the aforementioned set of controls, school linear time trends, and observable principal 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 in years before TAM was actually adopted. 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.

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). The 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), 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. As we discuss in a set of robustness tests, the CSDID estimates are qualitatively similar to conventional OLS estimates of equation (1), suggesting that failure of this assumption does not bias the baseline results. Accordingly, we focus on TWFE estimates in our main results.

5. 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 eligibility significantly increased takeup, as defined by enrolling in at least one TAM class, particularly among girls: exposure to TAM increased girls' participation by about 7pp. Takeup was similar for Black girls (7pp) and White girls (9pp).

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 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. As with takeup, there are significant impacts of TAM exposure on both measures of completion that are driven by girls: about 2% of girls finish their TAM coursework and earn a certificate when their cohort is exposed to TAM.

However, unlike in the case of takeup, there is a notable racial disparity in girls' TAM completion rates. About 3% to 4% of White girls completed TAM, which is an order of magnitude larger than the completion rate of Black girls. We do not find any significant effect on TAM course completion or earning a TAM certificate for Black girls. Notably, though, 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 and Hansen 2010). In our sample, though, it appears that Black girls are not taking 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 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, 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 Educational Attainment

Engaging with a program like TAM may increase educational attainment for at least two reasons. First, earning a high school diploma and college degree are prerequisites for entering the teaching profession. Second, 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 prospects and plans by exposure to TAM. Accordingly, in Table 4 we report TWFE estimates of equation (1) for several different educational attainment outcomes that may be affected by exposure to TAM.

Column 1 of Table 4 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 (1.3pp/1%). The impact of TAM on high school graduation is largest for Black girls (2.2pp/3%), though the effect on White girls is significant as well (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). This suggests that TAM's impact is broader and motivates some students to re-engage with school. The high school graduation findings are similar to other recent studies of high school career programs, which stretch beyond teaching (Brunner, Dougherty, and Ross 2023; Dougherty 2018; Hemelt, Lenard, and Paeplow 2019).

Columns 2 and 3 of Table 4 report estimates for enrollment in two- and four-year institutions, respectively. Overall, exposure to TAM increased the likelihood of enrolling in a four-year college by 1.7pp, or 6%. This is partially at the expense of two-year enrollments, but TAM also seems to have created new post-secondary enrollees as well. As in the case of high school graduation, the four-year college enrollment effects are primarily driven by girls (2.6pp/8%), though unlike in the high school case, the magnitudes of these effects are similar for both Black and White girls.

Given that post-secondary enrollments increase in response to TAM, a natural follow-up question is whether graduation rates increase too, which we investigate in columns 4 and 5 of Table 4. These estimates are imprecisely estimated. However, the estimated effects on two-year graduation tend to be negative while those on 4-year graduation tend to be positive, again suggesting a shift from two- to four-year degree programs and degree attainment. For Black girls, the negative effect on two-year college graduation is not completely offset by potential increases in four-year graduation.

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 4 consider whether students earned an AA in education or BA in teaching, respectively. 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. On average across the sample, effects are null. However, exposure to TAM increased the likelihood of Black girls graduating with an AA in education by about 0.1pp, or 105%, and of White girls graduating with a BA in teaching by about 0.9pp, or 23%.

In sum, Table 4 shows that exposure to TAM affected educational attainment, mainly for female high school students, along several different margins. Moreover, on some margins these effects were notably different for Black and White girls, while on others, the effects were homogenous. These results provide two important takeaways. First, career-specific teacher-pathway programs like TAM may well increase educational attainment among students who ultimately are not interested in pursuing teaching. Second, the patterns observed in Table 4 suggest that programs like TAM create new teachers through at least two distinct channels: increasing attainment (extensive margin) and changing "always-college going" students' degree type/major (intensive margin).

5.3. TAM Effects on Becoming a Teacher

Having shown that Maryland students in participating high schools did participate in TAM (Table 3) and that exposure to TAM increased students' educational attainment and, in some instances, the likelihood of earning a degree in teaching (Table 4), we now investigate the program's impact on becoming a public-school teacher in the state. Specifically, in Table 5 we estimate versions of equation (1) in which the outcomes are indicators for teaching in a Maryland public school under various license types. The first column 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 of entering ninth grade. Overall, exposure to TAM increased the likelihood of students becoming a teacher by 0.6pp, or 47%. Consistent with estimated impacts on educational attainment, these effects are larger for girls (0.9pp/41%). 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 80% for Black girls) because, in the absence of TAM, Black girls are substantially less likely to become teachers than White girls.

Subsequent columns of Table 5 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, Mayer, and Decker 2006; Kane, Rockoff, and Staiger 2008). Second, these results will 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.

Columns 2 and 3 of Table 5 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 1, 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 (42% versus 81%). As expected, given that TAM's impact on entering teaching was driven by girls, patterns of absolute and relative differences are similar for this group (0.7pp/42% for traditional license and 0.3pp/80% for alternative license).

However, 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/165%), while White girls' entry is driven primarily by traditional licenses (1.2pp/39%). This finding is consistent with extant evidence that licensure tests and traditional teacher prep programs discriminate against Black women (Cowan et al. 2023; Goldhaber and Hansen 2010) and that, at least descriptively, Black women make up a much larger share of individuals in alternative preparation routes compared to traditional ones, both in Maryland and elsewhere (Bacher-Hicks et al. 2023; Backes and Goldhaber 2023; Blazar et al. 2024).

This result also suggests that TAM is operating in different ways for different demographic groups. 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 though 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 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 the avenues through which they may bolster educational attainment, interest in the teaching profession, and eventual entry into the teaching profession.

5.4. 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.

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.

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, for girls, and for Black girls. The effect for White girls is indistinguishable from zero and imprecisely estimated. Specifically, exposure to TAM increased Black girls' eventual average earnings by about $643 per quarter, or 17%. 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, for girls, and for Black girls; it is indistinguishable from zero for White girls. The point estimate of 0.16 indicates that TAM increased Black girl's average quarterly earnings by about 18%. This in itself 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 Black 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 20% overall and for Black girls, and 28% for girls, while controlling for becoming a teacher attenuates the effect by 7% for Black girls and 10% overall and for girls. 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, Blackwell, and Sen 2016) or the failure of sequential ignorability (Imai, Keele, and Tingley 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, Keele, and Tingley (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, 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, Li, and Lovenheim 2016; Andrews et al. 2022). Quantile estimates are plotted in Figure 1. Generally, the TAM effects on log wages for the 30th percentile and up are similar to those of 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. This is consistent with the distributional returns to an education major (Andrews et al. 2022). 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 suggests that in addition to heterogeneity by gender and race, TAM's impact varied across the earnings distribution.

5.5 Additional Subgroups

In Appendix Tables 3a and 3b, we report the effects of TAM access on outcomes for all additional race-by-gender subgroups not reported in the main results (i.e., boys, Black boys, White boys, Hispanic girls and boys, Asian girls and boys). While TAM adoption induces students in most of these additional subgroups to start TAM, takeup is substantially lower, generally around 1% to 3%, compared to Black girls (7%) and White girls (9%). Hispanic girls are the next most likely to take up the TAM offer (roughly 6%), though we do not find positive effects on subsequent outcomes for this group. Instead, positive and significant effects on educational attainment and career outcomes concentrate among Black boys and Asian girls.

For Black males, exposure to TAM impacts high school graduation (1.3pp/2%), four-year college enrollment (2.3pp/11%), and BA degree receipt (1.7pp/15%). Aligned to these patterns, wage effects also are positive (6%) but estimated imprecisely. We also observe a positive effect on BA in teaching (0.3pp/150%), but not the likelihood of teaching in a Maryland public school. Black boys induced by TAM to earn a BA in teaching may teach out of state or in a private school, which we cannot observe. Similarly, for Asian girls, TAM shifts college going from two-year institutions (-8pp/-20%) to four-year colleges (10.3pp/20%), as well as increases receipt of a BA (5.8pp/10%) and wages (18%). Because we do not find any effect on teaching degrees or becoming a teacher for Asian girls, we infer that TAM primarily benefits Asian girls through educational attainment.

6. Robustness and Sensitivity Analyses

In this section, we probe the robustness of our results to possible threats to identification and inference. First, following the modern DD literature, we implement Callaway and Sant'Anna's (2021) estimator (CSDID), which eliminates "forbidden" comparisons between early and late adopters. In Appendix Tables 4a and 4b, we reproduce the main results presented in section 5 of the main text (Tables 3 through 6) using the CSDID estimator. These estimates are less precise, as expected, but qualitatively similar to our preferred TWFE estimates reported in section 5. The similarity between TWFE and CSDID estimates is reassuring and suggests that heterogeneity in effect size across years does not bias the TWFE estimates.

Next, we replace the binary treatment in equation (1) with a full set of leads and lags in an event-study framework to probe the plausibility of the staggered parallel trends assumption (Roth et al. 2023). Panel A of Figure 2 examines parallel trends in 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 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 take 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 while the second year of adoption captures effects for four years of exposure.

Panel B of Figure 2 similarly examines trends in our primary outcome: became a teacher. In the pre period, trends are flat and indistinguishable from zero in the full sample, for girls, and for Black and White girls separately. Post-treatment estimates resemble the average effect reported in Table 5 and are consistent in magnitude in the first year of adoption and in later cohorts. To increase precision, we pool event-time period two with period one (i.e., cohorts one and two years post adoption). Similarly, we pool event-time period negative three with negative two. Coding of event-time indicators is shown in Appendix Table 1.

We report event-study figures for educational attainment and degree in Appendix Figure 1 and for teaching license and wages in Appendix Figure 2. By and large, pre-treatment trends are flat and indistinguishable from zero, while post-treatment trends mirror our main results. An exception is for wages, where pre-treatment trends are flat in levels but not in logs for Black girls, which is consistent with the idea that the parallel trends assumption can be sensitive to functional form (Roth and Sant'Anna 2023). For White girls, there is some evidence of a pre-trend in wages in both levels and logs (which also drives a pre-trend for girls as a whole). 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. One last point here is that in a multiple hypotheses sense it is unreasonable to expect parallel trends to fully hold across the large number of outcomes and subgroup analyses considered in the current study (Roth and Sant'Anna 2023). The bulk of the evidence, then, suggests that the main results are not compromised by failures of the parallel trends assumption.

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. Appendix Figure 3 presents leave-one-out estimates of the effects of TAM on take up, 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.

In Appendix Tables 5 and 6, we present several additional sensitivity analyses of the main result that access to TAM increased the likelihood of becoming a teacher. Appendix Table 5 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 5, 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 and 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 three columns of Appendix Table 5, 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 Appendix Table 2: TAM adoption is unrelated to within-school changes in the observable characteristics of students. Similarly, in column 3 we augment the baseline model to condition on observable 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 tend to occur at the same time as TAM (or in anticipation of TAM), and this does not change our findings.

In column 4 of Appendix Table 5, we add school-specific linear time trends to the baseline model (Angrist and 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 5, 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.

In Appendix Table 6, we estimate fixed-effect logit models (Chamberlain 1984; Wooldridge 2010) for the key binary outcomes of becoming a teacher and license type, which take the right hand side of equation (1) as their linear index. This exercise is motivated by the concern that the linear probability model (LPM) estimates discussed thus far may provide a poor approximation of the true program effects due to the presence of FE in the model and the skewed distribution of the binary dependent variables (Wooldridge 2010): becoming a public-school teacher in Maryland is quite rare: only 1.4% of 9th graders in the state ultimately become teachers. A quirk of FE-logit is that schools that have no variation in the outcome variable fall out of the likelihood function and are therefore excluded from the sample. For this reason, we also estimate analogous LPMs on these restricted samples and report these estimates alongside the FE-logit estimates.

In a nonlinear DD model like logit, interest is still in the interaction term (i.e., TAM indicator) and its partial effect on the conditional probability (Puhani 2012). However, because the FE-logit does not estimate the fixed effects and we are agnostic about their distribution, proper average partial effects (APE) cannot be computed. Instead, we focus on the sign and statistical significance of the FE-logit coefficient estimates and crudely approximate APE by scaling the coefficients by (Pr y = 1)*(1 - Pr(y = 1)). Importantly, the sign and statistical significance of the FE-logit coefficients reported in Appendix Table 6 align with those of the LPMs reported in Table 5. The LPM estimates on the restricted FE-logit samples are also similar in size and significance to the baseline LPM estimates reported in Table 5, suggesting that the results are not driven by the sample restriction. The approximate "APEs" are qualitatively similar to the analogous LPM estimates, though for Black girls they are notably smaller. This is likely because teaching is such a rare outcome for Black girls in the sample. Still, this does not diminish the fact that TAM exposure significantly increased Black girls' entry into teaching via nontraditional pathways.

In sum, the robustness checks and sensitivity analyses reported in this section reaffirm a causal interpretation of the results presented in Section 5. Specifically, we have shown 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 have also shown that the statistical and economic significance of the results is quite robust to a variety of different, but reasonable, modeling decisions.

7. 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 literatures on workforce development, occupational choice, and labor supply.

The effects are encouraging along several dimensions. TAM induces 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 newly offering teacher preparation programs at elite colleges. When coursework is offered at the high school level, though, the pool of prospective teachers is substantially larger compared to four-year colleges and universities. Further, Reback's estimates are local only to 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, Darling-Hammond, and Carver-Thomas 2016) and more recent concerns that interest in teaching is rapidly declining (Kraft and Lyon 2022), GYO programs like TAM can be a fruitful avenue for building pathways into the profession.

GYO programs like TAM may also help diversify the profession, which is a central goal of many program designers and policymakers (Valenzuela 2017; Gist, Bianco, and Lynn 2019). 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 and 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, or a three-fold difference.

For TAM, or any GYO program, to close this gap entirely, effects for Black girls would need to be much 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, program expansion efforts could target schools and districts with large populations of 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.

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 follow 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 take up the program at similar rates as White girls but are 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 appear to translate into a four-year teaching degree. Instead, Black girls induced by TAM to become teachers do 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 and Goldhaber 2023; Blazar et al. 2024). 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 and information transmission.

Beyond effects on teaching, TAM's impacts on educational attainment and wages compare favorably with other job training programs provided in high school or shortly thereafter (Bonilla 2020; Kemple and Willner 2008; Hemelt, Lenard, and Paeplow 2019; Page 2012; Dougherty 2018; Brunner, Dougherty, and Ross 2023; Bloom 2010). 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, Seyal, and Contreras (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, Kurlaender, and Grosz 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 (Sutcher, Darling-Hammond, and Carver-Thomas 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.

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Tables

Table 1. School Analytic Sample by Treatment Participation

	All Schools	Never TAM Schools	Sometimes TAM Schools	Always TAM Schools
	(1)	(2)	(3)	(4)
Asian	0.06	0.05	0.06	0.07
Black	0.35	0.37	0.37	0.29
Hispanic	0.10	0.10	0.09	0.10
White	0.43	0.43	0.42	0.46
Female	0.49	0.49	0.49	0.50
FARMS	0.36	0.40	0.31	0.31
Access to TAM	0.36	0.00	0.60	1.00
Started TAM	0.013	0.002	0.036	0.027
Finished TAM Courses	0.004	0.001	0.008	0.011
TAM Certificate	0.004	0.001	0.007	0.009
Become a Teacher	0.014	0.013	0.014	0.017
Schools	*210*	*137*	*20*	*53*
Observations	*318,753*	*189,783*	*36,060*	*92,910*

Notes: Sample restricted to 9th grade cohorts between 2009 and 2013. 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 set of schools are excluded from all analyses.

Table 2. Student Analytic Sample by Subgroups

	All	Girls	Black Girls	White Girls
	(1)	(2)	(3)	(4)
Panel A: TAM				
Access to TAM	0.096	0.096	0.092	0.090
Started TAM	0.007	0.012	0.011	0.014
Finished TAM Courses	0.002	0.003	0.001	0.006
TAM Certificate	0.002	0.003	0.001	0.006
Panel B: Outcomes				
High-School Graduation	0.895	0.918	0.890	0.947
2-Year College Enrollment	0.396	0.425	0.371	0.458
4-Year College Enrollment	0.293	0.328	0.304	0.358
2-Year College Graduation	0.082	0.093	0.042	0.126
4-Year College Graduation	0.265	0.316	0.214	0.402
Associate Degree Education	0.004	0.007	0.001	0.012
Associate Degree Teaching	0.003	0.005	0.001	0.009
Bachelor Degree Teaching	0.014	0.024	0.009	0.039
Bachelor Degree Education	0.015	0.026	0.009	0.042
Became a Teacher	0.013	0.022	0.009	0.037
Traditional License	0.010	0.017	0.003	0.031
Alternative License	0.003	0.004	0.005	0.004
Positive Earnings	0.549	0.565	0.600	0.553
Mean Quarterly Earnings (Non-Missing)	8912	8189	6439	9477
Log of Mean Quarterly Earnings (Non-Missing)	8.72	8.65	8.38	8.84
Observations	*225,843*	*110,245*	*40,142*	*47,092*

Notes: In Panel A, each row indicates the proportion of the subgroup students 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 is 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. Teacher 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 varies slightly across different outcomes; results tables include the exact number of observations included in each analysis.

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*
Observations	*225,843*	*225,843*	*225,843*
Girls	0.071***	0.019***	0.018***
	(0.011)	(0.005)	(0.005)
Control Mean	*0.004*	*0.001*	*0.001*
Observations	*110,245*	*110,245*	*110,245*
Black Girls	0.068***	0.004	0.003
	(0.014)	(0.002)	(0.002)
Control Mean	*0.003*	*0.001*	*0.001*
Observations	*40,142*	*40,142*	*40,142*
White Girls	0.083***	0.038***	0.034***
	(0.015)	(0.008)	(0.008)
Control Mean	*0.004*	*0.002*	*0.002*
Observations	*47,092*	*47,092*	*47,092*

Notes: Estimates in each cell come from separate two-way fixed effect (TWFE) models of the effect of TAM access on TAM 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. * $p<0.10$, ** $p<0.05$, *** $p<0.001$

Table 4. Effect of TAM Access on Educational Attainment and Degree

	High School Graduation	2-Year College Enrollment	4-Year College Enrollment	2-Year College Graduation	4-Year College Graduation	AA in Education	BA in Teaching
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
All	0.008**	-0.007	0.017**	-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*	*0.260*	*0.004*	*0.014*
Observations	*207,484*	*209,859*	*209,859*	*209,861*	*209,861*	*207,129*	*207,129*
Girls	0.013**	-0.014	0.026**	-0.005	0.009	-0.001	0.002
	(0.005)	(0.009)	(0.010)	(0.005)	(0.011)	(0.001)	(0.003)
Control Mean	*0.916*	*0.428*	*0.321*	*0.094*	*0.310*	*0.007*	*0.024*
Observations	*102,116*	*103,473*	*103,473*	*103,473*	*103,473*	*102,175*	*102,175*
Black Girls	0.022**	-0.007	0.027	-0.012**	0.006	0.001*	0.001
	(0.009)	(0.012)	(0.017)	(0.006)	(0.018)	(0.001)	(0.003)
Control Mean	*0.888*	*0.375*	*0.296*	*0.043*	*0.207*	*0.001*	*0.008*
Observations	*36,948*	*37,358*	*37,358*	*37,358*	*37,358*	*36,838*	*36,838*
White Girls	0.010*	-0.006	0.022	0.010	0.004	-0.002	0.009*
	(0.006)	(0.015)	(0.016)	(0.009)	(0.018)	(0.002)	(0.005)
Control Mean	*0.946*	*0.461*	*0.352*	*0.127*	*0.397*	*0.012*	*0.039*
Observations	*43,895*	*44,560*	*44,560*	*44,560*	*44,560*	*43,995*	*43,995*

Notes: Estimates in each cell come from separate TWFE models that include school and cohort fixed effects, as well as student and school-year covariates. Standard errors are clustered at the school level. * p<0.10, ** p<0.05, *** p<0.001

Table 5. Effect of TAM Access on Becoming a Teacher

	Became a Teacher (1)	Traditional License (2)	Alternative License (3)
All	0.006***	0.004***	0.002***
	(0.001)	(0.001)	(0.000)
Control Mean	*0.013*	*0.009*	*0.002*
Observations	*225,843*	*225,554*	*225,554*
Girls	0.009***	0.007**	0.003***
	(0.002)	(0.003)	(0.001)
Control Mean	*0.022*	*0.017*	*0.004*
Observations	*110,245*	*110,019*	*110,019*
Black Girls	0.007***	0.001	0.007***
	(0.002)	(0.002)	(0.002)
Control Mean	*0.009*	*0.003*	*0.004*
Observations	*40,142*	*40,072*	*40,072*
White Girls	0.014***	0.012***	0.001
	(0.004)	(0.004)	(0.002)
Control Mean	*0.036*	*0.031*	*0.004*
Observations	*47,092*	*46,976*	*46,976*

Notes: Estimates in each cell come from separate TWFE linear models that include school and cohort fixed effects, as well as student and school-year covariates. Standard errors are clustered at the school level. * p<0.10, ** p<0.05, *** p<0.001

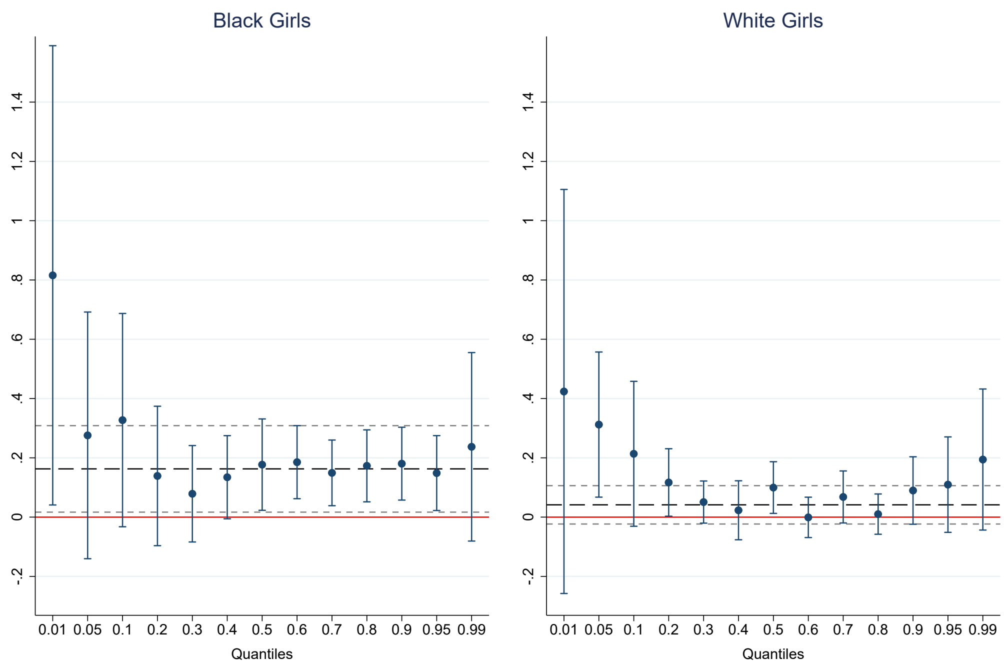
Table 6. Effect of TAM Access on Earnings

	Positive Earnings	Mean Quarterly Earnings	Log Earnings	Log Earnings (Control for Ed. Attainment)	Log Earnings (Control for Become a Teacher)
	(1)	(2)	(3)	(4)	(5)
All	0.004	271**	0.051**	0.040*	0.046**
	(0.008)	(127)	(0.020)	(0.021)	(0.020)
Control Mean	*0.553*	*4886*	*8.71*		
Observations	*182,167*	*100,034*	*100,034*	*96,103*	*100,034*
Girls	0.003	370**	0.080**	0.058*	0.072**
	(0.010)	(174)	(0.035)	(0.034)	(0.035)
Control Mean	*0.570*	*4615*	*8.64*		
Observations	*88,952*	*50,235*	*50,235*	*48,511*	*50,235*
Black Girls	-0.004	643*	0.163**	0.130	0.152**
	(0.016)	(334)	(0.074)	(0.081)	(0.074)
Control Mean	*0.608*	*3874*	*8.37*		
Observations	*32,671*	*19,608*	*19,608*	*18,836*	*19,608*
White Girls	0.005	-19	0.041	0.029	0.030
	(0.014)	(244)	(0.033)	(0.030)	(0.032)
Control Mean	*0.556*	*5220*	*8.83*		
Observations	*38,307*	*21,200*	*21,200*	*20,551*	*21,200*

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

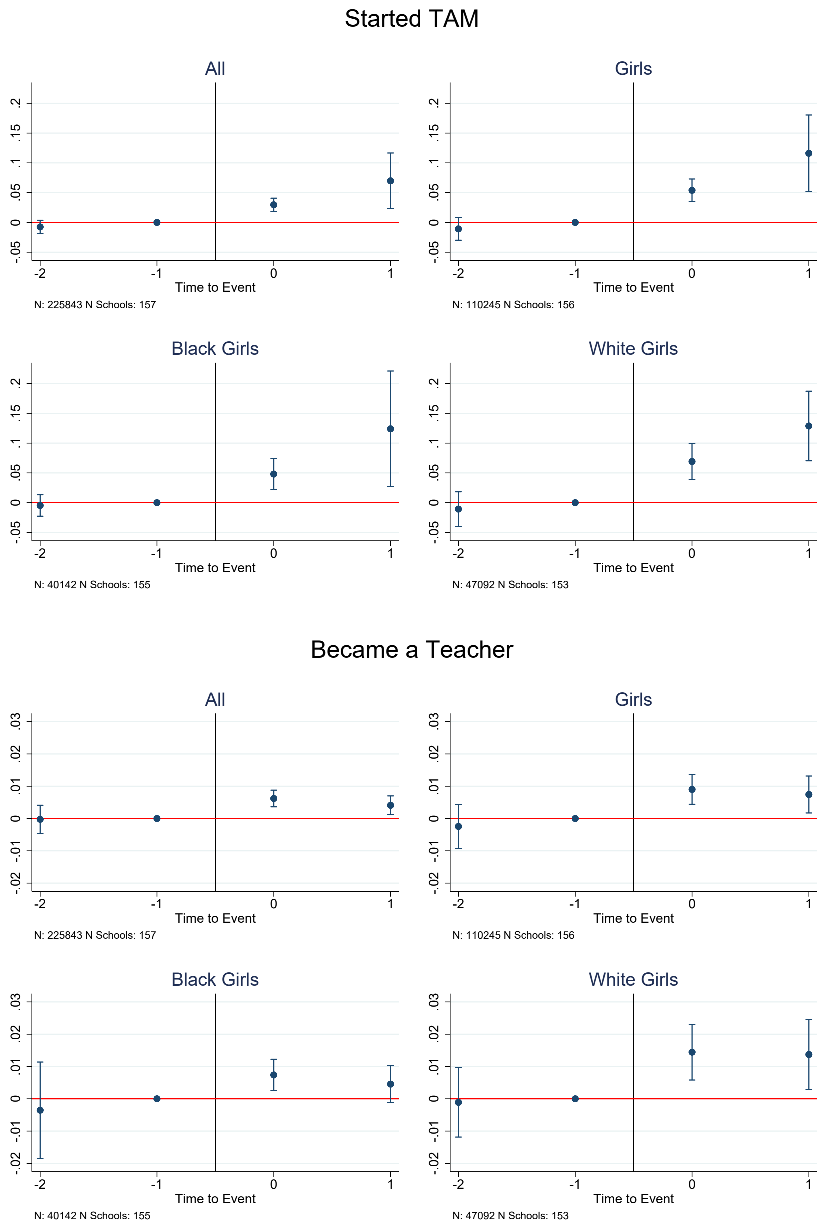
Figures

Figure 1. 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.

Figure 2. Event Study Analysis of the Effect of TAM Access on Starting TAM and Becoming a Teacher



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 Appendix Table 1. Standard errors used to compute confidence intervals are clustered at the high school level.

Appendix

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)
Black	-0.004	0.003
	(0.006)	(0.022)
White	-0.003	0.007
	(0.008)	(0.015)
Hispanic	0.002	0.000
	(0.004)	(0.010)
Asian	0.004	-0.004
	(0.003)	(0.015)
Multiple	0.001	-0.007
	(0.003)	(0.008)
Female	0.001	0.018
	(0.007)	(0.021)
ELL	0.002	0.015
	(0.003)	(0.024)
Special Education	-0.007	-0.009
	(0.005)	(0.013)
Math Scores	-0.018	-0.056
	(0.029)	(0.036)
ELA Scores	-0.003	-0.023
	(0.026)	(0.029)
P-Value on Joint Test	0.703	0.378
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) and change in principal one or two years prior to adoption (est. = 0.022, s.e. = 0.062). * p<0.10, ** p<0.05, *** p<0.001

Appendix Table 3a. Effect of TAM Access on Takeup, Completion, and Educational Attainment for Additional Subgroups

	TAM Start	Finished TAM Courses	TAM Certificate	High School Grad.	2-Year Coll. Enroll.	4-Year Coll. Enroll.	2-Year Coll. Grad.	4-Year Coll. Grad.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Boys	0.011**	0.001	0.001	0.005	-0.001	0.009	0.000	0.010
	(0.004)	(0.001)	(0.001)	(0.005)	(0.010)	(0.007)	(0.006)	(0.007)
Control Mean	*0.001*	*0.000*	*0.000*	*0.871*	*0.369*	*0.254*	*0.071*	*0.211*
Observations	*115,598*	*115,598*	*115,598*	*105,368*	*106,388*	*106,388*	*106,388*	*106,388*
Black Boys	0.018***	0.001	0.001	0.013*	-0.006	0.023**	0.005	0.017*
	(0.007)	(0.000)	(0.000)	(0.008)	(0.018)	(0.011)	(0.010)	(0.009)
Control Mean	*0.001*	*0.000*	*0.000*	*0.820*	*0.327*	*0.213*	*0.031*	*0.114*
Observations	*42,767*	*42,767*	*42,767*	*38,087*	*38,361*	*38,361*	*38,361*	*38,361*
White Boys	0.007**	0.001	0.001	0.002	0.000	0.005	-0.005	0.007
	(0.003)	(0.002)	(0.002)	(0.006)	(0.010)	(0.011)	(0.007)	(0.011)
Control Mean	*0.001*	*0.000*	*0.000*	*0.921*	*0.383*	*0.293*	*0.096*	*0.284*
Observations	*49,216*	*49,216*	*49,216*	*45,682*	*46,205*	*46,205*	*46,205*	*46,205*
Asian Girls	0.031**	0.003**	0.003*	0.005	-0.080**	0.103***	-0.034	0.058**
	(0.012)	(0.001)	(0.002)	(0.011)	(0.038)	(0.026)	(0.027)	(0.027)
Control Mean	*0.004*	*0.001*	*0.000*	*0.980*	*0.400*	*0.520*	*0.116*	*0.560*
Observations	*5,364*	*5,364*	*5,364*	*4,991*	*5,053*	*5,053*	*5,053*	*5,053*
Asian Boys	0.009	NA	NA	-0.020	-0.064*	0.007	-0.028*	-0.001
	(0.006)			(0.022)	(0.036)	(0.037)	(0.015)	(0.027)
Control Mean	*0.001*	*0.000*	*0.000*	*0.964*	*0.429*	*0.452*	*0.099*	*0.465*
Observations	*5,541*	*5,541*	*5,541*	*5,125*	*5,184*	*5,184*	*5,184*	*5,184*
Hispanic Girls	0.046***	0.003	0.002	0.017	-0.037	0.003	-0.015	0.005
	(0.012)	(0.002)	(0.001)	(0.017)	(0.033)	(0.029)	(0.017)	(0.026)
Control Mean	*0.002*	0.000	0.000	*0.825*	*0.394*	*0.182*	*0.078*	*0.167*
Observations	*10,417*	*10417*	*10417*	*9,441*	*9,512*	*9,512*	*9,512*	*9,512*
Hispanic Boys	0.010*	NA	NA	-0.001	0.026	-0.043**	0.023	-0.014
	(0.006)			(0.023)	(0.025)	(0.020)	(0.016)	(0.017)
Control Mean	*0.001*	*0.000*	*0.000*	*0.755*	*0.332*	*0.128*	*0.053*	*0.101*
Observations	*11,621*	*11,621*	*11,621*	*10,420*	*10,479*	*10,479*	*10,479*	*10,479*

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. NA indicates that too few individuals from a given subgroup achieved a given outcome for coefficients to be estimated. * p<0.10, ** p<0.05, *** p<0.001

Appendix Table 3b. Effect of TAM Access on College Major, Occupational Choice, and Earnings for Additional Subgroups

	AA in Education (1)	BA in Teaching (2)	Became a Teacher (3)	Traditional License (4)	Alternative License (5)	Log Quarterly Earnings (6)
Boys	-0.000	0.003**	0.002**	0.002***	0.001	0.019
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.020)
Control Mean	*0.001*	*0.004*	*0.004*	*0.002*	*0.001*	*8.78*
Observations	*104,954*	*104,954*	*115,598*	*115,535*	*115,535*	*49,799*
Black Boys	0.000	0.003**	0.001	0.001	-0.000	0.057
	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.049)
Control Mean	*0.000*	*0.002*	*0.002*	*0.000*	*0.001*	*8.43*
Observations	*37,697*	*37,697*	*42,767*	*42,749*	*42,749*	*18,444*
White Boys	-0.000	0.003	0.005***	0.004***	0.002	0.002
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.031)
Control Mean	*0.002*	*0.006*	*0.006*	*0.005*	*0.001*	*9.03*
Observations	*45,660*	*45,660*	*49,216*	*49,184*	*49,184*	*22,317*
Asian Girls	0.003	-0.013	-0.005	0.001	0.000	0.164**
	(0.004)	(0.010)	(0.012)	(0.011)	(0.001)	(0.071)
Control Mean	*0.004*	*0.024*	*0.013*	*0.011*	*0.002*	*8.87*
Observations	*5,009*	*5,009*	*5,364*	*5,355*	*5,355*	*1,881*
Asian Boys	-0.004	0.004*	-0.010	-0.004	-0.005	-0.032
	(0.006)	(0.002)	(0.007)	(0.004)	(0.005)	(0.089)
Control Mean	*0.001*	*0.002*	*0.003*	*0.001*	*0.001*	*9.00*
Observations	*5,135*	*5,135*	*5,541*	*5,539*	*5,539*	*1,974*
Hispanic Girls	-0.008*	-0.011	0.001	0.003	0.003	0.024
	(0.004)	(0.008)	(0.005)	(0.004)	(0.003)	(0.084)
Control Mean	*0.004*	*0.012*	*0.010*	*0.007*	*0.003*	*8.72*
Observations	*9,431*	*9,431*	*10,417*	*10,407*	*10,407*	*4,125*
Hispanic Boys	0.002	0.001	0.002	0.002	-0.000	-0.080
	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.109)
Control Mean	*0.000*	*0.002*	*0.002*	*0.001*	*0.000*	*8.91*
Observations	*10,382*	*10,382*	*11,621*	*11,617*	*11,617*	*4,055*

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. NA indicates that too few individuals from a given subgroup achieved a given outcome for coefficients to be estimated. * p<0.10, ** p<0.05, *** p<0.001

Appendix Table 4a. Effect of TAM Access on Takeup, Completion, and Educational Attainment using Callaway and Sant'Anna Estimator

	Started TAM	Finished TAM Courses	TAM Certificate	High School Grad.	2-Year Coll. Enroll.	4-Year Coll. Enroll.	2-Year Coll. Grad.	4-Year Coll. Grad.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All	0.047***	0.006*	0.005*	0.014**	-0.005	0.020*	0.002	0.012
	(0.012)	(0.003)	(0.003)	(0.006)	(0.010)	(0.012)	(0.004)	(0.012)
Control Mean	*0.002*	*0.001*	*0.001*	*0.893*	*0.398*	*0.287*	*0.082*	*0.260*
Observations	*225,843*	*225,843*	*225,843*	*207,484*	*209,859*	*209,859*	*209,861*	*209,861*
Girls	0.080***	0.013**	0.011**	0.022**	-0.006	0.035**	0.005	0.016
	(0.019)	(0.005)	(0.005)	(0.009)	(0.012)	(0.017)	(0.007)	(0.018)
Control Mean	*0.004*	*0.001*	*0.001*	*0.916*	*0.428*	*0.321*	*0.094*	*0.310*
Observations	*110,245*	*110,245*	*110,245*	*102,116*	*103,473*	*103,473*	*103,473*	*103,473*
Black Girls	0.082***	0.003*	0.003*	0.032	0.006	0.023	-0.003	0.009
	(0.024)	(0.002)	(0.002)	(0.020)	(0.017)	(0.032)	(0.007)	(0.031)
Control Mean	*0.003*	*0.001*	*0.001*	*0.888*	*0.375*	*0.296*	*0.043*	*0.207*
Observations	*40,142*	*40,142*	*40,142*	*36,948*	*37,358*	*37,358*	*37,358*	*37,358*
White Girls	0.097***	0.026***	0.023**	0.020**	-0.010	0.047*	0.020	0.027
	(0.021)	(0.010)	(0.010)	(0.009)	(0.021)	(0.025)	(0.013)	(0.024)
Control Mean	*0.004*	*0.002*	*0.002*	*0.946*	*0.461*	*0.352*	*0.127*	*0.397*
Observations	*47,092*	*47,092*	*47,092*	*43,895*	*44,560*	*44,560*	*44,560*	*44,560*

Notes: Estimates in each cell are obtained from the Callaway and Sant'Anna (2021) estimator (CSDID) and include student and school-year covariates. Standard errors are clustered at the school level. * p<0.10, ** p<0.05, *** p<0.001

Appendix Table 4b. Effect of TAM Access on College Major, Occupational Choice, and Earnings using Callaway and Sant'Anna Estimator

	AA in Education	BA in Teaching	Became a Teacher	Trad. License	Alt. License	Log Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
All	-0.001	0.002	0.005***	0.004**	0.002**	0.037
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.023)
Control Mean	*0.004*	*0.014*	*0.013*	*0.009*	*0.002*	*8.71*
Observations	*207,129*	*207,129*	*225,843*	*225,554*	*225,554*	*100,034*
Girls	-0.002	0.003	0.009***	0.007**	0.004***	0.088**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.001)	(0.042)
Control Mean	*0.007*	*0.024*	*0.022*	*0.017*	*0.004*	*8.64*
Observations	*102,175*	*102,175*	*110,245*	*110,019*	*110,019*	*50,235*
Black Girls	0.002**	0.001	0.006*	-0.000	0.007***	0.144
	(0.001)	(0.003)	(0.003)	(0.002)	(0.002)	(0.097)
Control Mean	*0.001*	*0.008*	*0.009*	*0.003*	*0.004*	*8.37*
Observations	*36,838*	*36,838*	*40,142*	*40,072*	*40,072*	*19,608*
White Girls	-0.003	0.010*	0.016***	0.014***	0.002	0.101**
	(0.004)	(0.005)	(0.005)	(0.005)	(0.002)	(0.046)
Control Mean	*0.012*	*0.039*	*0.036*	*0.031*	*0.004*	*8.83*
Observations	*43,995*	*43,995*	*47,092*	*46,976*	*46,976*	*21,200*

Notes: Estimates in each cell are obtained from the Callaway and Sant'Anna (2021) estimator (CSDID) and include student and school-year covariates. Standard errors are clustered at the school level. * p<0.10, ** p<0.05, *** p<0.001

Appendix Table 5. Sensitivity of Main Estimates on Becoming a Teacher to Alternative Specifications

	Main/ Preferred	No controls	Control for Principal Characteristics and Turnover	Control for School Time Trends	Exclude Post-Adoption Cohorts
	(1)	(2)	(3)	(4)	(5)
All	0.006***	0.006***	0.006***	0.008***	0.006***
SE Clustered at School Level	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
SE Clustered at School-Year	(0.001)				
SE Clustered at District-Year	(0.002)				
Control Mean	*0.013*				
Observations	*225,843*	*225,843*	*224,954*	*225,843*	*217,777*
Girls	0.009***	0.009***	0.009***	0.013***	0.009***
SE Clustered at School Level	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
SE Clustered at School-Year	(0.002)				
SE Clustered at District-Year	(0.003)				
Control Mean	*0.022*				
Observations	*110,245*	*110,245*	*109,784*	*110,245*	*106,324*
Black Girls	0.007***	0.007***	0.008***	0.011***	0.008***
SE Clustered at School Level	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)
SE Clustered at School-Year	(0.002)				
SE Clustered at District-Year	(0.001)				
Control Mean	*0.009*				
Observations	*40,142*	*40,142*	*39,796*	*40,142*	*38,809*
White Girls	0.014***	0.014***	0.012***	0.019**	0.014***
SE Clustered at School Level	(0.004)	(0.004)	(0.004)	(0.008)	(0.004)
SE Clustered at School-Year	(0.004)				
SE Clustered at District-Year	(0.005)				
Control Mean	*0.036*				
Observations	*47,092*	*47,092*	*47,019*	*47,092*	*45,670*

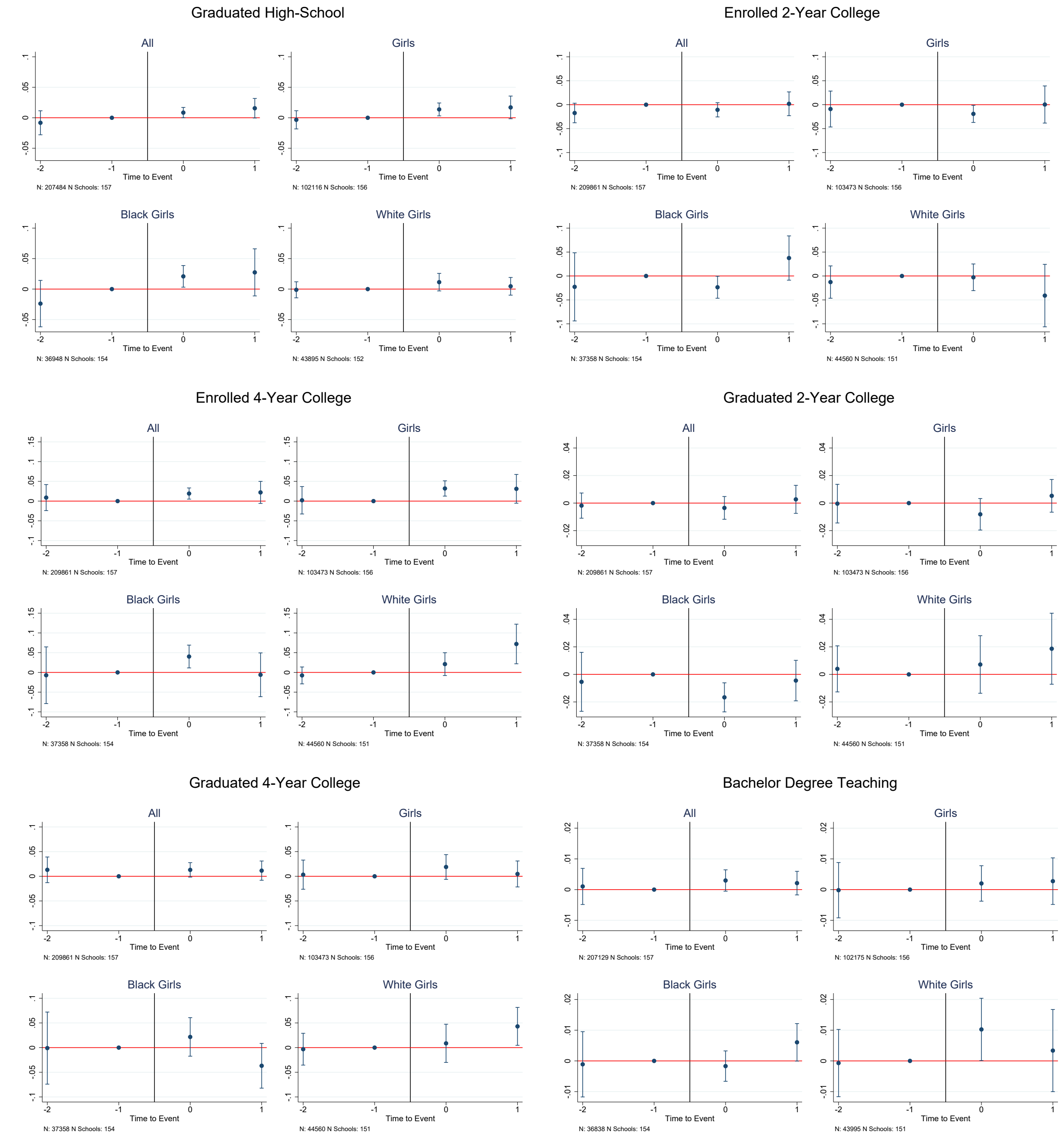
Notes: Estimates in each cell come from separate TWFE models of the effect of TAM on becoming a teacher (our main outcome). In column (1), we show the coefficients for this outcome using our preferred estimation approach, with standard errors clustered at three levels: school (our preferred method), school-year, and district-year. Column (2) reports the results when excluding covariates. Column (3) reports estimates after conditioning on an indicator for principal turnover and observable principal characteristics (i.e., gender, race/ethnicity, experience). Column (4) reports the results when including a school linear time trend. Column (5) 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 6. Effect of TAM Access on Becoming a Teacher and Teacher License (Logits)

	Became a Teacher		Traditional License		Alternative License	
	Logit	OLS	Logit	OLS	Logit	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
All	0.406***	0.006***	0.446***	0.005***	0.552**	0.002***
	(0.112)	(0.001)	(0.133)	(0.001)	(0.235)	(0.000)
APE	*0.005*		*0.004*		*0.001*	
Control Mean	*0.013*		*0.009*		*0.002*	
Observations	*219,126*	*219,126*	*205,794*	*205,794*	*204,101*	*204,101*
Girls	0.376***	0.009***	0.382***	0.007**	0.615**	0.003***
	(0.124)	(0.002)	(0.143)	(0.003)	(0.281)	(0.001)
APE	*0.008*		*0.006*		*0.002*	
Control Mean	*0.022*		*0.017*		*0.004*	
Observations	*107,341*	*107,341*	*100,487*	*100,487*	*93,645*	*93,645*
Black Girls	0.495*	0.008***	0.189	0.001	0.876**	0.008***
	(0.293)	(0.002)	(0.470)	(0.002)	(0.397)	(0.002)
APE	*0.004*		*0.001*		*0.003*	
Control Mean	*0.009*		*0.003*		*0.004*	
Observations	*34,393*	*34,393*	*24,449*	*24,449*	*27,385*	*27,385*
White Girls	0.395***	0.014***	0.412**	0.012***	0.354	0.002
	(0.151)	(0.004)	(0.164)	(0.004)	(0.523)	(0.002)
APE	*0.014*		*0.012*		*0.001*	
Control Mean	*0.036*		*0.031*		*0.004*	
Observations	*46,395*	*46,395*	*46,189*	*46,189*	*37,357*	*37,357*

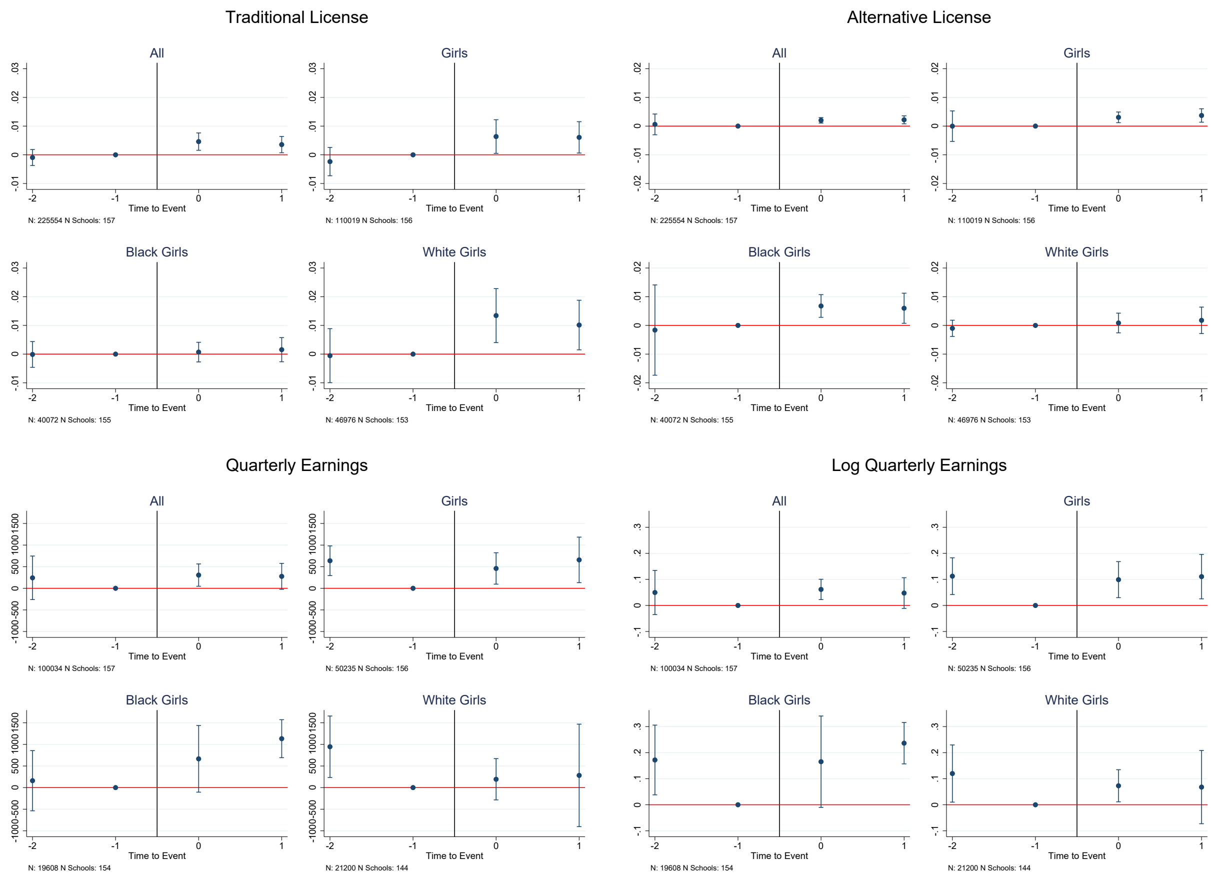
Notes: Logit estimates come from separate logistic regression models that include school and year fixed effects, and student and school-year covariates. Standard errors are clustered at the school level. The approximate average partial effect (APE) is estimated by multiplying the logit coefficient by p*(1-p), where p is the mean of the outcome. We also present OLS estimates are estimated from the same sample as the logit models, which drop units without identifying variation. * p<0.10, ** p<0.05, *** p<0.001

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

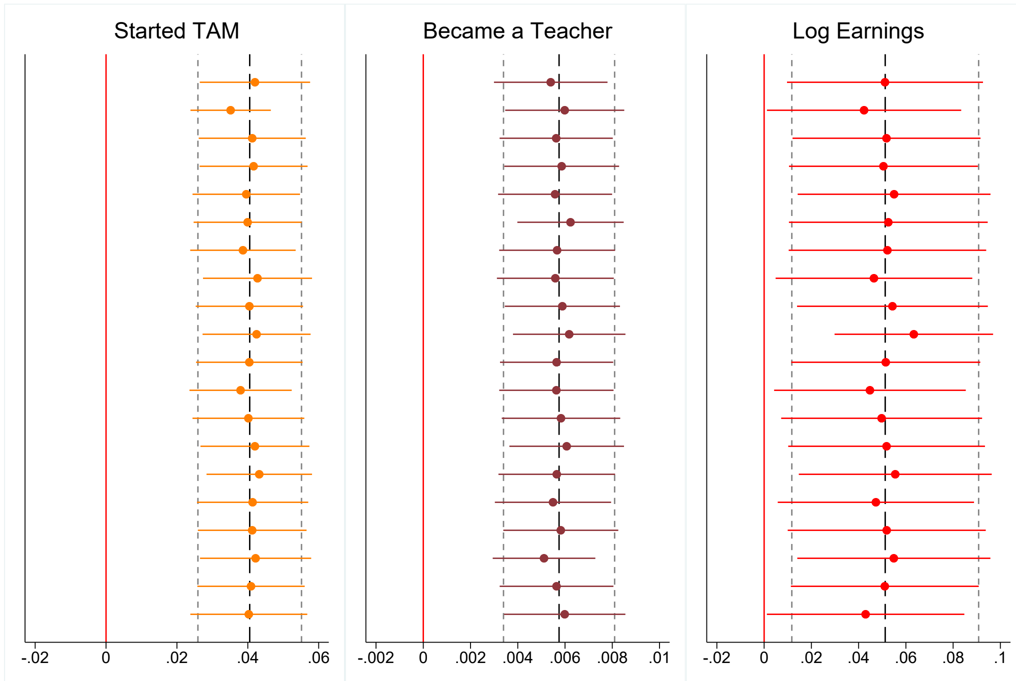


Notes: This figure reports event study point estimates and 95% confidence intervals from regression specifications that include lead and lag indicators for access to TAM as well as school and cohort fixed effects. Student and school-year covariates are excluded. The event time variable on the x-axis 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 Appendix Table 1. Standard errors used to compute confidence intervals are clustered at the high school level. This note applies to all event study figures in the appendix.

Appendix Figure 2. Event Study Analyses of the Effect of TAM Access on Labor Market Outcomes



Appendix Figure 3. 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.