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Some Promises are Worth More than Others: How "Free Community College" Programs impact Postsecondary Participation, Destinations, and Degree Completion

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"Free college" programs are widespread in American higher education. They are discussed as addressing college access, affordability, inequality, and skills shortages. Many are last-dollar tuition guarantees restricted to use at single community colleges. Using student-level data spanning the transition to college, we investigate how two similar local community college tuition guarantees in Pennsylvania affected college-going outcomes. The Morgan Success Scholarship has large impacts on community college attendance and associate degree attainment. The program diverts students away from four-year colleges, though much of this effect is temporary. Meanwhile, we find little evidence that the Community College of Philadelphia's 50th Anniversary Scholars program has any impact on college-going behavior. We suggest reasons for divergent findings and offer suggestions for practice.

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Some Promises are Worth More than Others: How "Free Community College" Programs impact Postsecondary Participation, Destinations, and Degree Completion

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Abstract

"Free college" programs are widespread in American higher education. They are discussed as addressing college access, affordability, inequality, and skills shortages. Many are last-dollar tuition guarantees restricted to use at single community colleges. Using student-level data spanning the transition to college, we investigate how two similar local community college tuition guarantees in Pennsylvania affected college-going outcomes. The Morgan Success Scholarship has large impacts on community college attendance and associate degree attainment. The program diverts students away from four-year colleges, though much of this effect is temporary. Meanwhile, we find little evidence that the Community College of Philadelphia's 50th Anniversary Scholars program has any impact on college-going behavior. We suggest reasons for divergent findings and offer suggestions for practice.

Some Promises are Worth More than Others: How "Free Community College" Programs impact Postsecondary Participation, Destinations, and Degree Completion

There is, today, a widespread consensus that college is too expensive (Nguyen et al., 2023), and this perception is likely implicated in the unprecedented enrollment crisis currently afflicting American higher education. Systemwide, enrollment was lower in 2020 than it was in 2008 despite population increase, and community college enrollment is 30% below its 2008 level. Nonparticipation, not demographics, is driving the current decline; the direct high school enrollment rate has fallen almost yearly since 2009 and, by 2021, hit its lowest point (61.8%) since 2001 (National Center for Education Statistics, n.d.).

Amidst these concerns, and amidst federal inaction to address them, actors at the state and local level have created scores of "free college" or "free tuition" programs (often called "Promise" programs[1]). These programs are pitched as dramatically reducing college costs, usually through a guarantee that tuition will be fully covered for eligible students (a "tuition guarantee", which we discuss below). Many if not most of these programs, both state and local, are usable only at community colleges (Jones et al. 2020; College Promise 2021).

Community college tuition guarantees seem capable of both increasing overall postsecondary participation and buttressing enrollment at applicable colleges. However, at present we have limited knowledge of the impacts of community college tuition guarantees and

[1] The definitions of "Promise program" and "free college program" are contested. For this paper, I define a free college program as a full tuition guarantee granted on an entitlement basis to eligible students attending applicable colleges. Such a program is *local* if it is administered by an agency within a state (rather than a state government), though this may include a state university. Usually, though not always, eligibility is geographically limited to an area within a state (e.g., county, municipality, community college district).

even less about the effects of *local* community college programs despite their prevalence. And we know very little about "free community college" programs' effects on outcomes beyond college enrollment. Such knowledge is crucial to inform policy creation at the local, state, and federal levels.

In what follows, we examine the effects of two local community college tuition guarantees (i.e., "Promise programs")— the Morgan Success Scholarship (in Tamaqua) and the Community College of Philadelphia's 50th Anniversary Scholars Program on postsecondary enrollment, destinations, and degree completion. These programs are both located in Pennsylvania and are similarly designed but differ in setting (a small industrial town versus a major city) and vintage (2003, 2015). To preview findings, we find that the Morgan Success Scholarship has strikingly large effects on eligible students but detect little evidence of impacts of the Philadelphia program on key outcomes. These divergent findings suggest that community college tuition guarantees *can* strongly shift college-going behavior but are not guaranteed to do so.

Theory and Prior Research

Theoretical Framework

Most research on financial aid and college-going behavior is guided by human capital theory. Students are alleged to weigh the full lifetime costs and benefits of college attendance for themselves (since costs and returns differ across individuals), attending college if the latter outweighs the former. Financial aid increases college participation by lowering costs to students, shifting the margin of attendance (e.g., Hoxby, 2004). While the intuition behind this theory is

well-supported—attendance is responsive to college pricing—its specifics are undermined by the opacity of American higher education pricing and uncertainty regarding college benefits.

In the United States, the "sticker" price (i.e., listed tuition and fees) often varies drastically and unpredictably from out-of-pocket costs to students and families for two reasons. First, prices are often reduced considerably by federal and state grant programs. Federal need-based grant aid is determined through a complex algorithm involving several factors beyond household income and is thus nearly impossible to calculate in advance (Dynarski et al., 2022). It also requires timely and yearly submission of a lengthy document (the Free Application for Federal Student Aid, or FAFSA), which many students fail to complete (Kofoed, 2017). Further compounding this, states have created a thicket of additional need- and merit-based programs with various eligibility criteria, some of which require submission of additional forms. Second, colleges craft individualized "aid" packages (better thought of as partial tuition waivers) either as a matter of general policy (e.g., "no-loan" programs) or to entice select students to enroll. These practices are particularly widespread in the private nonprofit four-year sector (Cheslock & Riggs, 2021). The result is that students cannot know how much they will be asked to pay at a given college—that is, the "price" to them—until after they have applied and been admitted.

Not only is pricing uncertain but most students and families do not understand that college pricing is structured in this fashion. Many students and families have a vague understanding of need-based grants or of institutional "aid" practices; they do not grasp that the listed price is *almost always* much higher than that which individual students pay (De La Rosa, 2006; Reavis, 2022 p. 11; Monaghan, 2023). Accordingly, students and families frequently overestimate college prices. This tendency is more pronounced for a) lower-priced colleges (especially community colleges) and b) among lower-income and minoritized students and

families—that is, by the most price-sensitive students, for the schools they are most likely to attend (Bleemer & Zafar, 2018; Grodsky & Jones, 2007).

Even if college pricing were completely transparent and predictable, the college-going decision would be made under considerable uncertainty. Students cannot know whether they will complete their intended degree (nor in how much time), nor can they know their *individual* lifetime college premium (Oreopoulos & Petronijevic, 2013). Students also systematically underestimate non-pecuniary returns to college education—improvements in health, job satisfaction, marital stability, and children's educational attainment, among other outcomes (McMahon, 2009). Price overestimation and benefit underestimation likely reduce college attendance far below the level predicted by human capital theory. If higher education produces social benefits—positive externalities beyond private returns—then the gap between "optimal" and actual participation will be even larger (McMahon, 2018).

It is relative to these considerations that the "college Promise movement" must be understood. Promise programs are wildly diverse in terms of structure, and even experts do not agree on a coherent and concise definition for them (e.g., Miller-Adams, 2015; Perna & Leigh, 2018). However, at least since the mid-2010s, they have become synonymous with "free college" or "free tuition" programs. These are usually last-dollar tuition guarantees: programs that cover the "gap" between tuition (or tuition and fees) and existing grants, thus "promising" that the price to eligible students (in terms of eligible expenses) will be $0. Such programs reduce both costs *and* the complexity and uncertainty of the price mechanism. Researchers have referred to the latter as Promise programs' "messaging effects" (Harnisch & Lebioda, 2016 p. 10; Monaghan 2023).

Depending on the student and the program, the relative weight of cost-reduction and messaging can vary drastically. As Andrews (2014) and Harris (2013) argue, generous four-year applicable tuition guarantees may increase participation both by lowering real costs to students and by assuring students that the cost they face will be low. That is, such programs' "messaging" informs students about real cost reductions. On the other hand, because community college tuition is often less than a full Pell grant, a community college tuition guarantee may deliver little additional aid, particularly to lower-income students. A community college tuition guarantee limited to lower-income students, or to a college that serves a mostly lower-income population, will mostly *only* involve messaging (Miller-Adams, 2015 p. 45). However, this message may be sufficient to boost college participation among lower-income students (Bell and Gándara 2021; Perna and Smith 2020).

Community college tuition guarantees likely shift college-going behavior along two margins (Mountjoy, 2022). They will entice some students to enroll who otherwise would not have. They will *also* entice some students to enroll immediately in a community college who otherwise would have gone to a four-year college. Researchers refer to these effects as "democratization" and "diversion" respectively (Rouse, 1995). A large body of research suggests that, among bachelor's intending students, enrolling directly in a community college dramatically lowers one's chances of earning a bachelor's degree (Monaghan & Attewell, 2015; Schudde & Brown, 2019). While program effects along the first margin will increase overall educational attainment, those along this second margin may reduce it. A community college tuition guarantee's aggregate effect on attainment may be positive or negative depending on the sizes of these countervailing effects.

Additionally, tuition-guarantee programs will not change behavior unless students and families know of their existence. Programs are, therefore, crucially dependent on effective networks of communication, and particularly on direct-service personnel in high schools (Reavis, 2022). Depending on the target population, communication may need to be buttressed by proactive supportive services, particularly around FAFSA completion and enrollment support to reduce summer melt.

Prior Empirical Research

Tuition-guarantees may change postsecondary behavior through both cost-reduction and perception-realignment (i.e., "messaging") mechanisms, with the former downstream from and contingent upon the latter. There is abundant evidence that lower prices boost college participation. Denning (2017) finds that community college enrollment increases by 5.1pp for every $1000 in reduced costs; Acton (2021) estimates a 3.5pp increase for the same cost decrease. Price also seems related to persistence; in a meta-analysis of 43 studies, Nguyen et al. (2019) found that $1,000 in additional grant aid increases persistence and completion by 2-3pp. Anderson (2020) finds that additional state need-based aid increases degree attainment among technical college students, with a larger impact on lower-income students. There is also evidence that messaging that *seems* to reduce costs can also, in and of itself, induce enrollment. Dynarski et al. (2021) conducted a randomized control trial in which the guarantee to lower-income youth of free tuition (given acceptance) to a state flagship university, absent any additional aid, doubled their enrollment rates at that university. Monaghan and Attewell (2023) show that a program promising "free community college" led an additional 1,300 students to enroll at the eligible college, despite providing additional funding to only 30 students.

There is a fair amount of research on college "Promise" programs, but most of it focuses on generous, local programs applicable at four-year colleges (e.g., in Kalamazoo, El Dorado, Pittsburgh, Syracuse, and Buffalo; for a review, see Swanson et al., 2020). These programs have been found to have strong impacts on college-going, particularly (and usually only) at four-year colleges, and smaller but still sizeable impacts on retention and completion (Swanson & Ritter, 2021; Page et al., 2019; Bartik et al., 2021; Bifulco et al., 2019). Such programs are, however, quite exceptional among "Promise" programs, most of which, both state and local, are usable only at community colleges (Perna & Leigh, 2018; College Promise, 2021). Since community colleges have much lower tuition than four-year public colleges, community college tuition guarantees are decidedly less generous than the well-studied Promise programs. Studies of programs like the Kalamazoo and Pittsburgh Promise are, therefore, of limited value to understanding the potential impact of community college tuition guarantees—i.e., of most "Promise" programs.

Several studies use IPEDS[2] data and differences-in-differences (DiD) designs to estimate the impacts of local and state community college Promise programs, most of which are tuition guarantees, on enrollment counts at eligible colleges. The Tennessee Promise, which has been particularly well-studied, increased first-time enrollments at eligible colleges by as much as 40%, some of which came at the expense of public four-year colleges (Nguyen, 2020; Bell, 2021a; House & Dell, 2020; Lee et al., 2022). Collum (2022), investigating the "adult" extension of the Tennessee Promise (Tennessee Reconnect), found that the program increased enrollments by up to 25%. Lee et al. (2022), in a descriptive analysis, document much smaller enrollment increases

[2] The Integrated Postsecondary Data System (IPEDS) contains college-level data reported by colleges to the US Department of Education and is publicly available.

consequent upon the Oregon Promise's launch. Li and Gándara (2020; Gándara & Li, 2020) examine the effects of 33 local programs at 32 community colleges. They find that these programs increased first-time, full-time enrollment by between 9-20%, depending on the comparison group. Consistent with the "messaging" mechanism—the marketing of programs as "free college"— effects were similar regardless of first- or last-dollar design or whether they fully or partially covered tuition. As these studies use college-level data, they suggest but do not directly measure program impacts on student behavior. College enrollment gains can reflect diversion away from non-eligible schools as well as net participation increases, though studies take laudable steps to measure such spillovers (e.g., Li and Gándara, 2020; Bell, 2021a).

There are just three studies that use student-level (state administrative) data to measure the effects of statewide community college tuition-guarantees on postsecondary behavior. Munoz et al. (2016), using a DiD design to study Missouri's A+ program (which predates the Tennessee Promise), estimates a program-induced 5.3pp increase in community college attendance. This program seems to have a larger diversion than democratization effects, as its impact on four-year public college matriculation (-3.8pp) is larger than its impact on overall college-going (+1.5pp). Gurantz (2020) studied the first two years of the Oregon Promise using DiD. In both years, the program increased community college enrollment by 4-5pp. In the first year, this came mostly from reductions in four-year public enrollment, while in the second year, it derived from increased overall participation. Finally, Hodara and Childress (2021), using a fuzzy regression discontinuity design, estimated that the Oregon Promise induced, for students on the margin of qualification, a 21-30pp increase in the probability of immediate college enrollment and a 5-24pp increase in the probability of any enrollment within two years. They also estimate that the program increased the chance of one-year persistence by 11-48pp and of

longer-term persistence or completion by 7-46pp. None of the other studies provide estimates of effects on outcomes beyond initial enrollment.

Finally, there are five studies that examine the impacts of *local* last-dollar community college tuition guarantees. Pluhta and Penny (2013), in a descriptive paper examining an anonymous program, document an increase in the rate of matriculation of eligible high school students to the program college from 4-8% in the years immediately prior to the program to 17-23% in the years following. Carruthers and Fox (2016) studied Tennessee Promise forerunner Knox Achieves (in Knox County, TN) using both DiD and propensity-score matching (PSM) methods. Their DiD analysis estimates a program effect on community college enrollment of 3.1-4.9pp, nearly all of which represents increases in overall college enrollment rather than diversion from four-year colleges. Propensity-score analyses suggest much larger effects: a 29-34pp increase in community college enrollment, a 21-24pp increase in overall college-going, and a 5-13pp decrease in four-year matriculation. Importantly, they document program effects that are similar for free-lunch eligible students, who would mostly not receive any funding from the program, and higher-income students, who would. In a follow-up study, Carruthers et al. (2023) found that after nine years Knox Achieves increased associate degree completion by just under a percentage point (but a 25% increase relative to a 3.3% baseline) and had small and statistically nonsignificant effects on bachelor's attainment (positive) and earnings (negative). Anderson et al. (2023) show that the introduction of a tuition guarantee at Milwaukee Area Technical College led to a 5pp increase in enrollment at that college by eligible students, half of which represented new enrollment rather than diversion, with larger impacts on lower-income students. Finally, Bell (2021b) and Bell and Gándara (2021) use DiD to examine the effects of Tulsa Achieves on post-matriculation outcomes. They find that the program increased the likelihood of vertical transfer

(i.e., to a four-year college) by 7-15pp and of bachelor's completion by 1-2pp, with larger impacts for Black and Latinx students, but had no impact on grades, credit accumulation, or persistence.

Some of these estimates require further clarification for policy relevance. Knox Achieves required students to opt *into* the program in the fall of their senior year, and on average, just 18% of students did so. For Carruthers and Fox's PSM estimates, treated students were those who were *both* geographically eligible *and* had elected to participate; *these* students were then matched to non-geographically eligible students. Resulting estimates capture combined effects of program availability and elective participation. As what may lead students to opt in (versus not) is imperfectly captured in administrative data; unmeasured selection likely biases these estimates. The smaller DiD estimates more credibly measure program availability impacts and are more policy relevant. Hodara and Childress estimate much larger impacts than Gurantz because they are capturing local average treatment effects for students around the eligibility threshold. These estimates are robust but do not generalize to the full state population. Finally, Bell's estimates from Tulsa Achieves diverge from other studies of community college tuition guarantees in finding positive effects on four-year participation and graduation. This is because they use *college*, rather than state, administrative data. Their data doesn't span the high school to college transition, and so both their treatment and comparison groups are already selected into community college. The impact of a community college tuition guarantee on four-year attendance occurs by diverting students away from a four-year school, and Bell's data was gathered after this had taken place. Accordingly, these negative impacts on four-year outcomes—which would be *program effects*—are controlled away through the sampling frame.

Our study contributes to the accumulating evidence on last-dollar community college tuition guarantees. Our data, while having important limitations (discussed below), is student-level and spans the high school to college transition. Additionally, we can examine longer-term program effects—up to eight years after high school completion in the case of the Tamaqua program and up to six years in the case of Philadelphia's. Such longer-term outcome measures are essential to understanding the extent and durability of program effects, particularly in terms of diversion away from the four-year sector. The potential of community college tuition guarantees to unwittingly *lower* eventual educational attainment among some students (usually higher-achieving, middle-income students), or even to lower *aggregate* attainment, is a serious concern. Our data will allow us to address, though not resolve, these questions.

Data and Methods

Data

Our primary data source is complete student-level K-12 academic records from the Pennsylvania Department of Education (PDE) for academic years (AY) 2010-11 through 2020-21. PDE gathers information on all students enrolled in any public or private school in Pennsylvania during the period in question; it is complete population data[3]. Pennsylvania began contracting with the National Student Clearinghouse (NSC) to obtain linked postsecondary data for its students beginning with the graduating class of 2013. Therefore, our analytic dataset is limited to students enrolled in twelfth grade in Pennsylvania high schools between AY 2012-13 and 2019-20; we have no postsecondary data for the graduating class of 2021. For most

[3] As our data is not derived from probability sampling, it is unclear if frequentist inference is appropriate. Since this practice is standard in such situations, and as alternatives are not well-established, we follow suit. However, we do not give inordinate interpretive weight to statistical significance except when using nonparametric methods.

analyses, we further restrict to high school graduates. The full analytic dataset contains over one million student-level observations.

PDE data includes enrollment and graduation dates and identifies students' schools and districts, permitting measurement of peer composition. The linked NSC data presents semester-by-semester data by college of enrollment, allowing us to establish the order of institutions attended and timing of attendance. NSC also records degrees achieved: date received, granting institution, and degree type.

PDE data has some inconvenient limitations. Most seriously, PDE does not collect data on academic performance – neither course grades nor GPA. This is partially compensated for by detailed data on course-*taking*. Pennsylvania also conducts standardized achievement tests for its students, and PDE collects these scores. However, between 2011-12 and 2013-14, the state switched its 11th grade test from the PSSA to the Keystone test, and the 2013-14 graduating class was apparently never tested in 11th grade. Additionally, even among tested cohorts, coverage is incomplete, and empirical analysis shows that having a test score (as well as performance) predicts college going. The other major limitation is standard for administrative educational data: imprecise measures of family resources. While detail regarding household income and caretaker educational attainment would be ideal, PDE has indicators for economic disadvantage and free/reduced lunch eligibility.

We make use of two other sources of data. First, we use college-level enrollment data from IPEDS for some supporting analyses. Second, we were provided, by local district officials, data on college destinations of Tamaqua Area High School graduates from the years 2001-2004, which we use in descriptive analyses.

Methods

Propensity-score Matching

The Morgan Success Scholarship (MSS) first began in 2003, but PDE only began collecting student records in 2010 and postsecondary data in 2013. A differences-in-differences analysis exploiting program onset is thus infeasible. As we discuss below, however, we obtained local district records on postsecondary outcomes for years immediately before and after MSS's introduction, and this will help build credibility. But for main analyses we estimate program effects through propensity score matching (PSM) with regression adjustment.

PSM was developed in 1983 (Rosenbaum & Rubin, 1983) and has been heavily used for decades. Briefly, an analyst uses a probit or logit model (we use probit) to estimate a subject's probability of exposure to a "treatment" conditional on observables. This probability is the "propensity score". Next, using one of several algorithms, one locates, for each "treated" case, the untreated case(s) most similar in terms of propensity score. We used three nearest neighbors matching in our main analyses. Based on this selection, cases (treated and untreated) are assigned weights. The resulting weighted samples approximate, as well as observational samples could, an experimental and matched counterfactual control group. If matching succeeds, then the two groups will be well-balanced in terms of relevant confounders, and selection bias will be minimized. The researcher can compare means of outcomes between weighted samples to estimate treatment effects.

A major advantage of PSM over regression analysis when control cases are in abundance is that PSM restricts the comparison sample to cases like the treated group, reducing non-overlap on key confounders, which can bias regression estimates (Gelman & Hill, 2006; Benedetto et al.,

2018). Otherwise, PSM, like regression, adjusts for bias only on the observables (Morgan & Winship, 2015). If all factors relevant to treatment assignment are measured, treatment and comparison groups would have had identical outcomes in the absence of the treatment (the Conditional Independence Assumption, or CIA, also called ignorability), and PSM provides an unbiased estimate of the causal effect (Stuart, 2010). However, as CIA is a large and untestable assumption, PSM estimates are not generally as credibly causal as those from true quasi-experimental methods (e.g., regression discontinuity designs).

Regression models the outcome, and PSM models the treatment. One can obtain the advantages of both through regression adjustment or doubly-robust estimation (Yao et al., 2021). We use propensity score matching to select our comparison sample and assign weights. We then estimate the treatment effect by regressing the outcome on both treatment and the whole set of covariates used in the matching equation, plus cohort dummies. Doubly robust estimators will be correct if *either* the treatment or outcome model is correctly specified. It also makes estimates more efficient (Vermeulen & Vansteelandt, 2015).

The credibility of PSM for causal identification depends ultimately on data quality (i.e., covariate richness sufficient to approximate meeting the CIA) and on the plausibly exogenous nature of treatment assignment. If treatment assignment is not exogenous, then it must be precisely modeled. But the true model is impossible to know outright, and the likelihood of adequately measuring all relevant confounders is slight. But if treatment *is* plausibly exogenous, at least conditional on the observables, then causal identification is much more credible.

We address covariate richness below through discussion of variables; we address plausible exogeneity now. In the case of MSS (which we detail below), the "treatment" is program eligibility, which is a function of school district residence. PDE data measures this

precisely. There are two major threats to validity. First, families (and therefore students) may select themselves into the eligible district to take advantage of MSS. We believe this to be unlikely to have occurred at considerable scale because 1) the population of the eligible municipality (Tamaqua, PA) continued to decline after MSS's introduction, 2) this population decline mirrors that of many other (ineligible) communities in the county, and 3) the eligible municipality's age distribution resembles that of the rest of the county. Moreover, the estimated impacts of much more generous, four-year applicable Promise programs on residential decisions have been small (Bartik & Sotherland, 2015; Leigh & Gonzalez-Canche, 2021; LeGower & Walsh, 2017). Second, *other* unmeasured factors correlated with college-going behavior, but not directly related to MSS, may lead families to move to (or remain in) Tamaqua, producing in Tamaqua a student population less (or more) likely to attend college than comparison students. This is impossible to rule out, but we believe it can be credibly addressed through matching. Additionally, as mentioned above, we provide descriptive evidence of immediate program impacts through district-provided data, and this strongly suggests exogeneity.

Differences-in-differences

We can investigate the Community College of Philadelphia's (CCP) 50th Anniversary Scholars program through DiD. DiD is appropriate for the identification of local average causal effects when 1) panel data is available spanning treatment onset (i.e., with pre- and post- data for both experimental and control cases), and 2) unmeasured confounders credibly exist, violating the CIA. With DiD, there is no need to assume baseline comparability between treated and control groups. Instead, DiD requires that, in the absence of treatment, the groups would have exhibited parallel outcome *trends* (the "parallel trends assumption") (Lechner, 2010; Angrist & Pischke, 2009). DiD also requires that there are no confounding events that would affect the

experimental cases differently than the control cases. Neither assumption is empirically testable, though parallel *prior* trends make parallel counterfactual trends more credible (Kahn-Lang & Lang, 2020). We examine prior trends below, and though we know of no confounding event, we also cannot rule it out.

In DiD, one estimates, first, the difference between pre- and post- outcome measurements for both experimental and control groups. Then, one takes the difference between these differences (hence, the name). Typically, DiD is implemented in a regression framework, with the parameter of interest being the interaction between pre/post and treatment/control indicator variables. Researchers may include various covariates, both time-varying and time-invariant, to improve the efficiency of estimates. In our data, the "treatment" variable is an indicator for enrollment in the school district of Philadelphia, and the pre/post indicator differentiates 12th graders prior to 2015 (when the program was introduced) and 12th graders in that year or later. We include a large set of covariates (the same set used in our Tamaqua analysis). Our model takes the form:

$$Y_{ij} = \alpha + \beta_1 Philadelphia + \beta_2 Post + \beta_3 Philadelphia * Post + \beta X + \epsilon_{ij}$$

in which β_3 is the parameter of interest.

Variables

Our student-level outcomes are binary measures of postsecondary behavior: any postsecondary participation, enrollment in specific sectors (community college, four-year college), and degree completion (associate degree & bachelor's degree). We measure each of these within varying time windows following high school graduation: one year, two years, four years, and six years. Thus, each variable measures whether a student performed the behavior

within the time allowed, e.g., enrolled in any four-year college within two years of high school graduation (1=yes, 0=no).

For different time windows, our analytic samples shift because there are different sets of graduating cohorts for which we have the necessary postsecondary data to measure the outcome. Since we have postsecondary data covering academic years 2013-14 through 2020-21, we can measure one-year outcomes for all graduating cohorts (2013-2020). But as we only have one year of postsecondary data for 2020 graduates, we can examine two-year outcomes only for the 2013-2019 cohorts, and so on.

Our target independent variables are binary measures of geographic program eligibility. For both programs, eligibility was limited to students from specific school districts (Tamaqua Area School District and the School District of Philadelphia), which we capture through binary indicators of district enrollment. In the case of Philadelphia's program, eligibility was also restricted by graduation cohort, so we interact a binary measure of Philadelphia enrollment with a binary measure for eligible cohort within a differences-in-differences framework (discussed above).

In study designs such as ours, control variables should come as close as possible to exhaustively measuring confounders relevant to postsecondary enrollment and completion that aren't plausibly downstream from the treatment variable. Luckily, the literature on the determinants of postsecondary enrollment and completion is voluminous and highly convergent. College participation, destination, and completion are consistently predicted by prior academic behavior, socioeconomic status, gender, other measures of categorical (dis)privilege, and student peer environment (Voss et al. 2022; Flores et al. 2017).

Prior academic performance is the area in which our data are most limited. As mentioned above, PDE does not collect grade point averages, course grades, or pass/fail outcomes. It does, however, collect exhaustive data on course-taking and harmonizes them at the state level. The literature suggests that curricular rigor matters for college outcomes (Woods et al., 2018), but there is little guidance beyond this. We adopted an empirical strategy, regressing college enrollment on a set of course-taking variables and other measures and keeping those with a statistically significant predictive relationship. We measure, across a student's high school career, the number of courses taken in art, foreign language, practical/vocational fields, and social studies, as well as the number of dual enrollment (college and high school) and Advanced Placement/International Baccalaureate taken. All these positively predict college enrollment, except for practical/vocational courses (which have the opposite relationship). We also include dichotomous measures indicating whether a student ever took calculus, precalculus, algebra 2, geometry, trigonometry, physics, chemistry, biology, and a 3- or 4-level language course (all positively predict enrollment). The other available measure of academic preparation is state skills tests, which we do not include in main analyses because of the limitations discussed above. We do include test scores[4] in robustness analyses, and their inclusion/exclusion did not have an appreciable impact on findings.

Students' socioeconomic and categorical (dis)advantages are also predictive of educational outcomes. As we discussed above, our data does not have robust measures of family resources. For this, we include a binary indicator for free lunch eligibility and another for

[4] We assembled composite math and verbal measures by selecting Z-standardized Keystone scores (algebra and literature respectively) for those who had them and supplementing with Z-standardized PSSA scores (math; English language arts, reading, and writing, respectively) for those missing Keystone scores.

economic disadvantage[5]. Since female students are more likely to enroll in and complete college (Buchman & DiPrete 2006), we include an indicator for gender. PDE combines race and ethnicity together, and we dichotomize their classifications to measure minoritized status (1=Black, Latinx, or Native; 0 otherwise). We dichotomized student age to create an indicator for students older than 18, as older students likely did not graduate earlier because of insufficient credits and are probably less academically oriented. The other variables that we include reflect educational categorizations: current special education status, former special education status, English language learner status, disabled status, and participation in a "gifted and talented" program.

Prior research suggests that peer composition may also impact college participation directly or through impacting high school academic outcomes (Sacerdote, 2011). We leverage student-level measures and school identifiers to create the following peer composition variables: percentage minoritized, percentage female, percentage economically disadvantaged, and percentage disabled. We also measure the school year-specific dropout rate, the school population (logged), the district population (logged), and the per-capita district suspension rate. All these variables had an independent predictive relationship with college enrollment in our data, net of student-level variables.

(*Table 1 about here*)

Programs

[5] Economic disadvantage is determined by school districts using various measures of program participation (e.g. TANF or Medicaid). Non-overlap with FRL likely occurs mostly in Community Eligibility Provision districts, which include Philadelphia.

Tamaqua, Pennsylvania, is a small borough in rural Schuylkill County, about 40 miles northwest of Allentown. At its peak in the 1930s, Tamaqua's population was 13,000; today, it stands at under 7,000, the decline driven by the collapse in the local coal industry. It is 82.5% Non-Hispanic White, 12.8% Latino, and 9% multiracial. Tamaqua's 2021 median household income of $46,423 is low for both its county ($57,785) and state ($67,587), and accordingly, its poverty rate (20%) is higher (13% for the county and 12% for the state). Just 17.8% of Tamaqua's adults have a bachelor's degree (like the county but half that for Pennsylvania). It is served by the Tamaqua Area School District (TASD), which also includes outlying rural townships.

The Morgan Success Scholarship has been available to TASD graduates since 2003. It is a last-dollar, tuition-only guarantee applicable to Lehigh Carbon Community College (LCCC, the main campus of which is about 25 miles southeast in Schnecksville). To be eligible, one must graduate from Tamaqua Area High School after attending there since 11th grade. It is unrestricted by income, and the only merit criteria is the requirement of regular high school graduation. Temporal restrictions are tight: MSS is available for the first four semesters immediately following high school graduation; students who take the first semester off forfeit that semester's funding, though they may receive funding for the next three. This effectively restricts by age. Students must apply to the program and file FAFSA yearly. To maintain eligibility, a student must attain Sufficient Academic Progress (SAP)[6] and complete nine credits per semester. MSS is funded by a local philanthropy (the John E. Morgan Foundation) and managed by LCCC. The characteristics of this program are summarized in Table 1.

[6] SAP is required to remain eligible for federal need-based grants and involves meeting a GPA threshold (usually 2.0) and completing 2/3 of attempted credits.

MSS was launched two years before the Kalamazoo Promise. Foundation board members created the scholarship in part to ensure enrollment at a new LCCC branch campus in town, which the foundation also funded. The desire to maximize enrollment also informed the program's lack of income or merit criteria. The foundation estimates that the program costs roughly $200,000 per year.

The Community College of Philadelphia celebrated its 50th institutional birthday in 2015 by launching the 50th Anniversary Scholars Program. Available first to that year's graduating class, this program is a last-dollar tuition & fee guarantee usable at CCP for up to six semesters. Eligibility is restricted by residence (reside in Philadelphia and graduate from a Philadelphia high school), enrollment timing (immediately after high school graduation, with funding available for three years (if full-time) or six years (if part-time) after first enrollment), Pell grant eligibility, and citizenship (U.S. citizens or legal residents only). There are also merit criteria: regular high school graduation (excluding GED completers) and placement into college-level English and math at CCP (via high school GPA or ACCUPLACER scores). Students must file FAFSA by a specified date, enroll in a degree program, and enroll in at least six credits initially. To maintain eligibility, they must maintain six-credit enrollment, continuous enrollment, Pell eligibility, Philadelphia residence, a 2.5 GPA, and SAP. They also must complete 12 credits (part-time students) or 24 credits (full-time) in the first year, refile FAFSA, and meet with their assigned counselor. See Table 1 for a summary of the program.

CCP's 50th Anniversary scholarship likely funds a very small number of students per year at minimal cost to CCP. Most CCP students are Pell-eligible, and a fair number likely meet the academic requirements. However, given the correlation between income and academic achievement, far fewer will meet *both* income and merit requirements. Of these, undocumented

students (who are expensive) are excluded, and most of the remainder will have nearly all their tuition covered by need-based federal and state grants. Continuing eligibility requirements—particularly continuous enrollment—will further reduce the number of funded qualifiers. It is also likely that many more students think they are receiving the scholarship than are funded. In a promotional video made by CCP (Community College of Philadelphia, 2016), two of the three featured students say that the scholarship is, in the words of one of them, "helping me by paying for books". The 50th Anniversary Scholars Program doesn't cover books, but community colleges often automatically apply need-based grants left over after tuition and fees to books. It is likely that these students' tuition, fees, and books were covered by Pell and state grants, not the program, and that the program is not funding them at all. Similar "symbolic" free college programs have been described elsewhere (Monaghan & Attewell, 2023; Monaghan, 2023).

Results

Tamaqua's Morgan Success Scholarship

We begin our analysis of program impacts by examining, in Table 2, descriptive statistics for Tamaqua, the rest of Schuylkill County, and the rest of Pennsylvania. In some ways, Tamaqua students seem statistically more likely to attend college, but not in others. Tamaqua is an economically struggling community but is far from the most disadvantaged locale in the state or even in the county. Its share of students who are free lunch eligible (34%) is close to that of the county (38%) and state (37%). It has a higher share of 12th graders older than 18 (10% vs. 6% elsewhere), which is typically an indicator of disadvantage. Conversely, it is much whiter than the state; only 5% of Tamaqua students are from minoritized populations, compared with 21% in Pennsylvania. The population of its high school is small compared with high schools in the rest of the state (but, again, not with the county), and (log) school population is negatively

related to college enrollment. Compared with the rest of the state, Tamaqua students take fewer social studies, dual enrolment, or AP/IB courses and are less likely to take calculus. They are, however, more likely to take physics, and take more art courses on average. Again, descriptive statistics suggest neither uniform relative advantages nor disadvantages.

(*Table 2 about here*)

The fourth column of Table 2 provides descriptive statistics for the propensity score matched comparison sample gathered from across the state, and the fifth column provides *p*-values from *t*-tests comparing this group with the Tamaqua sample. This comparison sample is very well-matched to the Tamaqua sample on most measures. Even where differences are statistically significant, they are small. For instance, the difference in art and dual enrollment course-taking amounts to, on average, 0.12 art courses and 0.06 dual enrollment courses (favoring the matched comparison group). Conversely, 89% of Tamaqua students take a biology class, compared with 85% of comparison students. As above, where differences attain statistical significance, they do not uniformly favor (in terms of postsecondary outcomes) either Tamaqua or the comparison group.

As noted above, PDE data doesn't cover the period during which the Morgan Success Scholarship was first introduced. However, Tamaqua Area School District kept its own records for this period and shared them with us. Figure 1 presents two postsecondary outcomes for Tamaqua high school graduates: the share of graduates who enrolled in some postsecondary education and the share of postsecondary enrollers who went to community college. The district's data covers the period from 2001 (two years prior to the 2003 MSS launch) through 2004 (two years after). While there is no comparison group, the differences occasioned by program launch are compellingly large. The rate of college-going jumped eight percentage

points, from 86% in 2002 to 94% in 2003; the slight increases between 2001-2 (three percentage points) and 2003-4 (one percentage point) suggest yearly secular increases in college-going over this period far smaller than the increase in the program year. Meanwhile, the share of collegegoers opting for community college bounds up a whopping *51 percentage points*, from 12% to 63%. Again, year-on-year changes before and after are nowhere near this large.

(*Figure 1 about here*)

(*Figure 2 about here*)

Postsecondary behavior is disaggregated in Figure 2, and we compare combined pre-program year outcomes with combined post-program outcomes. The share of graduates who enroll in community college increased by 51 percentage points (note that the denominator here is all graduates, whereas the denominator for community college in Figure 1 is all collegegoers). This increase came at the expense of every other category of postsecondary behavior. The share of Tamaqua graduates enrolling in four-year colleges fell by 29 percentage points, and this involved declines in the share of students enrolling in state universities (e.g., Penn State University), state comprehensive colleges (e.g., West Chester University), and other four-year colleges (private or out-of-state). Ten percentage points fewer graduates enrolled in "other PSE" – mostly less than two-year programs – and another ten percent fewer did not enroll in college. Overall, about 3/5 of the increase in community college enrollment was "diversion" from the four-year sector, and the other 2/5 was democratization, or upward diversion in the system.

The above results are descriptive but more than suggestive. In Table 3, we present evidence from our regression-adjusted propensity score matching analysis. We draw on four overlapping samples; for one-year outcomes, we are able to use our full sample (2013-20

graduates). For longer-term outcomes, we restrict progressively to earlier cohorts. The first two columns are unadjusted means for Tamaqua and comparison groups. That is, they show mean outcomes for the comparison group selected by PSM prior to application of weights and regression adjustment. The "adjusted difference" applies both.

(*Table 3 about here*)

The first panel of the table presents one-year outcomes. At one year after high school graduation, Tamaqua graduates were sixteen percentage points more likely than comparison students to enroll in any postsecondary education. This advantage is realized entirely in the community college sector; the difference in community college enrollment is 36 percentage points, favoring Tamaqua. This estimate is consistent with predicted program effects and broadly consistent with Figures 1 and 2 in both direction and size. Tamaqua students were 17 percentage points less likely to enroll in a four-year college. Thus, one-year estimates suggest equally sized initial democratization and diversion effects.

The second panel moves on to two-year outcomes. Two years after high school completion, associate degree completion is a possibility. By this point, only 6% of Tamaqua graduates had earned an associate degree, but this is a large increase relative to 1% associate degree attainment in the comparison group. The entire degree/certificate completion advantage (+4 percentage points) of the Tamaqua students occurs through the associate degree difference. College attendance and four-year attendance differences were slightly reduced relative to the one-year estimate, but the community college attendance estimate is the same.

There is more movement in effect sizes when we get, in the third panel, to four-year outcomes. Specifically, the difference in four-year college attendance, at just -6 percentage

points, is down ten percentage points. Clearly, between two and four years, a sizeable share of Tamaqua students transferred from community college to a four-year school. The associate degree gap is now 11 percentage points, and the raw estimates for both groups are significantly higher. The expansion in this effect from two to four years reflects that many students take more than two years to complete "two-year" associate degrees (we will address shifting cohort effects below) and high rate of initial community college enrollment by Tamaqua students. By four years, students have begun earning bachelor's degrees, and Tamaqua youth trail their matched comparisons by five percentage points. The difference in overall degree completion is similarly sized but positive.

Finally, because four-year degrees often take longer than four years to complete, the final panel analyzes six-year outcomes. Effect sizes in any postsecondary attendance and in community college attendance are remarkably stable across all our estimates. Differences in four-year attendance, associate completion, and overall degree completion are quite like four-year estimates. The major point to note is that the difference in bachelor's attainment is now just two percentage points and nonsignificant. Still, given other estimates, along with our possession of population (non-sampled) data, we suspect that the point estimate reflects a "real" difference.

Our next analysis, in Table 4, is consistent with the above interpretations. Here we examine program effects for the timing of educational milestones for those who achieved these milestones. For obvious reasons, samples differ for each outcome (e.g., time to first community college enrollment can only be measured for those who ever enrolled in community college). The MSS not only shifted shares of students achieving various milestones; it sped up or slowed down the achievement of outcomes in congruent ways. Tamaqua students who ever went to college did so more quickly (by about 2.2 months on average). They also more rapidly enrolled

in community college (by 9.2 months), earned any degree (3.1 months), and earned an associate degree (5.5 months). But they took longer to enroll in four-year colleges (by 5.2 months) and to earn bachelor's degrees (1.7 months). The consistency of these estimates with those above lends further confidence in our small point estimate of the difference in bachelor's attainment.

(*Table 4 about here*)

Before moving on, we address a few points. First, there is the matter of cohort effects. Over the period 2013-2020, there was a year-on-year secular decline in direct college enrollment rates, and neither Pennsylvania nor Tamaqua were exempt from this trend. This impacts our estimates in Table 2, and specifically, the relationship between estimates of the same outcome for different years out from high school completion. Extending each additional year requires us to drop a (later) cohort for which that time-horizon's postsecondary data is not available. Earlier cohorts have higher underlying rates of college enrollment (see Appendix Table A1), leading the sets of estimates to be imperfectly conformable with each other.

The second point regards our various statistical adjustments. A careful reader will note that in Table 2 the differences between unadjusted differences (i.e., the difference between unadjusted Tamaqua and comparison group means) are very similar to adjusted differences. This is not typical in propensity score matching; matched differences are often far smaller than unmatched differences. One reason for this could be—and this is our suspicion—that neither weighting nor regression is doing much "work" here, so the large observed differences reflect "real" underlying differences generated by the MSS program. We explore this further in Table A2, comparing Tamaqua students to those elsewhere in Schuylkill County. Here as well, for most outcomes, raw "naïve" descriptive differences are also almost identical in magnitude to fully adjusted differences. These further builds support for treatment exogeneity. However,

given that we do not have a properly causal design, and that we are lacking some crucial predictors of educational attainment, it is also possible that this is explained by specification error.

Third, we re-estimated one-year outcomes using several different specifications – not conditioning on graduation, including state achievement test scores and attendance rates as matching and regression control variables, restricting the comparison group to students from the same county, and using 5- and 1-nearest neighbors matching. Our estimates rarely budged more than a few percentage points in any estimation, and substantively, the results were nearly identical to Table 2 estimates. We conclude that the patterns we documented reflect program effects rather than statistical artifacts.

Finally, we noted above that tuition guarantees can motivate behavioral changes both by shifting real costs and/or by shifting apparent costs through "promising" a low tuition price. We believe that the Morgan Success Scholarship pays the tuition of many Tamaqua students. Tamaqua is not desperately poor, Pennsylvania's community colleges are relatively expensive, and the program doesn't condition eligibility by income. However, we are also certain that *some* Tamaqua students have all or most of their tuition fully paid by federal and state grants and that the MSS pays little to nothing towards these students' tuition. If a tuition-guaranteeing "Promise" program effects act solely through real cost reduction, we should see much smaller program effects for lower-income Tamaqua students than for their higher-income peers. In Table A4, we address this by re-estimating one-year outcomes, restricting analysis to free lunch eligible students. Estimated effects are nearly identical to the main program effects. This suggests that the "message" of free college has similar effects (at least in Tamaqua) regardless of whether it is backed up by real cost reduction.

All evidence we presented suggests that Tamaqua, Pennsylvania's Morgan Success Scholarship strongly refashions the educational trajectories of many eligible students. Specifically, it leads a sizeable share of students to enroll in community college who otherwise would not have enrolled anywhere, initially diverts a very large share of students from four-year colleges to community college, and considerably increases the proportion of students who earn associate degrees. After four to six years, because of the program, Tamaqua has fewer high school graduates with no postsecondary education, many more who attended community college but earned no credentials, considerably more with a terminal associate degree, and slightly fewer with a bachelor's degree. On balance, the program expands access to postsecondary education more than it diverts students away from four-year degrees, though it does appear to do this as well.

(*Figure 3 about here*)

Community College of Philadelphia's 50th Anniversary Scholars Program

We begin our investigation of the 50th Anniversary Scholars program (hereafter 50AS) by examining trends in CCP's enrollment using IPEDS data. Figure 3 presents yearly first-year and continuing enrollment at CCP and at all other Pennsylvania community colleges, dividing yearly enrollments for each group by enrollment in 2014 (the year preceding 50AS) to place them on the same scale. Because of its eligibility criteria, we expect the program to impact only first-year enrollment and only at CCP. In keeping with national trends, community college enrollments fell in Pennsylvania over this period. Enrollment of first year CCP students appears to buck this trend slightly; it increases between 2014 and 2015 by about 8.5%. At first glance, this is evidence of a program impact. CCP saw first-year enrollment increases in just two other years over this period, both of which were less than 2%.

But consider, first, that *clear* tuition-guarantee impacts can be considerably larger for comparable programs. Monaghan and Attewell (2023) record an enrollment spike of more than 100% in the year following the introduction of a similar program in another big-city community college. Second, a closer examination reveals that several year-on-year first-year enrollment increases of 8.5% or higher happened elsewhere in the Pennsylvania community college system during this period at colleges that did not introduce tuition-guarantee scholarships. Specifically, of all 140 year-to-year enrollment percent changes in the 14 colleges of the system over an eleven-year period (one needs two years per each percent change calculation), there were 16 instances (more than 10% of observations) with enrollment increases greater than +8.5% and another seven instances of increases of between +4 and +8.5.

How likely was such a change likely to occur by chance—that is, because of something other than a new tuition-guarantee program? We examined this using a permutation test[7], using enrollment increase as the dependent variable, reporting the results in Table A5. Our permutation *p*-value suggests a more than 10% chance of observing an enrollment increase of 8.5% or larger by chance, failing to reject the null hypothesis. We also performed permutation tests predicting enrollment, interacting a dummy for CCP by a dummy for the year 2015 or later. In this case, we find that the apparent enrollment increase at CCP in eligible years (of between -21 and 289 students, depending on the time window) is obtained by chance at least 36% of the time.

[7] Permutation reshuffles outcome values among cases a specified number of times, re-estimating regression coefficients with each reshuffling. The *p*-value is the share of reshufflings in which the target coefficient is equal to or greater than the original estimate. It thus provides a nonparametric estimate of the likelihood of seeing a given result by chance.

Thus, we cannot be sure whether the uptick in first-year enrollment at CCP occurred *because* of 50AS. But did the program impact the enrollment behavior of students in its district? We begin to examine this in the three graphs making up Figure 4. We compare enrollment probabilities among Philadelphia school district (PSD) graduates with those of four comparison groups: all graduates elsewhere in the state (excepting Tamaqua and Pittsburgh, which also have Promise programs), students in other low-income (>50% economically disadvantaged) and high-minoritized (>50%) districts, students in other large districts (greater than 10,000 students), and a comparison group assembled by 1-1 propensity score matching.

(*Figure 4 about here*)

We are looking for two things. First, are enrollment trends prior to 2015 parallel (i.e., examining the parallel trends assumption). Second, is there a discontinuity observed for eligible students at the program year that is *not* observed for the comparison groups? We are looking for an increase in community college enrollment, an increase in any postsecondary enrollment (evidence of democratization), and a possible decrease in four-year enrollment (evidence of diversion). Admittedly, we have only two pre-program years, so parallel trends cannot be examined well. Still, for all outcomes, trends were reasonably parallel with at least some comparison groups prior to 2015. We do not see any indication of a discontinuity in 2015 for PSD students for any postsecondary enrollment or for four-year enrollment. There is, however, a slight increase in community college enrollment observed for Philadelphia students that is sharper than for any comparison group. The shift is about +2 percentage points, which is neither so small as to be easily dismissed nor so large as to be a clear indication of a program effect.

We formally test for program effects using a differences-in-differences design, the results of which appear in Table 5. The target parameter is *Philadelphia*Post*. We examine impacts on all three outcomes using all four comparison groups. When the comparison group is the rest of the state, there is a small (2.5pp) increase in any postsecondary enrollment, suggesting democratization. However, this derives not from an increase in community college enrollment (+0.04pp) but from an increase in *four-year college* enrollment (+1.7pp). Since the program does not reasonably increase direct four-year enrollment, this is probably unrelated to the program. A different story emerges when the comparison group is the low-income, high-minoritized districts. Here, there is a small, nonsignificant (+1pp) increase in overall enrollment, a larger (+2.9pp) significant increase in community college enrollment, and a larger (-2pp) decrease in four-year enrollment. This suggests a small diversion effect, with possibly a tiny democratization impact as well. When the comparison group is large districts, we have a story closer to the first comparison. There is a small but significant increase in any enrollment (+1.7pp), coming more from an increase in four-year enrollment (0.9pp, nonsignificant) than from community college enrollment (0.5pp, nonsignificant). Again, we interpret this as a null program result with a slight hint of democratization. Finally, when we use the propensity-score matched comparison group, we find a clear null effect: a negative (-1.4pp, nonsignificant) effect on college-going, driven by a decline in four-year participation (-2.1pp, significant at $p<0.01$) and no change in community college attendance.

(*Table 5 about here*)

Our main analysis presents one very clear finding: the 50AS program did not have strong, clear effects on the college-going behavior of Philadelphia students. This is a clear contrast with our results for Tamaqua, where the MSS-induced behavior in eligible students diverges sharply

from any comparison group we can muster. Here, we are left to adjudicate whether the program had *any* effect.

We conducted numerous other analyses to provide more clarity (Table 6). Using each comparison group, we performed three specification changes: not conditioning on graduation, including test scores and attendance as controls, and shortening the study window to two years after program introduction (in case effects were short-lived). We also used an additional comparison group: districts which are both large and low-income and highly minoritized. Together with our analyses in Table 4[8], this results in 20 separate tests of the three groups of program effects. Results appear in Table 5. To interpret, we class a result as *democratization* if there is an increase in any college-going of +1 or greater *and* a larger increase in community college enrollment than in four-year enrollment, regardless of statistical significance. We class a result as *diversion* if there is a *decrease* in four-year enrollment of more than 1pp accompanied by an *increase* in community college enrollment of at least 1pp (again regardless of statistical significance). We class other results as null findings. These criteria are chosen deliberately to be generous in identifying positive program effects. Using these criteria, we classify three out of twenty analyses as providing evidence of democratization, nine out of 20 as providing evidence of diversion, and eleven out of twenty as providing evidence of neither. We performed additional analyses restricting only to free lunch eligible students and to those who had taken a less academic curriculum, using various comparison groups, with no divergent findings (analyses not shown).

(*Table 6 about here*)

[8] We include Table 4 results as the first row for each group, indicated by (T4), for comparison.

We conclude that the Community College of Philadelphia's tuition guarantee Promise program, the 50th Anniversary Scholars program, did not have a sizeable impact either on enrollment at the college nor on the college-going behavior of Philadelphia students. More cautiously, we suggest that the program probably did not have any impact on either, though there are hints that it may have slightly and temporarily boosted enrollment of first-year students, and it may have changed the college-going trajectories of a small number of students. To the extent that it had an impact on post-high school behavior, it is more likely that the program diverted students away from four-year colleges and to community college than that it expanded access to postsecondary education.

Discussion

We are faced with two similar programs with very different results. One program, in Tamaqua, clearly impacts college-going behavior. The other program, in Philadelphia, more likely than not has little to no impact. How can one account for these divergent results?

Let us *first* rule out some explanations. Tamaqua is much smaller than Philadelphia. However, a large eligible population need not rule out an impactful program. It is true that some successful large-city Promise-style programs are much more generous than 50AS (e.g., the Pittsburgh Promise) and that successful statewide community college programs have more resources to leverage for support. However, the large-city, large community college tuition guarantees studied by Monaghan and Attewell (2023) and Anderson et al. (2022) provide evidence that a program very similar to CCP's can increase the college's enrollment sharply while boosting college going among eligible students. These two studies help rule out two other possibilities: that the 50AS program is unsuccessful because it is overly restrictive in terms of eligibility or because it likely provides little in real cost reduction. Both programs in question in

these studies had elaborate (in fact, very similar) eligibility criteria, and Monaghan and Attwell provided direct evidence that next to no funding was delivered. "Symbolic" tuition reductions *can* cause real enrollment changes through messaging alone. CCP's program did not.

We cannot provide firm answers as to why, but we advance suggestions. First, the Tamaqua program appears deeply anchored in its target community. The fact that the community is small probably facilitated this, and Figures 1 and 2 suggest that program effects were immediate. Interviews we conducted with several officials in TASD suggest that the program is very well-known in the community and incorporated into postsecondary planning as a matter of course. We suggest that the 50AS program has not achieved sufficient institutionalization within Philadelphia's public high schools to become incorporated into decision-making.

Most likely, the failure was and is one of communication. Monaghan and Attewell report that the community college whose program they investigated 1) announced the program nearly a year in advance of its launch and 2) devoted significant resources to saturating local high schools with information and recruiters, leaning heavily on established relationships with these schools and their counselors. Accordingly, the program was, at least in its first year, repeatedly communicated to eligible students. We strongly suspect that CCP did not engage in such organizational mobilization. Our evidence for this is the late date of the program announcement: April 2015 (Benshoff, 2015). This announcement came at the end of the academic year after many students had already made decisions about whether and where to attend college. It left CCP, and high schools, little time to get the word out about the program. Still, CCP could have learned from this misstep and redoubled efforts for the following cohort. Evidence from our analyses suggests that this did not occur.

Taken together, our analyses suggest that a tuition guarantee, by itself, will not necessarily have any impact. If a program falls in the forest and no one hears it, it will not shift enrollment patterns. It is not monetary incentives nor even apparent monetary incentives that directly change behaviors, but the reshaping of cognitions and motivations through communicative interventions. Unless they have effective control of defaults, policymakers, to impact behavior, must sufficiently occupy cognitive space to realign taken-for-granted conceptions of opportunities. Changes in monetary consequences (i.e., real incentives) are neither necessary nor sufficient, though certainly they are helpful. What is necessary is penetrating the life-worlds of program targets. In the case of community college tuition guarantees, this means reaching students either directly or indirectly through their school administrators, school counselors, teachers, parents, community members, and peers. We suspect that incorporating high school personnel is the surest route to students' brain space.

We also suspect that a program's *simplicity* contributes to its effectiveness in shifting behavior, holding communicative intensity constant. Tamaqua's program is reasonably straightforward: everyone who graduates from its high school, regardless of grades or income, gets two free years at the local community college. The most complicated feature is its temporal restriction, but even this is easy to summarize: you have two years after high school to use it or lose it. We suspect that students may not fully understand that "tuition free" doesn't mean "completely free", and we also doubt that many grasp the last-dollar design. CCP's program is more complicated in terms of eligibility criteria. Still, colleges can equivocate in promoting such a program, describing it as "free college", and still change behavior with few negative consequences. What matters, again, is the simplicity of *messaging*; the message's correspondence to real practice is somewhat separate.

Conclusions

We summoned evidence from two tuition-guarantee community college "Promise" programs to further knowledge of how financial aid can impact postsecondary behavior. In the case of Tamaqua's Morgan Success Scholarship, we found clear evidence of strong program effects. The program increases college attendance overall and shifts students to eligible community college instead of four-year colleges. Over the longer term, the democratization effect remains, increasing the share of graduates with some college but no degree and the share of graduates with a terminal associate degree. The former group is larger than the latter. In addition, those who enroll in community college and earn associate degrees do so more rapidly than they would have without the program. The diversion effect appears to lessen over time as substantial numbers of initial community college enrollers transfer to four-year colleges and earn degrees. The program reduces the share of high school graduates who ever attend four-year college and probably slightly decreases the share who ever earn a bachelor's degree. Those who attend a four-year college and who earn a bachelor's take longer to do so. More bachelor's-holders earn an enroute associate degree.

In contrast, the Community College of Philadelphia's 50th Anniversary Scholars Program seems to have little impact on eligible students or even on its own enrollment. We found hints that the program boosted the college's enrollment of first-year students in the year it went into effect, but this increase was too small to be conclusive. We also found some evidence that the program may have diverted a small share of students to community college who would have gone to a four-year school and even less evidence that it marginally boosted college-going overall.

Our study has contributed to the burgeoning literature on "Promise programs", even as we question the usefulness of this analytic category. Our findings demonstrate the importance of detailed investigations of individual programs. The literature on "Promise" programs has spent too long either presuming program impacts rather than empirically establishing them or generalizing empirical findings from a few highly unique programs to all "Promise" programs, despite most programs' lack of resemblance to the researched program. We contribute to the literature on community college tuition guarantees (i.e., "free community college" programs) and even more to the very small literature on *local* community college programs. As we emphasized, there are scores, if not hundreds, of such programs, but they have been nearly entirely passed over by researchers.

We conclude that these programs can have strong impacts. They can induce both democratization and diversion, with the former effect seeming larger and more permanent. They can impact behavior both through resource provision and cognitive realignment (messaging). But they also can have little to no impact. We suspect that the determinative difference lies in effective organizational mobilization to impact the cognitive environment of program targets (students). Future research can and should pair rigorous estimation of program effects with a detailed examination of program design, implementation, and communication.

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Tables and Figures

Table 1: Characteristics of Morgan Success Scholarship and 50th Anniversary Scholars Program

	Morgan Success Scholarship	50th Anniversary Scholars Program
Setting	Small town	City
Eligible population	@150/year	@6800/year
Year launched	2003	2015
Generosity	Tuition only	Tuition and fees
	4 semesters	6 (full-time) or 12 (part-time) semesters
Applicability	Single community college	Single community college
Residential eligibility	Attend target high school since 11th grade	Reside in municipality; graduate from municipality high school
Temporal eligibility	First 4 semesters after graduation	Enroll immediately after graduation
Income eligibility	None	Pell-eligibility
Merit eligibility	Regular HS completion	Regular HS completion; place into college-level math and English
Citizenship eligibility	Requires FAFSA completion	U.S. citizens and legal residents only

Procedural eligibility	FAFSA & program application	FAFSA by deadline; enroll in degree program
Enrollment eligibility	None	6 credits
Continuing eligibility	Complete 9 credits/semester; file FAFSA; SAP	Enroll 6 credits; complete 12/24 credits per year (full-time/part-time); continuous enrollment; File FAFSA; Pell eligibility; Philadelphia residence; 2.5 GPA; SAP

Table 2: Descriptive statistics (variable means) comparing Tamaqua 12th graders with those in their state and county, and with a propensity-score matched comparison group

	Pennsylvania	Philadelphia	Tamaqua	PS-matched comparison	*p* (Tamaqua vs. comparison)
Age>18	0.06	0.11	0.10	0.10	0.80
Female	0.50	0.53	0.49	0.49	0.98
Minoritized	0.21	0.71	0.05	0.05	0.94
Free lunch eligible	0.37	0.94	0.34	0.37	0.20
Disability	0.14	0.17	0.20	0.22	0.35
Special education	0.13	0.14	0.18	0.20	0.35
Gifted	0.05	0.06	0.04	0.04	0.77
English language learner	0.02	0.07	0.01	0.01	0.90
School population (log)	7.03	6.88	6.45	6.43	0.26
District suspension rate	0.04	0.04	0.03	0.04	0.90
Total art courses	2.35	2.01	2.84	2.96	0.03
Total language courses	2.01	1.78	2.27	2.21	0.15
Total social studies courses	4.53	3.84	3.32	3.29	0.52
Total vocational courses	3.60	1.63	3.16	3.18	0.75
Total dual-enrollment courses	0.33	0.02	0.29	0.36	0.03

Total AP/IB courses	1.35	1.11	0.93	0.88	0.24
Calculus	0.23	0.11	0.11	0.11	0.99
Precalculus	0.32	0.33	0.40	0.40	0.98
Geometry	0.78	0.81	0.67	0.65	0.33
Trigonometry	0.23	0.01	0.03	0.03	0.80
Physics	0.44	0.34	0.57	0.53	0.13
Chemistry	0.79	0.82	0.80	0.79	0.55
Biology	0.90	0.81	0.89	0.85	0.08
Language 3 or 4 level	0.41	0.12	0.42	0.38	0.11
N	*900,408*	*54,946*	*8,644*	*1,176*	*1,111*

Source: Pennsylvania Department of Education

Table 3: Propensity score matching outcomes for results of Morgan Success Scholarship, 2013-2020 graduating classes. Comparison group identified by PS matching. Adjustment refers to PS-derived weighting and regression adjustment.

	Unadjusted means		Adjusted		
	Tamaqua	Comparison	difference	SE	p
One-year outcomes (N=4,202)					
Any postsecondary enrollment	0.74	0.57	0.16	0.01	<0.001
Enrolled at community college	0.48	0.11	0.36	0.02	<0.001
Enrolled at four-year college	0.31	0.48	-0.17	0.01	<0.001
Two-year outcomes (N=3,627)					
Any postsecondary enrollment	0.74	0.70	0.14	0.01	<0.001
Enrolled at community college	0.50	0.23	0.36	0.02	<0.001
Enrolled at four-year college	0.34	0.54	-0.15	0.02	<0.001
Any degree/certificate	0.06	0.02	0.04	0.01	<0.001
Associate degree	0.06	0.01	0.05	0.01	<0.001
Four-year outcomes (N=2602)					
Any postsecondary enrollment	0.76	0.73	0.14	0.02	<0.001
Enrolled at community college	0.55	0.29	0.37	0.02	<0.001
Enrolled at four-year college	0.45	0.59	-0.06	0.02	0.001
Any degree/certificate	0.33	0.32	0.05	0.02	0.015
Associate degree	0.17	0.05	0.11	0.01	<0.001
Bachelor's degree	0.18	0.27	-0.05	0.02	0.003
Six-year outcomes (N=1,482)					
Any postsecondary enrollment	0.78	0.74	0.15	0.02	<0.001
Enrolled at community college	0.57	0.29	0.38	0.03	<0.001

Enrolled at four-year college	0.46	0.60	-0.08	0.02	0.001
Any degree/certificate	0.46	0.47	0.06	0.03	0.028
Associate degree	0.22	0.08	0.13	0.02	<0.001
Bachelor's degree	0.31	0.41	-0.02	0.02	0.309
Enrolled in graduate school	0.02	0.03	0.00	0.01	0.587

Source: Pennsylvania Department of Education

Table 4: Estimated impacts of the Morgan Success Scholarship, 2013-2020 high school graduates. Comparison group identified by PS matching. Adjustment refers to PS-derived weighting and regression adjustment.

	Unadjusted means				
	Tamaqua	Comparison	Adjusted difference	SE	*p*
Time (in years) to:					
First postsecondary enrollment (*N*=3,141)	0.32	0.43	-0.18	0.03	<0.001
First community college enrollment (1,984)	0.56	1.17	-0.77	0.06	<0.001
First four-year enrollment (*N*=1,868)	0.95	0.54	0.43	0.06	<0.001
First degree (*N*=1,183)	3.50	4.00	-0.26	0.07	<0.001
First associate (*N*=531)	2.80	3.35	-0.46	0.11	<0.001
First bachelor's (*N*= 730)	4.32	4.22	0.14	0.06	0.014

Source: Pennsylvania Department of Education

Table 5: Differences-in-differences estimates of 50th Anniversary Scholars program effect on college-going behavior of Philadelphia public high school graduates, using four comparison groups. LIHM=Low-income, high-minoritized district; PSM=propensity score matched comparison

	State	LIHM	Large district	PSM
PSE enrollment, 1Y				
Philadelphia SD	0.019	-0.002	-0.002	0.034
	(0.004)[a]	(0.006)	(0.006)	(0.007)[a]
Post	-0.011	0.005	0.009	0.020
	(0.001)[a]	(0.004)	(0.003)[b]	(0.006)[a]
Philadelphia*Post	0.025	0.010	0.017	-0.014
	(0.004)[a]	(0.007)	(0.006)[b]	(0.007)
Community college enrollment, 1Y				
Philadelphia SD	-0.030	-0.033	-0.064	-0.012
	(0.004)[a]	(0.006)[a]	(0.006)[a]	(0.005)[c]
Post	-0.002	-0.015	0.000	-0.004
	(0.001)[c]	(0.004)[a]	(0.003	(0.006)
Philadelphia*Post	0.004	0.029	0.005	0.000
	(0.004)	(0.006)[a]	(0.005)	(0.007)
Four-year enrollment, 1Y				
Philadelphia SD	0.049	0.025	0.050	0.049
	(0.004)[a]	(0.004)[a]	(0.006)[a]	(0.006)[a]
Post	-0.001	0.020	0.018	0.028
	(0.001)	(0.006)[a]	(0.003)[a]	(0.005)[a]
Philadelphia*Post	0.017	-0.020	0.009	-0.021
	(0.004)[a]	(0.006)[a]	(0.005)	(0.007)[b]
N	889,915	114,938	157,989	86,340

Source: Pennsylvania Department of Education [a]$p<0.001$, [b]$p<0.01$, [c]$p<0.05$

Table 6: Robustness tests for differences-in-differences analysis of 50th Anniversary Scholars program

Comparison group	Condition on graduation	Test & attendance controls	Short study window	One year enrollment estimates: Any PSE	Community college	Four year college	Interpretation
State (T4)	Y	N	N	*0.025*	0.004	*0.017*	Null
State	N	N	N	*0.017*	-0.003	*0.018*	Null
State	Y	Y	N	*0.016*	*0.011*	0.001	Democratization
State	Y	N	Y	*0.018*	*0.008*	*0.005*	Democratization
LIHM (T4)	Y	N	N	0.010	*0.029*	*-0.020*	Democratization/Diversion
LIHM	N	N	N	0.005	*0.021*	*-0.017*	Diversion
LIHM	Y	Y	N	-0.012	*0.027*	*-0.041*	Diversion
LIHM	Y	N	Y	-0.002	*0.021*	*-0.027*	Diversion
Large dist (T4)	Y	N	N	*0.017*	0.005	0.009	Null
Large dist	N	N	N	*0.024*	0.008	*0.013*	Null
Large dist	Y	Y	N	0.005	0.008	-0.012	Null
Large dist	Y	N	Y	*-0.024*	*-0.024*	*-0.014*	Null
PSM (T4)	Y	N	N	-0.014	0.000	*-0.021*	Null
PSM	N	N	N	-0.014	0.000	*-0.021*	Null
PSM	Y	Y	N	*-0.039*	0.000	*-0.045*	Null
PSM	Y	N	Y	-0.015	0.014	*-0.038*	Diversion
LIHM, large dist	Y	N	N	0.001	*0.037*	*-0.047*	Diversion
LIHM, large dist	N	N	N	0.005	*0.033*	*-0.039*	Diversion
LIHM, large dist	Y	Y	N	*-0.028*	*0.029*	*-0.070*	Diversion
LIHM, large dist	Y	N	Y	*-0.032*	0.021	*-0.072*	Diversion

Source: Pennsylvania Department of Education; Estimates in italics are significant a $p<0.05$

Figure 1: College-going outcomes for Tamaqua SD 2001-2004 graduating classes. Morgan Success Scholarship was first available for the 2003 cohort. PSE=any postsecondary enrollment. CC/PSE=community college enrollment as a % of postsecondary enrollment. (Source: Tamaqua Area School District)

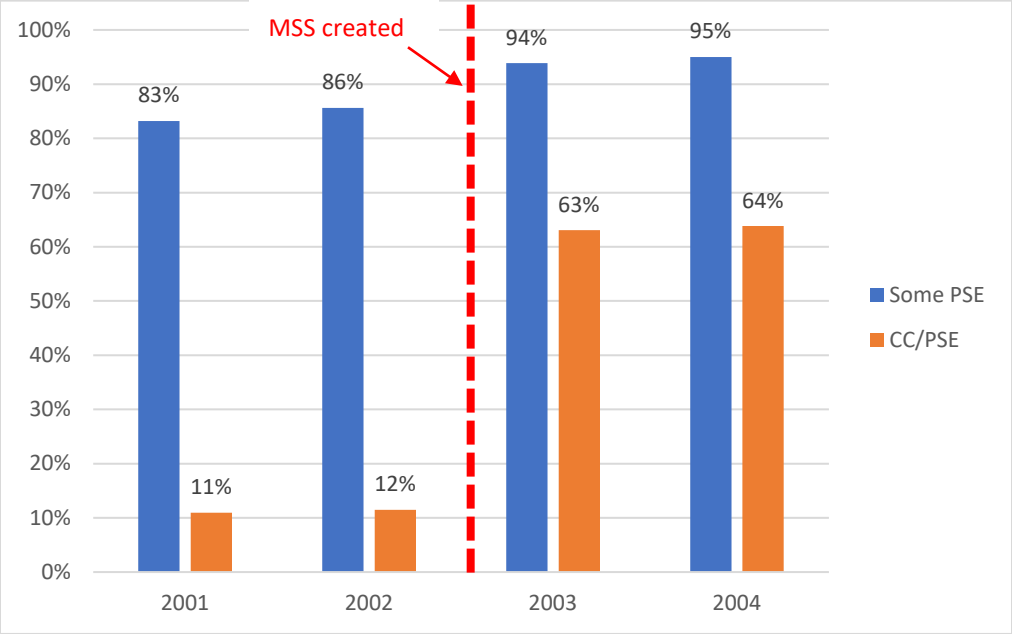


Figure 2: Detailed postsecondary outcomes for Tamaqua PA graduating classes immediately prior to (2001-2) and after MSS (2003-4) (MSS=Morgan Success Scholarship) (Source: Tamaqua Area School District).

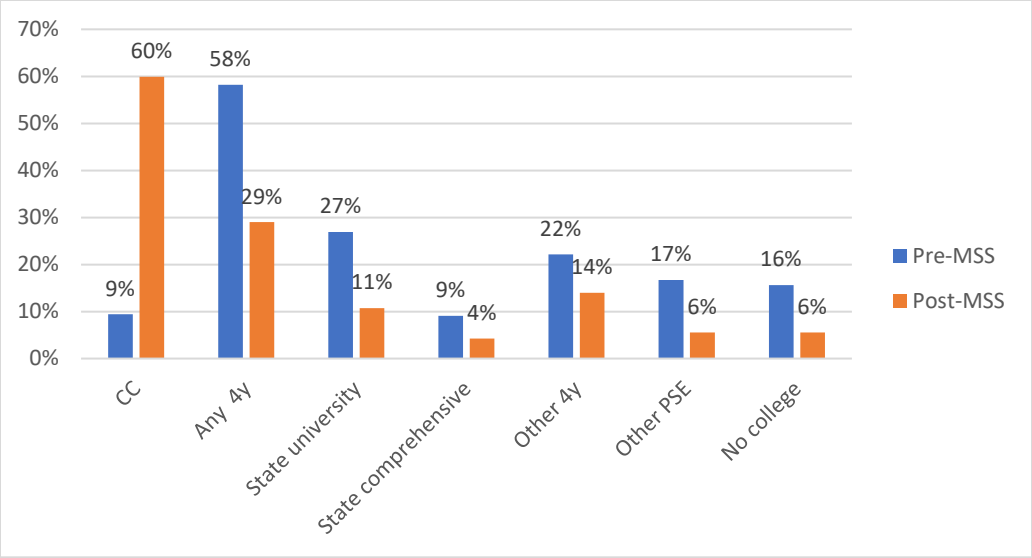


Figure 3: Full-time equivalent fall enrollment trends for first-time freshmen (FTF) and continuing students at the Community College of Philadelphia (CCP) and at other Pennsylvania community colleges. All enrollments standardized to 2014. CCP's 50th Anniversary Scholars program introduced for the fall 2015 entering cohort. Source: IPEDS.

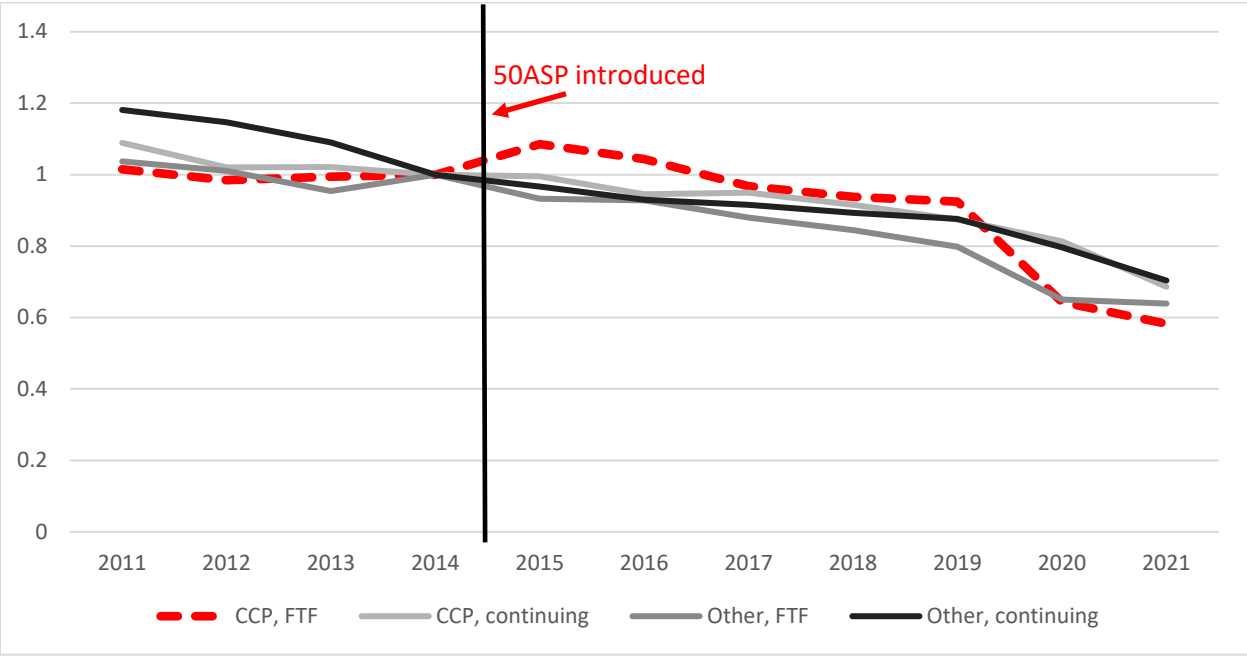
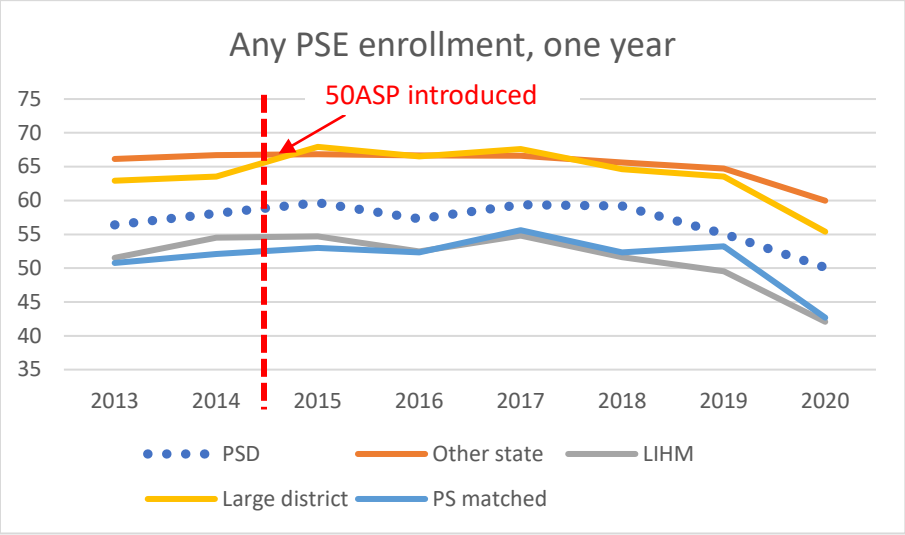
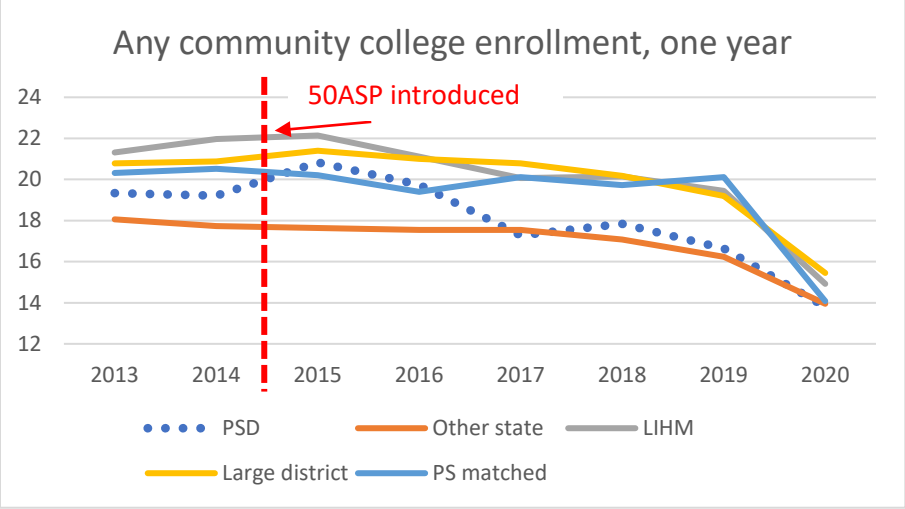
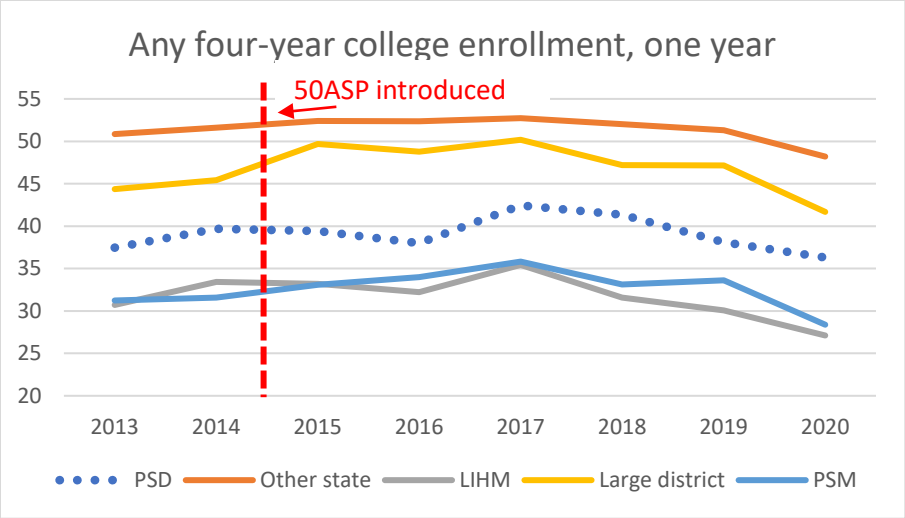


Figure 4: College-going trends for graduates of Philadelphia public schools (PSD) and comparison groups, 2013-20. LIHM=low-income, high-minoritized. PS=Propensity score.







Appendix Tables

Table A1: Unadjusted outcomes by cohort, Tamaqua & Schuylkill County, 2013-2020

Year	Tamaqua	Other Schuylkill County	Difference
One-year PSE enrollment			
2012	76.55	53.51	23.04
2013	68.75	55.75	13
2014	71.53	56.9	14.63
2015	69.66	58.76	10.9
2016	69.13	55.39	13.74
2017	68.48	54.93	13.55
2018	68.48	54.93	13.55
2019	64.34	54.36	9.98
One-year community college enrollment			
2012	48.28	8.45	39.83
2013	52.08	8.43	43.65
2014	52.78	8.15	44.63
2015	42.76	7.73	35.03
2016	43.62	6.76	36.86
2017	40.61	6.61	34
2018	40.31	4.84	35.47
2019	41.29	5.67	35.62
One-year four-year college enrollment			
2012	32.41	46.57	-14.16
2013	18.06	48.27	-30.21
2014	25	50.69	-25.69
2015	31.03	52.38	-21.35
2016	32.89	51.01	-18.12
2017	33.94	50.47	-16.53

2018	31.78	50.19	-18.41
2019	32.26	43.44	-11.18

Source: Pennsylvania Department of Education

Table A2: Tamaqua vs. other Schuylkill County graduates, 2013-20.

			Difference		
	Tamaqua	Other Schuylkill County	Raw	Matched unweighted	Matched weighted adjusted
One-year outcomes (N=4,202)					
Any postsecondary enrollment	69.98	54.88	15.1	16.72	15.82
Enrolled at community college	45.15	7.18	37.97	36.67	35.86
Enrolled at four-year college	29.76	49.09	-19.33	-16.37	-16.61
Four-year outcomes (N=2602)					
Any postsecondary enrollment	73.18	61.71	11.47	3.33	14.21
Enrolled at community college	53.23	15.92	37.31	26.58	37.18
Enrolled at four-year college	43.47	54.71	-11.24	-0.1368	-6.3
Any degree	31.77	27.94	3.83	0.09	4.69
Associate degree	16.09	4.65	11.44	11.31	10.76
Bachelor's degree	17.19	23.36	-6.17	-8.9	-4.7

Source: Pennsylvania Department of Education

Table A3: Robustness analyses using alternative samples and estimation methods

	Unadjusted means				
	Tamaqua	Comparison	Adjusted difference	SE	p
Main (Table 2) estimates (*N*=4202)					
Any postsecondary enrollment	0.74	0.57	0.16	0.01	<0.001
Enrolled at community college	0.48	0.11	0.36	0.02	<0.001
Enrolled at four-year college	0.31	0.48	-0.17	0.01	<0.001
Not conditioning on graduation (*N*=4487)					
Graduation	0.94	0.88	0.10	0.01	<0.001
Any postsecondary enrollment	0.70	0.60	0.16	0.01	<0.001
Enrolled at community college	0.45	0.16	0.34	0.02	<0.001
Enrolled at four-year college	0.30	0.46	-0.15	0.01	<0.001
Test scores & attendance covariates (*N*=3,537)					
Any postsecondary enrollment	0.76	0.67	0.15	0.01	<0.001
Enrolled at community college	0.47	0.17	0.35	0.02	<0.001
Enrolled at four-year college	0.34	0.53	-0.16	0.02	<0.001
Schuylkill County comparison group (*N*= 1,122)					
Any postsecondary enrollment	0.74	0.58	0.18	0.07	0.015
Enrolled at community college	0.48	0.08	0.42	0.07	<0.001
Enrolled at four-year college	0.31	0.52	-0.23	0.07	0.002
5 nearest neighbors matching (N=6117)					
Any postsecondary enrollment	0.74	0.65	0.15	0.01	<0.001
Enrolled at community college	0.48	0.17	0.35	0.02	<0.001
Enrolled at four-year college	0.31	0.51	-0.17	0.01	<0.001
1-1 matching (*N*=2177)					

Any postsecondary enrollment	0.74	0.65	0.16	0.02	<0.001
Enrolled at community college	0.48	0.17	0.36	0.02	<0.001
Enrolled at four-year college	0.31	0.51	-0.17	0.02	<0.001

Source: Pennsylvania Department of Education

Table A4: Estimated impacts of Morgan Success Scholarship on low-income students, 2013-20 high school graduates (*N*=1,405)

	Unadjusted means		Adjusted		
	Tamaqua	Comparison	difference	SE	*p*
Any postsecondary enrollment	0.589	0.4882	0.1576	0.0268	<0.001
Enrolled at community college	0.4528	0.1708	0.33	0.02684	<0.001
Enrolled at four-year college	0.1544	0.3235	-0.152	0.0219	<0.001

Source: Pennsylvania Department of Education

Table A5: Permutation tests for Community College first time freshman enrollment trends, 2012-20

Outcome	Estimate	permutation *p*	Sample
% change	0.1203	0.109	2012-20
Enrollment	-20.69	0.507	2012-20
Enrollment	190.76	0.361	2012-17
Enrollment	244.06	0.365	2012-16
Enrollment	289.26	0.365	2012-15

Source: IPEDS