What Knox Achieved: Estimated effects of tuition-free community college on attainment and earnings

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Online Appendix: Supplementary Figures and Results

Event Study Results

Intent-to-treat results reported in the paper rely on the following simple two-way fixed effects specification,

$$Y_{isct} = \alpha_t + \alpha_c + KA_{ct}\gamma + X_{isct}\beta + \varepsilon_{isct}, \quad \text{(A1)}$$

where KA_{ct} is equal to one for Knox County 12th graders in 2009-2011 cohorts who were eligible for Knox Achieves. Here, we discuss results from an event study extension of Equation (A1):

$$Y_{isct} = \alpha_t + \alpha_c + \Sigma_{t\neq 2008}\, Knox_c * Cohort_t\gamma_t + X_{isct}\beta + \varepsilon_{isct}, \quad \text{(A2)}$$

where $Cohort_t = \{2007, 2009, 2010, 2011\}$, omitting 2008, the last pre-treatment cohort. Findings complement the Table 2 balancing analysis but are not directly reconcilable with Equation (2) estimates of leave-one-out predicted $\tilde{Y}_{(-i)}$. Figure A1 plots γ_t estimates (circles) and 95% confidence intervals (vertical bars) derived from cluster-robust standard errors. Each panel helps to visualize results from one pre-treatment falsification test—for the 2007 cohort—as well as post-treatment effects for each cohort 2009-2011.

Panels I-II for credits within 2 and 4 years of high school indicate that Knox County's class of 2007 was insignificantly different from the next pre-treatment cohort. Relative to the omitted class of 2008, the first treated cohort experienced gains in credit accumulation, but confidence intervals widened over the next two cohorts. Panel III suggests that the likelihood of attaining a certificate and no higher fell with the 2010 cohort (perhaps because other degrees were more likely), but this pattern was not evident for either of the other two treated cohorts. Regarding associate's attainment, Panel IV depicts a fairly large and positive treatment effect for the first Knox Achieves cohort that subsequently tapered for the following two. Panel V suggests that Knox County's pre-treatment class of 2007 completed more bachelor's degrees than

expected relative to the 2008 class. Later, eligible cohorts also completed more bachelor's degrees, although – much like what we find for associate's attainment – estimated effects tapered for the last two cohorts. Finally, Panel VI shows that log earnings 9 years after high school were insignificantly lower for Knox County students prior to Knox Achieves, remained insignificantly lower for two treated classes, and climbed to par for the 2011 cohort.

Event study results suggest that, prior to 2009, outcomes were conditionally well-balanced between Knox and other counties, with the exception of bachelor's degrees. Predicted effects on bachelor's attainment were *less* likely based on observables (Table 2) but *more* likely among one Knox County pre-treatment cohort. These insights, combined with the large magnitude of imprecise bachelor's attainment coefficients relative to associate's coefficients, lead us to discount causal inferences about effects of the program on bachelor's receipt. We do not rule out positive effects on bachelor's attainment, but the magnitude of imprecise coefficients for this outcome may be driven by imbalanced potential outcomes.

Estimated Effects on Any Certificate or Associate's Receipt

Our main results for college completion focus on a student's highest college attainment among certificates, associate's degrees, and bachelor's degrees. Figure A2 depicts results for *any* certificate or associate's receipt (Panels I and III) along with copies of highest attainment results for comparison (Panels II and IV, also found in Figure 1). Results suggest that Knox Achieves accelerated certificate receipt, although imprecisely. Gains in certificate attainment rose to 2.8 percentage points 3 years after high school and tapered to 1.2 percentage points thereafter (Panel I). These certificates were often earned on the way to other degrees, however, because we detect no effect on the likelihood that a student earned only a certificate (Panel II).

Estimated effects on the likelihood of any associate's attainment (Panel III) follow a similar pattern as the likelihood of highest associate's attainment (Panel IV), climbing over the 4 years following high school and then plateauing at about 1.1 percentage points for any associate's and 0.8 percentage points for highest associates. The 0.3-point gap between those two estimates suggests that most of the effect on associate's degree receipt was among students who did not go on to earn a bachelor's degree within 9 years of high school.

Table A1 reports results for any certificate and associate's receipt for each of the subgroups described by Tables 4-6. Comparing control means across the five tables gives us a sense of how often each population "stacks" these credentials. Between 4-8% of each subgroup earns a certificate (Table A1) and 3-6% earn a certificate and no higher degree. However, our estimated treatment effects on any certificate receipt versus certificate-high attainment suggest that Knox Achieves increased the likelihood of that pathway, albeit not significantly (Figure A2). The biggest gap in any versus highest certificate attainment is for higher-achieving students – 8% earn at least a certificate whereas half that many stop with a certificate. Between 2-8% of students in each population earn an associate's degree. This degree is least common among Black students (2%) and most common among higher-achieving students (8%). The likelihood of

associate's attainment without a bachelor's degree is 1-5%, again with Black students being least likely and higher-achieving students being most likely.

Regression results are in accord with our main results for highest attainment, in that positive effects on associate's degrees are more pronounced for lower-income and higher-achieving students. As noted in the main paper, there are bigger differences between any and highest associate's degrees for Black and female students. Black students were insignificantly 0.6 percentage points more likely to attain an associate's and no higher if they were eligible for Knox Achieves (Table 6), but significantly 1.4 percentage points more likely to earn an associate's degree at all (Table A1). Any associate's degree was insignificantly 1.3 percentage points more likely for women (Table A1), whereas the increased likelihood that a woman attained an associate's and no higher was only 0.8 and marginally significant (Table 6). This pattern, although imprecisely estimated, suggests that Knox Achieves may have led more Black and female students to follow an associate's to bachelor's route.

Estimating Potential Selection into Public Postsecondary Institutions and In-State Earnings

Table A2 applies Equation (A1) to the likelihood of enrolling in a private or out-of-state college (where we cannot observe degree outcomes) as well as the likelihood of non-missing, in-state, UI-covered earnings at selected intervals 1-9 years after high school. Columns (2)-(3) partition the samples into students with below-median and above-median achievement, respectively.

Knox Achieves eligibility reduced the likelihood of enrolling in a private or out-of-state institution by 2.3 percentage points, although this is not precise according to Ferman and Pinto (2019) p-values. Selection out of our observable degree sample could be problematic if students with a different likelihood of completing college were particularly likely to make this substitution. Columns (2)-(3) support this possibility, because lower-achieving students were not more or less likely to enroll in private or out-of-state colleges, while higher-achieving students were 2.6 percentage points less likely to do so. Neither estimate is precise, but nevertheless we err on the side of caution in our main results, which omit 5% of Knox Achieves participants with the most college credits at 4 years. In the next section we describe results when we modify that bounding assumption.

Looking to the rest of Table A2, Knox Achieves is linked to small, imprecise, and mostly negative changes in the likelihood of having observable earnings. These changes are small, imprecise, and mostly positive when we partition by achievement (we can reconcile the change in sign from the 7-11% of students with missing achievement), and no clear pattern emerges for selection into earnings among students in one or the other achievement division.

Addressing Potential Selection into Public Postsecondary Institutions

Table A3 illustrates the sensitivity of our main intent-to-treat findings to different degrees of bounding for potential non-random selection into the sample of students with observable college

credit and completion data. Lee (2009) addressed a similar sample selection problem in an analysis of a job training program on wages that could only be observed for workers, acknowledging that employment could itself be affected by the program. Following Lee's proposed "trimming" solution, results reported in the main body of the paper exclude the top 5% of Knox Achieves participants in terms of credit accumulation within four years of high school, accounting for an extreme form of positive sample selection. The necessary monotonicity assumption is that Knox Achieves could only *increase* the likelihood of enrolling in a public Tennessee institution, and it could not increase observability for some students but increase attrition for others. We believe that this assumption is plausible for the public college enrollment margin.[1]

Column (1) of Table A3 repeats baseline Equation (1) and (A1) findings, which can also be found in Table 3 of the main paper. Column (2) trims the participant subsample by 10% rather than 5%, resulting in smaller and still-insignificant effects of Knox Achieves availability on college credits, certificate or bachelor's degree attainment, and earnings. Estimated effects for associate's attainment are very similar in magnitude and significance. Column (3) lists estimates with no trimming, raising some coefficients but again leaving our inferences unchanged. We conclude that omitting or including the top credit-earning Knox Achieves participants has very little bearing on results.

Addressing Potential Selection into In-State Earnings

Table A4 lists results for log earnings among different sample criteria that attempt to address omissions in the UI data. Column (2) regressions omit students who left the UI data as late as 6 years after high school, if not earlier. If Knox Achieves led to more out-of-state or entrepreneurial opportunities for inherently high-wage students, their selection out of the wage data might bias earnings results downward. We determine attrition from terminal runs of missing data as in Grogger (2012). For example, Column (2) coefficients are from regressions that omit students with no earnings in the 6^{th} – 9^{th} years after high school, as well as anyone who had a string of missing earnings from year $t = 2$ through $t = 9$ (students with missing earnings in all years are already excluded). Results for 1, 3, and 5 years after high school change very little. Results for years 7 and 9 are identical by design to baseline findings in Column (1). Column (3) specifications omit students who enrolled out of state, and again, coefficients and significance indicators are very similar to Column (1).

The specifications reported in Columns (4) and (5) omit the bottom and top 5%, respectively, of Knox Achieves participants in terms of their earnings as of each interval after high school. These bounding exercises are similar to that of Table A3, but under the assumption that the program helped lower-earning students gain employment in in-state, UI-covered occupations (Column 4)

[1] As noted in the main text, monotonicity is less viable on the question of attrition from observed in-state earnings covered by unemployment insurance, since the program's effects on college-going could increase the likelihood of any work as well as the likelihood of having more out-of-state opportunities for work.

or influenced higher-earning students to work in these occupations (Column 5). Although each trimming assumption changes the sample by a small number of students, estimated effects on earnings are notably different in Columns (4) and (5) compared to Column (1). Omitting the lowest earning participants at each point in time increases estimated returns from 1.4% to 4.1% at year 7 and from -3.0% to -0.3% at year 9. Omitting the top earning participants, however, decreases year 7 returns to 0.1% and decreases year 9 returns to -4.2%. None of these estimated returns are precise, however, and wide degrees of unexplained variation in earnings may be responsible for these swings. Nonetheless, outmigration or selection into UI-covered jobs do not appear to be responsible for inconclusive effects of Knox Achieves on earnings.

Treatment-on-the-Treated (TOT) Estimates

OLS Results

Now, we turn to estimates of the effect of individual-level participation in Knox Achieves. We estimate the following:

$$Y_{isct} = \alpha_t + \alpha_c + KAPartic_{isct}\gamma + X_{isct}\beta + \varepsilon_{isct}, \quad \text{(A3)}$$

The "treatment" variable $KAPartic_{isct}$ in Equation (A3) is an indicator variable equal to one for students who signed up to learn more about Knox Achieves. To make causal inferences about γ with confidence, we rely heavily on the X_{isct} vector of observable student, school, and county features to control for factors that may influence students' interest in a free community college program as well as their later outcomes. It is plausible that unobservable features affected both $KAPartic_{isct}$ and Y_{isct} outcomes, but nevertheless, it is helpful to contextualize preferred intent-to-treat effect estimates against conditional differences in Y_{isct} between participants and other students. In addition, we explore the extent to which linear Equation (A3) results are sensitive to much more flexible functional forms alongside data-driven model selection (Hansen, Chernozhukov, and Belloni 2014), and we gauge the potential severity of selection on unobservables using coefficient stability methods proposed by Oster (2019).

Column (1) of Table A5 lists results when the X_{isct} vector is limited to cohort fixed effects. This is a nearly unconditional estimate of relative college credit and accumulation gaps between Knox Achieves participants and other students. Table A5 shows that they were 3.0 percentage points more likely to earn a certificate (and no higher) than their peers in Knox County, throughout the state, and in earlier cohorts. They were 7.1 percentage points more likely to attain an associate's degree, 5.7 percentage points less likely to earn bachelor's degree, and they earned about as much as other students 9 years after high school.

Some of these gaps narrow when we add the basic set of controls used in the main paper. The Column (2) model estimates a 6.5 percentage point gain in associate's receipt versus a 7.1-point gain in Column (1). Participants' shortfall in bachelor's degree receipt widens to 6.7 percentage points, and estimated effects on 9th year earnings increase to a statistically significant 5% gain (Column 2). Coefficient changes combined with gains in explained variance (R^2) lead to the

inference that controls are informative and that a sizable degree of selection on unobservables would be necessary to explain away Column (2) conditional gaps. Estimates of Oster's (2019) δ parameter are underneath R^2 values in Columns (2)-(4). If the true treatment effect on associate's attainment were zero, for example, selection into Knox Achieves based on unobservable determinants of earning an associate's would have to be 7.1 times as informative as selection according to the basic X_{isct} vector components.

Specifications reported in Column (3) of Table A5 expand the X_{isct} vector from 30 elements to over 800 by including quadratic functions of continuous variables and interactions between and among all binary and continuous variables in X_{isct}. Point estimates for college credit gains are nevertheless similar under this more saturated and more flexible model, suggesting the Knox Achieves participants accumulated more credits within two years but fewer within four, which is consistent with their higher rate of two-year credential receipt and lower rate of bachelor's receipt. Between Column (2) and (3), the negative effect of participation on bachelor's receipt shrinks from 6.7 to 5.3 percentage points. Associate's attainment effects narrow somewhat from 6.5 percentage points in Column (2) to 6.2 percentage points in the saturated Column (3) model, and 9th year earnings effects increase from 5.0% to 6.1%. Even though we added many more observable controls to the linear model, Oster's (2019) δ indicates that proportionate selection on unobservables would still have to be quite large to completely explain treatment-on-the-treated effect estimates.

Lastly, Column (4) reports results from a specification of Equation (A3) with an intermediate degree of flexibility relative to baseline and saturated models. Specifically, we estimate Equation (A3) using a set of controls identified by least absolute shrinkage and selection (LASSO). We follow the post-double-selection method prescribed by Hansen, Chernozhukov, and Belloni (2014) to recover interpretable Equation (A3) estimates of γ from a large, data-driven set of controls. Point estimates change little relative to the flexible Column (3) specification.

Matching Results

Table 1 of the main paper illustrates that the statistical profile of Knox Achieves participants was notably different from non-participants in terms of demographics, achievement, and family income. This difference may mean that models such as Equation (A3) will depend too heavily on extrapolation when forming estimates of the conditional gap in outcomes between treated and untreated students (Imbens 2015). In such cases, matching estimators can be a suitable alternative to linear models.

The intuition with matching is to pair each Knox Achieves participant with a quantitatively similar non-participant and interpret the average difference in outcomes across matched pairs as the treatment effect. We take two complementary approaches to defining similarity between treated and control students. We first pair participants to similar students in terms of several observable features in X_{isct}: gender, race, Hispanic ethnicity, repeating the 12th grade, free or reduced-price lunch, junior year earnings, number of counties since 8th grade, ACT composite,

English and math end-of-course exam scores, schoolwide percent Black or Hispanic, and the county unemployment and poverty rates. Mahalanobis matching computes a distance metric describing the similarity of this control vector between every *i, j* pair of students and matches treated students to the untreated nearest neighbor in terms of this distance metric. For Mahalanobis treatment effect estimates, we compute standard errors according to Abadie and Imbens (2006). Figure A3 depicts the average standardized gap in observable features before and after matching Knox Achieves participants to non-participating Knox County students and students elsewhere in the state. As one would hope, matching on these covariates limits observable differences between Knox Achieves participants and comparison students.[2]

Our second matching approach is to pair Knox Achieves participants with non-participants according to their estimated likelihood of signing up for the program. We estimate this propensity by logit for seniors who had the opportunity to sign up, i.e., Knox County classes of 2009-2011. Predictive factors include all variables represented in X_{isct} of Equation (1). Parameter estimates are mapped to seniors in other counties and pre-program cohorts. We use a nearest neighbor propensity score estimator where matches are constrained to be within two percentage points (i.e., a caliper of 0.02), within the range of overlap between treated and control propensity (i.e., with common support), and below the top one percent of treated students' propensity distribution. Figure A4 plots the distribution of predicted propensity by Knox Achieves participation. Although participants generally had a higher propensity than non-participants, the area of common support is inclusive of them all.

Under both Mahalanobis and propensity score matching, we compute two sets of TOT estimates. The first allows Knox Achieves participants to be matched to any other 12th grader in the state, including their non-participating peers in Knox County. This technique could introduce bias from selection on unobservables, so in a second model we restrict the control reservoir to counties outside of Knox. The latter approach may lead to weaker matches on observables and cannot rule out omitted variable bias among Knox Achieves participants, but it will at least restrict comparison students to those who did not have the opportunity to take-up the program.

Table A6 presents treatment-on-the-treated estimates from Mahalanobis and propensity score matching procedures. The first takeaway is that treatment effect estimates are at times different under these four matching schemes and different from linear estimates reported in Table A5 Column (2) and copied to Table A6 Column (1) for comparison. Outcomes with the least consistent TOT effect estimates are credit accumulation after four years (-2.25 in the linear model but insignificant in matching models) and 9th year earnings (5.0% higher in Column 1 but as much as 12.6% lower in Column 4). These are the same outcomes for which intent-to-treat effect estimates are most inconclusive. Unfortunately, treatment-on-the-treated effect estimates

[2] Note that Mahalanobis matching minimizes the distance metric between matches without regard for which variables in the matching vector are most important for predicting college outcomes.

do not shed more light on how program eligibility shaped credit accumulation or 9th year earnings.

Matching results are more consistent for degree outcomes. Participant rates of certificate attainment were 1-2 percentage points higher than the counterfactual (with mixed statistical precision, much like ITT results for certificates). Associate's degree attainment was 6-8 percentage points more likely than the counterfactual, in agreement with linear treatment effect estimates. Bachelor's degree attainment was 4-5 percentage points lower among participants than matched peers, a slightly narrower gap than -6.7 in Column 1.

Comparison of Intent-to-Treat and Treatment-on-the-Treated Estimates

If countywide trends in postsecondary outcomes were driven entirely by Knox Achieves, and if spillover effects were minimal between participants and non-participants, we would expect TOT estimates to be about 6.5 times as large as intent-to-treat (ITT) estimates, based on 15.3% participation among eligible cohorts. As described above, TOT results for credit accumulation and 9th year earnings are difficult to reconcile with themselves, much less with ITT estimates. Taking 9th year earnings, for example, ITT models estimate that Knox Achieves eligibility led to 3% lower earnings. If this was entirely driven by program participants, we would expect them to have 19.5% lower earnings than the counterfactual. This is a larger decline in earnings than we estimate in any TOT model—the closest is a 12.6% decline in Table A6 Column (4), whereas the other TOT estimates range from -3.7% (insignificant) to +5.0% (significant).

TOT effect estimates point to 6-8 percentage-point higher rates of associate's attainment, and these are consistent across OLS and matching models. These gains would imply 0.9-1.2 percentage-point gains in associate's attainment countywide, which is close to the 0.8 percentage-point effect reported in the main paper. Participants were 4-7 percentage points less likely to attain a bachelor's degree, however, which would translate to a 0.6-1.1 point *decline* in bachelor's attainment that we do not see countywide. As discussed in the main paper, we can speculate as to why we come to different conclusions for the intent-to-treat effect of eligibility on bachelor's receipt versus the participant/non-participant gap in conditional bachelor's receipt. One possibility is *ex ante* imbalance in would-be bachelor's attainment, which we see conflicting evidence of in Table 2 and Figure A1. Selection bias in TOT estimates is another explanation. Finally, there may have been spillover effects from the program's general higher education advocacy to participants and non-participants alike.

References

Abadie, Alberto, and Guido W. Imbens. 2006. "Large Sample Properties of Matching Estimators for Average Treatment Effects." *Econometrica* 74(1): 235-267.

Ferman, Bruno and Christine Pinto. 2019. “Inference in Differences-in-Differences with Few Treated Groups and Heteroskedasticity.” *Review of Economics and Statistics* 101(3):, 452-467.

Grogger, Jeffrey. 2012. “Bounding the Effects of Social Experiments: Accounting for Attrition in Administrative Data.” *Evaluation Review* 36(6): 449-474.

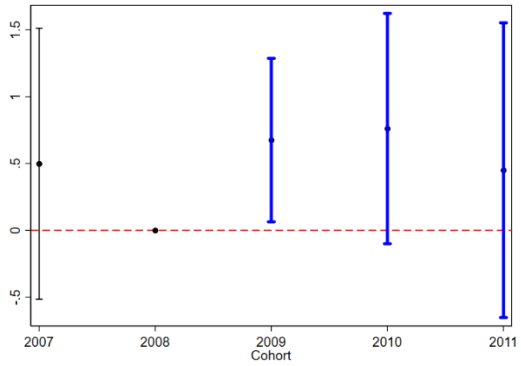
Hansen, Christian., Victor Chernozhukov, and Alexandre Belloni. 2014. “Inference on Treatment Effects After Selection Among High-Dimensional Controls.” *Review of Economic Studies* 81(2): 608-650.

Imbens, Guido W. 2015. “Matching Methods in Practice: Three Examples.” *Journal of Human Resources* 50(2): 373-419.

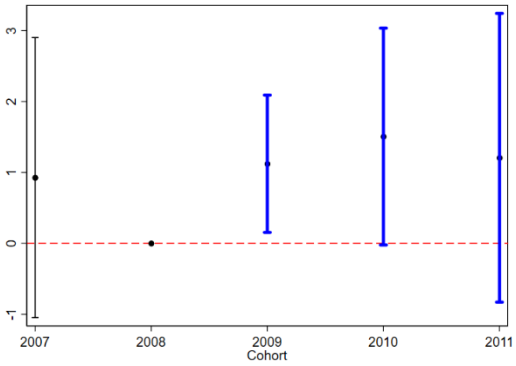
Lee, David S. 2009. “Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects.” *The Review of Economic Studies* 76(3): 1071-1102.

Figure A1. Event study estimates

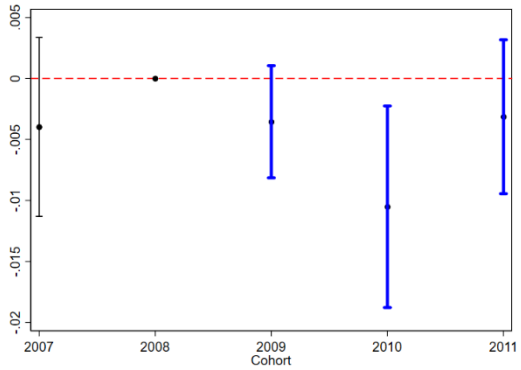
I. Credits within 2 years of high school



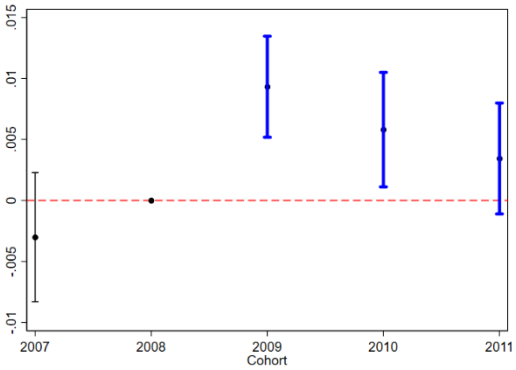
II. Credits within 4 years of high school



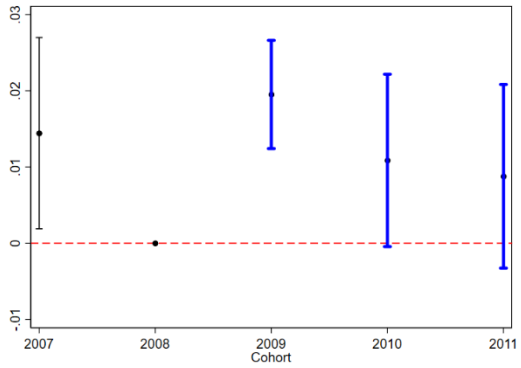
III. Certificate attainment



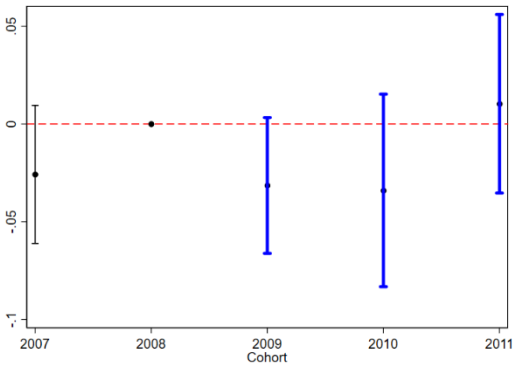
IV. Associate's degree attainment



V. Bachelor's degree attainment

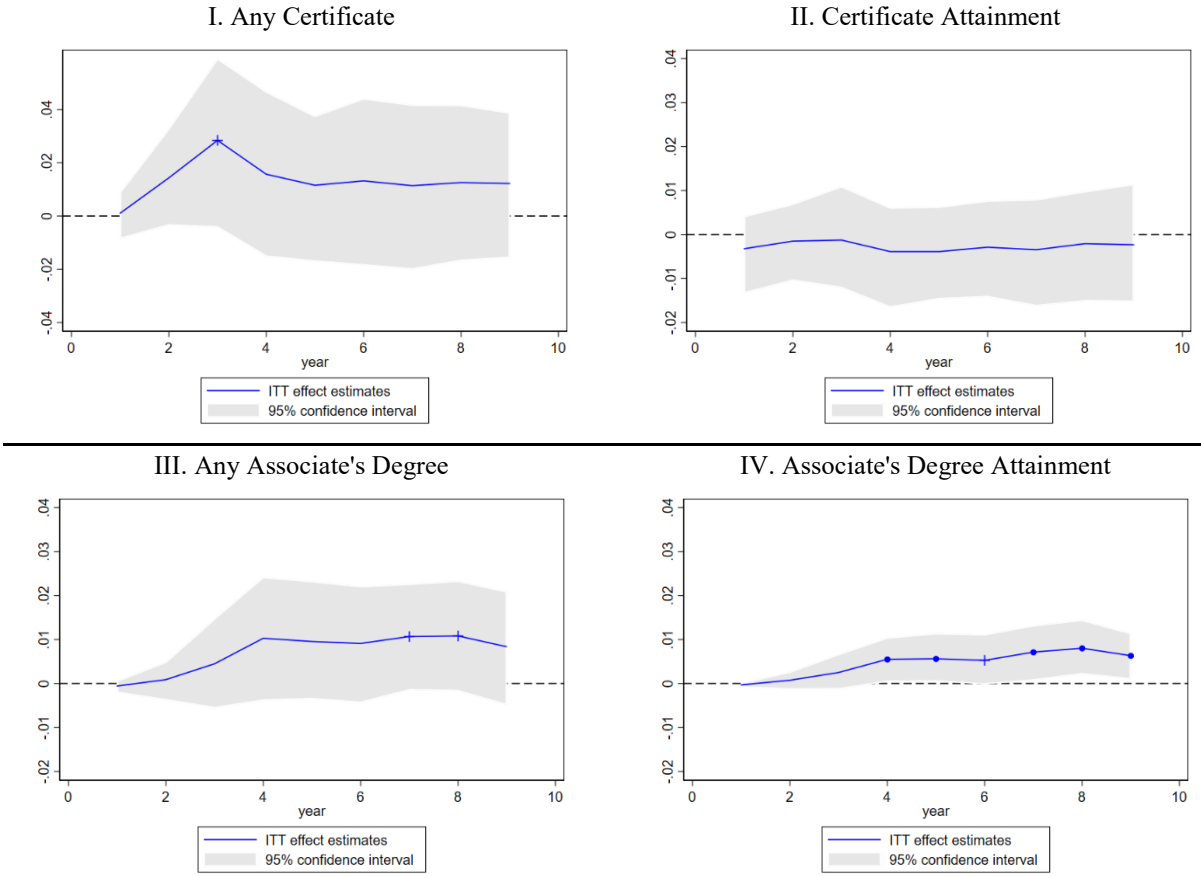


VI. Log in-state earnings nine years after high school



Notes: Each figure plots Equation (A2) event study estimates, omitting the Knox*2007 interaction, with 95% confidence intervals derived from robust standard errors that allow for clustering within counties.

Figure A2. Knox Achieves and certificate/degree completion and attainment, by years since high school



Notes: Each figure plots Equation (1) results for any college certificate or degree completion (left panels) versus highest attainment (right panels, also found in Figure 1 of the main paper), by years since high school, along with shaded 95% confidence intervals

+ $p < 0.10$, ● $p < 0.05$ (Ferman and Pinto 2019)

Table A1: Knox Achieves and any certificate or associate's receipt, by subsidized lunch, achievement, race, ethnicity, and gender

Subsample	(1) Not eligible for subsidized lunch	(2) Reduced-price lunch eligible	(3) Free-lunch eligible	(4) Below-median EOC achievement	(5) Above-median EOC achievement
Any certificate within 8 years of high school	0.013	0.007	0.018	-0.003	0.025
	(0.445)	(0.893)	(0.498)	(0.734)	(0.255)
	0.073	0.079	0.056	0.068	0.076
Any associate's within 8 years of high school	0.011	0.002	0.017	-0.008	0.025
	(0.398)	(0.952)	(0.287)	(0.289)	(0.122)
	0.070	0.052	0.031	0.035	0.084
Subsample	(6) Black	(7) Hispanic	(8) White	(9) Women	(10) Men
Any certificate within 8 years of high school	0.004	0.018	0.012	0.013	0.013
	(0.570)	(0.359)	(0.511)	(0.485)	(0.190)
	0.037	0.039	0.081	0.065	0.071
Any associate's within 8 years of high school	0.014**	0.001	0.01	0.013	0.009**
	(0.042)	(0.942)	(0.279)	(0.163)	(0.047)
	0.021	0.037	0.071	0.072	0.042

Notes: The table lists γ estimates from Equation 1, ITT estimates of the effect of Knox Achieves availability on any certificate or associate's attainment, across 10 student subgroups. See Table 4-6 for analogous treatment effect estimates for the likelihood that students attained a certificate or associate's and no higher credential within 8 years. Ferman and Pinto (2019) p-values are listed in parentheses. Control means are listed below p-values.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A2: Knox Achieves and selection into observed college and earnings outcomes

Subsample	(1) All TN 12th graders	(2) Below-median EOC achievement	(3) Above-median EOC achievement
Enrolled in a private or out-of-state college	-0.023	-0.006	-0.026
	(0.278)	(0.584)	(0.440)
	0.092	0.060	0.144
Non-missing earnings 1 year after high school	0.003	0.003	0.023
	(0.843)	(0.915)	(0.416)
	0.629	0.650	0.664
Non-missing earnings 3 years after high school	-0.019	0.001	-0.013
	(0.465)	(0.974)	(0.698)
	0.654	0.695	0.685
Non-missing earnings 5 years after high school	-0.008	0.010	0.006
	(0.739)	(0.685)	(0.774)
	0.683	0.722	0.712
Non-missing earnings 7 years after high school	-0.008	0.009	0.011
	(0.760)	(0.552)	(0.531)
	0.664	0.709	0.684
Non-missing earnings 9 years after high school	-0.003	0.006	0.025
	(0.937)	(0.742)	(0.291)
	0.639	0.684	0.655
Treated students (1st year)	14,698	5,789	7,835
All students (1st year)	347,049	154,883	154,883

Notes: The table lists γ estimates from Equation 1, ITT estimates of the effect of Knox Achieves availability on the likelihood of enrolling in private or out-of-state colleges and universities (for which we do not observe completion outcomes), and on the likelihood of having any observed in-state earnings 1-9 years after high school. Ferman and Pinto (2019) p-values are listed in parentheses. Control means are below p-values. Columns (2) - (3) exclude students with missing end-of-course achievement.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A3: Knox Achieves and postsecondary outcomes, under different sample trimming criteria

Sample	(1) Baseline 5% trim	(2) 10% trim	(3) No trim
THEC college credits within two years	1.301	1.085	1.489
	(0.263)	(0.319)	(0.170)
THEC college credits within four years	2.197	1.728	2.578
	(0.256)	(0.389)	(0.181)
Certificate attainment	-0.002	-0.002	-0.002
	(0.672)	(0.691)	(0.648)
Associate's degree attainment	0.008***	0.008***	0.008***
	(0.003)	(< 0.001)	(0.008)
Bachelor's degree attainment	0.011	0.006	0.014
	(0.355)	(0.585)	(0.210)
Log in-state earnings nine years after high school	-0.030	-0.034	-0.03
	(0.491)	(0.429)	(0.497)
Treated students	13,202	13,102	13,275
All students	314,973	314,872	315,047

Notes: The table lists γ estimates from Equation 1, ITT estimates of the effect of Knox Achieves availability on college persistence, attainment, and earnings. Column (1) repeats baseline estimates from Table 3, where we omit the top 5% of Knox Achieves participants in terms of college credits within four years of high school. Column (2) reports results from a specification where the top 10% of participants are omitted, and the Column (3) specification retains all Knox Achieves participants in the sample. Ferman and Pinto (2019) p-values are listed in parentheses.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

Table A4: Knox Achieves and earnings after high school, under different assumptions about sample selection

Subsample	(1) Baseline	(2) Without attriters	(3) Without out-of-state enrollees	(4) Without bottom 5% of participant earners	(5) Without top 5% of participant earners
In-state log earnings 1 year after high school	-0.017	-0.036	-0.023	0.007	-0.029
	(0.716)	(0.508)	(0.574)	(0.892)	(0.544)
In-state log earnings 3 years after high school	-0.011	-0.017	-0.024	0.018	-0.025
	(0.781)	(0.712)	(0.539)	(0.686)	(0.582)
In-state log earnings 5 years after high school	-0.007	-0.015	-0.011	0.020	-0.021
	(0.872)	(0.744)	(0.763)	(0.638)	(0.612)
In-state log earnings 7 years after high school	0.014	0.014	0.013	0.041	0.001
	(0.740)	(0.744)	(0.762)	(0.308)	(0.970)
In-state log earnings 9 years after high school	-0.030	-0.030	-0.034	-0.003	-0.042
	(0.449)	(0.473)	(0.406)	(0.929)	(0.344)
Treated students (1st year)	9,364	8,090	9,038	9,279	9,264
All students (1st year)	218,443	192,872	210,575	218,358	218,342

Notes: The table lists γ estimates from Equation 1, ITT estimates of the effect of Knox Achieves availability on log earnings 1-9 years after high school. Ferman and Pinto (2019) p-values are listed in parentheses. Column (1) repeats baseline results from Table 3 and Figure 2, Panel I. Column (2) omits students who left the earnings sample, i.e., who had no observed in-state earnings between six and nine years after high school. Column (3) omits students who enrolled out of state. Column (4) omits the bottom 5% of Knox Achieves participants in terms of in-state earnings, and Column (5) omits the top 5% of Knox Achieves participants in terms of in-state earnings.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

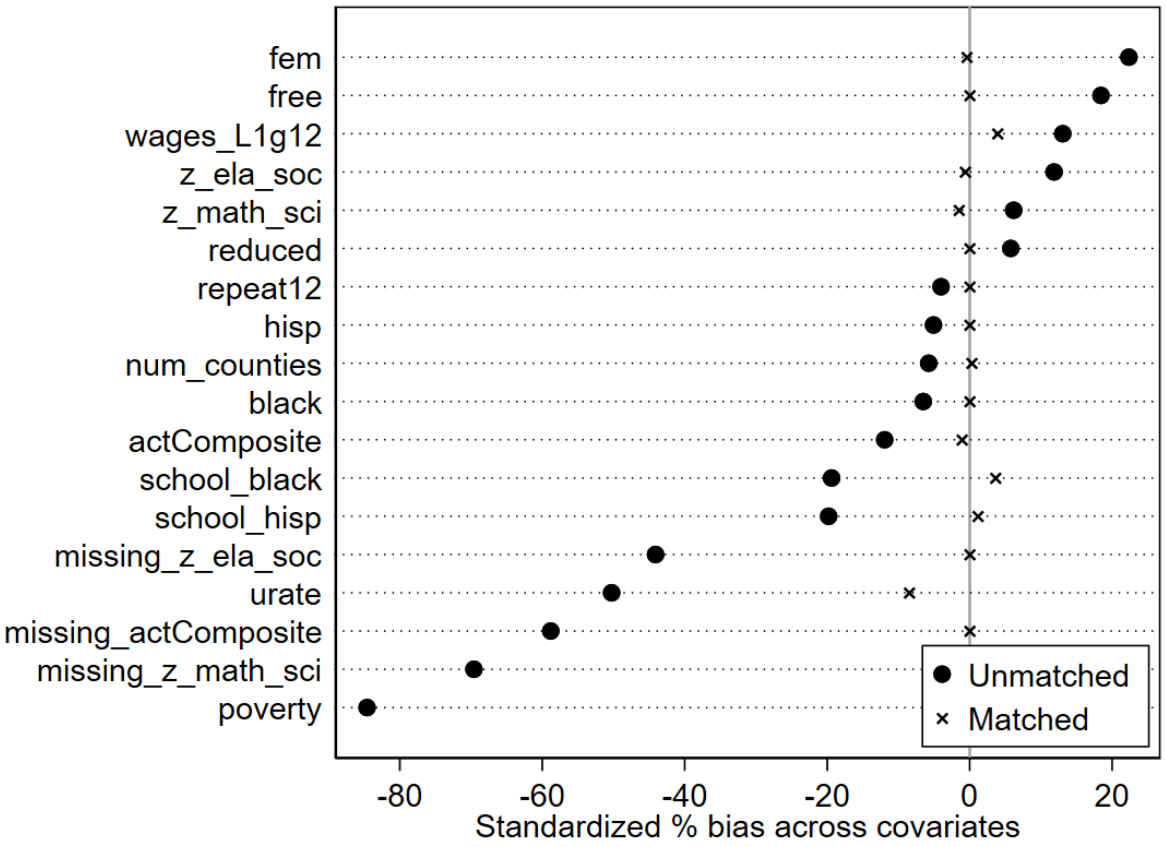
Table A5: Average treatment-on-the-treated effect estimates

Controls	(1) Cohort fixed effects	(2) Basic	(3) Flexible	(4) Selected features among flexible set
THEC college credits within two years	1.750***	1.297***	1.684***	1.734***
	(0.276)	(0.140)	(0.180)	(0.166)
R-squared	0.400	0.403	0.470	0.466
Oster delta estimate		1.306	1.838	1.988
THEC college credits within four years	-0.916	-2.247***	-1.287***	-0.940***
	(0.666)	(0.339)	(0.346)	(0.295)
R-squared	0.404	0.407	0.475	0.471
Oster delta estimate		-1.158	-0.761	-0.602
Certificate attainment	0.030***	0.023***	0.022***	0.022***
	(0.003)	(0.001)	(0.002)	(0.001)
R-squared	0.021	0.029	0.037	0.034
Oster delta estimate		-16.172	-20.467	-16.796
Associate's Degree Attainment	0.071***	0.065***	0.062***	0.062***
	(0.004)	(0.001)	(0.001)	(0.001)
R-squared	0.029	0.034	0.046	0.043
Oster delta estimate		7.146	5.614	6.047
Bachelor's Degree Attainment	-0.057***	-0.067***	-0.053***	-0.053***
	(0.006)	(0.003)	(0.003)	(0.002)
R-squared	0.269	0.271	0.325	0.321
Oster delta estimate		-6.494	-9.151	-8.902
Log in-state earnings nine years after high school	0.026	0.050***	0.061***	0.054***
	(0.020)	(0.010)	(0.006)	(0.005)
R-squared	0.099	0.103	0.122	0.117
Oster delta estimate		-14.312	-9.448	-11.165
Treated students	2,026	2,026	2,026	2,026
All students	314,973	314,973	314,973	314,973

Notes: The table lists γ estimates from variations of Equation (A3) for TOT estimates of the effect of Knox Achieves participation on college persistence, attainment, and earnings. In these specifications, $KAPartic_{isct}$ is a binary indicator equal to 1 for Knox Achieves participants. Robust standard errors, in parentheses below point estimates, allow for clustering by county. R-squared statistics are reported below standard errors, with Oster (2019) delta estimates below each R-squared. The Column (1) specification regresses each outcome against a KA participation indicator and cohort indicators. The Column (2) model include county and cohort fixed effects along with all X_{isct} control variables used in the main ITT specification. Column (3) adds a complete set of interactions between all control variables in the Equation (1) X_{isct} vector as well as squared values for continuous controls. Finally Column (4) regresses outcomes against a set of the Column (3) covariates that are selected by post-double-selection LASSO.

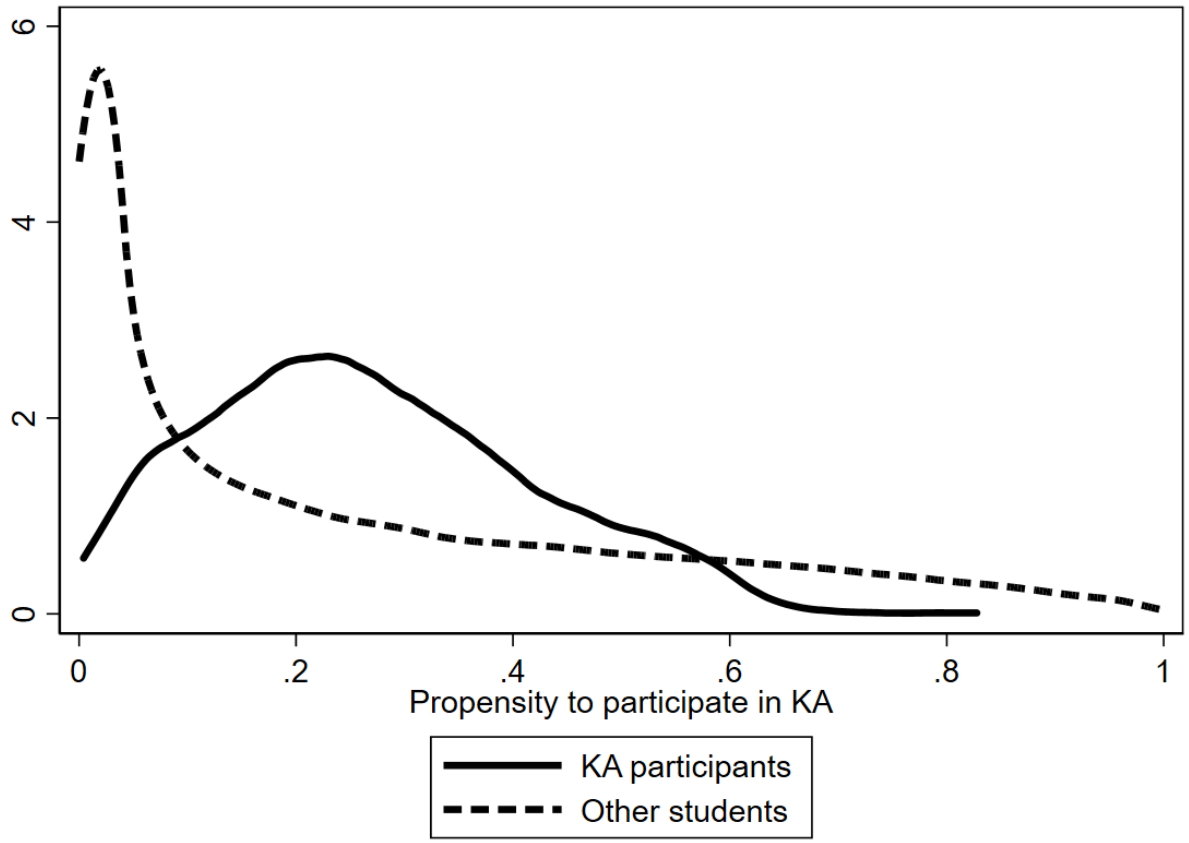
*** p < 0.01, ** p < 0.05, * p < 0.10

Figure A3. Unmatched and matched balance in student and school observable features



Notes: The figure depicts standardized differences in observed student, school, and county characteristics within unmatched (circles) and matched (x markers) samples. The donor pool includes all Tennessee 12th graders, 2007-2011. Treatment and control observations are matched by minimizing Mahalanobis distance metrics between vectors of these control variables.

Figure A4: Knox Achieves participation propensity



Notes: The figure depicts kernel densities of the estimated propensity to participate in Knox Achieves for actual participants (solid line) and ineligible non-participants (dashed line). Propensities are estimated by logit for Knox County 12th graders in the classes of 2009-2011. Factors in the logit model included X_{isct} variables described under Equation (1).

Table A6. Matching results: Knox Achieves participation and postsecondary outcomes

	(1)	(2)	(3)	(4)	(5)
Control reservoir includes Knox County non-participants		Yes	Yes	No	No
Method	OLS	Mahalanobis	Propensity score	Mahalanobis	Propensity score
THEC college credits within two years	1.297***	2.613***	2.23***	2.477***	2.390***
	(0.140)	(0.631)	(0.756)	(0.650)	(0.755)
THEC college credits within four years	-2.247***	-0.453	-0.622	-0.948	-0.478
	(0.339)	(1.115)	(1.318)	(1.160)	(1.320)
Certificate Attainment	0.023***	0.012	0.024***	0.014*	0.025***
	(0.001)	(0.008)	(0.007)	(0.008)	(0.007)
Associate's Degree Attainment	0.065***	0.060***	0.077***	0.062***	0.079***
	(0.001)	(0.009)	(0.008)	(0.009)	(0.008)
Bachelor's Degree Attainment	-0.067***	-0.045***	-0.049***	-0.038***	-0.052***
	(0.003)	(0.011)	(0.012)	(0.012)	(0.012)
Log in-state earnings nine years after high school	0.050***	-0.037	-0.011	-0.126***	-0.006
	(0.010)	(0.038)	(0.039)	(0.038)	(0.039)
Treated students on support	2,026	1,671	1,671	1,671	1,671
All students on support	314,973	221,504	195,144	213,887	187,533

Notes: The table lists Mahalanobis and propensity score matching results for postsecondary outcomes, with OLS Equation (A5, Column 2) results in Column (1) for comparison. For Column (2) – (5) models, we use a nearest-neighbor matching estimator, matching each Knox Achieves participant to one non-participant, with replacement, in terms of the Mahalanobis distance metric or the propensity score. Standard errors for Mahalanobis matching are computed according to Abadie and Imbens (2006). Propensity score estimates accommodate a 2 percentage-point caliper of participation propensity and trim the top one percent of Knox Achieves participants in terms of participation propensity.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$