Evaluating the Longer Term Impact of Early College High Schools on Workforce Outcomes

Report prepared for the U.S. Department of Education–Institute of Education Sciences

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Project Abstract

The Long-Term Impact of Early College High Schools Study—funded by the Institute of Education Sciences (IES)—aimed to explore the long-term impacts of early college (EC) high schools on students' academic outcomes (e.g., college enrollment, degree attainment) and outcomes after schooling. In this report—the second follow-up report on our initial EC impact study—we focus specifically on key questions about workforce, financial, and other life outcomes: What were the impacts of ECs on workforce, financial, and other life outcomes in the 12th to 14th years after expected high school graduation? Did the impacts of ECs vary by participant characteristics?

The analyses in this report focus on individuals who originally participated in 17 admission lotteries conducted by seven ECs for three cohorts of students, and examined outcomes after formal schooling. Key takeaways include the following:

1. Participants who were admitted to an EC, regardless of whether they attended the EC, did not experience a significant effect on any of the workforce, financial, and other life outcomes measured with survey data 12 to 14 years after expected high school graduation.
2. EC impacts on workforce, financial, and other life outcomes measured 12 to 14 years after expected high school graduation did not differ significantly by individuals' race/ethnicity, low-income status, or prior achievement.

While our initial impact study and first follow-up study found that attending an EC had effects on secondary and postsecondary enrollment, and completion for participants with different background characteristics, we found that attending an EC had no impact on any of the workforce, financial, and other life outcomes that we analyzed 12 to 14 years after participants' expected high school graduation. Future studies could consider examining workforce, financial, and other life outcomes in years before the 12th to 14th year after expected high school graduation using administrative data sources (e.g., IRS or unemployment insurance records). Further research may also examine if other factors, such as EC students' college major or local labor market opportunities, influence the impact of ECs on longer-term outcomes.

Introduction

Research has long indicated that obtaining a postsecondary degree or credential is likely to increase an individual's success in the workforce and lead to higher lifetime earnings (Belfield & Bailey, 2019). In 20221, the median earnings of bachelor's degree holders ($66,600) were 59% higher than those only having high school diplomas ($41,800; National Center for Education Statistics [NCES], 2024a). A substantial difference in earnings also existed between those with advanced degrees and those with bachelor's degrees, with median earnings 20% higher for the former than for the latter in 2022 (National Center for Education Statistics [NCES], 2024a). Previous studies have also demonstrated that those with college degrees performed better in challenging economic times than those with high school diplomas or without diplomas (Carnevale et al., 2015, 2018).

Not all students, however, have equal access to a rewarding postsecondary education. Over the past few decades, although college participation has steadily increased for the overall student population, gaps in both college enrollment and degree attainment rates between students from disadvantaged backgrounds and their more advantaged peers have persisted (De Brey, et al. 2021; Reber & Smith, 2023; National Center for Education Statistics [NCES], 2024b). Research on the causes of these gaps has shown differences in academic preparation and college entrance exam taking to be particularly important drivers (Holzman, et al. 2020; Reber & Smith, 2023). Thus, providing targeted college preparation to underrepresented groups may help narrow the persistent gaps in college access and completion.

Over the past 20 years, early colleges (ECs), a special type of dual enrollment program, have emerged as a promising way to reduce the persistent inequity in college access and success by giving underrepresented students a jumpstart on postsecondary education (Song et al., 2021). Jointly operated by school districts and postsecondary institutions and often located on college campuses, ECs are mostly small whole-school programs or programs within larger schools that are designed to offer students—particularly students traditionally underrepresented in higher education—the opportunity to earn an associate degree or up to 2 years of college credits during high school at no or low cost to their families. To achieve this goal, ECs provide students with combined high school and college experiences, as well as comprehensive academic and social supports to help ease the transition from high school to college.

Recent impact studies of ECs have consistently produced positive findings about the EC impact on participant outcomes (e.g., Edmunds et al., 2017, 2020; Song et al., 2021). Most of the existing research on ECs, however, focused on students' academic outcomes, particularly college enrollment and degree attainment. There has been limited research on EC impact on outcomes after formal schooling.

To fill in this gap, we administered a survey to examine longer term outcomes of participants in 17 admission lotteries conducted by seven ECs. These 17 lotteries were part of an earlier impact study conducted by researchers at the American Institutes for Research (which we refer to as "the original EC impact study" in this report), which leveraged randomized admission lotteries to obtain causal estimates of EC impacts on education outcomes through up to 4 years after expected high school graduation, and an initial follow-up study that assessed EC impacts on student outcomes with 4 more years of data. Administered from fall 2022 through spring 2023, the latest follow-up survey collected data on outcomes after formal schooling for participants in the original EC impact study during the 12th, 13th, or 14th year after expected high school graduation, depending on which year the respondent participated in a 9th-grade EC admission lottery. In addition to overall EC impacts on these outcomes, we examined whether impacts varied by participant characteristics because ECs were designed with a particular focus on serving students traditionally underrepresented in higher education. Extending the original EC impact study and the initial follow-up study, the second follow-up study addressed the following research questions (RQs) using data from the latest follow-up survey:

1. What were the impacts of ECs on workforce, financial, and other life outcomes in the 12th to 14th years after expected high school graduation?
2. Did the impacts of ECs vary by participant characteristics?

In the remainder of this report, we first provide an overview of the EC model and a review of existing research on ECs. We then describe the methods used to address the RQs and present the findings. We conclude the report by acknowledging study limitations and discussing the implications of study findings and directions for future research.

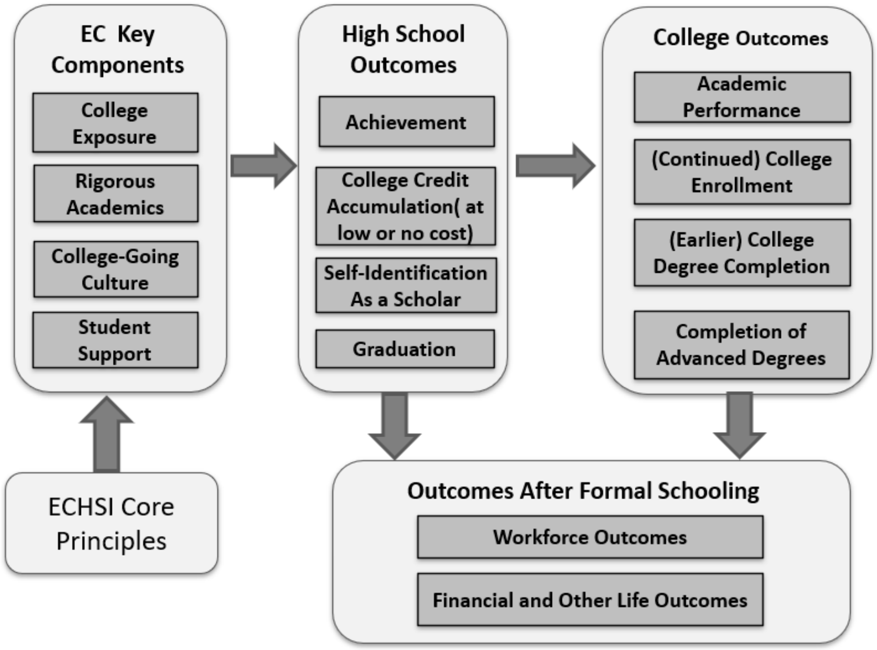
The Early College Model

Although the history of ECs dates back to the 1960s, the proliferation of ECs across the states did not start until the launch of the Early College High School Initiative (ECHSI) in 2002. Spearheaded by the Bill & Melinda Gates Foundation, the initiative aimed to improve the opportunity for students from disadvantaged backgrounds to earn a postsecondary credential and established five core principles that all ECs created under the initiative were expected to follow (Jobs for the Future, 2008):

1. ECs are committed to serving students underrepresented in higher education.
2. ECs are created and sustained by a local education agency, a higher education institution, and the community, all of which are jointly accountable for student success.
3. ECs and their higher education and community partners jointly develop an integrated academic program so that all students earn 1 to 2 years of transferable college credit leading to college completion.
4. ECs engage all students in a comprehensive support system that develops academic and social skills, as well as the behaviors and conditions necessary for college completion.
5. ECs and their higher education and community partners work with intermediaries to create conditions and advocate for supportive policies that advance the EC movement.

Guided by these principles, ECs were designed to provide students, particularly students from backgrounds underrepresented in higher education, with college exposure, rigorous academics, a strong college-going culture, and rich academic and social supports, all at little or no cost to students' families. These key components of EC experiences are expected to promote improved high school outcomes and better prepare students—particularly the target student population—for successful transition to college, which would, in turn, lead to better college outcomes, including an accelerated timeline and higher rates of degree completion. Conceivably, these improved outcomes at high school and college would have a positive influence on student outcomes after formal schooling, such as on workforce, financial, and other life outcomes, as shown by the EC theory of action, illustrated in Figure 1.

Figure 1. ECHSI Theory of Change



Although ECs—both those funded under the ECHSI and those created later—generally follow the same set of core principles, they vary in their structural features. Of the 148 ECs surveyed in a national evaluation of the ECHSI conducted by Berger and colleagues (2009), over two thirds (68%) were newly created small high schools, and the rest were either converted from existing high schools (27%) or created as programs within existing schools (5%). Since then, the number of ECs has been increasing rapidly. Through a landscape scan conducted as part of a larger study on ECs, we identified 1,006 ECs across 36 states as of March 2024. Although a larger share of ECs today operate as programs within schools, the small whole-school model remains an important form of EC, accounting for more than 40% of all ECs.

Research on Early Colleges

As ECs have been expanding nationwide, research on ECs has also been accumulating. Most existing studies in this area are descriptive or qualitative in nature. Results from these studies suggest that ECs are committed to providing personalized learning environments and tailoring learning opportunities to the academic needs of individual students (Thompson & Onganga, 2011; Ari et al., 2017). Such supportive learning environments are particularly beneficial for students who are traditionally underserved in higher education (Beall, 2016). In addition, the extensive supports and services provided by ECs, such as mentoring, career exploration events, and opportunities to participate in on-campus activities, enhance the college-going culture within ECs (Newton, 2008). Findings from descriptive studies also showed that EC students outperformed their peers in other high schools in the same districts in both proficiency rates on state assessments and high school graduation rate (Berger et al., 2009; Webb, 2014), despite the fact that ECs primarily served students from low-income families, first-generation college goers, and students who were traditionally underrepresented in higher education.

More recent EC studies based on quasi-experimental designs produced findings with stronger causal validity. Using propensity score matching, for example, Lauen and colleagues (2017) compared the performance of students at 70 ECs in North Carolina with the performance of a matched group of students who attended the same middle schools but did not attend ECs during high school. They found that EC students demonstrated significantly better outcomes than the matched comparison students in both high school and college, including higher test scores, lower ninth-grade retention rate, fewer absences, higher graduation rate, higher rate of enrollment at 4-year state colleges, and higher rate of associate degree attainment within 2 or 3 years after high school completion. Also focusing on ECs in North Carolina, Swiderski et al. (2021) estimated the impact of ECs using a propensity score weighting method. The study found that ECs had significant positive impacts on youth civic outcomes, resulting in a higher voting rate and lower criminal conviction rate for EC students, compared with their non-EC peers.

To date, the most rigorous causal evidence on the impact of ECs comes from three natural experiments based on admission lotteries. All three studies were rated by the What Works Clearinghouse (WWC) as "meets WWC standards without reservation," providing strong (Tier 1) evidence on the impact of ECs. The first two impact studies focused on ECs created as part of the ECHSI in the 2000s, including the study that is the basis of the follow-up study presented in this report and a study conducted by the SERVE Center at the University of North Carolina at Greensboro (Edmunds et al., 2012, 2013, 2017, 2020). Both studies found positive impacts of ECs on a variety of high school and postsecondary outcomes. Our original EC impact study, for example, found that ECs had positive impacts on high school achievement in English language arts (ELA) and both college enrollment and degree attainment 2 to 4 years after expected high school graduation (Berger et al., 2013, 2014; Haxton et al., 2016). Extending the original EC impact study with 4 more years of data, our initial follow-up study found that EC students continued to enroll in college and complete college degrees at significantly higher rates than control students up to 6 years after expected high school graduation (Song et al., 2021). Through an analysis of the financial costs and benefits of the ECs (Atchison et al., 2021), we found that the average estimate of lifetime benefits of enrolling in an EC was $57,682 per student. By comparison, the average cost per student was approximately $3,820 over 4 years of high school (which would be equivalent to $4,901 in 2024 dollars).

The findings from our original EC impact study and the initial follow-up study are largely consistent with the findings from the study conducted by the SERVE Center. Relying on a sample of 19 ECs in North Carolina, the SERVE study found that, compared with control students, EC students were more likely to be “on track for college” and had higher attendance rates, lower suspension rates, and higher levels of engagement (Edmunds et al., 2012, 2013). That study also demonstrated that ECs had positive impacts on college credit accrual during high school, high school graduation, college enrollment, and degree attainment (Edmunds et al., 2017, 2020).

Also relying on admission lotteries as a mechanism for random assignment, the third and most recent EC impact study assessed the early impacts of the Pathways in Technology Early College High School (P-TECH) Grades 9 to 14 model—a specific EC model with a strong career focus—on student outcomes during the first 3 years of high school (Rosen et al., 2020). Relying on data for students who applied for admission to seven P-TECH schools in New York City, this study found that the P-TECH model had positive impacts on the total number of college credits earned during high school, passing the ELA Regents exam, and attendance.

Overall, findings from existing research on ECs have been quite encouraging, which has prompted growing interest in expanding ECs across the country (Barshay, 2020; The White House, 2023; Warner, 2022; Webb & Mayka, 2013). However, the evidence base for ECs is still limited. Most of the existing impact studies on ECs, including all three studies based on natural experiments, focused on students' educational outcomes, and few prior studies examined EC impact beyond 6 years after high school graduation. One notable exception is the Swiderski et al. (2021) study, which assessed the impacts of ECs on youth civic outcomes, including voting and criminal convictions. However, to date, no peer-reviewed studies have been published that assess the impacts of ECs on workforce, financial, or other life outcomes. By focusing on these outcomes after formal schooling, the study in this report represents a significant addition to the existing evidence base on the EC impacts on longer term student outcomes.

Methods

As we explained earlier, this study is the second follow-up study built on a previous EC impact study, which was a multisite student-level randomized controlled trial with randomization based on admission lotteries. For the original study and this follow-up study, we defined “EC students” or “treatment students” as lottery applicants who were offered enrollment in an EC and “control students” as lottery applicants who were not offered enrollment, regardless of whether they actually enrolled in an EC. Below, we describe the sample, measures, and data sources, and the analytic approach used to address the two RQs concerning the EC impacts on workforce, financial, and other life outcomes 12 to 14 years after expected high school graduation.

Sample

The original EC impact study was a multisite natural experiment with student-level random assignment based on retrospective admission lotteries. ECs eligible for the study had to meet the following criteria: (a) enrolled students in Grades 9–12, (b) had high school graduates by 2011; (c) used lotteries in its admission processes for at least one of three incoming student cohorts (i.e., students who entered ninth grade in 2005–06, 2006–07, or 2007–08); (d) retained the lottery records; and (e) implemented the ECHSI as a whole-school program. The original study sample was restricted to ECs that were open by fall 2007 so that students in the study would have the opportunity to complete at least 2 years of college by the end of the study, in 2013.

Of the 154 distinct ECs open nationwide in fall 2007, 10 ECs across five states (North Carolina, Ohio, South Carolina, Texas, and Utah) met the criteria for inclusion in the original impact study. Of those 10 schools, six had lotteries in multiple years and three conducted multiple lotteries (i.e., “sublotteries”) within a single year,[1] leading to a total of 23 admissions lotteries (including sublotteries) across all 10 ECs and three student cohorts.

Of the 10 ECs included in the original impact study, three schools from North Carolina were excluded from the 2022–23 follow-up survey because the student contact information needed for the survey was not available. As a result, the sample for our follow-up survey (hereafter “the survey sample”) consisted of 870 students who were granted admission to ECs and 1,232 students who were not (for a total of 2,102 students) across 17 admission lotteries conducted by seven ECs for three cohorts of participants. By the time of the follow-up survey, the three

[1] The sublotteries were generally separate lotteries an EC conducted for applicants from different feeder schools or districts in a given year.

cohorts of lottery participants included in the survey sample were in their 12th, 13th, or 14th year after expected high school graduation, as shown in Table 1.

Table 1. Students' Expected Educational Progression From Ninth Grade to the 2022–23 Follow-up Survey, by Cohort

School year	Cohort 1	Cohort 2	Cohort 3
2005–06	9th grade		
2006–07	10th grade	9th grade	
2007–08	11th grade	10th grade	9th grade
2008–09	12th grade	11th grade	10th grade
2009–10	First year after HS	12th grade	11th grade
2010–11	Second year after HS	First year after HS	12th grade
2011–12	Third year after HS	Second year after HS	First year after HS
2012–13	Fourth year after HS	Third year after HS	Second year after HS
↓	↓	↓	↓
2021–22	13th year after HS	12th year after HS	11th year after HS
2022–23	**14th year after HS**	**13th year after HS**	**12th year after HS**

Note. HS = expected high school graduation. This table depicts the expected educational progression for each student cohort, assuming it generally takes 4 years to complete high school. Individual students may take more or less time to complete high school.

Although the existence of admissions lotteries at schools in the survey sample allowed us to obtain causal estimates of the long-term effects of ECs, those schools were not selected at random from the population of ECs open by fall 2007 (from which the original impact study sample was selected) and might not be representative of all ECs currently in operation. To help inform the generalizability of findings from this study, we compared the characteristics of the ECs in our survey sample with the characteristics of the population of ECs in 2007–08 and the population of ECs operating in the 2022–23 school year (as identified through our recent landscape scan). Compared with the current population of ECs, the ECs in our survey sample on average were much smaller, more likely to be in urban areas, and had a higher percentage of White students and somewhat lower percentages of Black and Hispanic students (see Table 2). The average enrollment size of the ECs in our survey sample was much smaller than that in the current population because the proportion of EC programs that operate as within-school programs (as opposed to the whole-school programs) has increased substantially over time.

Compared with the population of ECs in 2007–08, ECs in the survey sample had similar urbanicity but a lower percentage of low-income students and a higher percentage of White students. Depending on the extent to which school size, urbanicity, and the equity focus of EC programs may be associated with program effectiveness, these differences should be considered when interpreting the survey-based findings presented in this report.

Table 2. Average Characteristics of ECs in the Survey Sample, the Population of ECs in 2007–08, and the Current Population of ECs

School characteristic	ECs in survey sample (n = 7)	Population of ECs in 2007–08 (n = 154)	2022–23 population of ECs in CCD (n = 1,006)
Urbanicity			
Urban	50.0%	50.0%	35.9%
Suburban	20.0%	18.2%	27.1%
Town/Rural	30.0%	31.8%	37.0%
Student race/ethnicity			
Percent Asian/Pacific Islander	5.2 (3.6)	4.0	4.7 (1.5)
Percent Black	10.0 (3.8)	25.0	17.1 (7)
Percent Hispanic	32.0 (14.7)	30.0	36.0 (23.2)
Percent White	48.3 (58.0)	34.0	35.6 (26.6)
Percent low-income students	47.6 (56.0)	59%	NA
Average enrollment	430.0 (433)	211.0	816.0 (464)

Note: CCD = Common Core of Data. Data on the characteristics of the early colleges in the survey sample came from records collected in the original impact study and the 2007–08 CCD. Data on the characteristics of the population of early colleges in 2007–08 came from Berger et al. (2009). Data on the characteristics of the current population of ECs, which excludes P-TECH schools and college-based early college programs, came from the 2022–23 CCD. The percentages of students in different racial/ethnic categories and the percentage of low-income students are the average values of school-level percentages across schools. Medians are presented in parentheses where available.

Data and Measures

To collect information on workforce, financial, and other life outcomes for EC lottery participants, we surveyed individuals who originally participated in the 17 admissions lotteries. To locate these individuals for survey administration, we relied on contact information (i.e., addresses, phone numbers, and/or email addresses) for lottery participants and parents/guardians gathered from participants' original ninth-grade EC lottery applications, as well as any contact information from our initial follow-up study. In summer 2022, we provided this database of contact information (hereafter the "contact file") to the National Opinion Research Center (NORC), at the University of Chicago, which in turn conducted a contact validation process for each case.

Because the sample for this follow-up study had not been contacted in at least 10 years, NORC conducted extensive locating efforts to find the most up-to-date sample member contact information. These efforts included utilization of LexisNexis® Batch Services, a widely accepted locate-and-research tool available to government, law enforcement, and commercial customers, as well as TransUnion batch services, a similar service that provides addresses, telephone numbers, and email information; deceased indicators; and name updates. For participants still not located by these services, NORC-trained locators manually searched for the remaining, non-located participants by calling their phone numbers in the contact database, conducting internet searches for their contact information, and searching for them in the LexisNexis AML Insight™ pay-per-search web tool.

By the conclusion of the extensive location process, NORC located 1,867 (88.8%) of the 2,102 people who participated in the 17 EC lotteries included in this study. Of those located, about 1.6% were deemed to be deceased, incarcerated, terminally ill, or otherwise institutionalized. Although these people were not contacted for survey participation, they were still included when weighting our results to reflect the entire study sample.

After locating sample members, NORC first sent the link to the electronic follow-up survey[2] to those whom they were able to locate in September 2022, and then mailed a paper-and-pencil survey[3] to nonrespondents in November 2022. A $50 electronic gift card incentive was provided to each respondent for a completed survey. Those from the survey sample who did not respond to or complete the entire survey were sent multiple reminders to complete the survey through email, phone, or text messages. After approximately 5 months of survey administration, final reminders were mailed to non-responders with an additional $5 pre-incentive included in the mailing envelope. After 6 months, the survey was closed with 986 respondents in total (482 EC and 504 control) and an overall response rate of 47% (55% for EC and 41% for control).

[2] https://www.air.org/sites/default/files/2023-03/Early-College-ECG-FollowUp-Survey-Program-508-March-2023.pdf
[3] https://www.air.org/sites/default/files/2023-03/Early-College-PAPI-High-School-Experience-Survey-508-March-2023.pdf

The follow-up survey instrument included questions about participants' postsecondary attendance, completion, and their academic experience while in college. Additional questions covered labor market outcomes, such as employment and earnings, as well as other financial and life outcomes, such as family structure, housing, and usage of public assistance programs. Using the survey data, we constructed a number of measures of workforce, financial, and other life outcomes for EC lottery participants. These measures were the focus of the analyses presented in this report and are described in Table 3.

Table 3. Description of Survey Measures

Measure	Description
Workforce outcomes	
Employed	A binary variable equal to 1 for respondents who indicated that they were employed (i.e., doing any paid work or owning a business) at the time of the survey and 0 otherwise
Employed full time	A binary variable equal to 1 for respondents who indicated that they worked 40 hours per week or more in a typical week over the past month and 0 otherwise
Use of unemployment insurance benefits	A binary variable equal to 1 for respondents who indicated that they used unemployment insurance in the 3 years prior to the survey and 0 otherwise
Job alignment with goal	A binary variable equal to 1 for respondents who indicated that their current (or most recent) job "aligns/aligned well" or "partially aligns/aligned" with their long-term career goals and equal to 0 for respondents who indicated that their current (or most recent) job "is/was not related to [their] long-term career goals" or that they "have/had not established long-term career goals yet"
Financial outcomes	
Current annual employment earnings	A continuous measure of the annual earnings from employment based on respondent's reported weekly, monthly, or yearly earnings or salary at their current job(s), including any tips
2021 employment earnings	A continuous measure of the total annual income that a respondent earned from all jobs and business ventures in 2021 (before deductions for taxes, bonds, dues, or other items)

Measure	Description
Student loan debt ($0, greater than $40k, greater than $80k)	Three binary measures of the total student loan debt a respondent owed at the time of the survey: (a) a measure equal to 1 if the respondent had $0 of student debt and 0 otherwise, (b) a measure equal to 1 if the respondent had at least $40,000 of student debt and 0 otherwise, and (c) a measure equal to 1 if the respondent had at least $80,000 of student debt and 0 otherwise
Never worried about money	A binary variable equal to 1 for respondents who indicated that they never worried about having enough money to cover their regular expenses (e.g., food, transportation, and housing) in the past 12 months and 0 otherwise
Worried about money weekly or daily	A binary variable equal to 1 for respondents who indicated that they worried about having enough money to cover their regular expenses (e.g., food, transportation, and housing) once a week or more in the past 12 months and 0 otherwise
Ever used SNAP (in past 3 years)	A binary variable equal to 1 for respondents who indicated that they used SNAP (food stamps or food assistance) or WIC (Special Supplemental Nutrition Program for Women, Infants, and Children) benefits in the 3 years prior to the survey and 0 otherwise.
Other life outcomes	
Home ownership	A binary variable equal to 1 for respondents who indicated that they owned the house or apartment/condominium in which they were living at the time of the survey and 0 otherwise
Has (employer-provided) retirement account	A binary variable equal to 1 for respondents who indicated that they had a retirement plan provided by their employer or their spouse's employer at the time of the survey and 0 otherwise
Has (employer-provided) health insurance	A binary variable equal to 1 for respondents who indicated that they had health insurance provided by their employer or their spouse's employer at the time of the survey and 0 otherwise

Notes. SNAP = Supplemental Nutrition Assistance Program; WIC = Women, Infants, and Children.

In addition to survey-based outcome data collected in this follow-up study, we obtained covariate data on the background characteristics of lottery participants from state and district administrative records during our original impact study. Various participant characteristics were documented when lottery participants initially applied to their respective EC lottery in the eighth grade. These characteristics included gender, race/ethnicity, low-income status, and eighth-grade mathematics and ELA achievement scores (standardized based on state means and standard deviations). Because ECs were designed with an explicit goal of improving access to postsecondary education for students from racial and ethnic backgrounds traditionally underrepresented in higher education, we created a binary indicator for underrepresented minority (URM) status, coded 1 for students who did not identify as either White or Asian/Pacific Islander and 0 otherwise. Some participants had incomplete data on background characteristics, and data on background characteristics were unavailable for all students from one EC. In these cases, we used multiple imputation to impute missing data, as described in the "Handling of Missing Baseline Data" section.

Analytic Methods

Adjusting for Survey Nonresponse

A substantial proportion of the survey sample did not respond to the survey, and the survey respondents might differ systematically from nonrespondents, which could potentially lead to nonresponse bias. To minimize nonresponse bias, we constructed nonresponse-adjusted survey weights so that the weighted distribution of observable characteristics of the survey respondents more closely matched that of the full survey sample, which would ensure that the weighted EC impact estimates based on the survey respondents more closely matched the true impacts for the full survey sample. A detailed description of the methods we used to create nonresponse-adjusted weights and assess the performance of the weights can be found in Appendix A.

Approximately half of the follow-up survey sample was female, more than half of the sample identified with an underrepresented minority group, and nearly half came from low-income families, with small differences between treatment and control groups. After applying nonresponse weights to the sample of study participants who responded to the survey, the analysis sample both resembled the full follow-up survey sample and demonstrated baseline equivalence between students in the treatment and control groups with effect sizes ranging from -0.012 to 0.156 across the characteristics we examined (see Appendix Table A1).

Intent-to-Treat Analyses

Our main impact analyses addressing RQ1 are intent-to-treat (ITT) analyses, which estimate the impact of being offered admission to an EC through a lottery, regardless of whether the lottery participant actually enrolled in the EC. To estimate the overall ITT effects across lotteries, we constructed two-level models with individuals nested within lotteries. The treatment indicator was group-mean centered at the individual level to ensure that the comparisons of individuals in the treatment (EC) group and individuals in the control group were made *within* rather than *across* lotteries, thus producing unbiased impact estimates (Enders & Tofighi, 2007; Raudenbush, 1989). Consistent with the way we estimated ITT effects in our original EC impact study and the initial follow-up study, we modeled the intercept as a random effect to account for the clustering of individuals within lotteries. We modeled the treatment effect as fixed at the lottery level because the number of lotteries in the survey sample was too small to generate stable estimates of the variation in treatment effects across lotteries. A more detailed description of the ITT model can be found in Appendix A.

To address RQ2, we conducted differential impact analyses to examine whether the EC impacts on the survey-based outcomes differed for individuals with different background characteristics. Specifically, we explored whether the EC impacts differed significantly by individuals' underrepresented minority status, low-income status, or Grade 8 ELA or mathematics achievement by adding an interaction between treatment status and a potential moderator to the ITT impact model. In two of the 17 lotteries in the survey sample, all lottery participants identified as racial minorities; therefore, these two lotteries were excluded from the analysis of differential impact by underrepresented minority status. In addition, one lottery was excluded from the analysis of differential impact by low-income status because no individuals in this lottery were from low-income families.

Complier Average Causal Effect (CACE) Analyses

Although all lottery participants applied for admission to an EC, some lottery winners chose not to attend the ECs that offered them admission (i.e., no-shows). In addition, some participants who were not granted admission to an EC through the lottery were able to attend the same or a different EC through other means (i.e., crossovers). The presence of these noncompliers (i.e., no-shows and crossovers) in the ITT analyses meant that the ITT estimates represented the effects of being offered admission to an EC, rather than the effects of attending an EC, for individuals who complied with their lottery-based treatment assignment (i.e., compliers). Therefore, we supplemented the ITT analyses with Complier Average Causal Effect (CACE) analyses to estimate the effects of actually attending an EC for compliers.

For the CACE analyses, we identified no-shows and crossovers in the survey sample using district administrative records that indicated whether each lottery participant attended an EC in their first year of high school. We identified no-shows in 12 of the 17 lotteries included in the survey sample, with an overall no-show rate of 18.3% among winners of the 17 lotteries.[4] Crossovers were less common, occurring in only three lotteries in the survey sample, with an overall crossover rate of 1.9% among participants of the 17 lotteries who did not win.[5]

To estimate the effects of EC attendance on compliers, we used a two-stage instrumental variable approach, an approach shown to be well suited for use with randomized controlled trials (Angrist et al., 1996; Gennetian et al., 2005; Schochet & Chiang, 2009). Because admission to an EC via the admission lotteries was both positively correlated with EC attendance and unrelated to participants' background characteristics, we used lottery-based admission to an EC as the instrument for EC attendance in our CACE analyses. Because of these two characteristics of the instrument, we have reason to believe that our instrument satisfies both the inclusion restriction (i.e., the instrument affects treatment receipt) and the exclusion restriction (i.e., the instrument only affects participant outcomes through its effect on treatment receipt), which are necessary for producing unbiased CACE estimates. Further, since it is unlikely that lottery participants would act in direct opposition to the lottery results, the assumption of monotonicity (i.e., no defiers) underlying valid CACE estimates should also hold for our analyses.

We implemented the CACE analyses using a two-stage approach. In the first stage, we used a two-level logit model similar to the ITT model to estimate the probability of attending an EC during the first year of high school for each participant. In the second stage, we estimated the two-level model on our survey outcomes, using the predicted probability of EC attendance from the first stage as the primary independent variable. The estimate of the coefficient on the predicted probability of EC attendance from the second stage represents our estimate of the effect of EC attendance on compliers in the admission lotteries. Our CACE analyses did not take into account the uncertainty in the probability of EC attendance predicted at the first stage when estimating the second-stage model; thus, the standard errors of the CACE estimates may be somewhat underestimated.[6]

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[4] The EC with the highest no-show rate (about 50% for all three cohorts) was in a district that ran districtwide lotteries for all schools, and each student could participate in multiple school-specific lotteries. Some winners of the EC lottery in that district might also have won the lottery of a non-EC school that they preferred to attend.

[5] The lottery with the highest crossover rate (28.4%) was held by an EC located near another EC that was not part of the survey sample. We classified control students who attended this other EC as crossovers because their high school experiences were similar to those of the treatment students.

[6] As sensitivity analyses, we also conducted CACE analyses using a two-stage least-squares regression, which did take into account the uncertainty in the probability of EC attendance predicted at the first stage when estimating the second-stage model. However, two-stage least-squares uses a single-level linear probability model at both stages for binary outcomes.

Handling of Missing Baseline Data

While baseline information on participant background characteristics was available for most of the survey sample, not all study participants had complete data on all covariates. To address the potential selection bias due to missing data, we imputed missing covariate data using multiple imputation by chained equations (Raghunathan et al., 2001). The multiple imputation model included all outcome measures and covariates used in addressing the RQs, as well as lottery fixed effects, and the imputation was performed for the EC and control groups separately, using the sample of survey respondents. We generated 10 imputed datasets and conducted all analyses using each imputed dataset separately. We then combined estimates across the 10 datasets following standard multiple imputation combination rules, which take into account the uncertainty in imputed values both within and across the imputed datasets (Little & Rubin, 2002). Because imputed participant characteristics can vary across the imputed datasets, a distinct set of survey nonresponse weights was calculated for individuals in each of the 10 imputed datasets and used in the analysis of each imputed dataset. We conducted each impact analysis with imputed covariate data for survey respondents, excluding lottery participants with missing data on the outcome measure.

Although we were able to account for the clustering of students within lotteries with lottery fixed effects in a single-level model, the use of a linear probability model for binary outcomes is not entirely consistent with the logit model used for our ITT analyses. We therefore present estimates from the two-level logit model as our main CACE results for consistency and comparability across tables.

Results

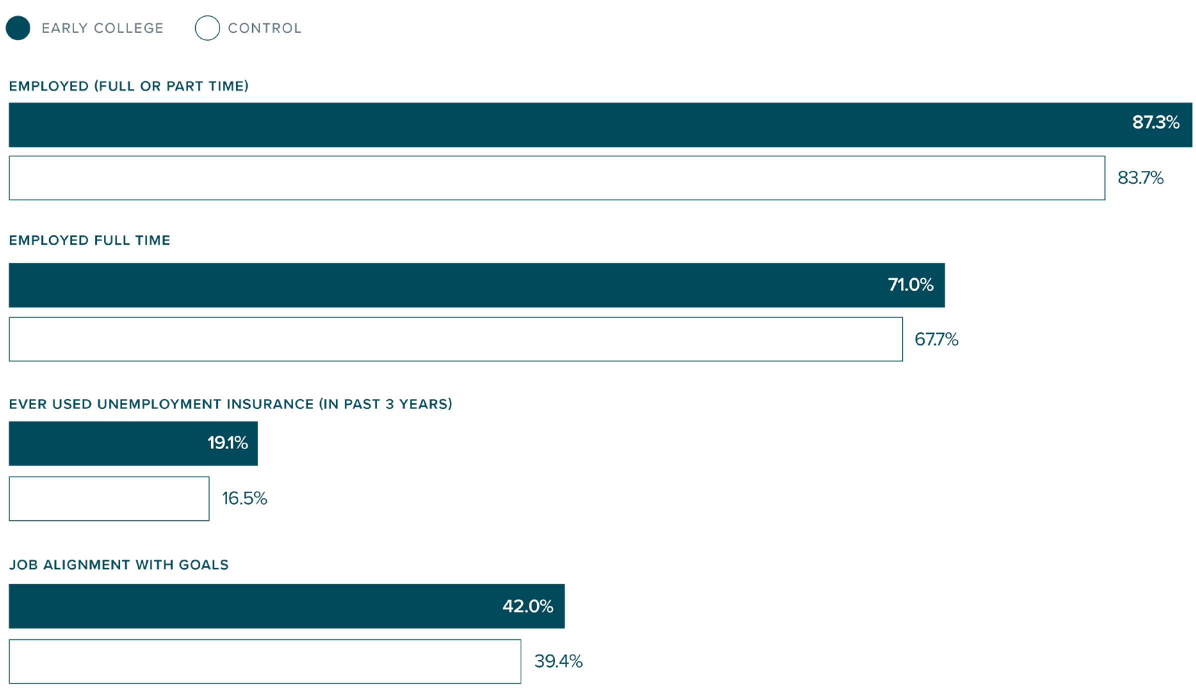
This section presents the results from this study, organized by research question. Tables that provide detailed study findings, including standard errors and effect sizes, can be found in Appendix B.

Impacts of ECs on Workforce, Financial, and Other Life Outcomes (RQ 1)

ITT Effects

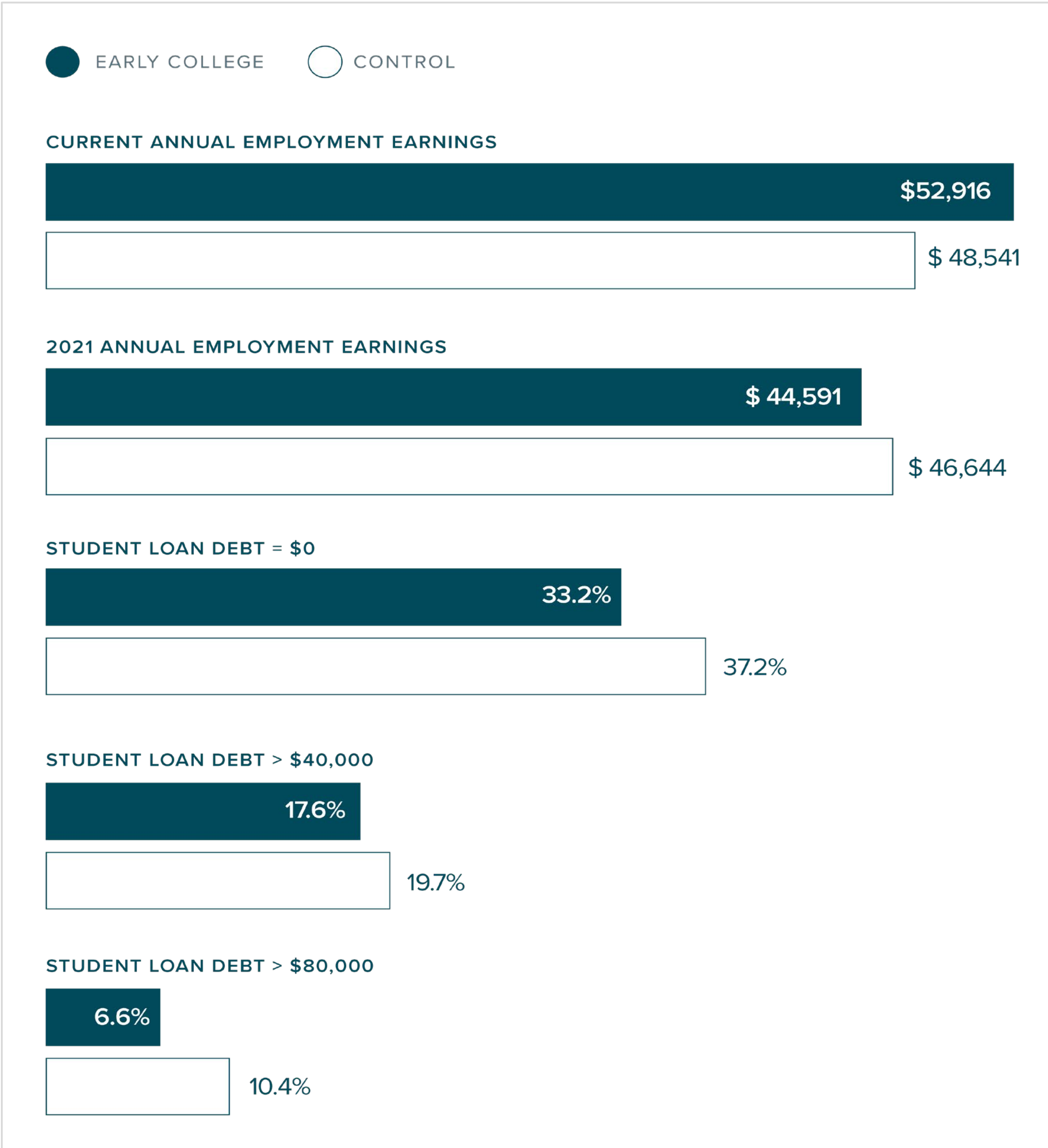
Figure 2 to Figure 4 present the estimated effects of EC admission on a variety of survey outcomes, based on ITT analyses with nonresponse-adjusted survey weights. These results suggest that being admitted to an EC had no significant effect on any of the workforce, financial, and other life outcomes measured with survey data 12 to 14 years after expected high school graduation, when most respondents were in their early 30s.

Figure 2. Estimated Effects of EC Admission on Workforce Outcomes 12 to 14 Years After Expected High School Graduation



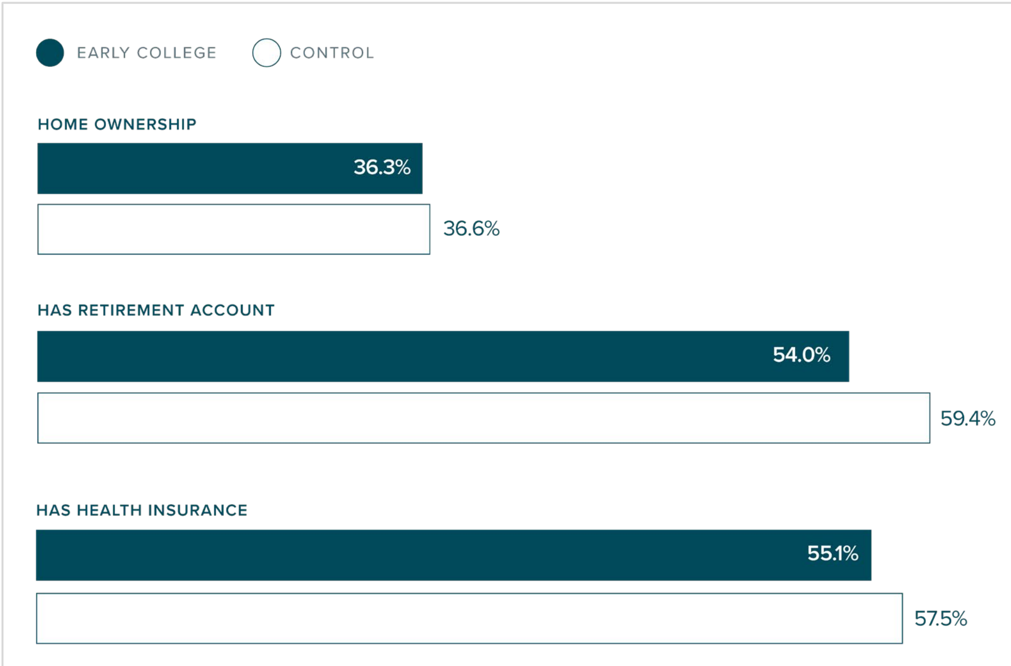
Note: The number of respondents for each outcome, ranging from 971 to 985, varied based on the number of survey participants who responded to the relevant survey question. Detailed findings can be found in Appendix Table B1. The EC group means are unadjusted means; the control group means were computed based on the unadjusted EC group means and the estimated EC effects.

Figure 3. Estimated Effects of EC Admission on Financial Outcomes 12 to 14 Years After Expected High School Graduation



Note: The number of respondents for each outcome, ranging from 978 to 986, varied based on the number of survey participants who responded to the relevant survey question. Detailed findings can be found in Appendix Table B1. The EC group means are unadjusted means; the control group means were computed based on the unadjusted EC group means and the estimated EC effects.

Figure 4. Estimated Effects of EC Admission on Other Life Outcomes 12 to 14 Years After Expected High School Graduation



Note: The number of respondents for each outcome, ranging from 978 to 980, varied based on the number of survey participants who responded to the relevant survey question. Detailed findings can be found in Appendix Table B1. The EC group means are unadjusted means; the control group means were computed based on the unadjusted EC group means and the estimated EC effects.

Complier Average Causal Effects

Although the ITT results reported above represented the effects of offering EC admission to winners of the admission lotteries, not all lottery winners actually attended the ECs to which they applied. We therefore supplemented the ITT analyses with CACE analyses, which estimated the effects of EC attendance on participants who complied with their lottery-based treatment assignment. Results from the CACE analyses are presented in Appendix Table B2. As we expected, the CACE estimates indicated that the effects of attending ECs were generally more favorable than the effects of being offered admission to ECs. For example, the CACE estimate for the outcome of job alignment with career goals was a 5.4-percentage-point difference favoring the EC group, compared with an ITT estimate of a 2.6-percentage-point difference favoring the EC group. However, as was the case with the ITT results presented in Figure 2 to Figure 4, there were no significant effects of EC attendance on any of the workforce, financial, and other life outcomes measured in our follow-up survey. We note that the CACE estimate for the effect of EC attendance on employment status, which has a *p*-value of 0.032, was no longer significant after applying the Benjamini-Hochberg correction for multiple comparisons (Benjamini & Hochberg, 1995).

Differential Impacts of ECs (RQ 2)

Because the ECs were designed with a particular focus on serving students traditionally underrepresented in higher education, it would be informative to understand whether ECs were particularly beneficial for individuals from traditionally underrepresented backgrounds. To address this issue, we conducted differential impact analyses to explore whether the EC impacts varied significantly based on the following characteristics: URM status, low-income status, and eighth-grade achievement in ELA and math. Overall, we found that the EC impacts on workforce, financial, and other life outcomes measured 12 to 14 years after expected high school graduation did not differ significantly by individuals' URM status or low-income status (see Appendix Table B3). While the estimated differences in impact were substantial in magnitude for some outcomes (e.g., a 14-percentage-point difference in impact on using SNAP benefits by URM status), none of those different impact estimates were statistically significant. It is possible that the lack of significant differential impacts may be due to insufficient statistical power in some cases. In particular, analyses of differential impact by URM status were based on a reduced sample size because two lotteries with a total of 181 URM respondents and zero non-URM respondent had to be excluded from these analyses.[7] In addition, one lottery with 29 respondents was removed from the analyses of differential impact by low-income status because no individuals within this lottery were from low-income families; all applicants from low-income families were offered admission to this EC without participating in the lottery and were thus not part of the study sample.

In our initial follow-up study based on the sample of our original EC impact study, we found that the EC impact on associate degree attainment was significantly stronger for individuals with higher prior achievement (as measured by performance on eighth-grade standardized ELA and math tests; Song et al., 2021). That finding led us to expect that ECs might also have stronger impacts on workforce, financial, and other life outcomes for individuals with higher prior achievement. However, as shown by the differential impact analysis results presented in Appendix Table B4, the EC impacts on workforce, financial, and other life outcomes measured 12 to 14 years after expected high school graduation did not vary significantly for individuals with different levels of prior achievement. While many of the differential impact estimates point to larger impacts for individuals with lower prior achievement, none of the estimates were statistically significant.

[7] This is not reflective of differential attrition on the basis of URM status in these two lotteries. All respondents in these lotteries were URM because all participants in these lotteries were URM.

Discussion

This paper presents the results regarding the impacts of ECs on outcomes after formal schooling—as well as differential impacts by participant characteristics—based on survey responses from participants in seven ECs included in our original EC impact study. Although prior literature has found statistically significant positive impacts of ECs on secondary and postsecondary educational outcomes (e.g., Edmunds et al., 2017, 2020; Song et al., 2021), results from this study did not demonstrate a significant impact of being admitted to an EC on any of the workforce, financial, or other life outcomes that we analyzed 12 to 14 years after participants' expected high school graduation. Nor did results from this study reveal any significant differences in the EC impacts on these outcomes for participants with different background characteristics, such as race/ethnicity, low-income status, or prior achievement.

It is possible that, although EC students and control students did not differ significantly in the survey-based outcomes in the 12th to 14th year after expected high school graduation, significant differences between the two groups may have been present in earlier years prior to the survey. Our previous analyses of degree attainment outcomes for participants in the original EC impact study found that EC students earned bachelor's degrees at significantly higher rates than control students during the first 5 years after expected high school graduation, but group differences were no longer significant in subsequent years. Those findings suggest that control students eventually "caught up" to EC students in terms of bachelor's degree attainment (Neering et al., forthcoming). A similar dynamic might have been present for the outcomes examined in this paper. It is possible that EC students entered the workforce earlier and earned more money than their control group peers during the first few years after formal schooling but that the differences between these two groups narrowed over time and were no longer significant by the time of our follow-up survey. However, because of concerns about potential recall bias in reporting annual earnings more than 10 years ago, our survey did not request information on earnings in each prior year. Moreover, asking about earnings during the pandemic could have resulted in attenuated group difference in the earnings reported if the pandemic disproportionately affected EC students or dramatically reduced earnings for both groups. As the follow-up survey only collected data on participant outcomes in the 12th to 14th year after expected high school graduation, we cannot say whether EC students and control students differed in workforce, financial, and other life outcomes at earlier points in time.

Whether ECs have a significant impact on lifetime earnings remains an open question. If ECs do not have an impact on annual earnings in the 12th to 14th year after high school graduation but instead have a cumulative impact on earnings in the years after high school graduation, then we might expect an influence on wealth over time. However, it would be rather challenging to measure wealth based on survey data, as questions about total savings, present value of assets (such as houses or businesses), and all types of debt (including mortgages, credit card debt, and student debt) would be both sensitive and burdensome for the survey respondents and thus lead to low response rates. Therefore, our follow-up survey did not include such questions. Moreover, the single-point-in-time nature of our follow-up survey does not allow for a complete understanding about the trajectory of earnings for respondents. As a result, our survey-based measures of current annual earnings, current student debt, and reported reliance on unemployment insurance are, at best, proxies for wealth and lifetime earnings. Future studies in this area may consider measuring lifetime earnings by obtaining administrative IRS or unemployment insurance data, if feasible, to track employment and earnings longitudinally. Admittedly, such data would not be sufficient to accurately measure wealth (because of missing information on spending and investments), but they could paint a clearer picture of the impact of ECs on cumulative earnings.

Although limitations of the survey data used in our analyses may make it difficult to observe significant EC impacts on workforce, financial, and other life outcomes, results of this study bring into question whether the key components of ECs would directly influence these outcomes. The ECs included in this study were designed to close historical gaps in college enrollment and degree attainment, but they did not have a particular career focus. Therefore, a higher degree attainment rate among EC students relative to control students may not have led to a higher average income if EC students were more likely to pursue degrees in fields with lower expected earnings than control students. This speculation is consistent with findings from recent research on returns to college majors, which revealed substantial variations in returns to both 4-year and 2-year college degrees by field of study (e.g., Andrews et al., 2022; Belfield et al., 2017).

As part of the larger study, we explored whether college major may have played a role in the way ECs affected participants' workforce and financial outcomes. We conducted post hoc analyses of the college majors of students in our survey sample using the National Student Clearinghouse data collected for addressing other questions.[8] Although information on students' majors was available for only 15% of those who enrolled in college but did not complete a degree, 83% of degree completers in our survey sample had information on their majors. To examine the potential relationship between EC attendance and major, we grouped college majors into two broad categories—majors in science, technology, engineering, and math (STEM) fields and majors in non-STEM fields—using the Classification of Instructional Programs (CIP) codes in the National Student Clearinghouse data.[9] Separate analyses by type of terminal degree suggest that EC students were somewhat (but not significantly) less likely to major in STEM—27% of EC students compared with 33% of control students majoring in STEM among bachelor's degree completers and 8% of EC students compared with 19% of control students among associate degree completers without a bachelor's degree. Detailed results from these analyses and results from analyses of the baseline equivalence of EC and control students included in these analyses are presented in Appendix Tables B5 and B6, respectively.

Because the above findings about college major are based on subsets of the full survey sample defined by measures (degree completion) affected by treatment status, they are exploratory in nature and may not have strong internal validity. Nevertheless, they may help shed light on the null results from this study for employment and earning outcomes. If EC students were less likely to major in high-earning fields (such as those in STEM) than control students, their post-college earnings might not necessarily be higher than those for control students despite the higher degree completion rate for EC students.

[8] We included the entire follow-up survey sample (2,102 people) in these analyses instead of the sample of survey respondents (986 people) to bolster analytic sample size and the generalizability of findings.

[9] STEM majors were identified as those that had a CIP code in any of the following fields (based on the first two digits of the code): engineering (14), engineering technologies/technicians (15), biological and biomedical sciences (26), mathematics and statistics (27), physical sciences (40), science technologies/technicians (41), health professions and related clinical sciences (51). The full list of CIP codes is available from the National Center for Education Statistics (2010).

More recently developed school models that involve college-level coursework, including Pathways in Technology Early College High Schools (P-TECH) and Innovation Career Pathways schools in Massachusetts, were designed to prepare students for local high-demand, high-wage careers. These programs often do not provide students with the opportunity to earn up to 60 college credits during high school, but they offer work-based learning opportunities to directly connect students with local employers, which may more directly affect students' workforce and financial outcomes. However, because these models were developed more recently, the longer-term impacts of these models have not yet been evaluated.

Finally, we acknowledge that this study relied on a purposeful sample of ECs that met certain study eligibility criteria. Therefore, findings presented in this paper may have limited external validity and only apply to the ECs similar to those included in the follow-up survey sample. In particular, the survey sample only included students who participated in lotteries at the 7 ECs between 2005–06 and 2007–08. Since then, many aspects of ECs themselves (e.g., school leadership and staffing, partnership with postsecondary institutions, and the student population served), as well as the educational context at the local, state, and national levels, may have changed. To strengthen the evidence base on the long-term impacts of ECs, more studies are needed, particularly studies that (a) include a sample of ECs that is more representative of the modern-day population of ECs, (b) measure participant outcomes with more objective extant data that are not self-reported, (c) examine more recent cohorts of students, and/or (d) collect data from each year after expected high school graduation to allow the examination of participant outcome trajectories over time.

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Appendix A. Additional Information About Analytic Methods

Calculating Nonresponse-Adjusted Survey Weights

To create the nonresponse-adjusted weights, we first created a base weight for each survey respondent and then rescaled the base weights to account for the nesting of respondents within lotteries. These two steps are described below.

Base Weights. Our approach to survey weighting relies on a propensity score weighting method (Mang et al., 2021). To create the survey weights, we first estimated the propensity to respond to the survey for each individual in the survey sample using the following individual-level logistic regression model with lottery fixed effects:[10]

$$\log\left[\frac{Responded_i}{1 - Responded_i}\right] = \beta_0 + \beta_1 EC_i + \boldsymbol{X_i}\theta + \gamma_i$$

where

- $Responded_i$ is a binary indicator of survey response, coded 1 if individual *i* responded to the survey and 0 otherwise;
- EC_i is a dummy indicator for treatment status, coded 1 if individual *i* won an EC admission lottery and 0 otherwise;
- $\boldsymbol{X_i}$ is a vector of individual characteristics (gender, URM status, low-income status at the time of admission lottery, and eighth-grade test scores in ELA and math); and
- γ_i is a vector of lottery fixed effects.

[10] As a robustness check, we also estimated the response propensities using a two-level model with individuals nested within lotteries. The response propensities estimated using both approaches were highly correlated, and the resulting EC impact estimates on survey outcomes were very similar.

After fitting this model on the survey sample, we calculate the fitted value, $\widehat{RespProb_i}$, which represents the probability of responding to the survey for each individual in the survey sample given the observable characteristics of the individual and the EC lottery they were in. The inverse of the fitted value for each individual provides the base survey weight, so that

$$Weight_i = \frac{1}{\widehat{RespProb_i}}$$

Scaled Weights for Impact Analyses. The base weights described above would have been sufficient to account for nonresponse bias had the survey respondents been independent observations. However, our survey data were of a clustered structure, in which individuals were clustered within EC admission lotteries. For such multilevel data, proper weights need to be applied to each level of the data to account for nonresponse across and within clusters, as noted by Mang et al. (2021). For our analyses of survey data using a two-level impact model, as described in the intent-to-treat (ITT) analyses section, this requires creating lottery-specific weights that were both applied to the lotteries included in the impact analyses and used to rescale the weights for respondents within each lottery. Because we received survey responses from at least some individuals in every lottery in the survey sample, all lotteries in the survey sample were represented in our impact analyses with a weight of 1 for each lottery. As a result, the rescaling of individual base weights within lotteries was simplified.

Specifically, to estimate the two-level impact model, we rescaled the individual base weights as follows, to account for the relative size of each lottery, so that the sum of the rescaled individual weights within a given lottery equals the total number of respondents in that lottery:

$$ScaledWeight_{ik} = Weight_{ik} \frac{n_k}{\sum_{i=1}^{n_k} Weight_{ik}},$$

where $ScaledWeight_{ik}$ is the rescaled weight for individual i in lottery k and n_k is the total number of respondents in lottery k.

To examine the performance of the nonresponse weights, we compared the average characteristics for the EC and control groups in the full follow-up survey sample and those of survey respondents before and after weighting. The first four columns of Table A1 indicate that individuals who responded to the survey were more likely to be female and less likely to identify as an URM or be from low-income families than those in the full survey sample. The survey respondents also had noticeably higher eighth-grade state test scores in both ELA and math than the full survey sample. Columns 5 and 6 of Table A1 show that the application of nonresponse weights resulted in average characteristics that more closely mirror those from the full follow-up survey sample. Columns 7 and 8 of Table A1 illustrate that the EC and control groups in this weighted sample did not differ significantly on any of the observed characteristics, with effect sizes ranging from -0.012 to 0.156 across the characteristics examined.

Table A1. Average Characteristics of Follow-up Survey Sample, Respondents, and the Weighted Sample of Respondents

Characteristic	Follow up survey sample		Survey respondents (unweighted)		Survey respondents (weighted)			
	(1) EC	(2) Control	(3) EC	(4) Control	(5) EC	(6) Control	(7) Effect size	(8) *p*-Value
Female	52.4%	52.6%	58%	57.9%	52.8%	51.4%	0.034	0.734
URM	57.1%	59.6%	53%	47.6%	58.0%	51.4%	0.162	0.410
Low-income	45.7%	42.1%	42%	40.8%	44.6%	43.6%	0.026	0.868
8th-grade ELA score	0.311	0.223	0.498	0.485	0.326	0.313	0.013	0.855
8th-grade math score	0.329	0.329	0.526	0.583	0.350	0.408	-0.058	0.431
Sample size	870	1,232	482	504	482	504		

Note. The EC group means are unadjusted means; the control group means are adjusted means computed based on the unadjusted EC group means and the estimated group mean difference. All baseline equivalence tests were conducted using two-level models that were similar to the main impact model and multiple imputation, as described in the “Handling of Missing Baseline Data” section. Effect sizes were computed using the Cox index for dichotomous measures and Hedges’ g for continuous measures, as recommended by the What Works Clearing House (WWC; 2022).

Intent-to-Treat Analyses

The specification of the random-intercept, fixed-slope hierarchical generalized linear model that we used to estimate the ITT EC impacts on binary outcomes is as follows:

Level 1 Model (Individual Level)

$$\log[\varphi_{ij}/(1-\varphi_{ij})] = \beta_{0j} + \beta_{1j} * EC_{ij} + \beta_{2j} * \mathbf{X}_{ij} + \sum_{m=2}^{m}(\beta_{3mj} * SUBLOT_{mij}) \quad (1)$$

where

- φ_{ij} is the probability of experiencing the outcome (e.g., being employed at the time of survey completion) for individual *i* in lottery *j*;
- EC_{ij} is a dummy indicator for treatment status (coded 1 if individual *i* in lottery *j* won the lottery and 0 otherwise, centered on the lottery mean);

- $\boldsymbol{X}_{ij}$ is a vector of individual characteristics, including gender, racial/ethnic minority status, low-income status, and eighth-grade state test scores in ELA and math, grand-mean centered;
- $SUBLOT_{mij}$ is a set of effect-coded indicators for the m sublotteries within a lottery with multiple sublotteries;[11]
- β_{0j} is the average outcome (in logits) among individuals in the control group in lottery j;
- β_{1j} is the difference in the average outcome between the treatment and control groups in lottery j;
- β_{2j} is the relationship between individual characteristic X and the outcome in lottery j; and
- β_{3mj} is the difference in the average outcome between individuals in the control group in sublottery m and individuals in the control group across all sublotteries in the given lottery with sublotteries.

Level 2 Model (Lottery Level)

$$\beta_{0j} = \gamma_{00} + u_{0j} \quad (2)$$
$$\beta_{1j} = \gamma_{10} \quad (3)$$
$$\beta_{2j} = \gamma_{20} \quad (4)$$
$$\beta_{3mj} = \gamma_{3m0} \quad (5)$$

where

- γ_{00} is the average outcome (in logits) among individuals in the control group across all lotteries,
- γ_{10} is the average difference in the outcome between the treatment and control groups across all lotteries,
- γ_{20} is the average relationship between individual characteristic X and the outcome across all lotteries,
- γ_{3m0} is the difference between the average outcome for individuals in the control group in sublottery m and the average outcome for individuals in the control group across all sublotteries in the given lottery with sublotteries, and
- u_{0j} is a random error associated with lottery j.

[11] For a given lottery with m sublotteries, $SUBLOT_{mij}$ was coded -1 for participants in the omitted reference sublottery (i.e., if m = 1), 1 for participants in sublottery m, and 0 for all the other participants. Because of the effect coding, treatment effect for such a lottery represents the equally weighted effect across the m sublotteries within the lottery. There is more than one set of sublottery indicators for each lottery with sublotteries in the Level 1 equation, although only one set is shown for simplicity.

The estimate of primary interest from the model is γ_{10} at the lottery level, which represents a precision-weighted, overall average treatment effect across all lotteries in the survey sample. For each binary outcome, the estimated treatment effect is in the logged odds ratio (logit) metric. Following the practice of the What Works Clearinghouse (2022), we converted the effect estimates into effect sizes using the Cox Index (i.e., logged odds ratio divided by 1.65) to facilitate the interpretation of the size of the treatment effects on binary outcomes.

For the two continuous outcomes (i.e., current and 2021 annual earnings from employment), we conducted the ITT analyses using a two-level hierarchical linear model that mirrored the model for binary outcomes described above, but the dependent variable in the model for continuous outcomes was designated as Y_{ij}. Effect sizes for continuous outcomes were computed using the formula for Hedges' g as recommended by the WWC (2022). For both earnings outcomes, we estimated the effects using three alternative models: a linear model with earnings in dollars, a linear model using the log of earnings instead of earnings in dollars, and a Tobit model with earnings in dollars to account for the floor censoring of dollars at 0. In all cases, our analyses yielded estimated effects with similar conclusions about statistical significance.[12]

[12] Tables of these additional analyses are available on request.

Appendix B. Detailed Study Findings

Table B1. Estimated Effects of EC Admission on Workforce, Financial, and Other Life Outcomes 12 to 14 Years After Expected High School Graduation

Outcome	(1) EC mean	(2) Control mean	(3) Mean difference	(4) Effect size	(5) *p* value	(6) Respondents (N)
Workforce outcomes						
Employed	87.3%	83.7%	3.7%	0.180	0.246	986
Employed full-time	71.0%	67.7%	3.2%	0.092	0.498	986
Ever used UI (in past 3 years)	19.1%	16.5%	2.6%	0.106	0.285	979
Job alignment with goals	42.0%	39.4%	2.6%	0.065	0.511	971
Financial outcomes						
Current annual employment earnings	52,916	48,541	4,375	0.046	0.398	984
2021 annual employment earnings	44,591	46,644	-2,053	-0.046	0.558	978
Student loan debt = $0	33.2%	37.2%	-4.0%	-0.106	0.160	986
Student loan debt > $40,000	17.6%	19.7%	-2.1%	-0.083	0.455	986
Student loan debt > $80,000	6.6%	10.4%	-3.8%	-0.301	0.178	986
Never worried about money	35.2%	37.0%	-1.8%	-0.048	0.617	980
Worried about money weekly or daily	28.3%	29.7%	-1.5%	-0.043	0.692	980
Ever used SNAP (in past 3 years)	20.0%	19.4%	0.6%	0.021	0.874	978
Other life outcomes						
Home ownership	36.3%	36.6%	-0.3%	-0.007	0.944	978
Has retirement account	54.0%	59.4%	-5.4%	-0.134	0.182	980
Has health insurance	55.1%	57.5%	-2.4%	-0.059	0.573	978

Note: UI = unemployment insurance. SNAP = Supplemental Nutrition Assistance Program. The number of respondents for each outcome refers to the number of survey participants who responded to the relevant survey question. The EC group means are unadjusted means; the control group means were computed based on the unadjusted EC group means and the estimated EC effects.

Table B2. CACE Estimates of EC Impacts on Admission on Workforce, Financial, and Other Life Outcomes

Outcome	(1) EC mean	(2) Control mean	(3) Mean difference	(4) Effect size	(5) *p*-value	(6) Respondents (N)
Workforce outcomes						
Employed	88.1%	79.8%	8.3%	0.379	0.032	986
Employed full-time	70.7%	63.4%	7.3%	0.201	0.250	986
Ever used UI (in past 3 years)	16.2%	15.3%	0.9%	0.042	0.762	979
Job alignment with goals	44.1%	38.7%	5.4%	0.135	0.257	971
Financial outcomes						
Current annual employment earnings	57,968	52,062	5,907	0.027	0.400	984
2021 annual employment earnings	46,921	49,742	-2,821	-0.019	0.553	978
Student loan debt = $0	38.7%	39.4%	-0.7%	-0.017	0.881	986
Student loan debt > $40,000	18.9%	19.7%	-0.8%	-0.029	0.829	986
Student loan debt > $80,000	6.5%	7.6%	-1.1%	-0.105	0.731	986
Never worried about money	39.7%	40.1%	-0.3%	-0.008	0.948	980
Worried about money weekly or daily	25.2%	31.2%	-6.1%	-0.183	0.223	980
Ever used SNAP (in past 3 years)	16.9%	19.2%	-2.3%	-0.094	0.666	978
Other life outcomes						
Home ownership	41.5%	38.2%	3.3%	0.085	0.548	978
Has retirement account	55.8%	58.7%	-2.9%	-0.072	0.589	980
Has health insurance	55.9%	55.1%	0.8%	0.019	0.880	978

Note: UI = Unemployment Insurance. SNAP = Supplemental Nutrition Assistance Program. The number of respondents for each outcome refers to the number of survey participants who responded to the relevant survey question. The EC group means are unadjusted means; the control group means were computed based on the unadjusted EC group means and the estimated EC effects.

Table B3. Differential EC Impacts on Survey Outcomes, by Individuals' Characteristics

Outcome	URM vs. non-URM individuals		Individuals from low-income families vs. individuals not from low-income families	
	Difference in impact	*p*-value	Difference in impact	*p*-Value
Workforce outcomes				
Employed	2.5%	0.791	2.0%	0.949
Employed full-time	-1.7%	0.853	8.0%	0.278
Ever used UI (in past 3 years)	-2.7%	0.500	-4.0%	0.320
job alignment with goals	5.7%	0.465	5.2%	0.447
Financial outcomes				
Current annual employment earnings	-7,747	0.431	10,617	0.353
2021 annual employment earnings	-613	0.908	661	0.932
Student loan debt = $0	7.6%	0.348	2.1%	0.829
Student loan debt > $40,000	-1.1%	0.980	5.8%	0.206
Student loan debt > $80,000	0.6%	0.677	4.6%	0.311
Never worried about money	-4.2%	0.592	-2.7%	0.743
Worried about money weekly or daily	3.8%	0.690	0.0%	0.987
Ever used SNAP (in past 3 years)	-14.4%	0.133	-10.4%	0.314
Other life outcomes				
Home ownership	1.4%	0.860	8.5%	0.260
Has retirement account	1.8%	0.729	-2.3%	0.740
Has health insurance	1.6%	0.805	0.3%	0.970

Note: URM = Underrepresented minority. UI = Unemployment Insurance. SNAP = Supplemental Nutrition Assistance Program. For each outcome measure, "Difference in Impact" is the estimated difference in EC impact between the two subgroups in percentage points or dollars. A positive difference in impact implies a larger impact for URM students than for non-URM students or for individuals from low-income families relative to individuals not from low-income families. Reported analytic sample sizes are based on the outcome "Employed," which is the first question on the survey used for analysis. Sample sizes for subsequent outcomes depend on the number of responses to that survey question and are a minimum of 787 (447 EC, 340 control) for analysis based on URM status and 932 (461 EC, 471 control) for analysis based on low-income status.

Table B4. Differential EC Impacts on Survey Outcomes, by Prior Achievement

Outcome	Prior ach. = 1 SD below average			Prior ach. = average			Prior ach. = 1 SD above average			Differential impact	
	EC group mean	Control group mean	Diff.	EC group mean	Control group mean	Diff.	EC group mean	Control group mean	Diff.	Odds ratio	*p*-value
Differential impact, by prior ELA achievement											
Employed	87.8%	83.1%	4.6%	89.2%	85.8%	3.4%	90.4%	88.0%	2.4%	0.938	0.711
Employed full-time	70.6%	64.8%	5.8%	74.1%	70.5%	3.6%	77.4%	75.7%	1.7%	0.919	0.616
Job alignment with goals	45.7%	33.0%	12.7%	41.3%	35.5%	5.8%	37.1%	38.1%	-1.0%	0.748	0.133
Used UI in past 3 years	22.9%	17.0%	5.8%	20.7%	17.5%	3.3%	18.7%	17.9%	0.8%	0.855	0.349
Current annual employment earnings	48,115	41,497	6,618	49,841	44,779	5,062	51,567	48,060	3,507	--	0.656
2021 annual employment earnings	42,136	39,626	2,510	41,408	42,117	-709	40,681	44,608	-3,928	--	0.331
Student loan debt = $0	19.6%	23.6%	-4.0%	22.2%	25.8%	-3.6%	25.1%	28.1%	-3.1%	1.040	0.837
Student loan debt > $40,000	14.9%	16.7%	-1.9%	15.5%	17.5%	-1.9%	16.2%	18.2%	-2.0%	1.002	0.991
Student loan debt > $80,000	2.7%	4.6%	-1.9%	3.2%	5.3%	-2.1%	3.8%	6.0%	-2.2%	1.037	0.879
Never worried about money	26.1%	26.1%	0.0%	27.6%	28.6%	-1.0%	29.1%	31.2%	-2.1%	0.951	0.825
Worried about money weekly or daily	27.3%	36.2%	-8.9%	30.1%	33.3%	-3.2%	33.1%	30.6%	2.5%	1.301	0.188
Ever used SNAP (in past 3 years)	15.7%	18.7%	-3.0%	16.8%	16.8%	0.0%	17.9%	15.0%	2.9%	1.238	0.224
Home ownership	37.5%	27.9%	9.6%	29.9%	26.9%	3.1%	23.4%	25.8%	-2.5%	0.752	0.114
Has retirement account	46.4%	48.7%	-2.3%	48.9%	53.4%	-4.5%	51.3%	58.0%	-6.7%	0.915	0.477
Has health insurance	40.6%	43.0%	-2.4%	48.9%	51.3%	-2.4%	57.3%	59.5%	-2.3%	1.003	0.987

Outcome	Prior ach. = 1 SD below average			Prior ach. = average			Prior ach. = 1 SD above average			Differential impact	
	EC group mean	Control group mean	Diff.	EC group mean	Control group mean	Diff.	EC group mean	Control group mean	Diff.	Odds ratio	*p* value
Differential impact, by prior mathematics achievement											
Employed	92.5%	86.4%	6.1%	89.8%	85.8%	4.0%	86.3%	85.2%	1.1%	0.751	0.147
Employed full-time	78.2%	70.9%	7.3%	74.8%	70.6%	4.2%	71.1%	70.2%	0.9%	0.843	0.313
Job alignment with goals	40.9%	32.3%	8.6%	40.1%	35.5%	4.5%	39.2%	38.9%	0.3%	0.837	0.243
Used UI in past 3 years	18.4%	16.4%	2.0%	20.1%	17.5%	2.6%	21.8%	18.6%	3.2%	1.028	0.874
Current annual employment earnings	50,475	41,601	8,874	50,511	44,781	5,730	50,546	47,961	2,585	--	0.376
2021 annual employment earnings	40,401	39,368	1,033	41,005	42,104	-1,098	41,609	44,839	-3,230	--	0.456
Student loan debt = $0	16.1%	19.9%	-3.8%	22.1%	25.8%	-3.7%	29.4%	32.7%	-3.3%	1.053	0.795
Student loan debt > $40,000	16.0%	18.9%	-2.9%	15.3%	17.5%	-2.1%	14.7%	16.1%	-1.4%	1.050	0.794
Student loan debt > $80,000	2.4%	4.6%	-2.2%	3.0%	5.2%	-2.2%	3.9%	6.0%	-2.1%	1.116	0.652
Never worried about money	23.1%	23.9%	-0.8%	27.2%	28.6%	-1.4%	31.7%	33.7%	-2.0%	0.978	0.920
Worried about money weekly or daily	28.7%	37.5%	-8.8%	30.3%	33.3%	-3.1%	31.8%	29.4%	2.5%	1.294	0.159
Ever used SNAP (in past 3 years)	16.7%	16.8%	-0.1%	17.3%	16.8%	0.4%	17.8%	16.8%	1.0%	1.039	0.879
Home ownership	28.2%	22.6%	5.5%	29.0%	26.9%	2.1%	29.9%	31.7%	-1.8%	0.829	0.320
Has retirement account	51.7%	53.6%	-1.9%	49.0%	53.4%	-4.4%	46.3%	53.2%	-6.9%	0.906	0.575
Has health insurance	53.5%	53.5%	0.0%	49.6%	51.3%	-1.7%	45.8%	49.1%	-3.3%	0.937	0.761

Notes. Ach. = achievement; Diff. = difference; ELA = English language arts; SD = standard deviation; UI = unemployment insurance. SNAP = Supplemental Nutrition Assistance Program. Analytic sample sizes vary by outcome. Across all outcomes, the sample sizes range from 971 (474 EC, 497 control) to 986 (482 EC, 504 control). The EC and control group probabilities for a given level of prior achievement (i.e., 1 SD below/at/above the state average) are predicted probabilities when all covariates other than prior achievement were set to their grand means. The values in the "Diff." columns may not match the difference between the EC and control group probabilities because of rounding. Odds ratio does not apply to continuous measures and therefore is not reported for current annual earnings. Reported *p*-values come from significance test on whether the coefficient associated with the interaction between treatment and the indicated prior achievement (ELA or math) is statistically different from 0.

Table B5. Differences Between EC and Control Students in the Probability of Earning a Degree in a STEM Field Among Degree Completers, by Level of Degree Completion

Outcome	EC Group		Control Group		Difference in Probability	Effect Size	P-value
	Probability	N	Probability	N			
Earning a bachelor’s degree in STEM [a]	27.0%	322	32.8%	249	-5.8%	-0.167	0.195
Earning an associate degree in STEM [b]	8.3%	105	18.7%	69	-10.4%	-0.565	0.097

[a]: Analysis was restricted to participants from survey sample who earned a bachelor’s degree within 10 years of expected high school completion and had degree major data from the NSC.

[b]: Analysis was restricted to participants from survey sample who earned an associate degree within 10 years of expected high school completion without earning a bachelor's degree (or higher) and had degree major data from the NSC.

Notes: The EC group probabilities are unadjusted probabilities; the control group probabilities were computed based on the unadjusted EC group probabilities and the estimated group differences (in logit). Effect sizes were computed using the Cox index for dichotomous measures as recommended by the WWC (2022).

Table B6. Differences in Background Characteristics Between EC Students and Control Students Included in Degree Major Analyses

Characteristic	(1) EC Group Mean	(2) Control Group Mean	(3) Mean Difference	(4) Effect Size	(5) P-value
Sample for analysis of earning a bachelor’s degree in STEM [a]					
Female	66%	66.9%	-1.1%	-0.029	0.815
Underrepresented Minority	62%	56.9%	5.2%	0.131	0.427
Low-Income	61%	56.1%	5.1%	0.127	0.369
Grade 8 ELA test score	0.610	0.582	0.028	0.028	0.726
Grade 8 math test score	0.595	0.735	-0.140	-0.140	0.084
Sample for analysis of earning an associate degree only in STEM [b]					
Female	67%	73.2%	-5.9%	-0.171	0.491
Underrepresented Minority	55%	66.7%	-11.6%	-0.296	0.435
Low-Income	67%	69.4%	-2.1%	-0.059	0.831
Grade 8 ELA test score	0.343	0.190	0.154	0.154	0.329
Grade 8 math test score	0.476	0.269	0.208	0.208	0.190

[a]: Analysis was restricted to participants from survey sample who earned a bachelor’s degree within 10 years of expected high school completion and had degree major data from the NSC. See Table A1 for analytic sample size.

[b]: Analysis was restricted to participants from survey sample who earned an associate degree within 10 years of expected high school completion without earning a bachelor's degree (or higher) and had degree major data from the NSC. See Table A1 for analytic sample size.

Notes: Obs.=observations. The EC group means are unadjusted means; the control group means are adjusted means computed based on the unadjusted EC group means and the estimated group mean differences. All baseline equivalence tests were conducted using two-level models that were similar to the main impact model with multiply imputed data. Effect sizes were computed using the Cox index for dichotomous measures and Hedges’ g for continuous measures, as recommended by the WWC (2022).

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