Does dual enrollment improve progression through college and earnings and do outcomes differ by sociodemographic characteristics or achievement levels? A quasi-experimental analysis of Colorado students

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Does dual enrollment improve progression through college and earnings and do outcomes differ by sociodemographic characteristics or achievement levels? A quasi-experimental analysis of Colorado students

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Abstract

Using a two-stage, matched design, we found dual enrollment students enrolled in college within one year of their expected high school graduation at higher rates than control students who did not take college classes in high school (OR = 3.06). For students that matriculated within one year after high school, compared to control, treatment students showed higher rates of persistence (OR = 1.30), and completion of "any" degree (OR = 2.08), a two-year credential within two years (OR = 2.87), and a four-year degree within four years (OR = 1.61). And five years after high school, treatment students had higher earnings (g = .079). Subgroup findings revealed no significant moderation effects of the treatment by income, race, gender, or achievement level. Prior quantitative studies have largely failed to consider the role of academic achievement, so that is an important contribution of this paper. Ours is also the first study we are aware of to examine the impact of dual enrollment on earnings. While awaiting future research replicating our achievement and earnings results, results suggest dual enrollment is a promising strategy for increasing postsecondary success and earnings after college.

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Dual enrollment (DE), also known as dual credit and concurrent enrollment, refers to the broad array of programs available to high school students that allow them to enroll in college-level coursework and simultaneously earn high school and college credit (An & Taylor, 2019; Villarreal, 2018). DE occurs through partnerships between high schools or school districts and colleges and universities and can be offered on a high school, community college, or university campus (Miller et al., 2017, 2018). In DE programs, college credit is determined by a student's course grade, reflecting a continuous assessment of overall performance throughout the semester that is more consistent with fundamental educational philosophies than the standardized test criterion used by other credit-based postsecondary transition programs such as Advanced Placement (Dutkowsky et al., 2006). The curriculum used in DE, though the same as that offered at a college, varies in pedogeological approach across programs. DE course type also varies and can include career and technical education (CTE) or academic courses that differ in difficulty and subject area.

Bailey & Karp (2003) describe three types of DE programs: (1) stand-alone college courses available to high school students, referred to as a singleton program; (2) a comprehensive program that offers multiple college courses, typically during the junior and senior year of high school; and (3) an enhanced comprehensive program that integrates college-level courses with support services. DE students might participate in a singleton program, or they could receive a more comprehensive program that, for some, leads to an associate’s degree (see Edmunds et al., 2012, 2017; Haxton et al., 2016; Song & Zeiser, 2019). Because some DE courses are taught at high school campuses, not all involve a true college experience with high school students and college students in the same classroom.

DE Debate and Study Purpose

Between the fall 2009 and 2020, the U.S. undergraduate enrollment rate in degree-granting colleges declined by nine percent (National Center for Education Statistics, 2022). Meanwhile, pervasive racial and ethnic disparities in college completion continue to exist. For example, the six-year graduation rate for first-time, full-time undergraduate students pursuing a bachelor’s degree who started college in fall 2010 was highest for Asian students (74%), followed by White students (64%), Hispanic students (54%), Pacific Islander students (51%), Black students (40%), and American Indian/Alaska Native students (39%) (de Brey et al., 2019). These realities underscore the need for policies to help students pursue and finish higher education – a need that is particularly urgent for those who face systemic barriers but want to attend college, including students of color and students who come from disadvantaged backgrounds (Boswell, 2001; Hoffman, 2005; Hugo, 2001). Advocates cite two reasons DE is seen as a policy mechanism for improving college outcomes that is inclusive of a variety of students.

First, DE provides more opportunities for college-level learning to high school students. The strongest predictor of a students’ likelihood of college attainment is the intensity and quality of their high school curriculum (Adelman, 2006). Research, however, has identified disparities in curricular offerings wherein low-income students disproportionately receive lower-quality instruction (Darling-Hammond, 2010). Studies have found that students who are exposed to rigorous courses show improved achievement and college outcomes, even after controlling for socioeconomic background (Welner et al., 2008). Expanding DE courses to more high schools and targeting them to students of all backgrounds could ensure traditionally underserved students have access to enriched, advanced curriculum (Karp et al., 2005; Venezia et al., 2003).

Second, DE creates multiple college pathways by enabling high school students to take academic courses on college campuses, college-led academic courses at their high school, or a hybrid of both (Puyear & Mills, 2001). In addition, some DE programs permit students to take post-secondary-level CTE courses (Estacion et al., 2011). Thus, DE benefits students with a variety of college aspirations, including those targeting certificate programs (Villearreal, 2018).

Opponents of DE programs raise several concerns. First, whether DE courses are sufficiently rigorous relative to college-credit only courses. Second, whether all high school students are prepared both academically and emotionally to satisfy the performance criteria expected of college-level courses. Third, and finally, critics raise concerns that note that limited access to DE courses for disadvantaged students might exacerbate existing college inequities (Miller et al., 2018; Speroni, 2011).

This study evaluates a Colorado DE program. Introduced in 2009, the program was intended to provide a mechanism for increasing college entrance rates immediately after high school graduation, improving college persistence and on-time postsecondary credential attainment, and improving earnings. In evaluating the program, we conduct secondary analyses to examine whether the relation between DE and outcomes differ by income, race, or achievement levels to test whether students equally benefitted from DE.

Theoretical Framework

Sociological and economic theories offer compelling explanations for how DE might increase postsecondary enrollment and completion (Villarreal, 2018). Applying social role theory, DE gives high school students a “head start” by teaching normative rules and behaviors that exist in often ill-defined postsecondary settings (Bailey et al., 2002; Collier & Morgan, 2008; Karp, 2012). By creating opportunities to role play a successful college undergraduate, DE prepares students for college prior to matriculation (Karp, 2012). Consistent with the theory of "schooling as experimentation" (Manski, 1989), DE also allows students to "test the waters" before making their postsecondary school choice, which may translate into better matches between students and institutions and improve outcomes. Critics, however, argue that social role theory renders a sense of universality, placing greater emphasis on social conformity than questioning social policies (Jackson, 1998). Social role theory guiding DE’s inputs, outputs, and outcomes therefore assumes institutes of higher education provide sufficient learning conditions and a welcoming environment for students representing a variety of multicultural and ethnic backgrounds.

Another framework underlying DE policies is human capital theory (Villarreal, 2018), which posits that differences in college outcomes are related to differences in academic preparation and achievement, availability of resources, and expected benefits of earning a degree (Becker, 1962, 1993; Paulsen, 2001). Students decide to continue their education beyond high school based on the direct and indirect costs associated with college, including the costs of tuition and fees plus the opportunity cost of foregone wages while in college which can be exchanged for higher long-term earnings (Becker, 1962; Villarreal, 2018). Within this framework, DE provides an opportunity for more secondary students to pursue a college certificate or degree, which will allow them to earn college credit before matriculation, thereby reducing time-to-degree, time out of the labor market, and increasing students' potential benefits of college attendance (Hoffman, 2005; Villarreal, 2018). This view, however, offers an individualistic idea of social mobility, which critics argue is harmful because it masks socio-economic class struggles (Rivera et al., 2019). Studies reveal earnings gaps by race and ethnicity, and these gaps increase with social class (Bayer & Charles, 2016; Farrell et al., 2020), indicating the human capital ethnic minorities possess is devalued in the labor market (Tomaskovic-Devey et al., 2005).

Existing Evidence on DE Impacts

Rigorous research (or designs that demonstrate high internal validity; see (Steeger, et al., 2021) comparing the outcomes of students who participate in DE to the outcomes of non-participants is sparse (Miller et al., 2018). Hemelt et al. (2020) published the first randomized controlled trial of DE, in which DE advanced algebra courses altered students' high school math course-taking, reduced enrollment in remedial math, and boosted enrollment in precalculus and AP math courses. Compared to students in the control group, students who participated in DE courses were more likely to attend four-year rather than two-year colleges, though the overall rate of college enrollment was unchanged.

Employing a quasi-experimental, instrumental variable (IV) analysis, Miller et al. (2018) found that, on average, participation in traditional DE programs taught in high school settings modestly improved college enrollment, completion, and time-to-degree (i.e., graduating from a four-year college within 6 years of high school graduation). Speroni (2011) also employed a quasi-experimental, regression discontinuity (RD), design to evaluate DE in Florida, where students are required to have a minimum unweighted GPA of 3.0 to be eligible for the state's program. Findings show that compared to control students, taking a DE algebra course had a large effect on college enrollment and graduation. However, when comparing students who take any DE academic course to students who take no DE courses, results show no significant effects.

Both IV and RD quasi-experimental designs are considered methodologically strong alternatives when randomized control trials are less feasible (St. Clair et al., 2014; Wing & Cook, 2013). The most recent intervention report released by the U.S. Department of Education's What Works Clearinghouse (WWC), which produces resources to help educators find evidence-based programs and to promote rigorous standards in education research, cited 35 evaluations of DE programs (U.S. Department of Education, Institute of Education Sciences, 2017). Most, however, are correlational in design. Only two (Haxton et al., 2016; Edmunds et al., 2012, 2017) meet WWC design standards that allow researchers to identify the causal impact of DE programs "without reservations", which is WWC's highest rating indicating strong evidence for an intervention's effectiveness – a designation that only experimental designs with low attrition qualify (ED, 2020). However, these studies evaluated early colleges, which is a slightly different, more comprehensive model than DE examined in the present study. Early colleges follow a five-year model that offers an opportunity to earn an associate's degree or up to 2 years of college credits toward a bachelor's degree during high school at no or low cost to the students (Haxton et al., 2016; Edmunds et al., 2012, 2017).

Three studies meet WWC standards "with reservations", a rating indicating weaker evidence of effectiveness because these studies each utilized quasi-experimental matching methods (An, 2013; Giani et al; 2014; Struhl & Vargas, 2012). An (2013) showed that eight years after high school, DE participation increased the probability of earning a bachelor's degree and earning any college degree. Giani et al. (2014) found that, compared to the control group, DE students were more likely to enroll in postsecondary education within one year after high school graduation, enroll in the second year of postsecondary education, and earn a postsecondary degree. Treatment students were also more likely to enroll in a four-year university within one year after high school graduation, enroll in the second year of the university, and earn a bachelor's degree within six years and four years.

An & Taylor (2019) published a systematic review of 122 quantitative, qualitative, or mixed methods empirical studies that examined high school and postsecondary outcomes of DE and noted several patterns concerning program impacts:

1. Many quantitative studies considered differential participation rates by race and ethnicity and socio-economic status (SES), but most did not account for important covariates (e.g., academic achievement) that potentially confound the race and ethnicity and SES disparities in DE participation.
2. College persistence is an understudied area of DE research; in addition, few studies have focused on the relationship between DE and degree completion.
3. Another understudied research question is whether DE reduces time-to-degree. And
4. DE has expanded so that participation is no longer exclusive to high-achieving, White, and high-SES students; however, only a handful of studies consider whether underrepresented students benefit from DE.

Our research contributes to the robustness of previous DE evaluation studies that utilized quasi-experimental matching methods (An, 2013; Giani et al; 2014; Struhl & Vargas, 2012) and addresses the limitations of the DE literature identified by An & Taylor (2019). In addition, to our knowledge, this is the first study extending DE outcomes to earnings.

Study Context

DE has been implemented in all fifty states for more than thirty years (Plucker et al., 2006; Bailey et al., 2002), though it has recently risen in popularity. Between 2002–03 and 2010–11, DE programs increased over seven percent annually, with 12 percent growth seen in schools serving a high proportion of racial and ethnic minority students (Thomas et al., 2013). An & Taylor (2019) note that "a nontrivial number of districts are associated with high enrollment rates among minority students as well as low White-minority gaps in AP and DE participation" (p. 3). In 2015–16, 69 percent of high schools offered DE opportunities (compared to 71 percent that offered at least one AP course; U.S. Government Accountability Office, 2018), and many heavily rural states utilize DE as a substitute for honors or AP courses (Kryst et al., 2018; Rivera et al., 2019). According to the Education Commission of the States (2018), 47 states (plus the District of Columbia) have a statewide policy of DE. Funding for, and implementation of, DE programs varies by state.

Defining Dual Enrollment in Colorado

In Colorado, school districts use per pupil revenue to pay the tuition for the postsecondary courses at the resident community college rate directly to the institution on behalf of the student. The college partner utilizes the state's Colorado Opportunity Fund stipend (per credit state allocation) to offset tuition costs at the institution. As of the 2021–22 school year, Colorado had 178 school districts containing 641 high schools (551 public and 90 private), and the state's higher education system is composed of 135 colleges and universities (31 public and 60 private institutions). School districts must enter into a cooperative agreement with a qualified institution of higher education outlining how college credits will be awarded, the negotiated tuition rate, and the establishment of an academic plan of study for students to support ongoing counseling and career planning. Eligible students must be less than 21 years of age and enrolled in the ninth grade or higher at a public school district in Colorado. A qualified student must satisfy prerequisites for the DE course and the college may require testing to demonstrate preparedness. DE instructors must meet faculty qualifications for an adjunct college instructor in Colorado, where it is common practice to have a master's degree in the subject area, or a master's degree with successful completion of at least 18 graduate-level credits in the subject area. For most classes in CTE, an instructor must be eligible to obtain a postsecondary credential in the specific CTE area from the college in which the faculty would be teaching. The district's college partner reviews and approves all instructors that are teaching DE.

Colorado is ranked 14th out of 50 states and the District of Columbia by state median district DE participation rate (Xu et al., 2021), with one in every four eleventh and twelfth graders dually enrolled (Colorado Department of Higher Education & Colorado Department of Education, 2021). When capitalized, "Concurrent Enrollment" refers to Colorado's statewide DE program detailed in the Concurrent Enrollment Programs Act (C.R.S. §22-35-101 et seq.), in which high school students may enroll tuition-free in postsecondary courses and earn college credits that are transferable to any Colorado public university (Witkowsky & Clayton, 2020; Witkowsky et al., 2020). There are other DE programs offered to high school students by colleges in Colorado that do not follow the state statutory guidelines. These DE courses are usually cash funded (i.e., students/families pay for the tuition), and course transferability varies.

The passage of the 2009 Concurrent Enrollment Programs Act marked a shift from smaller, limited participation programs to widespread DE open to all high school students supported by a dedicated funding model. Increased funding, coupled with legislative support and a well-defined credit articulation policy built the foundation on which DE participation grew (Witkowsky & Clayton, 2021; Witkowsky et al., 2020). Specifically, enrollment went from 9,349 Colorado high school students in 2010–11 to 53,245 students who participated in DE programs of any type in the 2019–20 academic year (75% of whom participated in the state-funded DE program) (Colorado Department of Higher Education & Colorado Department of Education, 2021). As of 2019–20, 98 percent of school districts and 91 percent of high schools in Colorado offered DE.

Xu et al. (2021) reported patterns of AP and DE enrollment by race and ethnicity across thousands of school districts in the U.S. and found most have racial equity gaps. This is the case in Colorado as well. As of 2019–20, most DE students (52 percent) were White, while 26 percent were Hispanic, and three percent were Black. These statistics reflect the demographics of K-12 students in Colorado, in which (as of 2019–20), 53 percent were White, 34 percent were Hispanic, and five percent were Black (Colorado Department of Education, 2021).

Research Questions

We pre-registered with the Registry of Efficacy and Effectiveness Studies (1705.1v1) three confirmatory research questions: (1) Is participation in DE related to college access, as measured by matriculation to college one-year post-expected date of high school graduation? For students who matriculate to college within one year of their expected high school graduation date, is participation in DE related to college success, as measured by (2) persistence from year 1 to year 2? and (3) completion of any postsecondary credential? In addition, three confirmatory questions related to on-time completion and earnings were preregistered with the Open Science Framework (osf.io/cqm7t). The sample for this set of questions focused on students who matriculated to college within one year of their expected high school graduation date and asked: what is the impact of DE on: (4) earning a credential within two years of expected high school graduation? (5) earning a four-year degree within four years of expected high school graduation date?, and (6) earnings five years after expected high school graduation date? Exploratory questions interacted condition by race, income, gender, and achievement level. The earnings confirmatory research question also included degree attainment as a moderator.

Methods

Sample

This project used a retrospective, quasi-experimental design relying on secondary analysis of historical data. We conducted a cohort-based longitudinal study that follows eleventh grade students who had an expected high school graduation date between 2010–11 and 2014–15. Cohorts were deemed eligible for inclusion in the study based on the school year in which students were in eleventh grade. Though the 2009 legislation made all high school students eligible to participate in DE courses in grades nine through twelve, most DE courses in Colorado are offered to eleventh and twelfth grade students. Statewide, nearly 40 percent of high school graduates in public high schools in Colorado participated in DE in 2019–20 (Colorado Department of Higher Education & Colorado Department of Education, 2021). Given the data available for the period of the present study, five cohorts were constructed starting when students were in eleventh grade and defined by their expected high school graduation year.

Design

While randomly assigning subjects to condition is the most effective approach for eliminating selection bias (Shadish et al., 2002), efforts to do so are limited by many forces, including universal policies like DE. As such, we employed a quasi-experimental design using multiple statistical techniques to adjust for pre-existing differences between conditions. To address both selection bias and differences in opportunity due to the inequitable availability of DE at different types of schools, we followed a two-stage matching design that included, first, crude matching at the school level and, second, propensity score matching (PSM) at the student level (Rosenbaum, 2010; Rosenbaum & Rubin, 1983). As noted in our limitations section (see Discussion), our design does not account for unobservable characteristics including motivation, self-efficacy, and other non-cognitive factors, each of which influence college access and success. These design limitations motivated our decision to preregister our research questions and analysis plan and thus guard against conducting too many analyses in a search for significant results, recognizing that incomplete or selective reporting biases results (Nosek et al. 2015).

Conceptual Model for Matching Variables

Our PSM design is grounded in Perna and Thomas' (2008) *Conceptual Model of Student Success*, which integrates economics, education, psychology, and sociology to provide a comprehensive understanding of the factors that influence college choice and success. According to the model, four interconnected layers influence student success along a continuum of indicators ranging from college readiness to college enrollment, college achievement, and post-college attainment. The internal context (i.e., students' attitudes, motivations, skills, and aptitude) comprises layer one. Layer two, referred to as the family context, theorizes that family factors influence student experiences and subsequent success. Layer three is the context of the educational institution, including resources and academic preparation relating to student success. The fourth layer considers external factors associated with the social, economic, and policy context that contribute to student success. We used this model to determine variables in the creation of our propensity score. This approach is taken by at least one other study that examined DE outcomes (Taylor, 2015) to adjust for multiple, theoretically meaningful contributors to student likelihood of selecting into the program.

According to May et al. (2013), "any study using propensity score methods should include a comprehensive logic model of the selection mechanism in order to identify the degree to which the propensity score model does or does not include key elements influencing the selection of program participants" (p. 6). Thus, we adapted the Perna and Thomas (2008) conceptual model based on a framework of inputs and predictors of high school students' participation in another credit-based transition program, the International Baccalaureate (IB) program (see May et al., 2013, p. 19). IB is a two-year program leading to a high school diploma honored by many universities internationally. Students in IB programs tend to come from higher income households, and IB serves a disproportionate number of students from White or Asian backgrounds (Perna et al., 2015). The demographics of DE students (compared to those who do not participate in DE) approximately parallel those of IB participants. For example, in analyzing who enrolled in DE in Colorado in 2019–20 (the most recent data available), a higher proportion of DE students were female than male (54 percent versus 46 percent); and White (52 percent) than Hispanic (26 percent), African American (three percent), or more than one racial or ethnic group (four percent) (Colorado Department of Higher Education & Colorado Department of Education, 2021). Rivera et al. (2019) found that socioeconomic status is a strong predictor of DE participation.

Following Perna and Thomas (2008) and May et al. (2013), our analytic model included measurable predictors at multiple levels (i.e., the individual, school, and community) associated with college outcomes while also accounting for measurable and unmeasurable elements of the process by which students are provided with the opportunity to enroll in DE. We did not, however, capture the family context (layer two of Perna & Thomas, 2008) due to limitations of our dataset. Concerning the fourth layer (Perna & Thomas, 2008), state-level variables were unnecessary since we focused on Colorado, thereby controlling for the college-related processes at the state level. Our data included eleventh grade students enrolled in high schools across Colorado. Just as individual characteristics vary, we presume that the high school contexts varied in ways that influenced the propensity for DE participation. Therefore, we used a two-stage matching process, involving both school and student-level characteristics, as described below.

Matching Process

We decided that it was important, *a priori*, for any matching strategy to account for the fact that some schools offer greater opportunity for DE than others, and that this opportunity impacts not only the likelihood of students selecting into the treatment, but also schools' likelihood of fostering beneficial college and postsecondary outcomes for students. Therefore, we defined a school-level treatment group for schools offering "ample" DE opportunities versus those offering "few" (see Supplemental File for further explanation). Groups were identified at the school-level using the state median number of DE credits attempted per enrolled student in 2008–09 (the baseline year), with "ample" opportunities represented by schools at or above the state median and "few" reflecting schools below the state median. The state median number of DE credits attempted per student in the baseline year was 0.1841, calculated by determining the mean number of DE credits earned (total DE hours attempted in 2008–09 divided by the school sample size in 2008–09) per school (n = 292 schools), and then determining the median of these 292 schools.

At the student level, treatment was defined as attempting any DE credits in the eleventh or twelfth grade while attending a school with "ample" DE opportunities. Treatment students were compared to a business-as-usual comparison group of eleventh and twelfth grade students who did not attempt any DE credits while attending otherwise similar high schools offering "few" DE opportunities. Baseline equivalence at both the school and student levels was established using Hedges *g* (with pooled standard deviations) for continuous variables and Cox's *d* for dichotomous measures, as recommended by the What Works Clearinghouse Version 4.1 Standards Handbook (ED, 2020). All data management and analyses were performed using R version 3.3.1 (R Core Team, 2017), with the R package "MatchIt" (Ho et al., 2011) used in both stages of the matching process. Distance measures were calculated using logistic regression for most variables rather than exact matching on covariates (with some exceptions noted below).

First, schools with ample DE opportunities were matched to their nearest neighbor schools offering few DE opportunities. Drawing from the list of variables identified by May et al. (2013) and Estacion et al. (2011), we included three school-level measured factors that influence the likelihood of enrolling in DE and college outcomes: (1) college-going rate, which represents the percent of students who enrolled in college the fall after graduating from high school; (2) mean ninth grade state standardized reading test score; and, (3) percent of students who qualify for free or reduced price lunch (FRL). At the community level, we exact-matched schools on geographic location (1 = rural, 0 = urban/suburban) because urbanicity strongly predicts DE offerings (Kryst et al., 2018). These factors were used to predict the odds of schools offering more or less than the state median number of DE credits. From the initial sample of 292 eligible schools (see Figure 1), 172 matched schools were retained (86 DE-ample, 86 DE-few).

We determined *a priori* that, if baseline equivalence was not achieved between all students attempting a DE course that follows the state's statutory guidelines at the matched schools with ample DE opportunities (treatment students) and all students who did not attempt DE credits at the matched schools with few DE opportunities (comparison students), we would use a second stage of one-to-one, nearest neighbor propensity score matching (PSM) without replacement (yielding equal comparison and treatment student group sizes) to match students across conditions by cohort, FRL status, ninth grade reading achievement test scores, minority status, and English language learner (ELL) status. Consistent with What Works Clearinghouse standards as outlined in the *Transition to College Review Protocol* (ED, 2019), we used a cutoff of better than 0.25 standard deviations for baseline equivalence on ninth grade reading scores and FRL status for each matched sample. Matching variables were included as controls to adjust for residual sources of observable variation remaining between groups. Tables 3 and 4 (see Results) display the baseline equivalence statistics for all baseline and analytic samples.

Because there remained sizeable baseline differences in student standardized test scores after school-level matching (g= 0.35), we proceeded with student-level matching for those enrolled in the remaining schools on: (1) ninth-grade state standardized reading test score; (2) FRL status; (3) under-represented minority status; (4) ELL status; and (5) school-level propensity scores from the previous matching model. While the sample was already restricted to comparable schools during the school-level match, the retained schools still displayed a range of propensity scores. School-level propensity scores were thus included in the student-level match so that students were matched not only to similar students, but to students with similar school-level characteristics that determined access and opportunity for treatment, without requiring that they could only be matched to similar students in their school's exact matched pair. Additionally, students were exactly matched on their expected high school graduation cohort to minimize the risk of unequal exposure (i.e., time available to attain each outcome) driving any group differences. In the samples assessing on-time degree attainment and earnings, we also examined whether students were roughly equivalent at baseline on gender (1 = female, 0 = male) and included gender as an additional matching covariate when further matching was needed.

We excluded treatment and potential comparison students with missing data on the key matching variables. Furthermore, if baseline equivalence was not achieved on key matching variables in the full analytic sample, treatment students with the least similar matched comparisons were omitted until baseline equivalence was achieved. For each outcome, we re-assessed the baseline equivalence of the analytic samples and proceeded with drawing a new, matched sample from the full, eligible population of students enrolled in the matched schools only if baseline equivalence was not maintained. If it was maintained, we proceeded to the next analysis with a subsample of students from the previous matched sample that were also eligible for the new outcome analysis, using listwise deletion to remove participants if outcome variables were missing. Most commonly, this occurred when there was insufficient follow-up data for later cohorts to attain the outcome (such as for 4-year degree attainment or earnings).

In summary, treatment and comparison students were never drawn from the same school. By selecting comparison students from schools offering "few" DE opportunities, and only retaining treatment students that attended schools with "ample" opportunities for DE, we tried to minimize the potential confound of comparison students self-selecting out of DE courses.

Inclusion and Exclusion Criteria

The evaluation-specific inclusion criteria were as follows. (1) Student records had to be linked to a school with 2008–09 baseline school-level data, meaning the school was in operation in 2008–09, the baseline year (i.e., prior to establishment of the state's statutory guidelines creating Colorado's DE; Witkowsky & Clayton, 2020). (2) The record had to be linked to a traditional high school in which students typically graduated in four years. The definition of "traditional school" included charter and innovation schools that offered high school degrees but excluded Early College High Schools (described further below). And (3) The record had to be linked to a school with a sample size of eleventh and twelfth grade students that included 70 or more students (per school), as "treatment" was defined first at the school level and then at the student level and we needed a minimum number of students per cohort eligible for DE.

Whole schools or subsets of students within schools were excluded when they were associated with one of the following three types of DE programs separate from Colorado's state-funded DE program. (1) *Early College High Schools*: These are state board-approved high schools that, through partnerships with institutions of higher education, enable students to earn 60 credits and an associate's degree (or a specified number of college credits) prior to graduation. Early colleges are exempt from Colorado's Concurrent Enrollment Act and thus were excluded from this study. (2) *ASCENT (Accelerating Students through Concurrent ENrollmenT)*: Students who have completed at least 12 credit hours of postsecondary coursework prior to completion of their twelfth-grade year may be eligible for the ASCENT Program. They remain students in their high school for one year following their twelfth-grade year, and the school receives ASCENT-specific per-pupil state funding that it uses to pay their college tuition at the resident community college rate. We excluded ASCENT students (though not the high schools they attend) because this is not the way most students in Colorado experience DE. And (3) "*Other" high school DE program:* Students can take a DE course outside of Colorado's state-funded DE program. These programs are administered directly by postsecondary institutions and do not fall under the state's statutory definition of Concurrent Enrollment. Students can receive college-level credit through these other DE programs, but they and their families may be required to pay for courses. Additionally, these courses are not required to transfer and/or apply to programs of study at other Colorado public colleges, so we excluded these students (although not the high schools they attend).

Data Sources

We used data from four sources: (1) Colorado Department of Education (CDE) provided student-level demographic and ninth grade achievement data as well as all school-level data. (2) Colorado Department of Higher Education (CDHE) provided student-level DE course-taking information as well as matriculation and completion information for in-state colleges and universities. (3) Through CDHE, National Student Clearinghouse (NSC) provided student-level matriculation and completion data for out-of-state colleges and universities. And (4) Colorado Department of Labor and Employment (CDLE) provided student-level quarterly earnings.

Cohorts and Outcome Measures

We examined a total of six confirmatory questions among four groups of dependent variables in this study related to (1) matriculation, (2) persistence, (3) attainment (i.e., obtaining any degree, and on-time completion, defined as earning a 2-year degree in two years or a 4-year degree in four years), and (4) earnings. Table 1 describes how these outcomes were analyzed using different numbers of cohorts based on the length of required follow-up period.

Matriculation. We coded the first outcome, college matriculation, as a binary variable that indicated whether students did or did not enroll in college within one year

Table 1. *Confirmatory Outcomes Assessed by Cohort*

Cohort	**(1) Matriculation**	**(2) Persistence**	**(3) Attainment (measured three ways)**			**(4) Earnings**
EHSG Year	**Within 1 YR of EHSG**	**Matriculate within 1 YR of EHSG, Persistence from YR1 to YR2**	**Matriculate within 1 YR of EHSG, "Any" Degree**	**On-time completion: Matriculate within 1 YR of EHSG, 2 YR Degree in 2 YRs**	**On-time completion: Matriculate within 1 YR of EHSG, 4 YR Degree in 4 YRs**	**Within 5 years of EHSG**
2011	X	X	X	X	X	X
2012	X	X	X	X	X	X
2013	X	X	X	X	–	X
2014	X	X	X	X	–	–
2015	X	–	–	–	–	–

Note: EHSG – Expected high school graduation date.
Postsecondary data encompass academic year (AY) 2011-12 though AY 2015- 2016. Earnings data encompass calendar year (CY) 2011 through CY 2018.

(during summer or fall immediately following graduation, or spring of the year after) of expected high school graduation date during the observation period.

We measured college success several ways, using samples that included only students who matriculated to college within one year of their expected high school graduation during the observation period.

Persistence and Completion of Any Degree. First, we coded college success as a binary variable indicating whether students persisted from year 1 to year 2 (enrolling in the fall of year 2 after enrolling in the summer, fall, or spring of year 1). We made an *a priori* decision to omit students who earned a credential (typically a certificate) in year 1 and did not enroll in year 2 from this analysis, under the assumption that these students (n=37) differ from the rest of the sample in motivation, which is difficult to adjust for with matching and might slightly overstate the treatment effect. Robustness checks including these students revealed no significant differences from the reported results. The second college success measure was whether students completed any certificate, associate's, or bachelor's degree.

On-Time Completion. On-time college graduation was another measure of college success; we differentiated completion of a 2-year degree or certification from a 4-year degree. Students enroll in public two-year community colleges to pursue a variety of goals. Some want to learn specific skills but not necessarily earn a credential. Others may want to obtain an industry-recognized certificate, take remedial courses to prepare for further postsecondary education, or take courses to prepare for transfer to a four-year college or university. In short, the goal for many community college students is not an associate's degree, and many who earn an associate's degree will seek a four-year degree. Thus, on-time completion was assessed as a binary variable (yes/no) using different samples, each analyzed with a separate model: (1) Earning a credential within two years of expected high school graduation date. This sample included students who initially enrolled in a 2-year institution and excluded students who started at a 2-year college and transferred to a 4-year institution within two years of their expected high school graduation date. Students earning any certificate or associate's degree within two years were classified as "yes", while those who did not were "no". (2) Earning a four-year degree within four years of expected high school graduation date. This sample included students who initially enrolled in a 4-year institution and those who enrolled in a 2-year college but transferred to a 4-year institution within two years of expected high school graduation. The sample also included those who initially enrolled in a 2-year college, earned a credential, and enrolled immediately in a 4-year institution. Students who earned a 4-year degree within four years were coded as "yes", and those who did not were "no."

Earnings. Both the CDHE and CDLE data sets provide social security numbers (SSN), which allows the departments to link data and examine earnings for students who attended a postsecondary institution in Colorado. However, the CDE data do not contain SSN. Thus, given the availability of data for the present study, the only opportunity to identify earnings outcomes for students was for cases in which the student matriculated to college. For this reason, the earnings analysis was limited to students who attempted at least one postsecondary course within a year of their expected high school graduation. Also, the sample evaluating earnings only included students with a SSN found in the CDLE unemployment insurance (UI) database, which requires them to have been employed in Colorado. The UI data, however, does not include all Coloradans in the labor force, omitting some in agricultural employment, military and federal civilian employment, railroad employment and those who are self-employed. Earnings five years after students' expected high school graduation date was a continuous variable. Quarterly data were summed for the fifth calendar year after a student's expected high school graduation date (e.g., earnings were summed from Q1 2017 to Q4 2017 for students whose expected high school graduation date was May 2012). We used fifth year earnings because it gave sufficient time for students to graduate from both 2-year and 4-year programs on time and return to the workforce.

Analysis Model

We assessed confirmatory questions using multi-level, or mixed effects, regression models. Mixed effects logistic regression models were used to estimate the odds of binary outcomes, with linear regression used to model our continuous outcome (earnings), while allowing us to simultaneously adjust for clustering (by including random intercepts for high schools) and student-level characteristics. Alternative model specifications including Poisson and zero-inflated negative binomial regressions were examined for the continuous outcome, though the simpler mixed-effects linear regressions provided the best model fit. Student-level controls included English language learner (ELL) status, urbanicity, income (FRL, free/reduced lunch), race, and ninth grade reading scores. Models assessing "any" credential attainment further included cohort (using 2011 as reference category) and tests for on-time degree attainment, while models assessing earnings included controls for both cohort (2011 referent) and gender (with female as the referent). We used the Bonferroni method of adjustment for multiple comparisons when running the confirmatory models. The basic analytic model was as follows.

$$Y_{ij} = \gamma_{00} + \gamma_{10}(PRE_{ij}) + \gamma_{20}(ELL_{ij}) + \gamma_{30}(Urbanicity_j) + \gamma_{40}(FRL_{ij}) + \gamma_{50}(Race_{ij}) + \gamma_{60}(Treatment_{ij}) + u_j + e_{ij}$$

Where:

- Y_{ij} is the outcome for student *i* in school *j*
- PRE_{ij} is the student ninth grade reading standardized test score
- ELL_{ij} is 1 if student *i* in school *j* is an English Language Learner, 0 otherwise
- $Urbanicity_j$ is 1 if students belonging to school *j* attended a rural school in 11th grade, 0 otherwise
- FRL_{ij} is 1 if student *i* in school *j* is low income (as measured by Free or Reduced Lunch price status), 0 otherwise
- $Race_{ij}$ is 1 if student *i* in school *j* is non-Asian or non-White, 0 otherwise
- $Treatment_{ij}$ is 1 if student *i* in school *j* is in treatment, 0 otherwise

And:

- γ_{00} is the student-level intercept
- γ_{10} is the effect of student level pretest, which is fixed at level 2
- γ_{20} is the effect of ELL status, which is fixed at level 2
- γ_{30} is the effect of Urbanicity, which is fixed at level 2
- γ_{40} is the effect of FRL status, which is fixed at level 2
- γ_{50} is the effect of Race, which is fixed at level 2
- γ_{60} is the treatment effect, which is fixed at level 2
- Together, $\gamma_{00} + u_j$ represents the random intercept for school membership
- e_{ij} is the error associated with student *i* in school *j*, with mean 0 and conditional variance ^2e
- u_j is the error associated with school *j*, with mean 0 and conditional variance ^2u
- σ2e is the unexplained variation at level 1
- σ2u u is the unexplained variation at level 2

In addition, for each confirmatory analysis we calculated marginal effects to assist with interpretation. For binary outcomes, these marginal effects give the predicted probability of the outcome for both treatment and comparison students while holding all other covariates constant at their means. With the continuous outcome, the marginal effects display the predicted level of earnings in dollars with all covariates at their means, by condition.

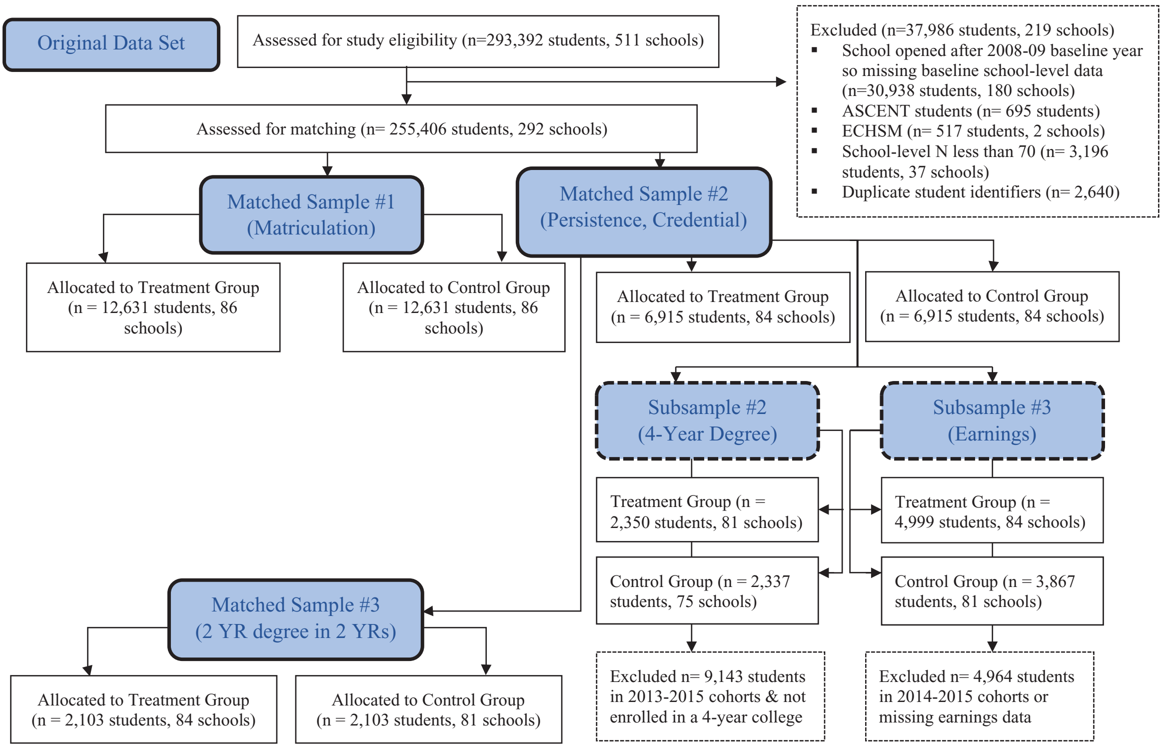
We ran separate, exploratory tests for moderation of effects on access, persistence, and attainment of "any degree" by including interaction terms between DE participation and (1) race and ethnicity; (2) free and reduced lunch (FRL; income) status; and (3) ninth grade reading test scores. We ran the same exploratory analyses for on-time degree completion and earnings, but also tested potential moderation effects of gender on both outcomes and credential/degree attainment on the earnings outcome. This resulted in 22 moderator analyses.

Results

Samples and Baseline Equivalence

Figure 1 displays the flow of students and schools through each of the various analytic samples using an adapted CONSORT diagram for randomized trials (Schulz et al., 2010). The initial student-level data set linking secondary records to postsecondary and earnings records included 293,392 students nested within 511 schools. A total of 37,986 students and 219 schools failed to meet inclusion criteria and were dropped prior to any matching. This number includes 695 ASCENT students (i.e., students

Figure 1. *Flow Chart Adapted from the CONSORT Diagram for Randomized Trials (Schulz et al., 2010)*



Note: ASCENT – Accelerating Students through Concurrent ENrollmenT; ECHSM – Early College High School Model. NSC – National Student Clearinghouse. "Matched Sample" refers to the analytic sample of students who met inclusion criteria for the research question and were retained in the sample after using propensity score matching at the student level.

who attended high school in five years) and 362 students nested within two early college high schools "approved" while the study period was ongoing, 3,196 students in 37 schools with school-level sample sizes less than 70 across cohorts, and 2,640 student duplicate observations where identifiers appeared in more than one school during their eleventh-grade year and only one was chosen at random and retained. The remainder and majority of exclusions (30,938 students in 180 schools) lacked school-level data at baseline. As such, 255,406 students in 292 schools were eligible for matching and inclusion in the evaluation.

In all, there were three matched samples and two subsamples used to assess outcomes across the confirmatory outcomes. Though this complicates external validity, we chose to minimize bias by establishing baseline equivalence following What Works Clearinghouse guidelines for each of our five samples (ED, 2020). These improvements in internal validity somewhat limit generalizing findings to the wider population of high school students in Colorado. Table 2 reports descriptive statistics and baseline equivalence for the initial, eligible sample, as well as for the analysis

Table 2. *Baseline Equivalence of Initial and Matched Samples*

	Initial Sample - Excluding Ineligible Schools and Students				**Matched Sample 1 - Matriculation**				**Matched Sample 2 - Year 1 to Year 2 Persistance, "Any" Credential**			
	Overall	**Treatment**	**Control**	**Trt vs. Cnt**	**Overall**	**Treatment**	**Control**	**Trt vs. Cnt**	**Overall**	**Treatment**	**Control**	**Trt vs. Cnt**
Baseline Characteristics	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Effect Size**	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Effect Size**	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Effect Size**
School Level												
% FRL	0.30	0.35	0.25	0.29	0.35	0.41	0.30	0.29	0.35	0.41	0.29	0.32
College-Going Rate	0.55	0.56	0.54	0.05	0.54	0.55	0.53	0.05	0.55	0.56	0.55	0.02
Urban	0.55	0.35	0.76	-1.07	0.59	0.59	0.59	0.00	0.60	0.58	0.61	0.08
Test Score Average	665.67	666.22	665.13	0.05	664.82	664.70	664.95	0.01	665.59	665.14	666.04	0.04
	(22.69)	(16.58)	(27.52)		(21.72)	(19.27)	(24.04)		(21.50)	(19.27)	(23.62)	
Student Level												
Racial and Ethnic Minority	0.34	0.40	0.39	0.03	0.40	0.40	0.40	0.00	0.34	0.36	0.33	0.08
FRL Status	0.30	0.37	0.34	0.08	0.38	0.37	0.38	0.03	0.32	0.33	0.32	0.03
ELL Status	0.07	0.04	0.07	-0.36	0.05	0.04	0.05	0.14	0.03	0.03	0.03	0.00
Test Score	666.03	677.01	660.78	0.35	676.21	677.01	675.41	0.04	683.48	683.87	683.08	0.02
	(49.99)	(39.68)	(51.36)		(42.37)	(39.68)	(44.89)		(39.14)	(36.65)	(41.47)	
Cohort												
2010-2011	0.20	0.17	0.20	-0.12	0.17	0.17	0.17	0.00	0.23	0.23	0.23	0.00
2011-2012	0.20	0.19	0.20	-0.04	0.19	0.19	0.19	0.00	0.25	0.25	0.25	0.00
2012-2013	0.20	0.20	0.20	0.00	0.20	0.20	0.20	0.00	0.25	0.25	0.25	0.00
2013-2014	0.20	0.21	0.20	0.04	0.21	0.21	0.21	0.00	0.27	0.27	0.27	0.00
2014-2015	0.20	0.23	0.20	0.11	0.23	0.23	0.23	0.00				
School N	292	146	146		172	86	86		168	84	84	
Student N	255406	12631	118111		25262	12631	12631		13830	6915	6915	

Notes: "Matched Sample 3" refers to the analytic sample of students who matriculated to a 2-year college within one year of expected high school graduation, met inclusion criteria for the 2-year degree in 2 years research question, and were retained in the sample after propensity score matching at the student level. Urban (1 = rural; 0 = urban/suburban); ELL – English Language Learner (ELL = 1, non-ELL = 0); FRL – Free & Reduced Lunch, a proxy for low-income (FRL = 1, non-FRL =0); Racial and Ethnic Minority (White/Asian = 0; All other = 1).

Table 3. *Degree Attainment and Earnings Analysis Sample Descriptive Characteristics*

	Matched Sample 3 - 2-Year Degree in 2 Years				**Subsample of Matched Sample 2 - 4 Year Degree in 4 Years***				**Subsample of Matched Sample 2 - Earnings 5 Years Post-Expected HS Grad+**			
	Overall	**Treatment**	**Control**	**Trt vs. Cnt**	**Overall**	**Treatment**	**Control**	**Trt vs. Cnt**	**Overall**	**Treatment**	**Control**	**Trt vs. Cnt**
Baseline Characteristics	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Effect Size**	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Effect Size**	**Mean (SD)**	**Mean (SD)**	**Mean (SD)**	**Effect Size**
School Level												
% FRL	0.35	0.41	0.29	0.32	0.35	0.41	0.28	0.35	0.35	0.41	0.29	0.32
College-Going Rate	0.55	0.56	0.55	0.02	0.58	0.57	0.58	-0.02	0.56	0.56	0.56	0.00
Urban	0.61	0.58	0.63	-0.13	0.58	0.57	0.60	-0.07	0.59	0.58	0.60	-0.05
Test Score Average	665.67	665.14	666.72	-0.08	666.76	665.51	668.11	-0.12	665.77	665.14	666.43	-0.06
	(20.76)	(19.27)	(22.29)		(21.14)	(19.28)	(23.03)		(21.49)	(19.27)	(23.68)	
Student Level												
Race and Ethnic Minority	0.46	0.46	0.46	0.00	0.27	0.29	0.25	0.12	0.35	0.35	0.34	0.03
FRL Status	0.45	0.44	0.46	-0.05	0.25	0.27	0.23	0.13	0.32	0.32	0.33	-0.03
ELL Status	0.05	0.05	0.05	0.00	0.01	0.01	0.01	0.00	0.03	0.03	0.03	0.00
Test Score	661.92	664.14	659.69	0.12	694.65	692.60	0.01	-0.12	683.58	685.01	681.72	0.08
	(36.44)	(36.62)	(36.14)		(35.83)	(33.16)	(38.23)		(38.49)	(36.68)	(40.65)	
Cohort												
2011	0.23	0.24	0.22	0.07	0.48	0.48	0.48	0.00	0.32	0.32	0.33	-0.03
2012	0.24	0.25	0.24	0.03	0.52	0.52	0.52	0.00	0.34	0.34	0.33	0.03
2013	0.26	0.26	0.27	-0.03					0.34	0.35	0.34	0.03
2014	0.26	0.25	0.27	-0.06								
2015												
School N	165	84	81		156	81	75		165	84	81	
Student N	4206	2103	2103		4687	2350	2337		8866	4999	3867	

* = This analysis sample includes the same matched sample used to assess year 1 to year 2 persistence from Table 3 (n=13,830) but drops all students in cohorts 2013-2015 and those who did not enroll in a 4-year college within 2 years of expected high school graduation. Data were not re-matched because baseline equivalence was maintained.

+ = This analysis sample includes the same matched sample used to assess year 1 to year 2 persistence from Table 3 (n=13,830) but drops all students in cohorts 2014-2015 or missing earnings data from the Colorado Department of Labor and Employment. Data were not re-matched because baseline equivalence was maintained.

matched samples used to evaluate matriculation, college persistence, and "any" credential completion. Table 3 reports these same statistics for outcomes related to on-time degree attainment and earnings. "Matched sample" refers to an analytic sample who met inclusion criteria for the research question and were retained in the sample after using propensity score matching at the school and student levels, as described previously. Samples designated "subsample" were comprised of a subset of students matched in an earlier, related analysis sample who were also eligible for a different outcome analysis and retained baseline equivalence without needing further matching since baseline equivalence was established following WWC guidelines. An easy way to differentiate these samples is that the three matched samples contain balanced numbers of treatment and control students, while this is not guaranteed for the two subsamples.

For the first and largest matched sample used to assess treatment impacts on matriculation, of 255,406 students eligible for matching, 25,262 were retained, 12,631 students per condition nested in 172 schools (86 per condition). A second matched sample of 13,830 students (6,915 per condition) in 168 schools (84 per condition) who matriculated to college within one year of their expected high school graduation date were drawn from the initial, eligible sample of 255,406 to examine college persistence and completion. From these, a second (n=4,687) and third (n=8,866) subsample were used to assess differences in on-time 4-year degree attainment and earnings, respectively. From the second matched sample (i.e., matriculaters), we drew a final matched sample (n= 4,206, with 2,103 per condition) to assess impacts on earning a 2-year degree in two years, which is unbalanced at the school level (n= 84 treatment, 81 comparison) because school propensity scores were less salient matching criterion for attaining balance in this sample than student-level characteristics (see Figure 1).

Postsecondary Access, Persistence, and Completion of "Any" Degree

Results show positive impacts of treatment on rates of college enrollment (*OR* = 3.06), persistence (*OR* = 1.30), and degree attainment (*OR* = 2.08) (Table 4). Marginal effects reveal that 77 percent of treatment students matriculated within one year of their expected high school graduation date compared to 52 percent of controls. For those who matriculated, 82 percent of students in the treatment group persisted from the fall of year 1 to the fall of year 2 compared to 77 percent of the control group. In addition, 37 percent of the treatment group that matriculated within one year of their expected high school graduation earned a credential compared to 22 percent of the control group. Exploratory analyses revealed no significant moderation effects of the treatment by income or minority status for any outcomes, but did demonstrate a statistically significant, negative interaction effect for achievement on matriculation. There were no subgroup effects for persistence and college completion. That is, treatment was shown to have somewhat stronger benefits for students with average to slightly below average achievement on matriculation, but the treatment helped all students equally to persist and earn a credential.

On-Time-Completion and Earnings

Findings were also positive and in favor of the treatment group, with medium-to-large effect sizes detected for on-time completion, as measured by earning a two-year credential in two years (*OR* = 2.87) and earning a four-year degree in four years (*OR* = 1.61; see Table 5). For students who matriculated to college within one year of their expected high school graduation date, 13 percent of students in the treatment group earned a two-year degree or credential within two years compared to 5 percent of

Table 4. *Hierarchical Linear Models Predicting Postsecondary Outcomes (Confirmatory Analyses)*

	Matriculation		**Year 1 to Year 2 Persistence**		**"Any" Credential**	
Covariates	**OR**	**(95% CI)**	**OR**	**(95% CI)**	**OR**	**(95% CI)**
School-Level Characteristics						
Treatment	3.06 ***	(2.86, 3.29)	1.30 ***	(1.17, 1.44)	2.08 ***	(1.89, 2.29)
Urban	0.97	(0.81, 1.16)	1.37 ***	(1.19, 1.59)	0.98	(0.85, 1.13)
Student-Level Characteristics						
Test Score	1.92 ***	(1.85, 1.99)	1.54 ***	(1.47, 1.61)	1.28 ***	(1.23, 1.34)
ELL	1.04	(0.90, 1.21)	1.57 **	(1.23, 2.00)	1.25	(0.96, 1.63)
FRL	0.70 ***	(0.65, 0.75)	0.66 ***	(0.60, 0.73)	0.81 ***	(0.73, 0.89)
Racial and Ethnic Minority	1.01	(0.94, 1.08)	0.96	(0.87, 1.06)	0.92	(0.84, 1.02)
Expected Graduation Cohort (vs. 2011)						
2012					0.83 **	(0.75, 0.92)
2013					0.41 ***	(0.37, 0.46)
2014					0.15 ***	(0.13, 0.17)
Student Intercept	1.28		3.17		0.68	
School Intercept σ	0.52		0.31		0.32	
BIC	28502		14019		15485	
ICC	0.08		0.03		0.03	
Student N	25,262		13,830		13,830	
School N	172		168		168	
Treatment Eff. Size	0.68		0.19		0.44	

Bonferroni-adjusted Significance Levels: * $p < .015$; ** $p < .0025$; *** $p < .00025$

Note. OR = Odds Ratio, 95% Confidence Intervals for Odds Ratios are calculated using the modified Wald method; Urban (1 = rural; 0 = urban/suburban); ELL – English Language Learner (ELL = 1, non-ELL = 0); FRL – Free & Reduced Lunch, a proxy for low-income (FRL = 1, non- FRL =0); Racial and Ethnic Minority (White/Asian = 0; All other = 1).

students in the control group. In addition, 26 percent of students in the treatment group earned a four-year degree in four years compared to 18 percent of students in the control group. Benefits of the treatment extended to earnings, as well. Five years after their expected high school graduation date, treatment students had on average significantly higher earnings across four quarters compared to control students ($g = .08$; $15,767.45 in treatment vs. $14,377.98 in control). Moderation analyses revealed no statistically significant results of differential impacts of the treatment by student characteristics.

Discussion

We compared eleventh and twelfth grade students who participated in Colorado's state-regulated DE program to eleventh and twelfth grade students who did not participate. Students matriculated within one year of their expected high school graduation date at a rate of 25 percentage points higher than control students. For those

Table 5. *Hierarchical Linear Models Predicting Time-To-Degree and Earnings (Confirmatory Analyses)*

	2-Year Degree Attainment in 2 Years		**4-Year Degree Attainment in 4 Years**		**Earnings 5 Years Post-Expected HS Grad ($)**	
Covariates	**OR**	**(95% CI)**	**OR**	**(95% CI)**	**B**	**(95% CI)**
School-Level Characteristics						
Treatment	2.87 ***	(2.26, 3.64)	1.61 ***	(1.37, 1.89)	1389.47 **	(588.11, 2185.91)
Urban	0.48 ***	(0.36, 0.63)	1.08	(0.88, 1.31)	940.27	(-41.39, 1924.07)
Student-Level Characteristics						
Test Score	1.25 **	(1.10, 1.41)	1.67 ***	(0.29, 0.52)	386.54	(-25.79, 796.61)
ELL	0.76	(0.39, 1.47)	1.15	(0.53, 2.50)	1927.86	(-449.90, 4311.57)
FRL	0.77	(0.61, 0.97)	0.71 **	(0.59, 0.87)	-189.73	(-1084.47, 711.92)
Race and Ethnic Minority	0.98	(0.77, 1.25)	0.85	(0.71, 1.03)	342.47	(-544.20, 1234.30)
Expected Graduation Cohort (vs. 2011)						
2012	0.77	(0.57, 1.03)	1.12	(0.97, 1.29)	-474.35	(-1375.69, 428.96)
2013	0.84	(0.63, 1.11)			863.73	(-34.37, 1764.58)
2014	0.79	(0.59, 1.06)				
Student Intercept	0.77		0.39		10907.10	
School Intercept σ	0.43		0.29		1417.70	
BIC	2686		4969		198402	
ICC	0.05		0.03		0.01	
Student N	4206		4687		8866	
School N	165		156		165	
Treatment Eff. Size	0.63		0.29		0.08	

Bonferroni-adjusted Significance Levels: * $p < .017$; ** $p < .0033$; *** $p < .00033$
Note: OR = Odds Ratio, 95% Confidence Intervals for Odds Ratios are calculated using the modified Wald method; Urban (1 = rural; 0 = urban/suburban); ELL – English Language Learner (ELL = 1, non-ELL = 0); FRL – Free & Reduced Lunch, a proxy for low-income (FRL = 1, non-FRL =0); Racial and Ethnic Minority (White/Asian = 0; All other = 1).

who matriculated, compared to the control group, students in the treatment group persisted from the fall of year 1 to the fall of year 2 at a rate of five percentage points higher and earned a credential at a rate of 15 percentage points higher. Findings are consistent in magnitude and effect to the studies that meet standards with reservations according to the Department of Education's What Works Clearinghouse that also utilized quasi-experimental matching methods instead of experimental methods (see An, 2013; Giani et al; 2014; Struhl & Vargas, 2012).

This study also adds to the literature examining on-time degree completion. Compared to control, treatment students earned a two-year degree or credential within two years and a four-year degree in four years at a rate of eight percentage points higher. Similarly, Miller et al. (2018) found DE significantly increased the likelihood of graduating from a four-year college within six years by 2.6 percentage points (smaller than our results, but over a longer follow-up period where the authors note that treatment effects faded over time). Benefits also extended to earnings. Five years after high school, treatment students had (on average) significantly higher earnings across four quarters ($15,767.45) compared to control students ($14,377.98).

Subgroup analyses demonstrated that DE did not disproportionately improve outcomes for traditionally underrepresented groups, as would be necessary to eliminate disparities in college completion rates; instead, similar treatment effects were observed across groups. This contributes to a body of quasi-experimental studies that find mixed results on whether DE reduces inequity. Miller et al. (2018) found that low-income students and students of color did not benefit as much from DE delivered in traditional high school settings when compared with non-low-income and White students. Taylor (2015) and Speroni (2011) found that DE policies positively affected all students, but low-income and students of color displayed smaller effect sizes for some outcomes. An (2013) found that first-generation DE participants (i.e., students with parents who did not attain a 4-year college degree) were seven percent more likely to earn a bachelor's degree compared with first-generation control students, though treatment explained less than one percent of the variance after controlling for other factors. Taken together, these studies indicate that DE does little to explicitly reduce existing educational inequities by race or income.

Limitations

Despite matching on observable variables at both the student and school levels, it is possible that unobserved factors associated with, for example, students' academic and career expectations and aspirations and parents' education influence students' selection into the treatment and are correlated with college success (Taylor, 2015). While randomly assigning subjects to condition is the most effective approach for eliminating unobserved bias (Shadish et al., 2002), an experimental study was not feasible since DE is a universal policy in Colorado. However, by ensuring that students were never drawn from the same school and defining the treatment at both the school and student-level, we attempted to minimize confounding from the self-selection of comparison students out of DE courses. Still, we cannot rule out the role unobserved characteristics played on selection into treatment and outcomes.

A second limitation relates to a common critique of propensity score matching (PSM), which is that the matched sample no longer represents the population from which the data were initially drawn and for whom the treatment is intended (Steiner & Cook, 2013). Indeed, in our largest matched sample, we only assess differences in outcomes for 10 percent of all eligible students. We attempted to dispel this concern in three ways. First, we documented the analytic samples via a modified version of the CONSORT diagram (Schulz et al., 2010) in Figure 1. For each of these samples, we also reported descriptive statistics and baseline equivalence, which readers can compare to the corresponding traits from the full, eligible sample in Tables 3 and 4. Finally, we only performed additional matching, when necessary, to meet What Works Clearinghouse standards (ED, 2019). Still, we acknowledge that these improvements in internal validity, which are rare in studies using PSM, come at some cost to external validity.

The third limitation is the decision to base the sample on cohorts of eleventh graders, since some students take dual credit as early as ninth grade. However, most Colorado high school students take DE courses starting in eleventh grade (Colorado Department of Higher Education & Colorado Department of Education, 2021). For those who take DE courses prior to eleventh grade, the majority would also take DE courses during the eleventh or twelfth grade and thus would be classified as treatment participants in the present study. This decision is consistent with other DE evaluation studies (Miller et al., 2017, 2018).

Fourth, this research does not answer critical questions about the mechanisms by which DE affects outcomes. We examined DE dichotomously (i.e., participated/did not participate) because Colorado does not collectively store student-level data categorizing DE courses completed by modality (i.e., taught online, face-to-face, or hybrid), location (on a college campus or high school campus), course content (academic or vocational), instructor of record (high school faculty or college faculty), or class composition (only high school students or a mix of high school and college students). Understanding how implementation influences outcomes is important research that some (e.g., Villarreal, 2018) have begun to study.

And finally, state policies vary in specifying components of DE, the articulation of college credits earned, funding requirements, and student populations and participant supports, making findings difficult to generalize across states. This limits external validity and caution should be exercised if applying these findings beyond the current sample.

Future Research

DE programs were initially designed for high-achieving students (Tobolowsky & Allen, 2016) and continue to be most readily available nationwide in schools that serve predominantly White, high SES students (Thomas, et al., 2013; Xu et al., 2021; Rivera et al., 2019). As high school students are provided with accelerated postsecondary pathways via DE – a trend that is increasing – the potential to do so equitably, however, is largely unknown (Taylor, 2015; Rivera et al., 2019). Further study is needed to examine whether or how DE reduces inequity.

Except for Miller et al (2018), prior studies have largely failed to consider the role of academic achievement (An & Taylor, 2019), so accounting for this variable is an important contribution of this paper. Ours is also the first study we are aware of to examine the relationship between DE and earnings. As such, replication and long-term follow-up are particularly important to (1) confirm our finding that the benefits of Colorado's state-regulated DE program do not vary by baseline academic achievement level; and (2) explore the mechanisms underlying the impact of the program on the earnings of young adults.

In addition, this study examined outcomes for immediate matriculation. Among Colorado's high school class of 2018, 57 percent enrolled in college after graduation (Colorado Department of Higher Education, 2020). Nationally, the immediate college enrollment rate for high school completers was 69 percent in 2018 (The Condition of Education, & National Center for Education Statistics, 2020). Future research will need to determine whether our findings hold for high school completers who delay matriculation or do not enter college (in terms of higher earnings).

Finally, because our dataset did not capture students' experience in DE, this study is a "black box" policy evaluation in that the results demonstrated effects but did not provide an explanation for those effects (Taylor, 2015). Variation in how DE is delivered (including modality, course content, instructor of record, and class composition) could clarify the different conclusions researchers have made about the effectiveness of DE. Mediation analyses (for example) will provide more explanations of the causal mechanisms upon which DE is based.

Conclusion

This study provided estimates on the overall impact of a DE state policy. Our results suggest that Colorado's DE policy (Witkowsky & Clayton, 2020) had substantive positive impacts on increasing progression through college and earnings regardless of racial and ethnic minority or income status, gender, and academic achievement. This is promising for broadly increasing college access and success, and as a potential pathway to economic independence for young adults. Many advocates, however, see DE as a strategy for improving outcomes for a diverse population of students, including racial and ethnic minority students and students from low-income families. Consistent with prior research (e.g., Taylor, 2015), this study underscores the need for more robust support for DE to have meaningful impact on reducing educational inequalities. Our results do not add to this discussion, though we did find more equitably favorable benefits of DE than prior studies.

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