Does Online Course-taking Increase High School Completion and Open Pathways to Postsecondary Education Opportunities?

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Abstract

Recent substantial increases in high school graduation rates have been linked anecdotally to the expansion of online course-taking for credit recovery. Online course-taking that supports high school completion could open new opportunities for postsecondary education pursuits. Alternatively, poorer quality online instruction could diminish student engagement and learning and discourage persistence toward graduation and further education. Using quasi-experimental methods with data from an eight-year longitudinal study of online course-taking in high schools, we find positive associations between online course-taking in high school and credits earned, high school graduation, and college enrollment. Our results leave open the question of whether online course-taking supports learning that will lead to longer-term postsecondary education and labor market success.

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Introduction

For more than a decade, compelled by the federal mandate under No Child Left Behind (NCLB) to report graduation rates, states have sought to identify policy levers for increasing high school graduation rates. The Every Student Succeeds Act (ESSA), passed in December 2015 to replace NCLB in governing K–12 public education policy in the U.S., continues this focus on high school graduation rates as a core academic performance indicator of federal and state public school accountability systems. The ESSA accountability system is also evolving, however, in that it provides states with greater flexibility to create a more "holistic" evaluation of school quality and student success. In addition to graduation rates and student achievement (standardized test scores), ESSA requires at least one other performance measure that is valid and reliable statewide, including, for example, measures of student engagement, access to advanced coursework, postsecondary readiness, or others that gauge students' ability to think critically and work collaboratively (Darling-Hammond et al., 2016). Moving to accountability systems with multiple indicators that allow for a broader assessment of the skills and competencies students need to succeed after graduating from high school could also help to deter the gaming of traditional performance measures (test scores and graduation rates), that is, efforts to "hit the target" that "miss the point" of increasing student learning.

Nationally, the most recently available graduation rate statistics (updated in January 2019) reported an adjusted cohort graduation rate (ACGR) for public high school students of 84.6 percent (for the 2016-17 school year), the highest rate since it was first measured in 2010–11 (at 79%) (Valentine, 2018). Concerns have been raised, however, about whether these trends reflect real advances in student learning and academic success, given that high school student performance on the National Assessment of Educational Progress (NAEP)[1] and Programme for International Students Assessment (PISA) has stagnated over this same period. In some states, the rise in high school graduation rates has been particularly dramatic (PISA, 2016). In Alabama, for example, the on-time graduation rate rose from 72 percent in 2010-11 to 86 percent in only three years (2013-14), and Florida reached another high in 2018, also with a graduation rate of 86 percent, representing an increase of more than 23 percentage points in a decade (from 62.7%), according to the state's department of education (Postal, 2018). Importantly, the latest increases in on-time graduation rates in Florida also narrowed the gaps between the performance of white students and black and Hispanic students, as well as for students from low-income families.

Some have linked the recent substantial increases in high school graduation rates to the proliferation of "credit recovery" programs, in which students repeat failed courses in an alternative (e.g., online) and sometimes abbreviated format (Dynarski, 2018; Malkus, 2018). For example, the Gadsden County School District in Florida, which had a 43 percent graduation rate in 2010, was searching for a way to more quickly boost its graduation rate. It turned to an online credit recovery program (EdOptions) to help students who had failed in-person classes to graduate on time, because "it was getting results" in other districts across the state (Kirsch, 2017). As Gadsen County increased its reliance on online credit recovery, its graduation rate rose to 68.4 percent in 2016. Other large metropolitan school districts, such as Nashville, Los Angeles and the District of Columbia, have likewise seen dramatic increases in their high school graduate rates (of more than 15-20 percentage points) after introducing online credit recovery programs (Kirsch, 2017; Malkus, 2018). Again, however, the lack of comparable, broad-based

[1] The NAEP is the largest ongoing, nationally representative assessment of student math and reading achievement. (See https://nces.ed.gov/nationsreportcard/).

increases in high school test scores where these programs have rapidly expanded has prompted the question of whether they are adding value to students' learning (The Economist, 2019). This is a serious concern, given that approximately three-fourths of U.S. high schools are now offering digital instruction opportunities to help students who have failed a course regain credit, stay on track for graduation, and complete their high school degree.

On the one hand, given that a high school degree is generally required to enroll in postsecondary education programs, online instruction that enables or supports high school degree completion could open opportunities that might not otherwise be available for student postsecondary education pursuits. Of three million high school completers in 2015, 69 percent enrolled in college by the following October, which represents an increase in the immediate college enrollment rate of six percentage points since 2000 (NCES)[2]. In addition, online course-taking typically offers options for flexible, "anytime, anywhere" access to instruction that may allow students who are struggling in traditional classrooms to make progress toward graduation in other settings. The online setting may also provide additional opportunities for individual counseling and goal-setting that could influence educational engagement and aspirations or help to accommodate the needs of students with disabilities (Darling-Aduana, Good & Heinrich, forthcoming). At the same time, the fact that students are often assigned to online course-taking after failing a course in a traditional classroom or being removed for behavioral problems raises concerns about ability grouping, which has been associated with unequal access to quality learning opportunities and increases in achievement gaps between high- and lower-achieving students (Brighouse, Ladd, Loeb & Swift, 2018). And if online instruction substitutes poorer quality digital instruction for better quality live instruction, student engagement and learning

[2] See: https://nces.ed.gov/fastfacts/display.asp?id=51.

could diminish, and students could be discouraged from persisting toward graduation or pursuing further education beyond high school.

We have undertaken a longitudinal study of the implementation of an online instructional program in a large, urban school district in the Midwest, which began offering online course-taking opportunities in 2010 primarily, but not exclusively, for high school students falling behind in their academic progress toward graduation (i.e., credit recovery). Nearly every high school in the district has enrolled students in online courses in at least one year over our study period. Further, by the 2016-17 school year, about 20 percent of all credits accrued in the district's middle and high schools were completed online, and 40 percent of graduating seniors had completed at least one course through the online course-taking system. The large-scale dataset that we have assembled in this study links technology vendor data to student school records—from 2010-11 to 2017-18—and provides information on students' online (and traditional) course-taking that allows us to construct detailed, student-level measures of the intensity, duration and types of online course-taking over time. We have also linked data from the National Student Clearinghouse (NSC) and the U.S. News and World Report (USNWR) that provide information on student participation in postsecondary education and the quality of institution attended (by several USNWR indicators). In addition, since 2015, we have also conducted more than 300 observations of student and classroom use of the online instructional program and more than 30 interviews with instructors and district staff.

We use these data to address the following key questions. First, does online course-taking (primarily for credit recovery) increase high school graduation rates? To better understand this relationship, we also explore associations between other measures of progress toward high school graduation (e.g., credits earned and grade point average) and online course-taking. Next, we examine whether there a link between online course-taking in high school and college enrollment. That is, if we find that online course-taking increases high school completion, does this in turn contribute to higher rates of postsecondary education enrollment? Do we see differences in where students enroll (e.g., 2-year vs. 4-year colleges and institutional quality) that relate to whether they took courses online in high school and their intensity of online course-taking?

In our analysis, we first examine the relationship of online course-taking to short-term measures of engagement and persistence in high school, including attendance, suspensions, course grades and grade point average and course credits earned. We then estimate the relationship between online course-taking and our primary outcomes of interest—high school graduation and college enrollment. We employ fixed-effect models, pooling data across the eight school years and adjusting for stable student, school, grade and year fixed effects. We also employ inverse probability weighting with regression adjustment, a double-robust estimator, to align the observed characteristics of online course-takers and non-users at baseline and assess the relationship of online course-taking (including intensity of use) to student short- and longer-term outcomes. In addition, we conduct several sensitivity tests of our model assumptions and compute nonparametric bounds for our estimates under less stringent assumptions about selection into treatment. Overall, we find positive associations between online course-taking in high school and credits earned (progression toward graduation), high school graduation, and college enrollment, although indicators of college quality suggest that high school students who took courses online entered postsecondary educational institutions of lower quality. Despite these gains, our results leave room for questioning whether online course-taking contributes to student learning that will lead to longer-term postsecondary education and labor market success.

Theoretical Perspectives Informing the Proliferation of High School Online Course-taking and Evidence on its Effectiveness

In framing this research, we draw on theoretical perspectives grounded in "new institutionalism" in education (Meyer & Rowan, 2006) to understand school district motivations for adopting and implementing online course-taking in high schools. Scholars bringing this theory to their investigations of educational organization and practice call attention to important changes in the political and social environments of public schools that have spurred demands for increased accountability for student outcomes, while reducing confidence in the public sector to deliver on them. NCLB, for example, followed on the broader new public management (NPM) reforms of the 1990s that encouraged the devolution of government responsibilities to the private sector (Hood, 1991) to promote flexibility, choice and accountability for results (Public Law 107–110—8 January 2002). Rowan (2006: 16) describes how this has prompted a "heightened concern with educational productivity" and the embracing of an increasingly "technical theory" of education that affords a growing role for private actors and "big business." He points to the private sector's expansion into home schooling, charter schools and supplemental educational services that are becoming an institutionalized part of public education.

We likewise argue that there is a new level of penetration by private actors into the "technical core" of public education, extending beyond the more limited roles of supplying standardized textbooks or tests to the provision of curricular content and the delivery of core course instruction in public schools. New institutionalism suggests that institutional reform of the technical core of public education is typically motivated by the identification of a performance problem (Rowan, 2006), such as the concern around low high school graduation rates that motivated annual reporting of graduation rates by states under NCLB. These early accountability efforts illuminated not only national graduation rates of 68 percent (of those entering 9th grade and graduating with a regular diploma in 2001), but also major disparities in the high school graduation rates of minorities (Blacks - 50%, American Indians - 51%, and Hispanics - 53%) and a lack of consistency and accuracy in the calculations (Orfield, Losen, Wald, & Swanson, 2004). The report by Orfield et al. (2004: 2) declared an educational crisis, in which the U.S. education system was allowing a "dangerously high percentage of students to disappear from the educational pipeline before graduating from high school." Moreover, their report described students who felt "pushed out" of high school because of their poor performance on standardized tests or the severe problems they were experiencing outside of school that made it difficult for them to progress toward graduation, with these glaring inequalities contributing to a national *civil rights* crisis.

Described as a "byproduct" of the NCLB reforms, credit recovery programs began proliferating after the passage of NCLB, with the basic objective to provide students who were falling behind academically the opportunity to "recover" credits through primarily online options (McCabe & St. Andrie, 2012). Through the lens of new institutionalism, credit recovery is essentially a technical fix for the problem of high school students lagging in their accumulation of credits needed for graduation. While there is no federal or uniform state definition of what constitutes credit recovery and minimal oversight of the burgeoning programs, McCabe and St. Andrie identified one of the clearer definitions of credit recovery in the North Carolina State Board of Education's Policy Manual, which characterizes credit recovery as "a block of instruction that is less than the entirety of the Standard Course of Study for that course," with the length of the credit recovery course not fixed by "seat time" but rather "dictated by the skills and knowledge the student needs to recover.[3]"

Public monies have been made available for the expansion of credit recovery via Title I funding, the Individuals with Disabilities Education Act (IDEA), Enhancing Education Through Technology (EETT), and other federal funding (e.g., American Reinvestment and Recovery Act). These funds have largely been diverted to contracts with private educational companies such as Apex Learning, Edgenuity and Pearson Education that are supplying the surging demand for online credit recovery programs, particularly in large, urban school districts such as Los Angeles (LA) Unified, Chicago Public Schools, Houston Independent School District, Miami-Dade and others (Clough, 2016a). With ensuing record increases in high school graduation rates—for instance, LA Unified's achievement of a 75% graduation rate in the 2015-16 school year after a 54% rate was projected in fall 2015 (Hansen, 2017)—growing concerns about the quality of education provided through these online courses and the absence of monitoring and regulation have been raised (Ahn & McEachin, 2017; Heppen, Sorenson, Allensworth, Walters, Rickles, Stachel Taylor, & Michelman, 2017; Heinrich, Darling-Aduana, Good and Cheng, forthcoming). The International Association for K-12 Online Learning (Powell, Roberts & Patrick, 2015: 10) was particularly blunt in its criticism of online credit recovery programs, noting that they are "low-cost, have very low levels (if any) of teacher involvement, and require very little of students in demonstrating proficiency. They are used primarily because they are inexpensive, and they allow schools to say students have 'passed' whether they have learned anything or not." Still, while acknowledging some unease about the fast pace of credit recovery and accelerating graduation rates, state and local educational agency leaders are mostly defending their use.

[3] https://www.nrms.k12.nc.us/cms/lib011/NC01800012/Centricity/Domain/64/gcs_m001.pdf

Former Texas Education Commissioner, Robert Scott, remarked that “any tool that helps get kids credit toward graduation is certainly worth having” (Thevenot & Butrymowicz, 2010), and LA Unified’s Chief Academic Officer, Frances Gipson, argued that “whether it’s online or any other credit recovery course, it’s the same” (Clough, 2016b).

Gipson highlights a key question: is a credit a credit, no matter how it is attained, or should we be concerned about whether shunting students into credit recovery reduces their quality of learning opportunities and later outcomes (i.e., beyond graduation)? The most rigorous evidence to date from an experimental study of online course-taking for recovery of algebra credits (vs. a face-to-face option) in Chicago Public Schools found that students in the online course had significantly lower end-of-course posttest scores and lower credit recovery rates compared to those in the face-to-face course (Heppen et al., 2017). Similarly, in a comparative interrupted time-series of North Carolina credit recovery programs, Viano (2018) a found that online credit recovery course offerings were associated with a decline in student test scores and graduation rates. That said, if the alternative to credit recovery programs is pushing students out of high school, as Orfield et al. (2004) suggested, and large, resource-constrained urban school districts are unable to bolster blended learning and instructional supports, reduce class sizes, and undertake other measures to improve student progress toward graduation, then credit recovery programs that “fix” the performance problem—move students to graduation and reduce disparities in graduation rates—may be the most cost-effective option available to these school districts. Furthermore, school districts lose state funding when students drop out or leave for alternative programs outside the district. But as Burch (2009) argued, the new institutionalism underlying many recent policy reforms—including those contributing to the rise of online credit recovery—is an inadequate lens for attending to questions about equity and social justice, such as how the problem of low and disparate high school graduation rates is rooted in deeper societal and economic inequalities. She cautions against settling for "simplistic" solutions offered by the market to complex challenges in which "too many children in communities of color are lost" in our public education system (2009: 19).

Study Samples, Data and Measures

Approximately one quarter of high school students access course instruction online in a given year in the urban school district we are studying, up from about five percent of all high school students in the first year (2010-11) the online instructional program was used. Across the district, we found that the proportion of high school students taking online courses in any one of the 46 high schools (during our study period) ranged appreciably both between and within high schools over time (e.g., from zero to more than 93 percent). Our interviews with district staff and teachers suggested that school-level administrative and staffing decisions, in conjunction with the types of student bodies served, were some of the most important factors in determining which and how many students were directed to take courses online (Heinrich et al., forthcoming). For example, in alternative high schools, about a third of the student body was taking courses online, and the rate of online course-takers was more than 80-90 percent in some of these schools, such as those serving pregnant and parenting teens and students returning to the classroom from incarceration or expulsion. In our prior research, we found that measures of student educational performance, particularly student course failures in the preceding school year, were the strongest individual predictors of online course-taking in our study district.

We link the student record data provided by the district for high school students from the 2010-11 through 2017-18 school years to data from the vendor of the online instructional program for this same period, matching about 85 percent of the cases on average.[4] This particular technology vendor provides online courses to school districts in all 50 states, including eight of the 10 largest districts in the nation, which use the program primarily for credit recovery (Clough, 2016b). The vendor data include detailed information on students' online courses and their engagement with the online instructional system (for each session a student logged in), as well as measures of their course progress, completion and online course grades. The student record data include demographic information, absences and suspensions, course credits earned and grade point average (GPA), ACT scores, and standardized test scores. We also merged data on school characteristics, including school type, geographic location and others that are made publicly available on the district's website.

Our (outcome) measures of progress toward graduation are defined as credits earned and GPA at the end of the academic year. We also examine two measures of academic achievement, reading and math standardized test scores (scaled scores from spring MAP and STAR assessments).[5] We use a measure of high school graduation that is not limited to four-year (on-time) graduation but captures graduation as reported in the district student records. College enrollment (in 2-year and 4-year colleges) measures were obtained through our study district from the NSC data, which is currently the most comprehensive national student-level college enrollment data available.[6] In addition, the USNWR provided detailed information on its college quality indicators, including for national universities, national liberal arts colleges,

[4] In related work (Heinrich, Darling-Aduana, Good & Cheng, forthcoming), we show that the subsample of data with matched student record-technology vendor data is representative of all students taking courses online in this school district.
[5] MAP and STAR are nationally normed standardized assessments that the school district administers locally in certain grades. Because the district transitioned form MAP to STAR during the course of the study, and to aid interpretation, we used standardized scores in the analysis as a means of equating scores from one year to the next.
[6] To track how many of their students go on to college and where, high schools use StudentTracker® reports from the NSC Research Center, which were created to enable schools to measure their effectiveness in supporting student postsecondary education success: https://nscresearchcenter.org/workingwithourdata/.

regional universities, regional colleges and historically black colleges and universities, as well as for first tier, second tier and unranked institutions. We use the measures of overall institutional quality (scores), peer institutional ratings, freshman retention rate, and graduation rate, and we also created an indicator variable for whether a given postsecondary institution is included the USNWR database.[7]

In examining the relationship between online course-taking in high school and progress toward graduation, high school completion, college enrollment and institutional quality, we construct two treatment-comparison samples for our analyses. In both sets of analyses, our identification of the average effect of online course-taking comes from students who switch to or from online course-taking (from or to traditional course-taking) from one year to the next in high school, which is the case for about a quarter of the students in our sample. In these analyses, we aim to adjust for factors that influence selection into online course-taking, including student demographics; prior year course performance and failures, credits earned, GPA and standardized test scores; student absences, and information on school characteristics that are associated with the availability of online course-taking on campus. Because course failure was the strongest predictor of online course-taking by high school students in this district (consistent with a credit recovery focus), we also estimate our models on a subsample consisting only of students who failed a course in the prior year, where some go on to repeat the course online (35 percent in our sample) and others do not. For courses required for graduation, which describes the majority of cases in our sample, students who did not repeat the course online repeated the course in a face-to-face instructional environment. Table 1 presents descriptive

[7] There are many well-known limitations to describing the USNWR indicators as *quality* measures of postsecondary institutions, including the concern that they can be "gamed" by institutions that take actions to raise their measured performance without increasing quality. For a more in-depth discussion, see: O'Neill, Cathy. *Weapons of Math Destruction*. New York: Broadway Books, 2016.

statistics on our analysis samples, with the first two columns comparing the characteristics of all high school students in this district to online course-takers, and the second panel showing the characteristics of the subsample of those who failed a course (both online course-takers and those who did not attempt recover a course online).

With sizeable samples, we observe mostly small yet statistically significant differences between students taking courses online and those not taking courses online in high school across most characteristics shown in Table 1. Comparing all high school students, the largest differences are the (higher) percentages of Black, low-income and special education-eligible students taking courses online; the lower proportions of ELL, Asian and 10th grade students taking courses online, and lower GPAs, more absences and higher rates of course failure among those in online courses. When we restrict the sample to students who had failed a course in the prior year, most of the differences between the students taking courses online (vs. not) are considerably smaller, and many of the differences are no longer statistically significant as well. In addition, there is now a lower proportion of students with special educational needs taking courses online (vs. not online) in a given year (in the sample limited to students who have failed a course in the prior year).

As shown in Figure 1.a, the proportion of high school students in the district that failed a course in the prior academic year is high (nearly two-thirds in 2011-12) but generally declining over time (to about 56% in the 2017-18 school year). At the same time, four-year cohort high school graduation rates[8] closely parallel the trend in course failure, albeit moving in the opposite direction, with a sharper rise in high school completion following the 2014-15 school year. Over this same period, the percentage of high school students taking courses online in a given year

[8] https://wisedash.dpi.wi.gov/Dashboard/portalHome.jsp.

was increasing—to 18 percent in 2011-12 (from 5% the previous year) to approximately a quarter of high school students in 2012-13—and then ranging between 25-30 percent through the 2017-18 school year. Figures 1.b and 1.c show that average credits earned in a school year and student grade point averages among high school students in this district were likewise largely increasing over the study period. In effect, the trends we observe in student progress toward graduation—in credit accumulation, in particular—appear consistent with the anecdotal evidence suggesting that online course-taking (primarily for credit recovery) may be associated with the rise in high school graduation rates. We now turn to our analysis that attempts to assess whether this relationship is plausibly causal, that is, whether the expansion in online course-taking is contributing directly to rising high school completion, and thereby potentially to increases in postsecondary education enrollment as well.

Methods

With longitudinal (panel) data covering eight school years, we employ fixed effects models to estimate the average effects of online course-taking on high school outcomes (such as credits earned), including school fixed effects (π_s) that capture school attributes that are stable over time; student fixed effects (δ_j) that adjust for student characteristics that are unchanging over time; and grade by year fixed effects (μ_{gt}):

$$A_{jst} = \alpha D_{jt} + \beta_1 X_{1jt} + \beta_2 A_{jst-1} + \beta_3 P_{st} + \delta_j + \pi_s + \mu_{gt} + \varepsilon_{jst} \qquad (1)$$

In the above model, A_{jst} is the outcome of interest for student j attending school s in year t; D_{jt} is an indicator if the student took courses online in year t; X_{1jt} are student characteristics at the start of the school year in which instruction is accessed online (including student demographics, credits earned in the prior year, special educational needs, etc.); A_{jst-1} is the lagged (prior year) value of the outcome; P_{st} are school characteristics, including the percent of students in a given school that access online instruction, and ε_{jst} is the random error term. Identification of the average effect of online course-taking in this model comes from students who take courses online in some but not all years that we observe them in high school, which is the case for many students in our sample. In addition, we also estimate the fixed effects models with interactions between online course-taking and grade level (distinguishing 11th and 12th graders from underclassmen), given that district administrative and instructional staff reported in interviews that 9th and 10th graders were less compatible and effective users of the online course-taking system, an insight that was confirmed in our prior empirical research (Heinrich et al., forthcoming). For the estimation of high school graduation and college enrollment outcomes, which we only observe at one time point, our models only include grade and year fixed effects. We acknowledge that our fixed effect models will only identify *effects* of online course-taking if we can reasonably assume that no other unobserved, time-varying factors influenced online course-taking and student educational outcomes (the conditional independence assumption).

Because the assumption that no other unobserved, time-varying factors at student, school and grade levels had influenced online course-taking and student educational outcomes is a relatively strong one, we additionally estimate inverse probability weighting models with regression adjustment (IPWRA), a double-robust estimator that aims to align the observed characteristics of online course-takers and non-users in the baseline year in assessing the relationship of online course-taking to student outcomes. We also use this doubly robust estimation method to examine the effects of the number of years of online course-taking (our measure of treatment intensity) on student outcomes. The IPWRA method uses probability weights from a model that predicts treatment status—any online course-taking or the number of years of online course-taking—to obtain outcome-regression parameters that account for the fact that each student is observed in only one of the potential outcomes. The estimated inverse-probability weights are used to fit weighted regression models of the outcome for each treatment level and to obtain predicted outcomes for each student, and then the average treatment effects (ATE) are computed from these estimates of treatment effects.

Here we show the multi-valued treatment model used to estimate the effects of intensity of online course-taking (for 1, 2, 3 or 4 or more years), using the same covariates included in equation (1):

$$\widehat{ATE}_t = 1/(n\sum_{i=1}^{n}[D_{t,i})/(\hat{p}_t(Xi)Y_i + (1 - D_{t,}\mathrm{i})/(\hat{p}_t(Xi))\hat{\mu}_t(X_i)] \\ - 1)/(n\sum_{i=1}^{n}[1 - D_{t,i})/(1 - \hat{p}_t(Xi)Y_i + (1 - (1 - D_{t,i}))/(1 - \hat{p}_t(Xi))\hat{\mu}_0(X_i)] = \hat{\Delta}(t) - \hat{\Delta}(0) \qquad (2)$$

Regression adjustment models estimate separate regressions for each treatment level, so that again, $D_{t,i}$ is a binary variable that equals 1 if student i is in a given treatment state in year t and 0 if not. In the above equation, $\hat{p}_t(X_i)$ is the estimated propensity score for treatment t and $\hat{\mu}_t(X_i)$ estimates $\mu_t(X_i) = E[Y(t)|X]$ for $t \in \{0, 1, \ldots, T\}$. The ATE is estimated in a three-step procedure, where the true propensity score $p_t(X_i)$ is estimated first, in this case with a multinomial logit model; the true regression model $\mu_t(X_i)$ is estimated next, and then they are combined as in equation (2) to calculate the final result. The primary advantage of IPWRA is that the estimate for the ATE is consistent if either the model for the propensity score or for the potential outcome regression is correctly specified (the doubly robust property). The IPWRA model estimation also provides in part a robustness check on the fixed effects model results, although we do not claim to have overcome all limitations to the validity of causal inference due to selective differences between online course-takers (and the intensity of online course-taking) and non-users. For instance, we do not discount that there may be unobserved student differences, known to the educators who decide whether to assign students to retake a course in a traditional or online setting, that are also associated with their response to treatment and associated outcomes.

In light of the concerns about unobserved student differences, we also used partial identification methods developed by Manski and Pepper (2000) to estimate the effects of online course-taking under less stringent assumptions than are required for generating point estimates. We are particularly interested in whether the bounds on our parameter of interest include zero (in varying specifications for their estimation). Because this method requires both binary treatment and binary outcome measures, we generate these bounds for our estimates of high school graduation and college enrollment outcomes, including whether the postsecondary institution is rated by USNWR. We also estimated a specification using the number of credits failed in the prior year as the monotone instrumental variable (MIV), which assumes that the latent probability of a good outcome conditional on treatment assignment varies (weakly) monotonically with this variable. We performed these analyses with each possible specification: monotone treatment selection (MTS), both positive and negative; monotone treatment response (MTR), and the combinations MTS and MTR; MIV and MTS; and MIV, MTS and MTR.

Another challenge with the student fixed effects model specification is that it more heavily weights in the analysis students who switched to or from online course enrollment within our period of study. To examine the extent to which our results might generalize beyond these "switchers," we also estimate models predicting high school student outcomes using a value-added model specification that similarly incorporates students' lagged achievement data but does not include the more restrictive student fixed effects. In addition, recognizing a potential concern for inflated treatment effect estimates in our models due to regression to the mean when using the year prior to online course-taking as the baseline year in our estimation, we also perform our estimation on a restricted sample of students who have 8th grade data available to use as the baseline year. The results from this estimation examining associations between online course enrollment and student longer-term outcomes (graduation and college enrollment) are presented in the appendices, along with the value-added model results and nonparametric bounds estimates.

Research Findings

Progress Toward High School Graduation

We begin by examining the relationship between online course-taking and students' progress toward high school graduation, as assessed by the number of credits earned and their GPA, as well as students' standardized (math and reading) test scores as a measure of their learning in high school. Panel A of Table 2 presents the results of the fixed effects (school, student and grade-by-year) analysis, including the average effect estimates for all students, as well as the effects estimated with interactions between grade-level and online course-taking. On average, high school students earn 0.138 additional credits (relative to approximately seven credits attempted) in a given school year when taking courses online, compared to not taking courses online (over this study period). The effects estimated in the models with interactions by grade level show a very clear pattern of greater credits earned as students advance to their junior and senior years. In fact, the average effects are negative and statistically significant for underclassmen, and almost five times larger for 12th graders (0.632 additional credits earned) than the overall average effect size. The findings for effects on GPA parallel those of credits earned, again with a smaller average effect for all online course takers of 0.023 grade points (relative to an average prior GPA of about 1.4 for online course-takers), which masks considerable variation in effects by grade level. Underclassmen taking courses online experience a statistically significant decline in GPA of 0.092 grade points, whereas 11th and 12th graders gained through online course-taking by about 0.19 grade points on average. Interviews with instructional staff suggested that 9th and 10th grade students were lacking in the necessary self-regulatory learning strategies needed to succeed online, as well as the added motivation of upperclassmen striving to move more quickly toward graduation (Darling-Aduana et al., forthcoming; Heinrich et al., forthcoming). Given that credits recovered (after a prior course failure) replace the failing grade in a given student's record, we expected increases in GPA to follow in lockstep with credits recovered via online course-taking.

Panel B of Table 2 shows the results of this same analysis when we restrict our sample to only students who had failed a course in the prior school year (where about one-third subsequently take courses online). The patterns in the effects are entirely the same as with the full sample, but the magnitude of the coefficients is in most cases reduced, while the standard errors are slightly larger. We continue to see considerably larger, statistically significant effects of online course-taking on credits earned and GPA for the 11th and 12th grade students (vs. underclassmen) of about 0.4 to one half credit greater earned through online course-taking. For underclassmen, there is no longer a negative and statistically significant effect on credits earned, but a small, statistically significant negative effect on GPA (-0.046) persists. To the extent that standardized reading and math test scores proxy well for student learning in high school, we see mostly negative but *no* statistically significant coefficient estimates in either the full or restricted samples.

High School Graduation and College Enrollment

We hypothesized based on observed trends in high school progression through online course-taking and graduation rates in our study district, as well as existing anecdotal evidence on the relationship between online course-taking for credit recovery and high school graduation rates (Kirsch, 2017; Malkus, 2018), that we would identify a positive relationship between online course-taking for credit recovery and graduation rates. Overall, high school students in this district who engage in online course-taking enroll in an average of two online courses each year, although the median is one course and the 90th percentile is four courses, and there is a long right-hand tail extending to 29 courses in a single year. Online course completion rates were steadily increasing over the period of our study, from less than 20 percent in the first two school years to over 40 percent in the last several years.

Table 3 presents descriptive information on the three outcomes we estimate—graduated high school, enrolled in college (2-year or 4-year), and enrolled in a four-year college—by online course-taking and the number of years of online course-taking. High school graduation and college enrollment rates are (statistically significantly) higher for high school students with no online course-taking vs. any online course-taking—graduation rates are about 4 percent higher on average, and college enrollment rates (2-year and 4-year) are about 13-15 percent higher on average for those not taking courses online. These gaps generally increase with more years of online course-taking, where students taking courses online for four or more years have graduation rates that are roughly 14 percentage points lower than those with no online course-taking, and college enrollment rates are one-fourth of those with no online course-taking.

In Table 4, we present our findings on the relationship between high school online course-taking and high school graduation and college enrollment outcomes, showing the results from fixed effects (linear probability) regressions (with grade-by-year fixed effects and controlling for student and school covariates) and the IPWRA estimation, as well as for the full sample (Panel A) and restricted sample (Panel B, students who failed a course in the prior year). The results are remarkably consistent across both methodological approaches and study samples.

Focusing on our preferred (restricted) sample in Panel B, students taking courses online in this school district have, on average, high school graduation rates that are about 21 percentage points higher than similar students who do not take courses online, and their 2- or 4-year college enrollment rates are also about 6 percentage points higher (compared to similar students not taking courses online). The estimates for four-year college enrollment are mostly very small (less than 0.1%), and the signs on the college quality proxy measures are all negative (and statistically significant for the indicator that the institution was rated by USNWR and its peer reputation score). This suggests that while taking courses online may open access to postsecondary education for these high school students, they may be enrolling in lower-quality institutions.

Table 5 presents the results from IPWRA models that estimate high school graduation and college outcomes by the number of years online course-taking in high school, both for the full sample (Panel A) and restricted sample of students who failed a course in the prior year (Panel B). The results for high school graduation generally hold across all levels of high school online course-taking, and they are fairly consistent across the full and restricted samples. Focusing on the findings reported in Panel B, for one to three years of online course-taking, high school graduation rates are increasing with the number of years of online course-taking, but the estimate trends lower again for students taking courses online all four years. We see a similar pattern in the findings for the outcomes of 2- or 4-yr and 4-year college enrollment, where enrollment rates are higher with two years vs. one-year of online course-taking, but then they decline with three or four years of online course-taking. The coefficient estimates on the college quality measures are negative, and for the most part, increasingly negative with each additional year of online course-taking (although because of the small subsample size of students taking courses online all four years, we are unable to obtain estimates for this group for all measures).

Model Sensitivity Tests

Appendix A presents the results of the first sensitivity test, where we replicate the analysis presented in Table 2 using an alternative, value-added modeling approach (for both the full and restricted samples). This is intended to address the concern that the student fixed effects model specification more heavily weights students who switched to or from online course enrollment over the study period. The pattern of results shown in Appendix A is largely consistent with that in Table 2—with negative, statistically significant effects of online course-taking on credits earned and GPA for underclassmen and positive, statistically significant effects for 11th and 12th graders—although the average effects of online course-taking on credits earned and GPA for all students were (on the contrary) negative. Another departure from the results of our preferred specification (and from other findings of our research, Heinrich et al., forthcoming) is the average positive association of online course-taking with reading test scores in the value-added estimation with all students.

In addition, Appendix B presents the results of our estimation that further restricts our sample to students who have data available from their 8th grade year to use as the baseline year in the analysis. This sensitivity test is intended to address the potential concern that our treatment effect estimates may be inflated due to regression to the mean, although the tradeoff is a considerably smaller (and possibly selective) sample, given that not all high school students had 8th grade records in this school district. The estimates shown in Appendix B are consistent with our main results in that they show positive effects of online course-taking on high school graduation, but the estimates are only one-fourth to one-fifth the size of our main results. The effect estimates for college enrollment (2-year or 4-year) are considerably smaller than the results in Table 4, and they are no longer statistically significant for the 4-year college enrollment outcome. While these results generally suggest that our main specification estimates may be overestimated, they lend plausibility to the possibility of true, albeit smaller, effects of online course-taking on high school graduation rates.

Nonparametric Bounds

We also estimate the effects of online course-taking under less stringent assumptions about selection into treatment using partial identification methods (Manski and Pepper, 2000) for the four binary outcomes: high school graduation, two- or four-year college enrollment, four-year college enrollment, and whether a student's postsecondary institution is rated by USNWR. Although the procedure generates estimates for all possible combinations of assumptions (MTS, MTR and MIV), we focus on the results for those that are more plausible for the treatment and context we are investigating. In particular, we invoke monotone treatment selection (MTS), which tightens the bounds by assuming the expected potential outcomes move in a specific direction when comparing the treated and untreated; in our case, that is negative selection, which implies that students taking courses online are more likely to experience a bad outcome conditional on treatment assignment. Alternatively, we do not believe that monotone treatment response (MTR) is a reasonable assumption, given that students are most often not choosing to take their courses online, but are rather assigned to credit recovery and may not have expectations about whether they will be better or worse off in this program. Lastly, as our models estimating selection into online course-taking have found course failure in the prior academic year to be the strongest predictor of treatment, we use the number of credits failed in the prior year as a monotone instrumental variable (MIV) and assume that the latent probability of a good outcome conditional on treatment assignment varies (weakly) monotonically with this variable.

With varying assumptions, four outcome variables, two alternative samples (full sample and the subsample of students who failed at least one course in the prior year), and four different assumed error rates, we generated numerous bounds estimates. In Appendix C, we present a selection of these estimates. For one outcome, high school graduation, we present the results of the various possible specifications (given the MTS-negative and MIV assumptions and alternative samples and error rates), and for the other three outcomes, we present only the results of our preferred specification (MTS-n + MIV) and subsample (students who failed at least one course in the prior school year). We also show the worst-case bounds for high school graduation, which makes no assumptions about selection, but we believe this is unnecessarily restrictive given the credible information we have (confirmed in quantitative and qualitative analyses) about factors influencing online course-taking in high school.

The results of the nonparametric bounds presented in Appendix C (focusing first on high school graduation) show that the MTS assumption considerably improves (tightens) the bounds, as does restricting our sample to students who failed at least one course, although with MTS alone, the bounds do cross zero at the higher assumed error rates. Assuming the outcomes vary with the MIV (number of credits failed in the prior year) and estimating our preferred specification (MTS-n + MIV) with the subsample of students who failed at least one course (see the second page of Appendix C), we find that the estimated bounds rarely cross zero (when also assuming no false positives) for the four outcomes. For example, for high school graduation, our fixed effects and IPWRA models produced point estimates of 0.216 and 0.214, respectively, which lie within the majority of the bounds estimates for the preferred specification/subsample. For the college enrollment and institutional quality outcomes, the estimates from our fixed effects and IPWRA models are smaller than (outside of) most of the estimated bounds, with the exception of about a third of the estimated bounds that cross zero. The smaller and possibly more selective samples for estimating the postsecondary outcomes (i.e., estimated only for students who have exited high school) might contribute to the greater imprecision of the bounds. It is also possible that the other selective factors we control for in estimating the effects of online course-taking are important for reducing bias, and our smaller point estimates may be closer to the true effects of online course-taking on college enrollment outcomes.

Discussion and Conclusion

The growing use of online course-taking for credit recovery in U.S. high schools raises concerns about how public schools are responding to accountability pressures to raise high school graduation rates through an expanding role for private vendors in the delivery of core curricular content and instruction online. Through the lens of new institutionalism, as discussed above, credit recovery programs provide an efficient technical solution to the problem of course failure that sets high school students behind for graduation, particularly those who have struggled academically and with problems outside of school that heighten their risk of "disappearing from the educational pipeline before graduating" Orfield et al. (2004: 2). Our empirical examination of whether online course-taking (primarily for credit recovery) increases high school graduation rates in a large urban school district suggests that the "technical fix" is likely working as intended. That is, we find a plausibly causal, positive relationship between high school online course-taking and graduation rates, with effect sizes ranging from a lower bound of about 4.5 percentage points (in our most restricted sample) to an estimated 21 percentage point increase in graduation rates. Our analysis suggests an almost mechanical relationship that works mainly for upper classmen, who are able to replace failed courses with credits earned more quickly—an increase of approximately 0.4-0.6 credits in a given year—and more cost-efficiently online. In our study district, we have also found increasing proportions of students taking and passing online course pre-tests—which allow students to “test out of” and bypass some or all parts of online course instruction (and thereby complete courses in fewer sessions)—rising from about one-quarter of students in the first years of online course-taking to about two-thirds of students in recent school years (Heinrich et al., forthcoming). This use of online instructional programs is congruent with the goals of new institutionalism, which in principle value efficiency over more holistic learning outcomes (Meyer & Rowan, 2006). It is incompatible, however, with the move toward a more “holistic” evaluation of school quality and student success under ESSA.

Indeed, our results reveal mostly negative but statistically insignificant associations between online course-taking and student performance (test scores) on standardized math and reading tests, which is consistent with reports of stagnating high school student performance on NAEP tests as high graduation rates have risen. Our related research (Darling-Aduana et al., forthcoming; Heinrich et al., forthcoming) that reports in-depth on our classroom observations of online course-taking likewise engenders concern that the quality of learning opportunities may be poorer in these settings, which we found were lacking in live teacher interactions, content learning support, accommodations for students with special needs, and adequate student-teacher ratios. These findings and the broader, accumulating evidence base on credit recovery programs raise the question of whether the goal embedded in policies such as North Carolina’s, which articulates that the length of a credit recovery course should be “dictated by the skills and knowledge the student needs to recover,” is being pursued in practice.

At the same time, in following students after high school and considering their postsecondary education options, we do find some small, statistically significant, positive associations between high school online course-taking and college enrollment, We estimated increases in (2-year or 4-year) college enrollment of about 6 percent (or 1.5 percent in our specification accounting for regression to the mean), although our nonparametric bounds estimation leaves us with less confidence in the point estimates. If we believe even small increases in college enrollment are plausibly causal, then online course-taking that increases the likelihood of graduation may be opening the door for students to postsecondary education opportunities that they might not have otherwise had. Furthermore, online course taking could potentially increase student persistence in college if it strengthens students' self-regulatory behavior that college courses demand, although we are only measuring enrollment, not persistence, in this analysis.

Finally, we conclude by pointing to some of the limitations of our research design and analysis. We acknowledge that these findings are based on data from a single, large urban school district, and while it shares many characteristics with other large urban school districts using this same online instructional program (e.g., high poverty rate, largely serving students of color, and low resources), we do not make claims about the generalizability of these findings to similar school districts in the U.S. In addition, although we have employed rigorous quasi-experimental methods facilitated by the panel structure of our data and have conducted several sensitivity tests of assumptions about our samples and model specifications, our estimates are nonetheless subject to the usual threats about unobserved selection into treatment (online course-taking), attrition in our outcome measures, and other data errors.

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Figure 1.a

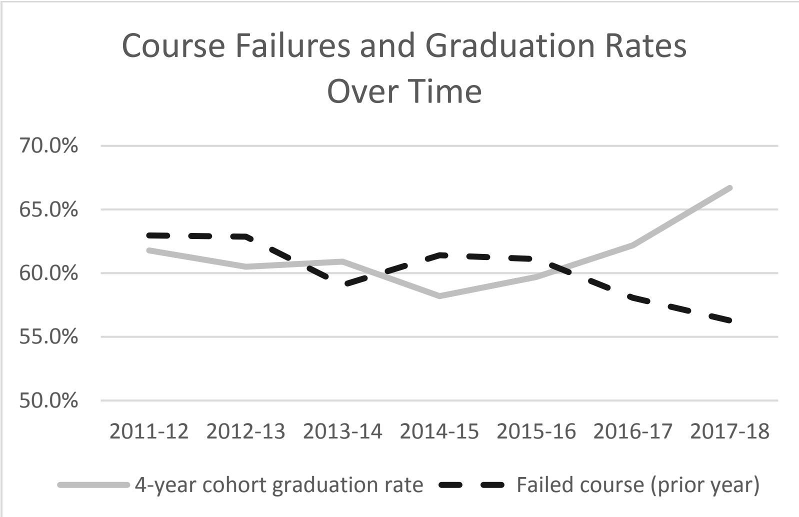

Figure 1.b

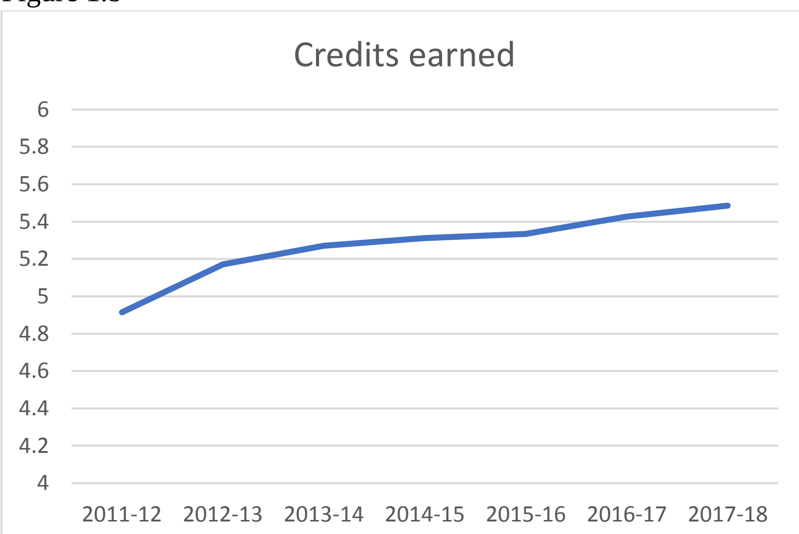

Figure 1.c.

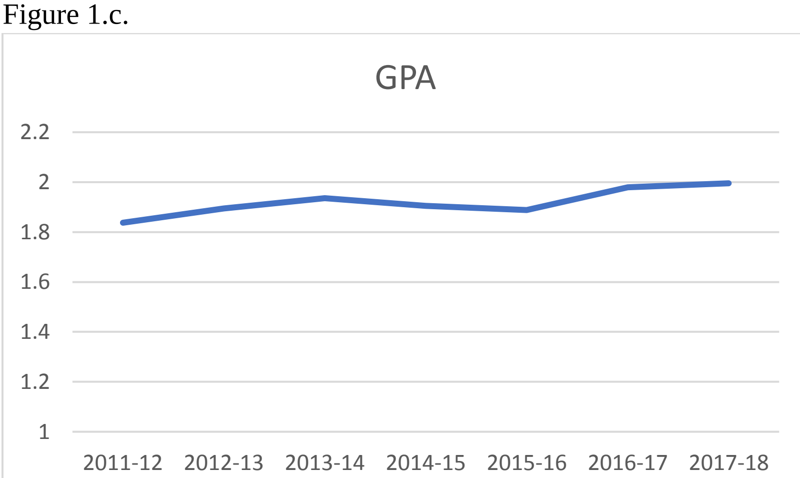

Table 1: Descriptive Statistics for Analytic Samples

	All Students (N=82,151)		Students Who Failed Course in Prior Year (N=49,541)	
	Not Online Course-taker	Online Course-taker	Not Online Course-taker	Online Course-taker
Female	0.505	0.455*	0.443	0.435
	(0.500)	(0.498)	(0.497)	(0.496)
Black	0.591	0.674*	0.674	0.693*
	(0.492)	(0.469)	(0.469)	(0.461)
Asian	0.081	0.030*	0.043	0.023*
	(0.273)	(0.171)	(0.202)	(0.149)
Hispanic	0.201	0.207	0.202	0.205
	(0.401)	(0.405)	(0.401)	(0.404)
Other Race	0.007	0.008*	0.007	0.009
	(0.081)	(0.092)	(0.086)	(0.094)
English Language Learner (ELL)	0.098	0.075*	0.101	0.073*
	(0.297)	(0.263)	(0.301)	(0.261)
Free/Reduced Price Lunch Eligible (FRL)	0.760	0.821*	0.840	0.832*
	(0.427)	(0.384)	(0.367)	(0.374)
Special Education Eligible (SPED)	0.205	0.235*	0.263	0.253*
	(0.404)	(0.424)	(0.440)	(0.435)
10th Grader	0.351	0.282*	0.343	0.284*
	(0.477)	(0.450)	(0.475)	(0.451)
11th Grader	0.307	0.333*	0.272	0.320*
	(0.461)	(0.471)	(0.445)	(0.467)
12th Grader	0.238	0.218*	0.195	0.199
	(0.426)	(0.413)	(0.396)	(0.400)
Failed One or More Courses in Prior SY	0.524	0.836*	1.000	1.000
	(0.499)	(0.370)	(0.000)	(0.000)
Credits Attempted in Prior SY	6.998	6.978*	7.061	7.048
	(1.173)	(1.472)	(1.314)	(1.381)
GPA in Prior SY	2.088	1.433*	1.352	1.197*
	(1.039)	(0.858)	(0.741)	(0.676)
Percent Absent	0.166	0.245*	0.246	0.266*
	(0.198)	(0.214)	(0.229)	(0.218)
N	61,311	20,840	32,123	17,418

Note: * Difference in means (between online course-takers and students not taking courses online) is statistically significant at p<0.05.

Table 2: Student Fixed Effect Models: Dependent Variables = High School Outcomes

Panel A:	All Students			
	All Students	9/10th Grade Students	11th Grade Students	12th Grade Students
Credits Earned (N=73,353)	0.138**	-0.168**	0.436**	0.632**
	(0.025)	(0.035)	(0.045)	(0.057)
High School GPA (N=82,151)	0.023**	-0.092**	0.194**	0.191**
	(0.007)	(0.009)	(0.012)	(0.015)
Reading Test Scores (Std.) (N=36,945)	-0.015	-0.024	0.026	-0.024
	(0.016)	(0.021)	(0.026)	(0.047)
Math Test Scores (Std.) (N=37,120)	-0.025	-0.020	-0.021	0.035
	(0.013)	(0.018)	(0.022)	(0.040)
Student Fixed Effect	Yes	Yes	Yes	Yes
Year & Grade Fixed Effect	Yes	Yes	Yes	Yes
Student Covariates	Yes	Yes	Yes	Yes
School Covariates	Yes	Yes	Yes	Yes

Panel B:	Students Who Failed a Course in Prior Year			
	All Students	9/10th Grade Students	11th Grade Students	12th Grade Students
Credits Earned (N=43,923)	0.172**	-0.045	0.395**	0.504**
	(0.033)	(0.043)	(0.061)	(0.082)
High School GPA (N=49,541)	0.017*	-0.046**	0.146**	0.084**
	(0.009)	(0.011)	(0.016)	(0.021)
Reading Test Scores (Std.) (N=22,069)	-0.032	-0.035	0.018	-0.040
	(0.020)	(0.025)	(0.034)	(0.068)
Math Test Scores (Std.) (N=22,158)	-0.029	-0.016	-0.038	0.018
	(0.017)	(0.022)	(0.029)	(0.057)
Student Fixed Effect	Yes	Yes	Yes	Yes
Year & Grade Fixed Effect	Yes	Yes	Yes	Yes
Student Covariates	Yes	Yes	Yes	Yes
School Covariates	Yes	Yes	Yes	Yes

Notes: *p<0.05 *p<0.01. Standard errors in parentheses. Student covariates include whether students failed a course, the number of credits attempted, and GPA pre-treatment as well as each student's race, gender, attendance, and English Language Learner, special education, and free or reduced-price lunch status. School covariates include the 16 schools enrolling the largest number of students in Edgenuity, school-by-year variables for student demographic characteristics, school type, and courses offered. Models predicting spring math and reading test scores also control for fall test scores.

Table 3: Graduation and College Enrollment Descriptive Outcomes by Online Course Enrollment

	Never Enrolled Online (N=21,133)	Enrolled Online				
		Enrolled Online (N=13,504)	For One Year (N=7,459)	For Two Years (N=4,488)	For Three Years (N=1,377)	For Four Plus Years (N=107)
Graduated High School	0.543	0.504	0.519	0.481	0.512	0.406
	(0.498)	(0.500)	(0.500)	(0.500)	(0.500)	(0.492)
Enrolled in College (2-Year or 4-Year)†	0.359	0.224	0.247	0.207	0.180	0.083
	(0.480)	(0.417)	(0.431)	(0.406)	(0.383)	(0.287)
Enrolled in a 4-Year College or University	0.213	0.088	0.099	0.078	0.056	0.016
	(0.409)	(0.283)	(0.298)	(0.268)	(0.231)	(0.128)
US News Rated Institution	0.376	0.124	0.147	0.088	0.058	0.010
	(0.484)	(0.330)	(0.354)	(0.284)	(0.235)	(0.102)
US News Peer Reputation	2.871	2.678	2.695	2.630	2.676	
	(0.538)	(0.466)	(0.459)	(0.484)	(0.468)	
US News Freshman Retention Rate	0.770	0.733	0.738	0.722	0.733	
	(0.098)	(0.094)	(0.092)	(0.099)	(0.091)	
US News Graduation Rate	0.539	0.473	0.480	0.454	0.463	
	(0.202)	(0.151)	(0.152)	(0.149)	(0.155)	

US News data not available for all students.

Table 4: Graduation and College Enrollment Outcomes: Baseline = Last Year Pre-Edgenuity

Panel A	Full Sample (N=37,068)	
	Fixed Effects	IPWRA
Graduated High School	0.254***	0.241***
	(0.006)	(0.006)
Enrolled in College (2-Year or 4-Year)†	0.094***	0.085***
	(0.005)	(0.006)
Enrolled in a 4-Year College or University	0.025***	0.011**
	(0.004)	(0.005)
US News Rated Institution (N=20,019)	-0.041***	-0.050***
	(0.007)	(0.009)
US News Overall Score (N=2,882)	0.041	-0.652
	(0.698)	(0.973)
US News Peer Reputation (N=5,242)	-0.038*	-0.070**
	(0.022)	(0.030)
US News Freshman Retention (N=5,234)	-0.003	-0.008
	(0.004)	(0.005)
US News Graduation Rate (N=5,235)	-0.006	-0.015
	(0.007)	(0.010)
Year & Grade Fixed Effect	Yes	Yes
Student Covariates	Yes	Yes
School Covariates	Yes	Yes

Panel B	Students Who Failed a Course in Pre-Treatment Year (N=23,254)	
	Fixed Effects	IPWRA
Graduated High School	0.216***	0.214***
	(0.007)	(0.007)
Enrolled in College (2-Year or 4-Year)	0.060***	0.058***
	(0.005)	(0.005)
Enrolled in a 4-Year College or University	0.008**	0.007**
	(0.004)	(0.004)
US News Rated Institution (N=10,755)	-0.029***	-0.030***
	(0.008)	(0.008)
US News Overall Score (N=398)	-1.343	-1.295
	(1.480)	(1.516)
US News Peer Reputation (N=943)	-0.069**	-0.074**
	(0.034)	(0.034)
US News Freshman Retention (N=940)	-0.008	-0.009
	(0.008)	(0.008)
US News Graduation Rate (N=938)	-0.011	-0.014
	(0.012)	(0.012)
Year & Grade Fixed Effect	Yes	Yes
Student Covariates	Yes	Yes
School Covariates	Yes	Yes

Notes: *$p<0.05$ **$p<0.01$. Standard errors in parentheses. Student covariates include whether students failed a course, the number of credits attempted, and GPA pre-treatment as well as each student's race, gender, attendance, and English Language Learner, special education, and free or reduced-price lunch status. School covariates include the 16 schools enrolling the largest number of students in Edgenuity, school-by-year variables for student demographic characteristics, school type, and courses offered.

[†]All students who attended college graduated high school.

Table 5: Graduation and College Enrollment Outcomes by Years Enrolled Online, IPWRA estimation: Baseline = Last Year Pre-Edgenuity

Panel A	Full Sample (N=37,068)			
	1 Year	2 Years	3 Years	4+ Years
Graduated High School	0.201***	0.295***	0.368***	0.248***
	(0.007)	(0.008)	(0.012)	(0.046)
Enrolled in College (2-Year or 4-Year)†	0.076***	0.105***	0.101***	-0.005
	(0.006)	(0.008)	(0.014)	(0.028)
Enrolled in a 4-Year College or University	0.015***	0.006	-0.004	-0.028*
	(0.005)	(0.007)	(0.011)	(0.016)
US News Rated Institution (N=20,019)	-0.029***	-0.077***	-0.092***	-0.056**
	(0.010)	(0.012)	(0.018)	(0.023)
US News Overall Score (N=2,882)	-0.611	-0.259	-2.192	
	(1.016)	(1.521)	(1.908)	
US News Peer Reputation (N=5,242)	-0.051*	-0.123**	-0.071	
	(0.031)	(0.048)	(0.068)	
US News Freshman Retention (N=5,234)	-0.002	-0.023**	-0.020	
	(0.005)	(0.009)	(0.016)	
US News Graduation Rate (N=5,235)	-0.005	-0.037**	-0.037	
	(0.010)	(0.015)	(0.023)	
Year & Grade Fixed Effect	Yes	Yes	Yes	Yes
Student Covariates	Yes	Yes	Yes	Yes
School Covariates	Yes	Yes	Yes	Yes
Panel B	Students Who Failed a Course in Pre-Treatment Year (N=23,254)			
	1 Year	2 Years	3 Years	4+ Years
Graduated High School	0.169***	0.274***	0.322***	0.280***
	(0.007)	(0.009)	(0.014)	(0.038)
Enrolled in College (2-Year or 4-Year)	0.050***	0.080***	0.056***	0.018
	(0.006)	(0.008)	(0.012)	(0.026)
Enrolled in a 4-Year College or University	0.008**	0.010*	-0.008	-0.011
	(0.004)	(0.005)	(0.007)	(0.012)
US News Rated Institution (N=10,755)	-0.022**	-0.034***	-0.061***	-0.054***
	(0.008)	(0.009)	(0.011)	(0.016)
US News Overall Score (N=398)	-1.060	-2.281	1.329	
	(1.590)	(2.046)	(4.500)	
US News Peer Reputation (N=943)	-0.041	-0.144***	-0.120	
	(0.036)	(0.046)	(0.115)	
US News Freshman Retention (N=940)	-0.009	-0.015	0.010	
	(0.009)	(0.011)	(0.017)	
US News Graduation Rate (N=938)	-0.005	-0.038**	-0.005	
	(0.013)	(0.016)	(0.032)	
Year & Grade Fixed Effect	Yes	Yes	Yes	Yes
Student Covariates	Yes	Yes	Yes	Yes
School Covariates	Yes	Yes	Yes	Yes

Notes: *$p<0.05$ **$p<0.01$. Standard errors in parentheses. Student covariates include whether students failed a course, the number of credits attempted, and GPA pre-treatment as well as each student's race, gender, attendance, and English Language Learner, special education, and free or reduced-price lunch status. School covariates include the 16 schools enrolling the largest number of students in Edgenuity, school-by-year variables for student demographic characteristics, school type, and courses offered.

Appendix

We presented our preferred specifications above when examining high school outcomes (implementing a student, school and grade-by-year fixed effects approach with lagged achievement information). However, it is important to understand the potential limitations of this approach, which more heavily weights students who switched to or from online course enrollment over the period of study. Below we present the results from our value-added models that incorporate lagged achievement data.

Appendix A: Value-Added Models Predicting High School Outcomes

Panel A	All Students			
	All Students	9/10th Grade Students	11th Grade Students	12th Grade Students
Credits Earned (N=73,353)	-0.019	-0.257**	0.382**	0.603**
	(0.017)	(0.023)	(0.036)	(0.046)
High School GPA (N=82,151)	-0.040**	-0.105**	0.130**	0.076**
	(0.005)	(0.008)	(0.011)	(0.013)
Reading Test Scores (Std.) (N=36,945)	0.040**	0.022	0.018	0.049
	(0.011)	(0.013)	(0.019)	(0.031)
Math Test Scores (Std.) (N=37,120)	-0.014	-0.028**	0.013	0.082**
	(0.008)	(0.011)	(0.016)	(0.025)
Student Fixed Effect	Yes	Yes	Yes	Yes
Year & Grade Fixed Effect	Yes	Yes	Yes	Yes
Student Covariates	Yes	Yes	Yes	Yes
School Covariates	Yes	Yes	Yes	Yes
Panel B	**Students Who Failed a Course in Prior Year**			
	All Students	9/10th Grade Students	11th Grade Students	12th Grade Students
Credits Earned (N=43,923)	0.049*	-0.110**	0.295**	0.415**
	(0.021)	(0.026)	(0.044)	(0.058)
High School GPA (N=49,541)	-0.008	-0.046**	0.111**	0.006
	(0.006)	(0.008)	(0.013)	(0.015)
Reading Test Scores (Std.) (N=22,069)	0.031*	0.028	-0.005	0.002
	(0.012)	(0.015)	(0.024)	(0.038)
Math Test Scores (Std.) (N=22,158)	-0.014	-0.015	-0.008	0.045
	(0.010)	(0.012)	(0.019)	(0.029)
Student Fixed Effect	Yes	Yes	Yes	Yes
Year & Grade Fixed Effect	Yes	Yes	Yes	Yes
Student Covariates	Yes	Yes	Yes	Yes
School Covariates	Yes	Yes	Yes	Yes

Notes: *p<0.05 **p<0.01. Standard errors in parentheses. Student covariates include whether students failed a course, the number of credits attempted, and GPA pre-treatment as well as each student's race, gender, attendance, and English Language Learner, special education, and free or reduced-price lunch status. School covariates include the 16 schools enrolling the largest number of students in Edgenuity, school-by-year variables for student demographic characteristics, school type, and courses offered.

In addition, the results below (Appendix B) address the concern about regression to the mean in our estimates when using the year prior to treatment as the baseline year. There was insufficient data to run the same analyses for long-term outcomes from US News.

Long-Term Outcomes: Baseline = 8th Grade (N=10,926)

	Fixed Effects	IPWRA
Graduated High School	0.045***	0.048***
	(0.009)	(0.009)
Enrolled in College (2-Year or 4-Year)	0.014**	0.016***
	(0.006)	(0.006)
Enrolled in a 4-Year College or University	0.001	0.002
	(0.003)	(0.003)
Year & Grade Fixed Effect	Yes	Yes
Student Covariates	Yes	Yes
School Covariates	Yes	Yes

Notes: * p<0.05 ** p<0.01. Standard errors in parentheses. Student covariates include whether students failed a course, the number of credits attempted, and GPA pre-treatment as well as each student's race, gender, attendance, and English Language Learner, special education, and free or reduced-price lunch status. School covariates include the 16 schools enrolling the largest number of students in Edgenuity, school-by-year variables for student demographic characteristics, school type, and courses offered.

Appendix C: Nonparametric Bounds Results

Partial identification methods were used to generate estimates the effects of online course-taking under less stringent assumptions about selection into treatment: monotone treatment selection (MTS)-negative and monotone instrumental variable (MIV), i.e., the number of credits failed in the prior school year.

Treatment: Enrolled in a high school course online
Outcome: Graduated high school
Sample/specification: All H.S. students; no MIV

Error Rate	Arbitrary Errors	No False Positives
No Monotonicity Assumptions (Worst Case Selection)		
0	[-0.513, 0.487] p.e.	[-0.513, 0.487] p.e.
0.05	[-0.563, 0.537] p.e.	[-0.563, 0.537] p.e.
0.10	[-0.613, 0.587] p.e.	[-0.613, 0.587] p.e.
0.25	[-0.763, 0.737] p.e.	[-0.763, 0.737] p.e.
MTS Assumption: Negative Selection		
0	[-0.033, 0.487] p.e.	[-0.033, 0.487] p.e.
0.05	[-0.145, 0.537] p.e.	[-0.129, 0.537] p.e.
0.10	[-0.272, 0.587] p.e.	[-0.225, 0.587] p.e.
0.25	[-0.669, 0.737] p.e.	[-0.588, 0.737] p.e.

Sample/specification: H.S. students who failed at least 1 course; no MIV

Error Rate	Arbitrary Errors	No False Positives
No Monotonicity Assumptions (Worst Case Selection)		
0	[-0.444, 0.556] p.e.	[-0.444, 0.556] p.e.
0.05	[-0.494, 0.606] p.e.	[-0.494, 0.606] p.e.
0.10	[-0.544, 0.656] p.e.	[-0.544, 0.656] p.e.
0.25	[-0.694, 0.806] p.e.	[-0.694, 0.754] p.e.
MTS Assumption: Negative Selection		
0	[0.101, 0.556] p.e.	[0.101, 0.556] p.e.
0.05	[-0.018, 0.606] p.e.	[0.017, 0.606] p.e.
0.10	[-0.145, 0.656] p.e.	[-0.067, 0.656] p.e.
0.25	[-0.574, 0.806] p.e.	[-0.385, 0.754] p.e.

Sample/specification: All H.S. students; MIV = number of failed credits in prior year

Number of pseudo-samples used in MIV bias correction: 100
Number of observations per MIV cell: Cell 1: 12905; Cell 2: 11130; Cell 3: 10635

Error Rate	Arbitrary Errors	No False Positives
MIV and MTS Assumptions: Negative Selection		
0	[0.350, 0.050] p.e.	[0.350, 0.050] p.e.
0.05	[0.194, 0.124] p.e.	[0.246, 0.087] p.e.
0.10	[0.063, 0.198] p.e.	[0.164, 0.124] p.e.
0.25	[-0.209, 0.419] p.e.	[-0.122, 0.235] p.e.

Treatment: Enrolled in a high school course online
Sample/specification: H.S. students who failed at least 1 course; MIV = number of failed credits in prior year *(preferred specification)*

Outcome: Graduated high school

```
Number of pseudo-samples used in MIV bias correction: 100
Number of observations per MIV cell: Cell 1: 7176; Cell 2: 5326; Cell 3: 5708

---------------------------------------------------------------------------
 Error Rate     | Arbitrary Errors                | No False Positives
---------------------------------------------------------------------------
MIV and MTS Assumptions: Negative Selection
  0             [  0.379,  0.187] p.e.            [  0.379,  0.187] p.e.
  0.05          [  0.274,  0.256] p.e.            [  0.325,  0.222] p.e.
  0.10          [  0.187,  0.326] p.e.            [  0.277,  0.256] p.e.
  0.25          [  0.031,  0.535] p.e.            [  0.139,  0.361] p.e.
---------------------------------------------------------------------------
```

Outcome: Ever enrolled in college (2-yr. or 4-yr.)

```
---------------------------------------------------------------------------
 Error Rate     | Arbitrary Errors                | No False Positives
---------------------------------------------------------------------------
MIV and MTS Assumptions: Negative Selection
  0             [  0.171,  0.238] p.e.            [  0.171,  0.238] p.e.
  0.05          [  0.061,  0.307] p.e.            [  0.148,  0.273] p.e.
  0.10          [ -0.025,  0.377] p.e.            [  0.129,  0.307] p.e.
  0.25          [ -0.127,  0.521] p.e.            [  0.072,  0.347] p.e.
---------------------------------------------------------------------------
```

Outcome: Ever enrolled in 4-yr. college

```
---------------------------------------------------------------------------
 Error Rate     | Arbitrary Errors                | No False Positives
---------------------------------------------------------------------------
MIV and MTS Assumptions: Negative Selection
  0             [  0.052,  0.274] p.e.            [  0.052,  0.274] p.e.
  0.05          [ -0.023,  0.342] p.e.            [  0.045,  0.308] p.e.
  0.10          [ -0.025,  0.385] p.e.            [  0.039,  0.317] p.e.
  0.25          [ -0.041,  0.487] p.e.            [  0.021,  0.317] p.e.
---------------------------------------------------------------------------
```

Outcome: Enrolled in USNWR-rated college

```
---------------------------------------------------------------------------
 Error Rate     | Arbitrary Errors                | No False Positives
---------------------------------------------------------------------------
MIV and MTS Assumptions: Negative Selection
  0             [  0.044,  0.178] p.e.            [  0.044,  0.178] p.e.
  0.05          [ -0.080,  0.252] p.e.            [  0.030,  0.215] p.e.
  0.10          [ -0.105,  0.315] p.e.            [  0.013,  0.241] p.e.
  0.25          [ -0.242,  0.426] p.e.            [ -0.112,  0.241] p.e.
---------------------------------------------------------------------------
```