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

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Abstract

Recent increases in high school graduation rates have been linked anecdotally to online course-taking for credit recovery. Online course-taking that supports high school completion could open opportunities for postsecondary education pursuits. Alternatively, poorer quality online instruction could diminish student learning and discourage persistence toward graduation and further education. Using quasi-experimental methods in an eight-year longitudinal study of high school online course-taking, we find positive associations between online course-taking, credits earned and high school graduation, and for those with limited online course-taking, small increases in college enrollment. However, we find significantly lower four-year college enrollments and lower-quality college enrollments for all online course-takers, leaving open the question of whether online course-taking 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).

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 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 (Powell, Roberts, & Patrick, 2015).

In this paper, we analyze online course-taking that is used primarily for credit recovery, but we also perform our analyses on a subsample of students who previously failed a course, allowing us to focus specifically on credit recovery facilitated through an online course system. We also acknowledge that some of the arguments for and against these courses could be relevant to face-to-face and school district-developed credit-recovery courses, as well as to other forms of online instruction.

On the one hand, given that a high school degree is generally required to enroll in postsecondary education programs, online credit-recovery courses that enable or support 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. Levin (2009) estimated the value of lifetime economic (gross) benefits to the public associated with an additional high school graduate to be about $209,000 (in 2004 dollars). 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 (Authors, 2019a).

On the other hand, the fact that students are often assigned to online credit-recovery courses 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 lowerachieving students (Brighouse, Ladd, Loeb & Swift, 2018). And if online instruction substitutes poorer quality digital instruction for better quality live instruction, student engagement and learning could diminish, and students could be discouraged from persisting toward graduation or pursuing further education beyond high school. Delivering poorer quality digital instruction also abdicates the moral imperative of education to support students' intellectual development and raises important equity considerations, given that the students assigned to these courses are frequently from predominately marginalized groups.

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 National Student Clearinghouse (NSC), College Scorecard, and U.S. News and World Report (USNWR) data that provide information on student participation in postsecondary education and the quality of institution attended. In addition, since 2015, we have 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 (Authors, 2019b).

We use these data to address the following key questions. Is there a link between online course-taking in high school, high school graduation 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? Furthermore, 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 order to provide context for these findings, we also explore, how online course-taking (primarily for credit recovery) affects progress toward high school graduation (e.g., credits earned and grade point average).

In the analysis, we begin by examining the relationship between online course-taking and 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, as well as school-by-cohort fixed effects models. We also employ inverse probability weighting with regression adjustment, a double-robust estimator, to assess the relationship of the intensity of online course-taking 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) and high school graduation, and for those with very limited online-course taking, small increases in college enrollment as well. However, we also find significantly lower four-year college enrollment and lower-quality postsecondary institutional enrollments for all online course-takers. Despite the positive outcomes, 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 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 (2012) 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 percent graduation rate in the 2015-16 school year after a 54 percent 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; Authors, 2019b). The International Association for K-12 Online Learning (Powell et al., 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. 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 directing students to 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) found that online credit recovery course offerings were associated with a decline in student test scores and graduation rates. In more recent work, Viano and Henry (2019) examined whether students taking courses for credit recovery were more likely to graduate and less likely to drop out than those taking traditional courses and found a lower likelihood of dropping out of high school associated with credit recovery, but also less learning as measured by end of course exams and ACT scores.

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. Levin (2009) estimated the unit (present value) cost of educational interventions found to increase high school graduation rates—early childhood education, class size reductions and teacher salary increases—as ranging from about $3,000 to $13,000 per student in 2004 dollars. To generate a comparable estimate of the unit cost for credit recovery, we obtained program cost information directly from our study district that accounts for the vendor contract and program coordinator salary, and then we adjusted these for inflation to 2004 dollars (the same as Levin's numbers).[4] The resulting cost estimate of $55-65 per student suggests that credit recovery may be as much as 50 to 200 times less expensive than other interventions shown to increase graduation rates. In addition, school districts lose state funding when students drop out or leave for alternative programs outside the district. If dropouts can be averted, Levin et al. (2006) showed (using National Educational Longitudinal Study 1988 data for the bottom third of students in socioeconomic status), small increases in postsecondary enrollment would also result.

As Burch (2009) argued, however, 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). Levin (2009: 5) likewise counsels that "fairness in access to good education is a matter of justice rather than simple economic rationality as measured by investment returns." In other words, when elevating principles of equity and justice in educational programming decisions, it becomes harder to see regaining credits required for high school graduation as a success if little is done to support students' intellectual development or prepare them well for postsecondary pursuits. Indeed, to the extent that rising graduation rates divert attention from underlying structural inequities and allow marginalized groups to continue to be underserved by educational institutions, then credit recovery programs may potentially do as much harm as good for these students. In this study, we investigate this latter concern, that is, whether as implemented in a large urban school district, online credit recovery might negatively affect students' post-high school educational opportunities, even if it increases the chances they complete high school.

Program Implementation

The school district in this study contracted with a single, third-party vendor (operating in all 50 states) to develop and deliver the course content in the online courses for all subject areas (English and language arts, math, science, social studies, and elective courses). The vendor provided training at the start of the school year to teachers in classrooms or lab-style settings where the online instruction was made available, including technical training on how to use the course-taking system and troubleshoot problems (e.g., student difficulties logging into the system). School district staff provided additional training, support and guidance to teachers in professional development sessions and communications during the school year, including information on practices for improving instructional delivery, such as encouraging student note-taking during online instructional videos, conducting weekly check-ins of student progress, and regularly monitoring student online course progress during class periods (Authors, 2019b). Although the courses content was developed by the vendor's team of curriculum developers and adapted as needed to meet state standards and district requirements, there was also a mechanism in place for lab instructors to flag content as incorrect or offensive, after which the vendor adjusted content accordingly. The district also provided credit-recovery course options in traditional classroom settings, with about half of all students who failed a course taking the courses in traditional classrooms and the other half enrolling online in a given year. Students enrolling online could work on their courses outside the regular school day as well, and our analysis showed that over 15 percent of time logged in the online course system was outside school hours.

Approximately one quarter of high school students in the study district accessed course instruction online in a given year, 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 among the most important factors in determining which and how many students were directed to take courses online (Authors, 2019b). For example, in one school, a new school principal wanted to understand more about the online course-taking program before committing instructional space for its use, and hence in her first year, only students who hadn't completed their online courses in the prior year were allowed to continue with the program (contributing to a steep decline in the rate of student online course-taking that year). In alternative high schools, about a third of the student body took courses online, although the rate of online course-takers was more than 80-90 percent in some schools, such as a school serving students returning to the classroom from incarceration or expulsion. In addition, over time, the school district discouraged online course-taking by students in the 9th and 10th grades, because of their lower reading levels and self-regulation for independent learning.

Overall, within schools, we observed substantial year-to-year variation in the percentage of students taking courses online, even when student characteristics such as the proportion failing their courses (one of the strongest individual predictors of online course-taking), eligible for free or reduced-price lunch, and scoring low on standardized tests was varying negligibly over time. As an illustration, one school with substantial year-to-year fluctuations in the proportion of its students taking courses online saw an increase of more than 50 percent, followed by a decline of 16 percent, and then another increase of nearly 30 percent, and another decline of 12 percent, and so on, even as student characteristics were stable. Moreover, our analyses predicting online course-taking (discussed further below) showed that school-level characteristics—including school-level demographics, course offerings (advanced, career and technical, service learning) and school type (alternative, charter, etc.)—accounted for more than two-thirds of the explained variation in the intensity of online course-taking (vs. less than one-third of explained variation accounted for by individual student attributes, including course failures). We leverage the potentially exogenous, school-level variation in the proportion of students taking courses online in our empirical analysis, discussed below.

Study Samples, Data and Measures

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.[5] 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.

Treatment and Outcome Measures

In defining student participation in online course-taking, we use an indicator variable to denote whether a student enrolled in at least one online course in high school, and we also measure the intensity of online course-taking among students enrolling in online courses. While we observed one student enrolling in as many as 35 online courses, approximately 60 percent of online course-takers enrolled in only 1-2 courses in one or two years of high school, and just a little over 10 percent of online course-takers enrolled in more than five courses during their time in high school. In developing our measure of online course-taking intensity, we factored in both the number of online courses taken and the number of years in high school that students enrolled in online courses. The categorical measure of online course-taking that we use in our analysis is defined as: (1) students who enrolled in 1-2 online courses in 1-2 high school years (60% of online course-takers); (2) students who enrolled in 3 or more online courses in 1-2 high school years (27% of online course-takers), and (3) students who enrolled in online courses in three or more years of high school (13% of online course-takers). The other (reference) category in this measure is students who did not take any of their high school courses online.

Our 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).[6] In a recent analysis of the evidence on the relationship between test scores and outcomes such as college attendance and earnings, Goldhaber and Özek (2019) conclude there is an abundance of evidence suggesting a causal link between test scores and later life outcomes. 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.[7] 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.[8] In addition, we use publicly available data from the College Scorecard[9] and information provided by the U.S. News and World Report (USNWR) to measure college quality. From the College Scorecard, we use measures of the type of degree awarded by the institution (predominant and highest degrees), level of research activity, first-year student retention rate, college completion rate, and whether the college has open admissions. Some of the same measures of college quality were available from USNWR but were more complete in the College Scorecard. Thus, we limit our use of the USNWR data to an indicator variable for whether a given postsecondary institution is included among the USNWR-ranked institutions (a measure of selectivity).[10]

Analysis Samples

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 primary treatment-comparison samples for our analyses. One, we compare students who took at least one course online in high school (a little over 40% of our sample) with students who did not complete any courses online. Two, given that after school-level factors are accounted for, the strongest *student-level* predictor of online course-taking in our study district was course failure (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 about half of these go on to repeat the course online. For courses required for graduation, which describes the majority of cases in our sample, students who did not repeat the courses online repeated them in a face-to-face instructional environment. And in an additional analysis, we further restrict our sample to students who have data available from their 8^{th} grade year to use as the baseline year in the analysis. This is intended to address the potential concern that our treatment effect estimates may be inflated because of regression to the mean, although as seen in the sample descriptive statistics, the tradeoff is a considerably smaller (and more selective) sample, given that not all high school students had 8^{th} grade records in this school district.

Table 1 presents descriptive statistics on the students in these three analysis samples, with the first two columns comparing the student-level characteristics of all high school students in this district to online course-takers, 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), and the third set of columns showing those with 8^{th} grade (baseline) student records. With sizeable samples, we observe mostly small yet some statistically significant differences between students taking courses online and those not taking courses online in high school across many student 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. The subsample of students with 8th grade (baseline) records is particularly distinctive, however, in their significantly higher proportions of males, Black students, students with special educational needs, substantially higher rates of absences, and very low GPAs, regardless of whether they were taking courses online. This is clearly a more selective sample of high school students in this district who are at greater risk of poor educational outcomes.

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[11] 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 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 describe the various analyses we undertake in the effort to assess whether the relationship between online course-taking and student high school and postsecondary education outcomes is plausibly causal, that is, whether the expansion in online course-taking for credit recovery is contributing directly to rising high school completion and thereby potentially influencing postsecondary education enrollments as well.

Methods

With longitudinal (panel) data covering eight school years, we employ fixed effects models to estimate the average associations of online course-taking with 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 time-varying 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 (Authors, 2019b).

As we only observed high school graduation and college enrollment outcomes at one point in time, our models for these outcomes cannot include student fixed effects. Instead, we implemented a school-by-cohort fixed effects model, as shown in the equation below. This fixed effects approach accounts for variation in assignment to and the implementation of online courses between schools and within schools across different cohorts. School enrollment was identified as the first high school the student attended (observed in the study data). Student cohorts were assigned based on when each student entered ninth grade. For students without ninth grade data, we assigned the cohort based on the year and grade of the first year of data available for that student. When using lagged covariates, we used eighth grade data (where available) as the baseline year. For students without eighth grade data, we used the first year of high school data available as the baseline year. We acknowledge that our fixed effect models would only identify *effects* of online course-taking if we could reasonably assume that no other unobserved, time-varying factors influenced online course-taking and student educational outcomes (the conditional independence assumption), which is not a claim we make in this analysis.

$$A_{jc} = \alpha D_j + \beta_1 X_j + \beta_2 A_{j-1} + \beta_3 P_j + \delta_c + \varepsilon_{jc} \tag{2}$$

In the above model with the school-by-cohort fixed effects (δ_c), A_{jsc} is the outcome of interest for student j attending a school in a given cohort c; D_j is an indicator if the student took courses online during high school; X_j indicates each student's fixed demographic characteristics (such as race, ethnicity and gender), as well as whether the student ever qualified for free or reduced-priced lunch, English Language Learner status, or special education services. A_{j-1} indicates the number of credits attempted, credits failed, GPA, and percent of days attended from the student's baseline year. P_j is a vector of school characteristics across a student's high school experience, including the maximum percentage of students in a school they attended that accessed online instruction and whether the student ever attended a school identified as an alternative school,[12] while ε_{jc} is the random error term.

In assessing how the intensity of online course-taking relates to high school graduation and postsecondary education enrollments using the categorical treatment measure, we estimate inverse probability weighting models with regression adjustment (IPWRA), a double-robust estimator that aims to align the observed characteristics of those with no online course-taking to the those of the three subgroups of online course-takers in their baseline years (as defined above). The IPWRA method uses probability weights from a model that predicts treatment status—0 online courses vs. 1-2 online courses in 1-2 high school years, 3 or more online courses in 1-2 high school years, or online course-taking in 3-4 years of high school—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.

The multi-valued treatment model used to estimate the effects of intensity of online course-taking, using the same covariates included in equation (1), is specified as follows:

$$\widehat{ATE}_t = 1/(n\sum_{i=1}^{n}[D_{t,i})/(\hat{p}_t(Xi)Y_i + (1 - D_{t,}\text{i})/(\hat{p}_t(Xi))\hat{\mu}_t(X_i)] \\ - 1)/(n\sum_{i=1}^{n}[1 - D_{t,i})/(1 - \hat{p}_t(Xi)Y_i + (1 - (1 - D_{t,i}))/(1 - \hat{p}_t(Xi))\hat{\mu}_0(X_i)] = \hat{\Delta}(t) - \hat{\Delta}(0) \qquad (3)$$

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 (3) 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). As with the fixed effects model results, we do not claim to have overcome all limitations to the validity of causal inference due to selective differences in the intensity of online course-taking. For instance, we do not discount that there may be unobserved student differences, known to the educators who decide when a student should retake a course online (vs. in a traditional 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.

More specifically, this method allows us to relax the conditional independence assumption that we imposed in our fixed effects and IPWRA models, replacing it with a weaker assumption that online course-taking is monotonically related to our outcomes of interest. In examining the results, we are particularly interested in whether the bounds on our parameter of interest include zero (in varying specifications for their estimation), which would give us less confidence in the direction of the estimated effects. 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 (i.e., increasing and decreasing functions); 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. Students with no online course-taking have a weight of zero in the analysis; there were no students with all (only) online course-taking during high school. 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. Since the student fixed effects and value-added models require different assumptions, consistent estimates across models would provide suggestive evidence that the assumptions specific to each specification were unlikely to be producing biased estimates (Angrist & Pischke, 2009). The results from the value-added and nonparametric bounds analyses are presented in the appendices.

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 0.139 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 over four times larger for 12^{th} graders (0.623 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 0.024 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 11^{th} and 12^{th} 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 (Authors, 2019a; Authors, 2019b). 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 0.4-0.5 more credits 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 (-0.047) persists. To the extent that standardized reading and math test scores proxy well for student learning in high school and correlate with their later life outcomes, as Goldhaber and Özek (2019) suggest, there is reason for concern that we see mostly negative (albeit 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 in a year, although the 90th percentile is five courses, and there is a long right-hand tail extending to 35 courses in a single year (which can include summer work). 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 main outcomes we estimate—graduated high school, enrolled in college (2-year or 4-year), and enrolled in a four-year college—as well as college quality indicators, by online course-taking and the intensity 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 8-12 percentage points higher on average for those not taking courses online. The gaps in college enrollment rates increase with greater intensities of online course-taking. For students taking courses online in three or more years of high school, college enrollment rates are one-third to one-half of those with no online course-taking. In addition, all indicators of college quality suggest that students taking online courses and continuing on to postsecondary institutions are attending lower-quality institutions. For example, students who enrolled in online courses are about 25 percentage points more likely to attend open-admissions colleges and 30 percentage points less likely to attend institutions that confer graduate degrees, and first-year retention rates and college completion rates are significantly lower (9 and 12 percentage points, respectively) as well.

In Table 4, we present findings on the relationship between high school online course-taking and high school graduation and college enrollment outcomes, showing the results from school-by-cohort fixed effects regressions (controlling for baseline student achievement) for the full sample, the sample restricted to students who failed a course in the prior year, and students who had 8th grade baseline data. The results are very consistent across the different study samples. Students taking courses online in this school district have, on average, high school graduation rates that are about 10-12 percentage points higher than similar students who do not take courses online, and their 2-/4-year college enrollment rates are also about 2 percentage points (or less) higher (compared to similar students not taking courses online). The estimates for four-year college enrollment are negative and around 2 percentage points or lower are statistically significant (except for the smaller 8th grade baseline sample), suggesting that students taking online courses are less likely to attend four-year colleges. Furthermore, the signs on college quality proxy measures all suggest that while taking courses online may open access to postsecondary education for these high school students, they appear to be significantly more likely to enroll in lower-quality, open admissions institutions with poorer reputations, retention rates, and completion rates, even after adjusting for student and school characteristics and in the more restricted samples of academically struggling, highly disadvantaged students.

Table 5 presents the results from IPWRA models that estimate high school graduation and college outcomes by the intensity of student 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 and in comparison with the school-by-cohort fixed effects models. In both Panels A and B, the results show that students at varying levels of online course-taking intensity are 8-12 percentage points more likely to graduate from high school (i.e., 8 ppts. at the lower end for those with three or more years of online course-taking in high school). For the outcomes of 2-/4-yr college enrollment, the coefficient estimates are mostly negative and statistically significant, generally showing small decreases (around 2 percentages points less likely to attend college), with a few exceptions. Again, the findings are very consistent with the school-by-cohort fixed effects models. For the full sample, the differences in the quality of institutions attended by students with more intensive online course-taking (vs. with no online course-taking or only 1-2 courses in 1-2 years) are noticeably larger and mostly statistically significant. The coefficient estimates on the college quality again suggest that students who enrolled in high school online courses and went on to postsecondary education were significantly more likely to be enrolled in lower-quality, open access institutions and to a larger extent as they enrolled in more online high school courses and/or over more high school years.

Model Sensitivity Test

Appendix A presents the results of the model sensitivity test where we replicate the analysis presented in Table 2 (outcomes in high school) 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 gives weight only to 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 in the value-added models. Another departure from the results of our preferred specification (and from other findings of our research, Authors, 2019b) is the average positive association of online course-taking with reading test scores in the value-added estimation with all students.

Nonparametric Bounds

We also present the estimated 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 student 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 B, 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 B (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), 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 approximately 0.10=0.12, which lies within the majority of the bounds estimates for the full sample in our preferred specification (3). For the 2-4 year college enrollment outcome, the estimates from our fixed effects and IPWRA models (shown for the sample of students who failed at least one course) are smaller than (outside of) most of the estimated bounds, and they are around the lower bound for 4-year college enrollment and the one college quality indicator. The smaller and possibly more selective samples for estimating the postsecondary outcomes (i.e., estimated only for students who have exited high school) might also contribute to the greater imprecision of those bounds, although as noted above, most of the bounds do not cross zero, which increases confidence in the estimated direction of the relationships we have modeled.

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 (relatively inexpensive) 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 positive (plausibly causal) relationship between high school online course-taking and graduation rates, with associations ranging from a lower bound of about 8 percentage points (in our most restricted sample) to an estimated 12 percentage point increase in graduation rates. These estimates correspond fairly closely with the rise in graduation rates we saw in our study district over this same period. In fact, our analysis suggests an almost mechanical relationship that works mainly for upper classmen, who are able to replace failed courses with online credits earned more quickly—an increase of approximately 0.4-0.5 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 (Authors, 2019b). 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 necessary to achieve goals of educational equity and social justice.

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 school graduation rates have risen. Our related research (Authors, 2019a; Authors, 2019b) 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. If school districts value equity and quality in educational opportunities and outcomes, they need to allocate more resources toward providing the types of instructor and student supports that will contribute to more effective content learning online and to preparing students for continuing education beyond high school.

In fact, in following students after high school and examining their postsecondary education options, we do find some small, statistically significant, positive associations between high school online course-taking and college enrollment, primarily for those with very limited online course-taking, i.e., no more than 1-2 courses over 1-2 high school years. We estimated increases in (2-year/4-year) college enrollment of about 2 percent for those with the lowest intensity of online course-taking. If we believe even small increases in college enrollment are plausibly causal, then online course-taking that increases the likelihood of graduation for a predominately lower performing student population may be opening the door for students to postsecondary education opportunities that they might not have otherwise had. However, we find only negative, statistically significant associations between online course-taking (of all levels of intensity) and four-year college enrollment across each of our student samples. In addition, we also find that students who take courses online in high school enroll in poorer quality postsecondary institutions, with lower average retention and completion rates, which casts some doubt on whether they will realize longer-term benefits from these investments.

Finally, we conclude by reiterating 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 vendor-developed 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 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. Course Failures and Graduation Rates, Credits Earned and Grade Point Average (GPA) Over Time

Figure 1.a

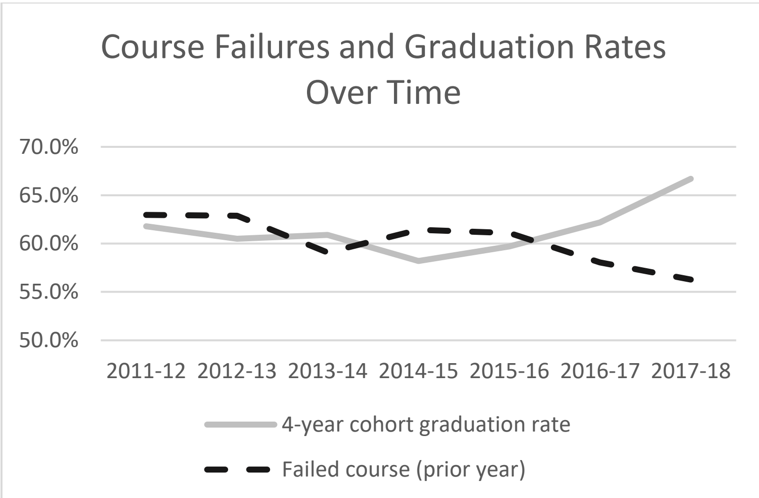

Figure 1.b

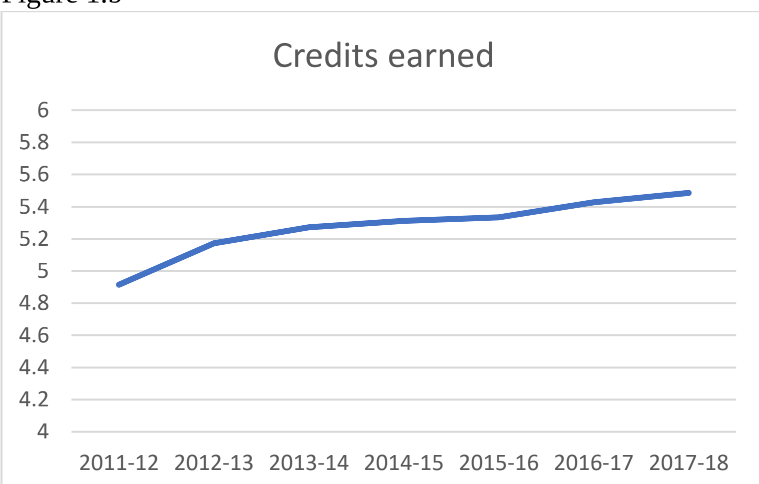

Figure 1.c.

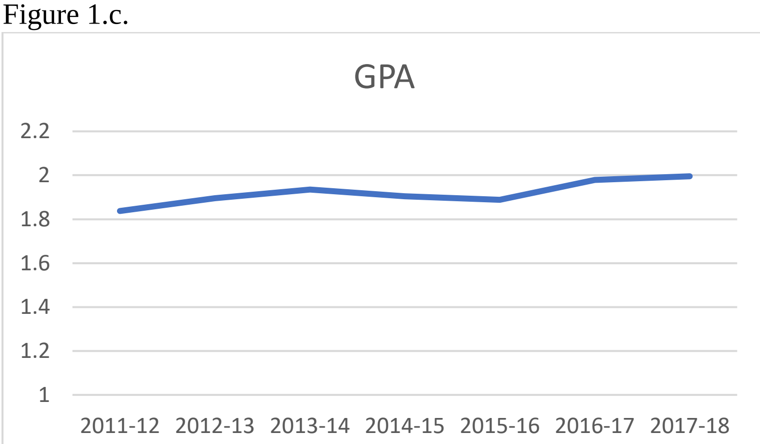

Table 1: Descriptive Statistics for Analytic Samples

	All Students (N=82,151)		Students Who Failed Course in Prior Year (N=49,541)		Students with 8th Grade Baseline Data (N=10,926)	
	Not Online Course-taker	Online Course-taker	Not Online Course-taker	Online Course-taker	Not Online Course-taker	Online Course-taker
Female	0.505	0.455*	0.443	0.435	0.383	0.396
	(0.500)	(0.498)	(0.497)	(0.496)	(0.486)	(0.489)
Black	0.591	0.674*	0.674	0.693*	0.691	0.730*
	(0.492)	(0.469)	(0.469)	(0.461)	(0.462)	(0.444)
Asian	0.081	0.030*	0.043	0.023*	0.034	0.012*
	(0.273)	(0.171)	(0.202)	(0.149)	(0.182)	(0.108)
Hispanic	0.201	0.207	0.202	0.205	0.207	0.189*
	(0.401)	(0.405)	(0.401)	(0.404)	(0.405)	(0.392)
Other Race	0.007	0.008*	0.007	0.009	0.009	0.010
	(0.081)	(0.092)	(0.086)	(0.094)	(0.094)	(0.098)
English Language Learner (ELL)	0.098	0.075*	0.101	0.073*	0.133	0.074*
	(0.297)	(0.263)	(0.301)	(0.261)	(0.340)	(0.263)
Free/Reduced Price Lunch Eligible (FRL)	0.760	0.821*	0.840	0.832*	0.884	0.879
	(0.427)	(0.384)	(0.367)	(0.374)	(0.320)	(0.326)
Special Education Eligible (SPED)	0.205	0.235*	0.263	0.253*	0.362	0.334*
	(0.404)	(0.424)	(0.440)	(0.435)	(0.480)	(0.472)
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 Baseline SY	0.524	0.836*	1.000	1.000	0.963	0.980*
	(0.499)	(0.370)	(0.000)	(0.000)	(0.189)	(0.139)
Credits Attempted in Prior SY	6.998	6.978*	7.061	7.048	6.469	6.619*

	(1.173)	(1.472)	(1.314)	(1.381)	(1.563)	(1.477)
GPA in Prior SY	2.088	1.433*	1.352	1.197*	0.640	0.598*
	(1.039)	(0.858)	(0.741)	(0.676)	(0.636)	(0.534)
Percent Absent	0.166	0.245*	0.246	0.266*	0.393	0.359*
	(0.198)	(0.214)	(0.229)	(0.218)	(0.282)	(0.246)
N	61,311	20,840	32,123	17,418	7,181	3,745

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)	0.139***	-0.163***	0.432***	0.623***
	(0.025)	(0.035)	(0.045)	(0.057)
High School GPA (N=82,151)	0.024***	-0.092***	0.195***	0.194***
	(0.007)	(0.009)	(0.012)	(0.015)
Reading Test Scores (Std.) (N=36,945)	-0.015	-0.022	0.024	-0.026
	(0.016)	(0.021)	(0.026)	(0.047)
Math Test Scores (Std.) (N=37,120)	-0.024*	-0.017	-0.024	0.031
	(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=44,408)	0.172***	-0.044	0.395***	0.494***
	(0.033)	(0.043)	(0.061)	(0.082)
High School GPA (N=49,657)	0.018**	-0.047***	0.148***	0.086***
	(0.009)	(0.011)	(0.016)	(0.021)
Reading Test Scores (Std.) (N=22,116)	-0.031	-0.034	0.018	-0.039
	(0.020)	(0.025)	(0.034)	(0.068)
Math Test Scores (Std.) (N=22,202)	-0.029*	-0.015	-0.041	0.016
	(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. All models include student, year, and grade fixed effects. 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 online courses, 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. Models predicting spring test scores also controlled for fall test scores.

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

	Never Enrolled Online (N=26,864)	Enrolled Online			
		Enrolled Online (N=20,249)	1-2 Courses in 1-2 Years (N=12,014)	3 or More Courses in 1-2 Years (N=5,414)	Enrolled in 3-4 Years (N=2,662)
Graduated High School	0.494	0.460*	0.486	0.518	0.501
	(0.500)	(0.498)	(0.500)	(0.500)	(0.500)
Enrolled in 2 or 4-yr. College†	0.316	0.200*	0.234	0.203	0.176
	(0.465)	(0.400)	(0.424)	(0.402)	(0.381)
Enrolled in 4-yr. College/Univ.	0.160	0.075*	0.097	0.073	0.054
	(0.366)	(0.263)	(0.296)	(0.259)	(0.226)
US News-Rated Institution	0.150	0.056*	0.072	0.035	0.044
	(0.357)	(0.229)	(0.259)	(0.183)	(0.206)
Open-Admissions College	0.459	0.746*	0.696	0.844	0.830
	(0.498)	(0.435)	(0.460)	(0.363)	(0.376)
Highest Degree: Associates	0.237	0.259*	0.268	0.253	0.259
	(0.425)	(0.438)	(0.443)	(0.435)	(0.438)
Highest Degree: Graduate	0.476	0.160*	0.217	0.095	0.091
	(0.499)	(0.367)	(0.413)	(0.293)	(0.287)
Predominant Degree: Assoc.	0.268	0.298*	0.313	0.299	0.281
	(0.443)	(0.457)	(0.464)	(0.458)	(0.450)
Very High Research (R1)	0.145	0.033*	0.049	0.011	0.012
	(0.352)	(0.178)	(0.216)	(0.105)	(0.109)
First-year Retention Rate	0.664	0.576*	0.592	0.546	0.549
	(0.153)	(0.127)	(0.132)	(0.111)	(0.110)
Completion Rate	0.363	0.242*	0.260	0.215	0.203
	(0.231)	(0.166)	(0.175)	(0.137)	(0.144)

Note: *Difference in means (between online course-takers and students not taking courses online) is statistically significant at $p<0.05$. US News and College Board data are not available for all students in the sample.

Table 4: Graduation and College Enrollment Outcomes

Method and Analysis Sample	School-by-Cohort Fixed Effects Model		
	Full Sample	Failed Course	8th Gr. Baseline
	N=39,508	N=24,466	N=10,925
Graduated High School	0.098***	0.106***	0.118***
	(0.004)	(0.005)	(0.009)
Enrolled in College (2-Year or 4-Year)†	0.008*	0.023***	0.027***
	(0.004)	(0.005)	(0.006)
Enrolled in a 4-Year College or University	-0.025***	-0.006*	-0.004
	(0.003)	(0.003)	(0.004)
US News Rated Institution	-0.051***	-0.024***	-0.040***
	(0.006)	(0.006)	(0.013)
Open-Admissions College	0.078***	0.062***	0.092**
	(0.011)	(0.014)	(0.040)
Highest Degree: Associates	0.015**	0.011	0.012
	(0.008)	(0.010)	(0.024)
Highest Degree: Graduate	-0.065***	-0.033***	-0.044**
	(0.008)	(0.009)	(0.019)
Predominate Degree: Associates	0.012	0.006	-0.009
	(0.008)	(0.011)	(0.025)
Very High Research Activity (R1)	-0.017***	-0.010***	-0.016*
	(0.004)	(0.004)	(0.009)
First-Year Retention Rate	-0.017***	-0.009**	-0.019*
	(0.003)	(0.004)	(0.011)
Completion Rate	-0.027***	-0.011**	-0.010
	(0.004)	(0.005)	(0.013)
Year & Grade Fixed Effect	No	No	No
School-by-Cohort Fixed Effect	Yes	Yes	Yes
Student Covariates	Yes	Yes	No
School Covariates	Yes	Yes	No

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 online courses, school-by-year variables for student demographic characteristics, school type, and courses offered.

Table 5: Graduation and College Enrollment Outcomes by Online Course-taking Intensity, IPWRA Estimation: Baseline = Last Year Pre-Online Course-taking

Panel A	Full Sample (N=33,975)		
	Enrolled in 1-2 online courses in 1-2 years	Enrolled in 3 or more online courses in 1-2 years	Enrolled in online courses in 3 or more years
Graduated High School	0.120***	0.127***	0.083***
	(0.012)	(0.018)	(0.028)
Enrolled in College (2-Year or 4-Year)†	0.026**	-0.018	-0.024
	(0.012)	(0.019)	(0.025)
Enrolled in a 4-Year College or University	-0.019***	-0.078***	-0.028
	(0.006)	(0.007)	(0.021)
US News Rated Institution (N=37,494)	-0.024***	-0.088***	-0.028
	(0.006)	(0.008)	(0.023)
Open-Admissions College (N=10,211)	0.080***	0.282***	0.112**
	(0.023)	(0.031)	(0.049)
Highest Degree: Associates (N=18,762)	0.036***	0.080***	0.026
	(0.012)	(0.022)	(0.030)
Highest Degree: Graduate (N=13,241)	-0.070***	-0.219***	-0.129***
	(0.014)	(0.024)	(0.027)
Predominate Degree: Associates (N=18,762)	0.040***	0.092***	0.009
	(0.012)	(0.023)	(0.029)
Very High Research Activity (R1) (N=18,762)	-0.024***	-0.071***	-0.045***
	(0.009)	(0.014)	(0.007)
First-Year Retention Rate	n.a.	n.a.	n.a.
Completion Rate	n.a.	n.a.	n.a.

Panel B	Students Who Failed a Course in Pre-Treatment Year (N=24,466)		
	Enrolled in 1-2 online courses in 1-2 years	Enrolled in 3 or more online courses in 1-2 years	Enrolled in online courses in 3 or more years
Graduated High School	0.079***	0.113***	0.079***
	(0.012)	(0.020)	(0.025)
Enrolled in College (2-Year or 4-Year)	0.012	-0.013	-0.052***
	(0.010)	(0.013)	(0.013)
Enrolled in a 4-Year College or University	-0.001	-0.014**	-0.028***
	(0.004)	(0.006)	(0.007)
US News Rated Institution (N=22,847)	-0.004	-0.020***	-0.019***
	(0.004)	(0.004)	(0.006)

Open-Admissions College	n.a.	n.a.	n.a.
Highest Degree: Associates (N=9,477)	0.019	-0.019	-0.031
	(0.014)	(0.019)	(0.019)
Highest Degree: Graduate (N=6,410)	-0.013	-0.089***	-0.085***
	(0.011)	(0.012)	(0.018)
Predominate Degree: Associates (N=9,477)	0.017	-0.014	-0.048**
	(0.014)	(0.020)	(0.020)
Very High Research Activity (R1) (N=9.477)	-0.005	-0.024***	-0.029***
	(0.005)	(0.004)	(0.004)
First-Year Retention Rate	n.a.	n.a.	n.a.
Completion Rate	n.a.	n.a.	n.a.

Notes: *p<0.05 **p<0.01. Standard errors in parentheses. Student covariates include whether students failed a course (only in Panel A models), 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 online courses, school-by-year variables for student demographic characteristics, school type, and courses offered. For several measures of college quality above with estimates not available, the IPWRA models did not converge.

Appendix

As our preferred specification when examining high school outcomes (a student, school and grade-by-year fixed effects approach with lagged achievement information) more heavily weights students who switched to or from online course enrollment over the period of study, we also present here results from our alternative 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)	-0.021	-0.255***	0.377***	0.596***
	(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)	0.042***	0.024*	0.015	0.051
	(0.011)	(0.013)	(0.019)	(0.031)
Math Test Scores (Std.) (N=37,120)	-0.014*	-0.027**	0.012	0.081***
	(0.008)	(0.011)	(0.016)	(0.025)
Student Fixed Effect	No	No	No	No
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=44,008)	0.044**	-0.112***	0.289***	0.406***
	(0.021)	(0.026)	(0.044)	(0.058)
High School GPA (N=49,657)	-0.008	-0.047***	0.111***	0.007
	(0.006)	(0.008)	(0.013)	(0.015)
Reading Test Scores (Std.) (N=22,116)	0.033***	0.030**	-0.008	-0.000
	(0.012)	(0.015)	(0.023)	(0.038)
Math Test Scores (Std.) (N=22,202)	-0.012	-0.013	-0.008	0.044
	(0.010)	(0.012)	(0.019)	(0.029)
Student Fixed Effect	No	No	No	No
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 online courses, school-by-year variables for student demographic characteristics, school type, and courses offered. Models predicting spring test scores also controlled for fall test scores.

Appendix B: 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 1: 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 2: 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 3: 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 3)*

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.)

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---------------------------------------------------------------------------
 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.
---------------------------------------------------------------------------
```

Endnotes

[1] The NAEP is the largest ongoing, nationally representative assessment of student math and reading achievement. (See https://nces.ed.gov/nationsreportcard/).
[2] See: https://nces.ed.gov/fastfacts/display.asp?id=51.
[3] https://www.nrms.k12.nc.us/cms/lib011/NC01800012/Centricity/Domain/64/gcs_m001.pdf
[4] Levin (2009) described four approaches that he used to estimate the cost of educational interventions: (1) accounting for the specific resources used, such as personnel, facilities, materials, etc.; (2) using market and quasi-market or shadow prices to place costs on these resources; (3) obtaining total costs for the intervention as well as average costs and marginal costs per student; and (4) analyzing the distribution of cost burdens among governmental and nongovernmental entities and clients to find out who was paying for the intervention. In our estimation of the costs of credit recovery, we followed Levin in accounting for the costs of the specific resources used in operating a credit recovery program, obtaining the prices directly from the school district.
[5] In related work (Authors, 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.
[6] 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.
[7] We do not restrict our measure of high school completion to those who graduate in four years because whether (versus when) students earned their diploma was of greater interest in this study. Furthermore, data on students' first year in the district was missing for many students, making an accurate calculation of the four-year graduation rate possible only with a restricted sample.
[8] 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/.
[9] https://collegescorecard.ed.gov/data/
[10] 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.
[11] https://wisedash.dpi.wi.gov/Dashboard/portalHome.jsp.
[12] Ideally, if all students were enrolled in the district for each of four years of high school and had attended only one high school, giving us data on each student for each year of high school enrollment, then the school measures included in our models would be identical among students in the same school-by-cohort group. However, with considerable variation among students in the number of years of data available for their time in high school and relatively high rates of transfer between high schools, these variables were not always constant across students in the same school-by-cohort. Thus, the inclusion of these variables aims to control for (to the extent possible) differences in assignment to online course-taking and other school-based variations in school experiences not associated with online course-taking.