Online Education in Community Colleges: Access, School Success, and Labor-Market Outcomes

By

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Dedication

I would like to dedicate this dissertation to my family and friends who have supported me, helped me, and cheered me on through the struggles and successes of graduate school. In particular, I dedicate this to my ever-supportive husband, Hans, and my daughter, Elsa, who have helped me to focus on what is important in life, to keep charging ahead, and to keep smiling even when the going gets rough. I also dedicate this to my mom, Susan, my dad, Clayton, and my brother, Derek, who have been and will always be there for me and be cheering me on, regardless of where I am and what I am doing.

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Chapter 1. Estimating the Impact of Online Education on Labor-Market Outcomes

Abstract

This paper provides the first evidence on the effect of online education on labor-market outcomes. The analysis herein uses administrative data on individuals who enroll in a statewide community-college system paired with unemployment-insurance records from the state. Together, these data track the educational attainment and earnings of over 100,000 first-time college students. I use an individual-fixed-effects estimation strategy and show that students who complete courses in the online format experience larger earnings gains than their peers who complete courses in the traditional, face-to-face format. Estimates show large benefits to completing online coursework in the years immediately following initial enrollment, when a student may still be enrolled or may have just exited college. Estimates also show that earnings fall less during enrolled periods for students who enroll in online courses. These findings suggest that online education allows students to acquire college credit at a lower opportunity cost. In the long run, estimates show that there is a large, positive benefit associated with completing any amount of online credits but no significant dosage effect of completing greater amounts of online coursework.

1 Introduction

Online education in college is expanding dramatically. In a survey of over 2,800 colleges and universities, more than 6.7 million students attempted an online course in the fall of 2011 (Allen and Seaman, 2013). This represents a 9.3% increase over the number reported in the fall of 2010 and a 319% increase over the number reported in 2002.

The expansion of online education has important implications for the education of our country's workforce, because it may help expand access to higher education by both lowering the cost of college and making college more flexible. In fact, President Obama's recent plan to make college more affordable highlights online education as a mechanism for achieving this goal.[1] President Obama also announced a goal of having the highest proportion of college graduates in the world by 2020.[2] Attaining the latter goal will require more than helping traditional college students transition from high school to college and succeed; it will require helping non-traditional students – likely working adults – gain access to and be successful in college (Lane, 2012). Online education is, in theory, one way we can provide a more flexible style of college that is appropriate for these non-traditional students.

It is important that we understand the effects of this educational innovation on students' outcomes. Existing research focuses only on shorter-term, in-school outcomes, such as exam scores and grades. These findings do not necessarily generalize to important, non-school outcomes such as earnings and employment. This paper provides the first evidence on the differential effect of online, college education, relative to traditional, face-to-face, college education, on labor-market outcomes.

There are a number of reasons why online education may have a different effect on earnings and employment than face-to-face education. First, online education may lead to a different level of human capital accumulation, if it is of a different quality. If so, human capital theory suggests that students who acquire online education should earn different wages in the labor market. Second, online education may have lower opportunity costs, both in terms of forgone earnings and forgone experience, because – thanks to the flexibility of the online format – students can remain more attached to the labor market while being enrolled in college. Students who take online courses may have higher earnings while enrolled and experience additional gains in earnings because of

[1] The White House, Office of the Press Secretary, Release August 22, 2013, `http://www.whitehouse.gov/the-press-office/2013/08/22/fact-sheet-president-s-plan-make-college-more-affordable-better-bargain-`, Accessed September 29, 2013. The relative costs of online education, however, are not well understood. This is discussed more in the concluding section.

[2] `http://www.whitehouse.gov/issues/education/higher-education`, Accessed October 4, 2013.

the work experience they accumulated while in school. Third, online education may help students persist further in college to complete more credits or earn credentials, which may be rewarded in the labor market. My research design controls for the quantity of credits completed and estimates the effect of online education on earnings via the former two pathways. The third channel may be an important additional channel through which online education affects labor-market outcomes that I do not capture in this paper.

This paper focuses on students who enroll in community colleges. Community colleges enroll the largest share of undergraduate students, 39%.[3] Furthermore, online instruction is most commonly found in this sector of higher education. In 2007, 96% of public, two-year institutions offered online courses (Parsad and Lewis, 2008), more than any other sector of higher education.[4]

This paper also focuses on older, non-traditional students (which I define as age 20 and older at the time of initial enrollment) within the community-college sector. Older students make up a large portion of the community-college student body – over 40% of community college students were age 25 or older in 2009 (Mullin, 2012). Older students are also more likely to participate in online education (Radford, 2011; Jaggars and Xu, 2010; Jaggars and Xu, 2011). Furthermore, online courses may be targeted precisely at this population; the most popular reasons colleges state for offering distance education are to meet student demand for flexible schedules and to provide access to college for students who would otherwise be without because of family, work-related, or geographic reasons (Parsad and Lewis, 2008).

Focusing on older students is also crucial in light of my estimation strategy. I employ an individual-

[3] Author's calculation from statistics in Table 226, U.S. Department of Education, Digest of Education Statistics, 2012.

[4] The next highest share is found in public, four-year institutions – 86% offered online courses in 2007. Furthermore, Public two-year institutions have the highest participation rate in "distance education"–22% of undergraduates at these institutions participate in such courses (Radford, 2011). "Distance education" is defined here as a course that "[is] not a correspondence course but [is] primarily delivered using live, interactive audio or videoconferencing, pre-recorded instructional videos, webcasts, CD-ROM or DVD, or computer-based systems delivered over the internet." Thus, distance education does not correspond perfectly with the fully online courses I evaluate in this paper, but this fact is still suggestive of the popularity of online courses in the community-college sector.

fixed-effects strategy that uses each individual as their own counterfactual by comparing post-college earnings and employment to pre-college earnings and employment. This estimation strategy alleviates many concerns about selection into online education by controlling for unobservable, fixed student characteristics that are correlated with both the amount of education students complete in the online format and their labor-market outcomes. It relies on pre-college earnings and employment being good measures of earnings and employment potential absent a college education. Pre-college earnings that we observe for younger/traditional students who move straight from high school to community college are likely from temporary, low-wage jobs held during school or over the summer, and may not represent what these individuals would have earned in the labor market without any higher education.[5] For older students, however, pre-college earnings are a better representation of their earning potential. I operationalize the individual-fixed-effects strategy using a unique panel of data that tracks the educational attainment and earnings for over 100,000 community-college students in state A.[6]

Online education comes in many forms ranging from Massive Open Online Courses (MOOCs), which are large-scale, open-access courses offered on the internet, to web-enhanced courses in which the instructors of face-to-face courses simply post readings and assignments for students to retrieve on the course's website. The online education studied here falls in the middle. This paper focuses on full-semester, college-level courses in which the instruction is delivered fully online. This is distinct from hybrid courses in which instruction is split between online and face-to-face delivery and students are required to be on campus for instruction at specific times each week. This is also distinct from online training sessions that attempt to teach a specific concept or skill in a single, short session. This study focuses on fully online education that is intended to replicate the learning that would occur in a comparable full-semester, face-to-face, college-level course.

I find that online education has a positive differential effect on labor-market outcomes; students

[5]In fact, these earnings might be especially low for individuals with higher earning potential if future earning potential is positively correlated with time spent on academic and extracurricular pursuits in high school.

[6]The state has been de-identified as per data-use agreements.

who complete online credits experience larger earnings and employment gains than their peers who complete courses in the traditional, face-to-face format. Linear, baseline specifications show that the benefit to online coursework occurs largely in the years immediately following initial enrollment, when a student may still be enrolled or have just recently completed their time in college. The positive differential effect serves to offset the early negative main effect of completing college credit that results as students pull away from the labor market to take classes. Estimates also show that earnings fall less during enrolled periods for students who enroll in online classes. These findings suggest that online education allows students to accumulate college credit at a lower opportunity cost. Semi-parametric specifications that allow for an effect of completing *any* online coursework and a dosage effect of completing larger amounts of online coursework show that the dosage effect is large and positive in the short run. Overtime, however, the dosage effect shrinks becoming negative, though small and imprecisely estimated. The effect of completing any online coursework is insignificant in early years but grows to be large, positive, and significant as time elapses since students' initial enrollment. Controlling for the fields in which students complete credits increases the dosage effect of online education. This suggests that students tend to take more online courses in lower-return fields, and failing to control for field of study may result in downwardly biased point estimates.[7]

The long run estimates that show a large fixed benefit associated with completing any online coursework but an insignificant dosage effect raise some concerns about lingering selection bias. Ideally, treatment effects should move with the size of the treatment. One potential source of bias may stem from differences in trends between those who do and do not participate in online education–such trends are not accounted for by the individual-fixed-effects estimation strategy used herein. If, for example, individuals who choose to participate in online education have faster growing earnings, this may cause upward bias in the estimates. This issue will be discussed further in the concluding section along with potential extensions to the analysis that could help to address

[7]Similar work-in-progress in another state suggests that this bias is not generalizable. Results in the other state show that accounting for field of credits decreases the differential effect of online credits on earnings suggesting that students tend to take online courses in higher-return fields.

such bias.

2 Related Research

This paper falls at the intersection of the job-training literature – which studies the effect of training programs on adult workers' labor-market outcomes – and the education-production-function literature – which studies the effect of educational inputs on students' outcomes.

Recent research on inputs in higher education shows that higher-quality college instructors (measured by academic rank, experience, and degree status) increase "deep learning" (i.e., learning measured in future courses) (Carrell and West, 2010). It also shows that adjunct instructors may positively impact students' subsequent interest in a given subject, especially for those more closely related to specific occupations (Bettinger and Long, 2010), and may induce greater learning (Figlio et al., 2013). Evidence shows that college class size has no effect on students' academic performance (Machado et al., 2008), though it may have a negative effect at the tails of the distribution (Bandiera et al., 2010).

The evidence on how well online education works in college is growing, but mixed. A recent meta-analysis by the U.S. Department of Education (2010) concluded that "students in online learning conditions performed modestly better than those receiving face-to-face instruction." This study has since received criticism because its broad conclusion is somewhat misleading (Figlio et al., 2013; Jaggars and Bailey, 2010). In fact, the superior online outcomes are driven by hybrid learning conditions in which students receive a mix of online and face-to-face instruction, and no significant differences were found between students in purely online and face-to-face conditions. Furthermore, many of the studies that compare fully-online to face-to-face instruction actually evaluate short training sessions as opposed to full-semester, college-level courses.

Some studies that do focus on fully online, full-semester, college-level courses do find that students in the online format perform better in terms of final grades and test scores.[8] The majority of the evidence, however, suggests that students' outcomes are worse in the online format. Withdrawal and dropout rates are typically higher in online courses relative to their face-to-face counterparts (Carr, 2000; Carpenter, Brown, and Hickman, 2004; Xu and Jaggars, 2011; Xu and Jaggars, 2013). Students in online courses score lower on exams, earn lower grades, and are less likely to pass the course (Brown and Liedholm, 2002; Coates et al., 2004; Jaggars and Xu, 2010; Jaggars and Xu, 2011; Xu and Jaggars, 2011; Xu and Jaggars, 2013; Figlio et al., 2013). Lastly, students who enroll online in early semesters are found to re-enroll at lower rates (Jaggars and Xu, 2010; Jaggars and Xu, 2011).

Little of this research establishes strong causal relationships. Xu and Jaggars (2011), Xu and Jaggars (2013), and Figlio et al. (2013) are exceptions. Xu and Jaggars (2011) employ propensity score matching to estimate the effect of online instruction on completion rates and grades in two introductory community-college courses. They control for a rich set of student- and school-level covariates and find that students in online sections are 11-15 percentage points more likely to drop out of the course and, conditional on completing the course, 7-10 percentage points less likely to earn a C or better. Propensity score matching generates causal estimates if the selection process into online courses depends only on the observable covariates. Unobservable covariates, however, such as work and family responsibilities, motivation, or ability, may be correlated with both enrollment in online courses and the course outcome and may bias the estimates. Xu and Jaggars (2013) builds upon this research by implementing an instrumental variables strategy, using travel distance to the student's institution as an instrument for enrollment in the online section of a course, to account for bias stemming from unobservable student characteristics. They continue to find that online instruction has a negative effect on course completion and grades. In related work, I am applying other quasi-experimental methods that remove bias stemming from unobserved course-

[8]To name a few: Navarro and Shoemaker, 2000; Schoenfeld-Tacher et al., 2001; Carpenter, Brown, and Hickman, 2004; Cavus and Ibrahim, 2007; and Washburn, 2012.

and student-level characteristics, course fixed effects, student fixed effects, and instrumental variables (using the share of seats in a course offered online as the instrument), to estimate the effect of online instruction on course-level success (defined as passing the class as opposed to failing or withdrawing). I find that students are 9-13 percentage points less likely to pass an online class.

Figlio et al. (2013) provides the most convincing causal evidence. They present experimental estimates of the effect of online instruction on exam scores for students in an introductory economics course at a large research university. They find that live instruction is modestly, though not significantly, superior to online instruction, but significantly superior for certain subgroups of students (males, Hispanics, and lower-achieving students).[9] In general, the evidence on the effectiveness of online education is mixed, but the most convincing estimates suggest that students' in-school outcomes are worse in the online format.

Although we certainly care about the impact of online education on in-school outcomes, such as course completion and exam scores, these findings may not generalize to non-school outcomes. The impact on non-school outcomes that are more closely linked to individuals' livelihoods, such as earnings and employment, are especially important. By addressing the effect of online education on older individuals' labor-market outcomes, my research links with the job-training literature that seeks to understand the impact of additional training and education on adult workers' outcomes. Evidence from this literature suggests that training provided for adult workers via the Workforce Investment Act increases quarterly earnings by \$300-\$450 per quarter (Andersson et al., 2013) and that a year of community-college retraining for displaced workers increases quarterly earnings by 7-13% (Jacobson et al., 2005a; Jacobson et al., 2005b). Community-college credentials are shown to increase older students quarterly earnings by \$300-\$2,400 with higher gains for Associate's Degrees and lower gains for Certificates (Jepsen et al., 2012). Furthermore, the job-training literature commonly employs the method used herein, individual fixed effects, to address issues of selection

[9] Bowen et al. (2012) also conducted a set of experiments in an introductory statistics course at several public universities. Their study, however, focused on differences between hybrid and face-to-face courses and found no significant differences in outcomes between these two modes of delivery.

bias.[10]

3 Theoretical Motivation

Human capital theory suggests that additional education contributes to a worker's stock of knowledge, making him/her more productive in the work place. A more productive worker will earn higher wages than a less productive worker. The stock of human capital, however, may not depend solely on the amount of education completed, but also on the quality of the education. Prior research shows that individuals attending higher-quality schools (with quality measured by inputs, such as teacher salaries or class size, or selectivity to name a few) earn higher wages (Dale and Krueger, 2002; Black and Smith, 2004; Black and Smith, 2006). Online education may be of a different quality than its face-to-face counterpart (as measured by differences in grades and exam scores across students in online and face-to-face classes) and, thus, may result in different levels of human capital accumulation. If this is true, online schooling should have a differential effect on employment and earnings. Previous research on online education is mixed, but the most convincing estimates suggest that online instruction may be inferior to traditional, face-to-face instruction, and, if this is true, one would expect the differential effect on earnings to be negative.

Enrollment in online courses also offers students greater flexibility, because they do not have to be on campus at any specific time to complete the course. Students who take online courses can remain more fully attached to the labor market, thus lowering their cost of schooling in terms of forgone earnings. Additionally, students can continue to accumulate work experience while in school, which, in and of itself, will have a positive effect on labor-market outcomes.[11]

[10] Many other studies also employ this approach to estimate returns to schooling (Arcidiacono et al., 2008; Bahr et al., forthcoming; Cellini and Chaudhary, 2012; and Turner, 2011).

[11] Ruhm (1997) and Light (2001) find positive wage gains for students who work during high school and college. Scott-Clayton (2012) documents a trend of increasing employment for college enrollees, but concludes that changes in the returns to work experience are not a likely cause of the increased in-school employment.

Signaling theory offers another popular explanation of how education can affect earnings (Spence, 1973). If, for example, employers believe that online education signals a lower level of motivation or committedness on behalf of the potential employee, then employers would offer a lower wage or be less likely to hire individuals who complete online schooling. For the schools in my data, transcripts do not contain information about whether any given credit was completed online. Additionally, the schools in the sample are not widely regarded as "online colleges" like the University of Phoenix or DeVry. Barring a potential employee disclosing their participation in online schooling, the consequences of signaling theory are not relevant for this study.

An individual will decide on the optimal amount of education to complete online by considering the differential quality and the differential costs of this type of education. The next section presents a two period, discrete choice model that formalizes this choice.

3.1 Discrete Choice Model

Consider the following two-period, discrete-choice model in which all individuals attend school during the first period (choosing between online schooling (I, for internet) and face-to-face schooling (F)), and then work during the second period. Individuals will choose online versus face-to-face based on which option offers them the highest total utility (i.e., the sum of utility between the first and second periods). In the first period, individuals who enroll face-to-face earn utility

$$U_1^F = -c(F) \tag{1}$$

where c(F) is the cost of face-to-face schooling. Individuals who enroll online earn utility

$$U_1^I = w_0 - c(I) \tag{2}$$

where w_0 is the wage an individual can earn without having completed any post-secondary education and c(I) is the cost of online schooling. Note that online students work while enrolled and thus face-to-face students have forgone earnings of w_0.[12]

In the second period, individuals who completed school face-to-face earn utility

$$U_2^F = w_1 \tag{3}$$

where w_1 is the wage an individual can earn after completing college. Individuals who completed school online earn utility

$$U_2^I = \delta w_1 + X \tag{4}$$

where $\delta >= 0$ represents the relative quality of online education and X is the earnings gain from acquiring work experience in the first period.

Thus, an individual will choose online schooling if

$$\begin{aligned} &U_1^I + U_2^I > U_1^F + U_2^F \\ &w_0 - c(I) + \delta w_1 + X > -c(F) + w_1 \\ &(\delta - 1)w_1 + X > c(I) - c(F) - w_0 \end{aligned} \tag{5}$$

where the left side of the inequality represents the differential earnings gain associated with online schooling, that incorporates the reduced wages but the boost from experience, and the right side of the inequality represents the differential cost (both direct and opportunity) of online schooling.[13]

[12] We could also allow face-to-face students to work and think of w_0 as the differential earnings between online and face-to-face enrollees.

[13] This exposition is obviously simplified. If one assumes, based on the most convincing prior literature, that online education is of lower quality than face-to-face education, then individuals would weigh the reduced quality of online education against the reduced costs to determine the optimal amount of online education. If, however, online education is in fact equal or superior to face-to-face education and has lower costs, we would expect to see everyone taking all online courses. I do not observe this in the data, suggesting that the presence of information constraints, supply constraints, and/or heterogeneity in preferences for instructional format induces students into the face-to-face format even if online is equal or superior in quality.

$c(I) <= c(F)$ because tuition is the same for online and face-to-face courses, but travel costs associated with online coursework may be lower.

This model abstracts from several details of reality, such as the fact that students often take a mix of online and face-to-face coursework, but it captures the idea that students will consider the differential costs and benefits of the two formats when making their enrollment choice. It also captures the idea that there may be a differential effect of online coursework operating through differential human-capital accumulation and differential work-experience accumulation.

The model also suggests interesting heterogeneity in selection into online education. If we allow first-period wages, w_0, to vary across individuals we would see that those with higher w_0 (i.e. higher opportunity cost) are more likely to select into online education. In later sections, I present evidence that those who select into online education were in fact earning more prior to enrollment, suggesting they would have a higher opportunity cost. If we allow the costs of online education, $c(I)$, and the costs of face-to-face education, $c(F)$, to vary across individuals, those with higher $c(F)$ would be more likely to choose online education.[14] One could imagine that females may have higher $c(F)$ because of greater childcare responsibilities, and evidence presented later shows that females are more likely to enroll in online education.

Although it would be interesting to separately estimate the differential effect of online schooling as it operates through human-capital accumulation and the differential effect of online schooling as it operates through experience, my paper will estimate the net of these two effects. From a policy perspective, this net effect is the most important parameter. Online courses will inherently have the feature of flexibility that allows students to remain more attached to the labor market. Thus, even if human-capital accumulation is lower in online classes and the differential human-capital-specific effect is negative, we may be willing to accept this if the differential experience-specific effect dominates and the net differential effect is positive.

[14] Heterogeneity in $c(F)$ and $c(I)$ can also capture differences in consumption value. Those who greatly prefer face-to-face instruction will have a lower $c(F)$.

4 Background on Online Education

There is substantial variety in online education. Online education ranges from Massive Open Online Courses (large-scale, open-access courses offered on the internet) to web-enhanced courses in which the instructors of face-to-face courses simply post readings and assignments for students to retrieve on the course's website. The online courses studied here fall in the middle. They are courses that are meant to replicate their face-to-face counterparts, but the instruction is provided fully online.[15] The online courses cover the same material and are of a similar size to the face-to-face counterparts (i.e. there are 25 students in the face-to-face section of Business 101 and 25 students in the online section). The courses are hosted by a Learning Management System, such as Blackboard or ANGEL. The same professors can be found teaching online and face-to-face courses, and the tuition is the same for online and face-to-face courses. That said, there is still a good deal of heterogeneity in the online courses studied here. For example, some online courses require in-person orientations and proctored exams and others do not. Unfortunately, I do not observe these characteristics in my data and can not control for them. Most importantly, however, the online courses I study are intended to recreate, as closely as possible, the learning that would occur in a comparable face-to-face course.

Table 1 presents information about the share of credits completed online. Approximately, 10% of all credits observed in my data are completed online. Table 1 also shows the distribution of online credits across a nine-field classification.[16] Online classes are offered across almost all fields, but are more popular in certain areas. Liberal Arts, Social/Humanities, Computer & Information Science, and Business are popular fields for online coursework. Health, Engineering & Science Technologies, and Other Technical (which includes courses in areas such as automotive mainte-

[15] Some hybrid courses, in which the instruction is split between online and face-to-face delivery, are observed in my data, but they are much less common. These courses are included in the face-to-face category for the purposes of this study.

[16] The nine field categories are assigned based on the two-digit Classification of Instructional Programs ("CIP") code of the course. The two-digit CIP code is assigned to each course based on its subject code. The two-digit CIP codes can be found here: `http://nces.ed.gov/ipeds/cipcode/browse.aspx?y=55` and the subject-CIP and CIP-9 field crosswalks are available upon request from the author.

nance, welding, etc.) are less popular fields for online coursework.

It is also important to note that online education in this context is not the same as online education at fully online institutions such as the University of Phoenix or Devry. Online education in this context should be viewed as a supplement to traditional, face-to-face education. The students in the study should be thought of as largely on-ground students who take a few classes face-to-face but rarely complete full programs of study online. Some of the community colleges do offer fully online degree programs, but many of them include some hybrid coursework that requires students to be on campus for instruction at designated times and hence are not "fully online" based on the definition used in this study.[17] Furthermore, it is rare to see students undertake fully online study. Among all the students in my sample, 2% of those who complete any credits do so exclusively online. Less than 1% of students who complete at least 10 credits do so exclusively online, and 0.6% of students who complete at least 20 credits do so exclusively online. I restrict the analytic sample to those who complete 50 or fewer credits online because a) students who complete more than this are rare and b) this context is more appropriate for estimating the effect of taking one additional course online as opposed to the effect of completing an Associate's Degree online.[18]

Figure 1 presents the distribution of total credits completed and total online credits completed among students who are ever online. The majority (68%) of students complete less than a year's worth of community-college credits (30 credits). Students who complete online credits typically only complete a few online courses (each worth 3 credits on average). Table 2 shows the total number of credits completed, total number of online credits completed, and the percent of students who completed all of their credits online by students' highest community-college credential. The statistics in this table echo the discussion in the prior paragraph; few students complete fully online degrees. For example, only 0.2% of students whose highest degree completed is an Associate's Degree take courses exclusively online.

[17] Note that community colleges in state A offer three types of credentials, Certificates, Diplomas, and Associate's Degrees, requiring 12-18, 36-48, and 64-76 credits, respectively.

[18] This only eliminates 0.4% of the students in the sample and 1.6% of the students who complete any online coursework.

5 Data

This paper uses data from all 58 community colleges in state A's community-college system. Administrative records from the community-college system provide demographic and transcript data. The transcript data provide information on all courses taken by the students, such as the title of the course, the subject area of the course, credits attempted, credits completed, and the instructional format of the course (online v. face-to-face).[19] Unemployment insurance (UI) records provide information on each student's earnings before, during, and after enrollment.[20]

The analytic dataset is a student-by-academic year panel that consists of first-time, for-credit students who enrolled in the 2001, 2002, and 2003 academic years and contains earnings observations from 1997-2009.[21] I observe earnings for all students for at least three years prior to enrollment and six years following initial enrollment. The sample contains individuals who enrolled between the ages of 20 and 45 and is restricted to observations of these individuals between the ages of 17 and 65.

Summary statistics for the sample are presented in column 1 of Table 3. They show that the average student enrolls at the age of 29.5. 40% percent of the sample is male and 60% of the sample is white. 36% of students take a remedial course in their first year. The average student completes 25.06 credits within six years of initial enrollment with 2.34 of those credits being online. The average student was earning $13,774 three years before they enrolled (this includes $0 observations).

[19]For the purposes of this analysis, I allow credits attempted to maintain the definition that was provided by the colleges meaning that courses from which students withdraw are not associated with positive credits attempted. The results do not change, however, if I associate positive amounts of credits attempted with courses in which students eventually withdraw.

[20]State UI records do not cover those who are federal government employees, self-employed, or work outside the state. Individuals in my sample for whom this applies will be coded as having $0 earnings. UI records are very commonly used for these types of analyses and research shows that estimates using UI data are similar to those using survey data (Kornfeld and Bloom, 1999).

[21]Details of the dataset and its construction can be found in the Data Appendix.

The summary statistics also provide a glimpse into the patterns of selection into online coursework. Columns 2 and 3 disaggregate the full sample by whether or not the student took an online course in their first year. Column 4 contains the difference between columns 2 and 3 and the associated t-statistic. The online-in-first-year distinction is used here, as opposed to an ever-online distinction, so as not to conflate online participation with persistence. Students who persist for longer and complete more credits have more opportunity to move themselves into an ever-online category making this binary distinction less useful. This distinction is simply for descriptive purposes. The following analysis will allow for a continuous measure of online participation as opposed to the simple dichotomization presented here.

Students who enroll in online courses upon entry into college are significantly different from their peers in many ways. Students who enroll online are, on average, 1.2 years older. Males and remedial students (defined by remedial enrollment in the first year) are less likely to enroll in online courses. These patterns of positive selection are also noted in other research (Carpenter, Brown, and Hickman, 2004; Jaggars and Xu, 2010; Jaggars and Xu, 2011; Xu and Jaggars, 2011). Students who enroll online in their first year complete more credits during their entire time in community college. Further, online enrollment in the first year is predictive of future online enrollment. Students who are online in their first year complete over 11 online credits in total within six years of initial enrollment compared to 1.35 online credits among students who do not enroll online in their first year.

Table 3 also shows that students who enroll online have higher earnings even before enrolling in college ($16,619 for students who enroll online compared to $13,453 for students who do not). A simple cross-sectional analysis might misattribute higher post-college earnings to online coursework when, in reality, students who completed online coursework may have been earning more absent any additional college credits.[22] The individual-fixed-effects analysis used in this study

[22] I have run several cross-sectional specifications, and the online differential falls significantly when controls for prior earnings are included as independent variables. This shows that the online differential is likely biased up for estimates that fail to control for prior earnings/earning potential.

accounts for these differences in pre-college earnings by comparing changes in earnings within individuals.

Figure 2 provides descriptive, visual evidence of selection into online coursework. The figure plots unconditional averages of yearly earnings (in 2011 dollars) with respect to years until/since first enrollment (these averages include zeros). Average yearly earnings are plotted separately for students who do and do not complete a course online in their first year. The bold lines indicate average yearly earnings for students who do not complete an online course in their first year (solid line) and students who do complete an online course in their first year (dashed). The thin lines indicate the share of students in each category that are still enrolled in state A's community-college system.

Figure 2 echoes the findings in Table 3. It shows that students who enroll online in their first year have higher earnings than students who do not. Importantly, students who enroll online have higher earnings *before* they enroll in community college. Thus, students who enroll online are different from their non-online peers at the outset, and students with higher earnings appear to be selecting into the online format. Figure 2 also shows that students who enroll online appear to have similar trends in earnings prior to enrollment but somewhat faster growth in earnings as time elapses after initial enrollment. Figure 2 shows that students who enroll online are more likely to still be enrolled as time elapses. Thus, their faster growth in earnings after enrollment is likely not a result of them completing their college education more quickly and having more time post-college to experience wage growth.[23] It should be noted that these are unconditional averages that only distinguish students based on whether they completed any online credits in their first year. The differences in earnings patterns shown here are simply motivational. My empirical strategy will control for additional characteristics of the students and will allow for a continuous measure of online participation.[24]

[23]Although not plotted here, trends of enrollment in any college, including other two-year and four-year colleges observed in National Student Clearinghouse Data, show that students who enroll online in their first year are also more likely to be enrolled in any college as time elapses.

[24]Appendix Figure A1 plots the share of students employed with respect to years until/since first enrollment. There

6 Empirical Strategy

The ideal way to identify the differential effect of online credits on earnings and employment would be to compare the earnings and employment of individuals who are identical in every way (i.e., had the same experience prior to enrollment, took the same number of courses in the same fields while enrolled, etc.) but who, randomly, took different amounts of these courses online. Unfortunately, online course-taking is not random. To approximate the ideal, I employ an individual-fixed-effects estimation strategy that uses the pre-schooling earnings of each individual as the counterfactual for his/her post-schooling earnings. I control for total credits completed so that I compare individuals who complete the same amount of college, but do so with varying amounts of online coursework. In other words, this estimation strategy compares the earnings gains of individuals who complete different amounts of online credits, conditional on the total number of college credits they completed.

The most basic fixed-effects specification is :

$$Y_{it} = \beta_1 TotalCredits_{it} + \beta_2 TotalOnlineCredits_{it} + \beta_3 X_{it} + \gamma_i + \theta_t + \epsilon_{it} \tag{6}$$

where Y_{it} is the earnings of individual i at time t. $TotalCredits_{it}$ is the total number of credits individual i completed prior to time t and $TotalOnlineCredits_{it}$ is the total number of online credits individual i completed prior to time t.[25] X_{it} is a vector of time-varying demographics including age, age squared, and the interactions of these two variables with gender and race. γ is a vector of individual fixed effects, θ is a vector of academic year fixed effects, and ϵ is an error term.

are no substantial differences in the unconditional share of students employed despite the obvious differences in earnings. Appendix Figures A2 and A3 replicate Figure 2 and Appendix Figure A1, but disaggregate students by whether they are *ever* online. Similar trends appear.

[25] *TotalOnlineCredits* is a subset of *TotalCredits* and can also be thought of as the interaction of *TotalCredits* with the share of credits completed that are online. The fraction would be set to 0 for students who complete no credits (this value would otherwise be missing because $x/0$ is undefined).

β_1 is the change in earnings associated with a one-credit increase in total credits completed prior to time t. β_2 is the additional change in earnings associated with a one-credit increase in total online credits completed prior to time t. The coefficient of interest, β_2, is the differential effect of online credits on earnings, conditional on the total number of credits completed. For an individual who completed 20 community-college credits, this model predicts that he/she would earn $20 * \beta_1$ more per year than an individual who completed none. For an individual who completed 20 credits, five of which were online, this model predicts that he/she would earn $20 * \beta_1 + 5 * \beta_2$ more per year than an individual who completed none and $5 * \beta_2$ more per year than the student who completed the same amount of two-year education not online.

It is important to note that my focus is on the differential effect of completing online credits, not the main effect of completing any credits. Thus, I am concerned about issues of selection into online credits conditional on total credits, but not selection into community college or selection into larger values of total credits (i.e. persistence). The type of selection that I worry about could occur if students who complete the same total amount of coursework but complete more of it online are higher ability, more motivated, or have higher earning potential (and thus might have higher earnings regardless of the level of their online participation), than their peers who complete the same number of total credits, but with fewer online. If ability, motivation, and earning potential are time-invariant characteristics of these students, then the fixed-effects approach controls for them and resolves concern about this type of selection bias.[26]

I extend the basic analysis in several ways.

First, I include three variables measuring contemporaneous enrollment in community college: a dummy for currently enrolled in period t, the number of credits being attempted in period t, and the number of online credits being attempted in period t. The coefficients on these variables tell

[26]The individual-fixed-effects analysis accounts for differences in levels of earnings across students who complete different amounts of online coursework, but it assumes that the pre-college earnings trends are the same. If students who complete more online courses have faster growing earnings even absent these additional online credits, then the individual-fixed-effects estimates may be biased up. This issue is discussed further in the conclusion.

us about the changes in earnings that occur during the periods of enrollment and provide insight into the differential effect of online enrollment on forgone earnings. Including these controls may also have implications for the coefficient of interest. If, for example, students who have previously completed more online credits are also more likely to be enrolled in community college at time t and be experiencing depressed earnings due to enrollment, failing to control for enrollment might lead to downwardly biased estimates of the differential effect of online credits. I do not include controls for contemporaneous enrollment in all specifications, however, because it could be considered an intermediate outcome that is influenced by prior online course-taking that one would not want to control away. I add these controls to two baseline specifications to observe their coefficients and any changes in the coefficient of interest, but I do not include them throughout the full analysis.

Second, I assess the time path of the differential effect of online credits using the following specification:

$$\begin{aligned} Y_{it} = &\sum_{j=1}^{8} \beta_j TotalCredits_{it} * \mathbb{1}(YearsSinceFirstEnroll = j) \\ &+ \sum_{j=1}^{8} \alpha_j TotalOnlineCredits_{it} * \mathbb{1}(YearsSinceFirstEnroll = j) + \delta X_{it} + \gamma_i + \theta_t + \epsilon_{it} \end{aligned} \tag{7}$$

where β_j is the coefficient on Total Credits interacted with an indicator for j year(s) after initial enrollment, and α_j is the coefficient on Total Online Credits interacted with an indicator for j year(s) after initial enrollment.[27]

Third, I estimate a nonparametric specification of credits in which I allow the main effect of Total Credits and the differential effect of Total Online Credits to vary by bins of credits as opposed to being linear. For expositional purposes, I focus initially on one year and six years after initial

[27] I observe at least six years after initial enrollment for all individuals in the same. I observe up to eight years after initial enrollment for some individuals. I only report coefficients on years one through six in the tables.

enrollment to compare the short-run and long-run effects.[28] I then summarize these effects for all of the years after initial enrollment using a parsimonious model that allows for an intercept shift for completing any online credits and a slope for completing each additional online credit (a dosage effect).

Fourth, I present the results of this parsimonious model for specifications with employment and logged earnings as the dependent variables. Most of the specifications use earnings measured in levels so as to include all of the $0 observations in the data. These estimates reflect the combined effect of credits on the extensive margin (employment) as well as the intensive margin (earnings conditional on employment). Focusing on employment and logged earnings separately allows me to disentangle the effects at the extensive and intensive margins, respectively.

Fifth, I extend the analysis to control for the field of credits students complete and allow the differential effect of online credits to vary by field. The variation in the differential effect of online credits across fields is interesting in and of itself. Weighted averages of these field-specific effects are also compared to the baseline specification in which the main effects of credits are restricted to have the same effect across fields. This comparison tells us about how controlling for field of study affects the coefficient of interest. If, for example, students tend to take online courses in fields that reap higher returns in the labor market, the coefficient on total online credits in the baseline specification will be biased up.

Controlling for total credits and total credits by field allows me to more closely approximate the ideal scenario in which I would compare identical students who randomly complete different amounts of coursework online. One problem with this, however, is that it may be over controlling and washing out some of the effect of online education. For example, if enrolling online helps

[28]For the one-year estimates, I restrict the analytic sample to include data from years prior to having any completed credits (call these periods ≤ 0), including the first period of enrollment (call this period 0), and the first year after initial enrollment (period 1). For the six-year estimates, I restrict the analytic sample to include data from years prior to having any completed credits, including the first period of enrollment (period 0), and the sixth year after initial enrollment.

students to complete more credits overall, then the differential effect of online credits, as it operates through its effect on persistence and credit accumulation, is controlled away by controlling for total credits. Or, if taking an online health course causes students not to take additional health courses when health is a high-return field, then the differential effect of online credits, as it operates through its effect on field selection, is controlled away by controlling for total credits by field. Future work will specifically address these outcomes of interest and assess how online coursework influences persistence and field selection. For now, I simply recognize that, to approximate the ideal, I may be over controlling for the effect of online credits, and the impact this has on the estimated differential effect of online credits is ambiguous.

Sixth, I replicate these specifications using credits attempted as opposed to credits completed. Prior research has found that completion and passing rates are lower in online courses than face-to-face courses (Carr, 2000; Carpenter, Brown, and Hickman, 2004; Jaggars and Xu, 2010; Jaggars and Xu, 2011; Xu and Jaggars, 2011; Xu and Jaggars, 2013). Given this, one might be concerned that the positive estimates presented thus far simply reflect the fact that passing online classes is more difficult and that students who are able to do so (i.e. those with greater ability or motivation) will reap higher returns in the labor market. If ability and motivation are fixed, these students' characteristics should be reflected in their pre-college wages and hence controlled for in the analysis. Thus, replacing credits completed with credits attempted serves as a robustness check. Furthermore, the estimates using credits attempted convey the wage premium for participating in online education after adjusting for the reduced likelihood of passing the class and tell us whether there is a benefit to choosing online coursework as opposed to simply being successful in it.

Finally, I explore heterogeneity in the differential effect of online credits. First, I compare the effects across older (30 years or older at initial enrollment) and younger (20-29 years old at initial enrollment) portions of my sample. The individual-fixed-effects strategy relies on pre-college earnings being a good measure of earning potential absent a college education. This is the rationale for restricting the sample to older individuals, 20 years and older at initial enrollment. Twenty years old is still relatively young, however, and the pre-college earnings observations for these individuals (observed at ages 17-19) may be less reliable proxies for earning potential than the pre-college earnings of someone who enrolls, for example, at the the age of 30. The comparison of effects between older and younger portions of the sample serves as a robustness check for the key findings. Second, I compare the effects for men and women. I do this because labor-market effects are commonly estimated separately for men and women and because prior research has shown that males fare worse than females in online classes (in terms of exam scores (Figlio, 2013)). It is of interest as to whether this difference also appears in the labor-market effects.

7 Results

7.1 Baseline Specification

The estimates from the basic specification (Equation 6) are presented in Table 4, column 1. These estimates tell us that each additional credit a student completes is associated with a $66 increase in earnings. If that credit is online, earnings increase by an additional $18. These estimates suggest that a year's worth of community-college credits (30 credits) increases earnings by 13.6% ($=$ $\$66 * 30/\$14,530$) relative to earnings two years prior to enrollment.[29] If that year of credits is completed entirely online, earnings increase by an additional 3.7% ($= \$18 * 30/\$14,530$). As discussed earlier, it is uncommon for students to complete this many credits online. The average student completes approximately 25 credits with 2.5 online. The average student would experience earnings gains of $1,650 (11.4%) from completing 25 credits and an additional $45 (0.3%) from completing 2.5 online.

[29] I choose to compare to earnings two years prior to enrollment to avoid comparing to earnings just prior to enrollment that may exhibit an "Ashenfelter's Dip" – the phenomenon in which earnings decline just prior to entering a job-training program (Ashenfelter, 1978).

Columns 2 through 4 progressively include controls for contemporaneous enrollment. Column 2 includes a dummy variable that is set equal to one during years when the individual is attempting credits. The coefficient tells us that, on average, earnings fall by $2,342 during periods of enrollment. Column 3 allows earnings to change proportionately with the intensity of enrollment by controlling for the number of credits attempted in year t. This tells us that earnings fall by $350 for each credit attempted. Column 4 allows earnings to change differentially for online credits attempted. The coefficients tell us that earnings fall by $376 for each additional credit attempted but fall by $297 less if the credit is online. For an individual who attempts 25 credits during an enrolled year, earnings fall by $7,416 ($= \$1,984 - \$376 * 25$) in this year. If that individual attempts 2.5 of these online, earnings only fall by $6,674 ($= \$1,984 - \$376 * 25 + \$297 * 2.5$) in this year. The coefficients provide evidence that online coursework may have a differential opportunity cost in terms of forgone earnings during enrollment.

The coefficient on the differential effect of online credits grows to $39 after including the three controls for enrollment. This suggests that students who have completed more online credits are also more likely to still be enrolled in community college and possibly experiencing depressed earnings because of this enrollment. Failing to control for enrollment biases down the differential effect of online credits completed. Evidence of this is also seen in Figure 2 which shows that students who enrolled online in their first year were more likely to still be enrolled in community college in future years.

7.2 Baseline Specification, Over Time

Table 5 presents results from the Equation 7 that show the time path of the differential effect of online credits.[30] The estimates in column 1 show that the main effect of completing credits in the early years after initial enrollment (years 1 and 2) is negative. This is akin to the "lock-in"

[30] The coefficients on the interactions with 7 and 8 Years Since 1st Enrollment are not shown because not all students are observed for this length of time. All students are observed for at least 6 years after initial enrollment.

effect that is often observed in the job-training literature in which training participants experience negative, short-run earnings impacts due to their participation in training that may inhibit their ability to be actively employed or seeking a job (Van Ours, 2004; Andersson et al., 2013). The estimates imply that the earnings of an individual one year after initial enrollment who completed one credit falls, on average, by \$137 more than an individual who did not manage to complete any credits during the first year of enrollment. The average number of credits completed by one year after initial enrollment is nine; the average student would experience earnings declines at this time of \$1,233 ($= \$137 * 9$). Over time, however, the negative effect disappears and the main effect of an additional credit levels off at about \$100. Six years after initial enrollment, the average student has completed 25 credits and experiences earnings gains of \$2,400 ($= \$96 * 25$), an increase of 13% over earnings two years prior to enrollment.

The time path of the differential effect of online credits is the opposite. The differential effect of an additional online credit is large and positive in the early years after initial enrollment but falls as time elapses. One year after initial enrollment, the differential effect of completing a credit online is \$148. The main effect of credits at this point is -\$137, meaning that, credit for credit, completing coursework online eliminates the negative earnings effects associated with completing coursework in general. That said, the average student only completes 0.5 credits online by one year after initial enrollment. The average student with nine credits in total and 0.5 online experiences earnings declines of \$1,159 ($= \$137 * 9 - \$148 * 0.5$). This is 6% smaller than the earnings declines of the student who did not complete any coursework online. Six years after initial enrollment, the differential effect of online credits is negative, but small and imprecisely estimated. The average student has completed 25 credits with 2.5 being online by this time and experiences earnings gains of \$2,375 ($= \$96 * 25 - \$10 * 2.5$), which are only 1% smaller than the student who completed nothing online.

Column 2 of Table 5 incorporates the three controls for enrollment that were progressively included in Table 4. The inclusion of these controls serves to increase the differential effect of online credits in most years following enrollment (years three through six). By six years after initial enrollment, the differential effect is positive and marginally significant ($20). Again this suggests that students who have completed online credits are also more likely to be enrolled in community college in later years and possibly experiencing depressed earnings because of this enrollment. The short-run differential effect in the first year, however, shrinks substantially, but is still large and positive. This may be because students who completed more online credits in their first year are also enrolled in more online credits in the subsequent year and the controls for contemporaneous enrollment are accounting for the positive differential effect of online enrollment on contemporaneous earnings.

Altogether, these estimates show that completing online credits benefits students in the short run and reduces the opportunity cost of acquiring schooling. This suggests that students may benefit from online coursework more immediately, possibly because they can remain working or can look for work while they complete it whereas students who complete face-to-face coursework are more detached from the labor market during their time of enrollment. Over time, however, the differential effect of online credits falls. Depending on the specification, the long-run effect is either a small, insignificant, negative effect or a modestly significant, positive effect suggesting that online coursework, at a minimum, does not significantly harm students' labor-market outcomes.

Do these estimates tell us anything about the separate experience- versus human-capital-specific effects of online credits that were described in the theoretical motivation? The short-run versus long-run estimates may shed some light on this question. One might imagine that any additional experience an individual accumulated while enrolled in college does not have a significant bearing on earnings several years later. Thus, the experience effect may be present in the short-run estimates, but not the long-run estimates. If the schooling one acquired instilled more or less human capital that an employer could learn about over time, the human-capital effects may show up as time elapses and be present in the long-run estimates. If the timing story told here applies, then these estimates imply that the experience-specific effect of online credits is positive and the human-capital-specific effect, though not precisely estimated, may be slightly negative (Table 5, Column 1). Future research is needed to investigate other ways to formally disentangle these two effects.

The net effect, however, is the most important from a policy perspective because one cannot separate the inherent flexibility of an online course that would allow students to accumulate additional experience from its potentially different capacity to instill human capital. And overall, the net effect suggests that online credits help students in the short run and do not significantly harm them in the long run.

7.3 Non- and Semi-Parametric Specifications of Credits

One concern with these basic specifications is that they assume a linear main effect of credits and a linear differential effect of online credits. To address possible non-linearities, I replace the linear terms with sets of dummies indicating bins of credits completed.[31]

For expositional purposes, I initially focus on the effects at one year and six years after initial enrollment. Columns 1 and 2 of Table 6 restrict to observations of individuals prior to having any completed credits (years since/until enrollment ≤ 0) and observations of them one year after initial enrollment. Columns 3 and 4 restrict to observations of individuals prior to having any completed credits and observations of them six years after initial enrollment. These estimates convey a different picture than the linear estimates in Tables 4 and 5. In the first year after initial enrollment (Column 1), having completed 1-3 credits online (this can be thought of as one course because one course is typically worth three credits) increases earnings by an additional \$497 relative to students who completed no credits online. Having completed 4-6 credits online increases earnings by \$1,782 relative to students who completed no courses online. In general, the effect of online

[31] I no longer include controls for current enrollment. As discussed in the empirical strategy section, enrollment could be seen as an intermediate outcome that is influenced by prior online course-taking. In this case, one might not want to control for it. The prior two sections showed that controlling for enrollment may actually increase the coefficient on the differential effect of online credits, especially in the long run.

credits is increasing in the number of credits though it tapers off after 21 credits and becomes insignificant after 30 credits, because few students have completed this many online credits within one year of enrollment.

Six years after initial enrollment (Column 3), having completed 1-3 credits online increases earnings by $1,286. Having completed 4-6 credits online or 7-9 credits online increases earnings by $1,525 and $1,287, respectively. These estimates suggest that, in the long run, the effect of online credits is largely fixed for any amount of online coursework completed. The effect tapers off after 30 credits, but it is also insignificant as few students complete this many online credits.

These results can be summarized with a more parsimonious, semi-parametric model that allows for an intercept shift for having completed any online credits and a linear term in the number of online credits completed. The latter can be thought of as the dosage effect of additional online coursework. Columns 2 and 4 show this parsimonious specification. One year after initial enrollment (Column 2) there is no significantly different change in earning for students who complete any online credits relative to those who do not, but the dosage effect of completing each additional online credit is large, positive, and significant at $280. Six years after enrollment (Column 4), however, there is a significant and large positive effect of having completed any credits online ($1,409), but a small and insignificant negative dosage effect.

Table 7 presents the coefficients from the summary specification for each of the six years following initial enrollment. The estimates in the columns labeled "Yr 1" and "Yr 6" reproduce the estimates from columns 2 and 4 of Table 6. The trend shows that the effect of having completed any online credits grows over time becoming particularly large and significant by three years after initial enrollment. The dosage effect, however, shrinks and becomes slightly negative, though insignificant over time. These estimates show that completing more coursework online benefits students in the short run, possibly because it allows them to remain more connected to the labor force while enrolled and thus attenuates earnings and employment losses that result from enrollment. Completing more coursework online may also allow to students to reap the benefits of the additional education more quickly and/or to accumulate valuable work experience while enrolled. In the long-run, however, the amount of coursework a student completed online does not have a significant effect on earnings but having completed *any* coursework online does. This suggests that completing coursework online, regardless of the amount, imparts a fixed benefit on students long-term, labor-market outcomes. Theoretically, it is challenging to find support for such a finding. And, we would expect that treatment effects should move with the size of the treatment. As such, this result raises some concern that selection issues may still be biasing these results. Analyses presented in the next three sections do uncover some significant, long-run dosage effects, but future work is still needed to investigate whether selection bias may be driving the fixed benefit associated with completing any online coursework. This is discussed further in the concluding section.

The parsimonious summary specification will be shown moving forward as is provides a simple summary of the differential effect of online credits.

7.4 Logged Earnings and Employment

Up until now, the outcome variable has been earnings measured in levels so as to incorporate all of the $0 observations in the data. Estimates presented thus far reflect the combined effect of credits on the intensive margin (earnings conditional on employment) and the extensive margin (employment). In order to look at these two margins separately, I present estimates similar to those in Table 7, but with logged earnings and employment as the dependent variables. Panel A of Table 8 shows the semi-parametric specification with logged earnings as the dependent variable, and Panel B of Table 8 shows the semi-parametric specification with employment as the dependent variable.[32] The short-term benefit of completing additional credits that was observed in Table 7 is observed

[32]Note that the sample sizes change for the logged earnings regressions. The number of observations falls because $0 observations are dropped when earnings are logged. The number of students falls because some students only have $0 earnings observations.

for both the intensive and extensive margins in Table 8. One year after initial enrollment there is no significant effect of having completed any online credits on earnings conditional on employment or on employment, but each additional credit completed is associated with a 1% increase in earnings conditional on employment and a 0.47% increase in employment. The long-term fixed benefit to completing any online credits is also observed at both margins. Six years after initial employment, having completed any online credits increases earnings conditional on employment by 6.2% and increases employment by 1.9%. At this point, however, each additional online credit completed is associated with a 0.35% decrease in earnings conditional on employment but a 0.07% increase in employment. The balance of these opposing effects results in the small and insignificant negative dosage effect of each additional online credit shown in Table 7.

Altogether, these estimates show that neither the intensive nor the extensive margin are solely responsible for the observed differential effects of online credits. The effects typically move in the same direction at each margin, with the exception of the dosage effect in the long term where a negative effect at the intensive margin is balanced against a positive effect at the extensive margin. In the long run, having completed additional online coursework appears to increase the likelihood of employment but decrease earnings conditional on employment.

7.5 Accounting for Field of Credits

One potential issue with the specifications thus far is that all credits, regardless of field, are restricted to have the same effect on earnings. Prior research shows that certain fields of study, especially quantitative and health fields, may reap higher rewards in the labor market (Jacobson et al., 2005a; Jepsen et al., 2012).

Table 9 presents estimates similar to those in Table 7 but allows the effect of credits and the differential effect of online credits to vary by the nine fields of study displayed in Table 1. The main effect of total credits is specified nonparametrically allowing for 12 bins of credits for each of the nine fields. The coefficients on these variables are not presented, both for ease of exposition and because the focus of the study is on the differential effect of online credits. Now, however, in addition to simply controlling for the total number of credits completed, this specification also controls for the fields in which students completed credits bringing me one step closer to the "ideal" experiment. Allowing the differential effect of online credits to vary by field also highlights interesting heterogeneity in the effect of interest across fields. Having completed any credits online in the field of Liberal Arts, Social/Humanities has a significant positive effect on earnings at all points in time after initial enrollment. The dosage effect of completing additional credits in this field, however, is not precisely estimated at any point in time and ranges from \$133 to -\$4. Having completed any credits online in the field of Computer & Information Science significantly reduces earnings in the short-run (by \$1,321 one year after initial enrollment and \$816.4 two years after initial enrollment) but significantly increases earnings in the longer-run (by \$739 five years after initial enrollment and \$659 six years after initial enrollment). The early negative effect of having completed any credits in this field is counterbalanced by the large positive dosage effect of completing each additional credit (\$674 one year after initial enrollment). Completing any online credits in Health has a positive effect on earnings in the short run (\$1,686 one year after initial enrollment), but each additional online credit in Health negatively affects earnings in the long run ($-\$180$).[33] The dosage effect in the field of Other Professional (this includes courses in criminal justice, court reporting, graphic design, social work, etc.) is positive and significant in all years following initial enrollment. The largest dosage effect is observed five and six years after initial enrollment in the field of Engineering & Science Technologies. There are interesting trends in other fields, but many of the point estimates are imprecise.[34]

[33] Maybe it is unwise to learn to stick someone with a needle in a virtual environment.

[34] F-tests of the null hypothesis that the coefficients on "Any Online" are equal across the nine fields fail to reject the null in each year. This means that the intercept coefficients are not statistically significantly different from each other. F-tests of the null hypothesis that the coefficients on "Total Online Credits" are equal across the nine fields fail to reject the null in year 1 but reject the null in years 2 through 6. This means that the slope coefficients are not statistically significantly different from each other in year 1 but are in later years.

To understand the average differential effect of completing any credits online or additional credits online after controlling for the field of credits completed, I generate weighted averages of the coefficients across the nine fields. I weight the "Any Online" coefficients by the share of students who have any online credits in each field (=#students with any online credits in field X/#students with any online credits) and I weight the "Total Online Credits" coefficients by the share of online credits completed in each field (=#credits completed online in field X/#credits completed online).[35] These weighted averages are seen in the lower panel of Table 9 and can be compared to the coefficients in Table 7 to understand how the differential effect of completing online credits changes after controlling for the field of credits completed. The dosage effect of completing any online credits increases in the short run (from -$68 in year 1 of Table 7 to $380 in year 1 of Table 9) but falls somewhat in the long run (from $1,409 in year 6 of Table 7 to $1,112 in year 6 of Table 9). The effect of completing additional online credits, however, increases at every point in time after enrollment. This suggests that students tend to take more online courses in fields that reap lower rewards in the labor market. Failing to control for the field of credits may bias down the differential effect of online credits.

7.6 Credits Attempted

Prior research has found that completion and passing rates are lower in online courses than face-to-face courses (Carr, 2000; Carpenter, Brown, and Hickman, 2004; Jaggars and Xu, 2010; Jaggars and Xu, 2011; Xu and Jaggars, 2011; Xu and Jaggars, 2013). Given this, one might be concerned that the positive estimates presented thus far simply reflect the fact that passing online classes is more difficult and that students who are able to do so (i.e. those with greater ability or motivation) will reap higher returns in the labor market. If ability and motivation are fixed, these students' characteristics should be reflected in their pre-college wages and hence controlled for in the analysis.

[35] The weights are calculated separately for each year after initial enrollment. The weights for the "Any Online" coefficients sum to more than one because students can complete "Any Online" credits in more than one field.

Thus, replacing credits completed with credits attempted serves as a robustness check. Furthermore, the estimates using credits attempted convey the wage premium for participating in online education after adjusting for the reduced likelihood of passing the class and tell us whether there is a benefit to choosing online coursework as opposed to simply being successful in it.

Panel A of Table 10 replicates Table 7 using credits attempted instead of credits completed. When using credits completed, the estimates of the intercept shift are slightly larger while the estimates of the dosage effect are 16-25% smaller. The dosage effect six years after initial enrollment is now marginally significant at -$27 for each additional online credit.

Panel B of Table 10 replicates the bottom panel of Table 9 that shows weighted averages of field-specific coefficients. Again, the estimates of the intercept shift are slightly larger while the estimates of the dosage effect are smaller. Although imprecisely estimated, the dosage effect is positive in all years.

Altogether, estimates using credits attempted versus credits completed produces largely similar results. The point estimates, however, are smaller reflecting the reduced likelihood of completing and passing online courses. These estimates of the adjusted wage premium associated with enrolling in online courses (as opposed to completing them) still show a short run benefit to attempting greater amounts of online coursework and a long run benefit associated with completing any online coursework.

7.7 Heterogeneity

Table 11 presents estimates of the differential effect of online credits separately for older (30 and older at initial enrollment) and younger (20 to 29 at initial enrollment) portions of the sample and for males and females. These results come from specifications in which the main effect of total credits and the differential effect of online credits are allowed to vary by field. Weighted averages of the field-specific online coefficients are presented. The key findings presented earlier are found for each of these subsamples. In the short run, there is a positive dosage effect of completing additional online credits (though it is not significant for younger individuals), and, in the long run, there is a large, positive effect of completing any credits online. Some interesting differences, however, appear between older versus younger individuals and males versus females.

Panels A and B compare older and younger individuals. This comparison serves as a robustness check for the estimation strategy that relies on pre-college earnings of each individual as good proxy for earning potential for that individual. This might be less likely to hold for younger workers. Fortunately, the two major findings largely hold for both groups – in the short run there is a positive benefit to completing additional online credits and in the long run there is a large positive benefit to having completed any online credits. That said, there are also differences. The short run dosage effect of additional online coursework is much larger for older individuals than for younger individuals, for whom it is insignificant. This may suggest that the flexibility offered by online courses that allows individuals to remain working or job-hunting while enrolled is particularly valuable for older individuals and less valuable for younger individuals who may have had a less-strong attachment to the labor market prior to enrollment.

Prior research suggests that males fare less well in online coursework than females in terms of exam scores (Figlio et al., 2013). Panels C and D compare the labor-market effects for males and females and find the opposite. The short run dosage effect of additional online coursework is larger for males than females as is the long-run benefit to having completed any online coursework. Theoretically, it is unclear why one might expect differences between males and females in the effect of completing online credits on earnings, especially after accounting for the fields of the credits completed. The analysis, however, accounts for nine, broad fields of study. It is possible that within these broad fields, males take online classes in higher-return sub-fields than do women.

8 Conclusion and Discussion

This study presents the first piece of evidence on the effect of online education on labor-market outcomes. Estimates are generated by applying an individual-fixed-effects estimation strategy to panel data on over 100,000 nontraditional students who enroll in community college in state A. Estimates show that online education has a positive differential effect on labor-market outcomes; students who complete online credits experience larger earnings and employment gains than their peers who complete courses in the traditional, face-to-face format. Linear, baseline specifications show that the benefit to online coursework occurs largely in the years immediately following initial enrollment, when a student may still be enrolled or have just recently completed their time in college. The positive differential effect serves to offset the early negative main effect of completing college credit that results as students pull away from the labor market to take classes. Estimates also show that earnings fall less during enrolled periods for students who enroll in online classes. These findings suggest that online education allows students to accumulate college credit at a lower opportunity cost. Semi-parametric specifications that allow for an effect of completing any online coursework and a dosage effect of completing larger amounts of online coursework show that the dosage effect is large and positive in the short run. Overtime, however, the dosage effect shrinks becoming negative, though small and imprecisely estimated. The effect of completing any online coursework is insignificant in early years but grows to be large, positive, and significant as time elapses since students' initial enrollment. Controlling for the fields in which students complete credits increases the dosage effect of online education. This suggests that students tend to take more online courses in lower-return fields, and failing to control for field of study may result in downwardly biased point estimates.

The individual-fixed-effects strategy used herein accounts for differences in levels of earnings across students who complete different amounts of online coursework, but it assumes that trends in earnings are comparable. If students who complete any online credits or more online credits have faster growing earnings even absent their online participation, then the individual-fixed-effects estimates may be biased up. It is of particular concern that this may be driving the estimates of the long run fixed benefit associated with completing any online credits. To test for differences in trends, I estimate a model using only data from the pre-enrollment period that includes a linear time trend, an interaction between the trend and an indicator for whether or not each individual completed any online credits in the future, and interactions of the time trend with future measures of the other variables in the model (eg. bins of total credits completed and total online credits completed). The coefficient on the interaction of the trend and the future measure of any online participation is positive and significant suggesting that individuals who participated in online education did have faster growth in earnings even before enrolling in college. The positive benefit associated with completing any online credits may be partially due to these differences in trends that are not accounted for by the individual-fixed-effects model. One approach to address this concern is to use a propensity score matching strategy in which I estimate the effect of completing any online credits on earnings six years after initial enrollment. Propensity scores can be estimated using a host of characteristics include pre-enrollment trends in earnings. Preliminary work using this approach suggests that there is still a large increase in earnings associated with completing any online credits, similar in size to that presented in this paper using the individual-fixed-effects strategy. Future research is needed to explore this strategy and other alternative estimation strategies further.[36]

As with all research, this study has some limitations in terms of generalizability. The students at these schools are primarily on-ground students who complete a portion of their coursework online; they are not fully online students. Thus, these estimates may not generalize to students who complete large shares of their coursework online or enroll in fully online programs, such as those offered by the University of Phoenix or Devry. This study focuses on non-traditional students (age 20 and older at initial enrollment), and the findings may not generalize to the full age-spectrum of

[36]Another potential approach is to estimate models that include student-specific time trends as in (Jacobson, LaLonde, and Sullivan (2005a)). This approach is not ideal because the trends are estimated off of the full panel, not just the pre-treatment data, and it is possible that the treatment could have an effect on the post-treatment trend.

college students. Finally, the data used here are from students at the community-college level, and the results may not generalize to students enrolled at other levels of higher education or to primary and secondary education. Future work is needed to assess the effectiveness of online education in other levels and sectors of higher education.

A weakness of this study is that it does not contribute to our understanding of how online course taking influences persistence and credential completion. My study controls for total credits and total credits by field so as to compare like students who complete different amounts of this coursework online and generate plausibly causal estimates of the differential effect of online coursework on labor-market outcomes. Future research is needed to address the effect of online course-taking on persistence in college, persistence within certain fields of study, and completion of credentials.

Additional research is also needed to understand the relative costs of online education. Popular opinion is that it is less costly for institutions to offer online courses, but the evidence on this is sparse (Xu and Jaggars, 2013). This opinion likely stems from the notion that online courses can serve larger numbers of students and not require the use of on-campus facilities thus reducing costs related to instruction and facilities. In the context studied here, however, online courses contain the same number of students as their face-to-face counterparts meaning that instructor costs are not lower though facility costs may be. Furthermore, conversations with community-college administrators suggest that the costs associated with subscription to an online course management system, such as Blackboard or Angel, are high, which may also preclude large cost savings. Future research is needed in this area. My results, however, show that students who complete online coursework actually experience improved labor-market outcomes. If costs of offering online instruction are not significantly higher than costs of offering face-to-face instruction, then my findings suggest that a cost-benefit analysis of online education would likely favor the expansion of online instruction.

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Data Appendix

Administrative data were provided from state A's community-college sytem covering all first-time, credit-seeking college students who enrolled between the fall of 2001 and the summer of 2010 (the 2001-2009 academic years). I use these data to create a student-by-academic year panel that tracks credit accumulation.

Unemployment insurance records were provided at the student-by-quarter level for 1996q1 through 2012q1. I collapse the data into a student-by-academic year panel by combining quarters in the following way: earnings for academic year 2003-2004 (which runs from September 2003 through August of 2004) are designated to be $= 1/3 * earningsfrom2003q3 + earningsfrom2003q4 + earningsfrom2004q1 + earningsfrom2004q2 + 2/3 * earningsfrom2004q3$. I then merge the two student-by-academic year panels. I restrict to observations of 1997 forward because I do not observe a full academic year of wages for 1996. I restrict to observations of 2009 and earlier because I do not observe credit accumulation after 2009. This results in a student-by-academic year panel tracking earnings from 1997 to 2009 and credit accumulation from 2001 to 2009.

I restrict the analytic sample to those who enrolled in the 2001, 2002, and 2003 academic years. This allows me to observe credit accumulation and earnings for at least six years following initial enrollment for all students. The sample provided from the community-college system was intended to contain first-time college students. I use National Student Clearinghouse data that contains information on enrollment in, and degree receipt from, other two- and four-year institutions to identify students who appear to have attended other institutions prior to their enrollment in community college but may have been missed. These students are dropped. I also drop observations that are missing important demographic and transcript information (gender, age, subject area of credits, or instructional format of credits). I drop outliers that earn more than $300,000 in any year, outliers that earn more than 200 credits overall, and outliers that complete more than 50 credits online.

Figure 1. Distribution of Credits

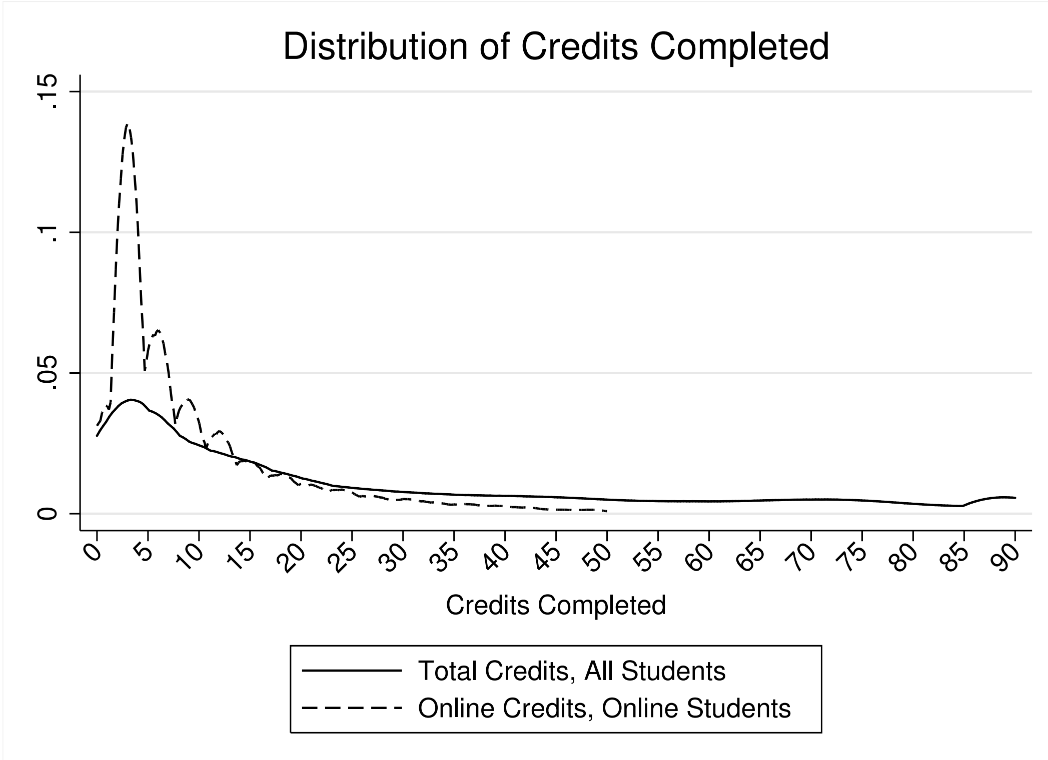


Figure 2. Earnings Relative to First Enrollment

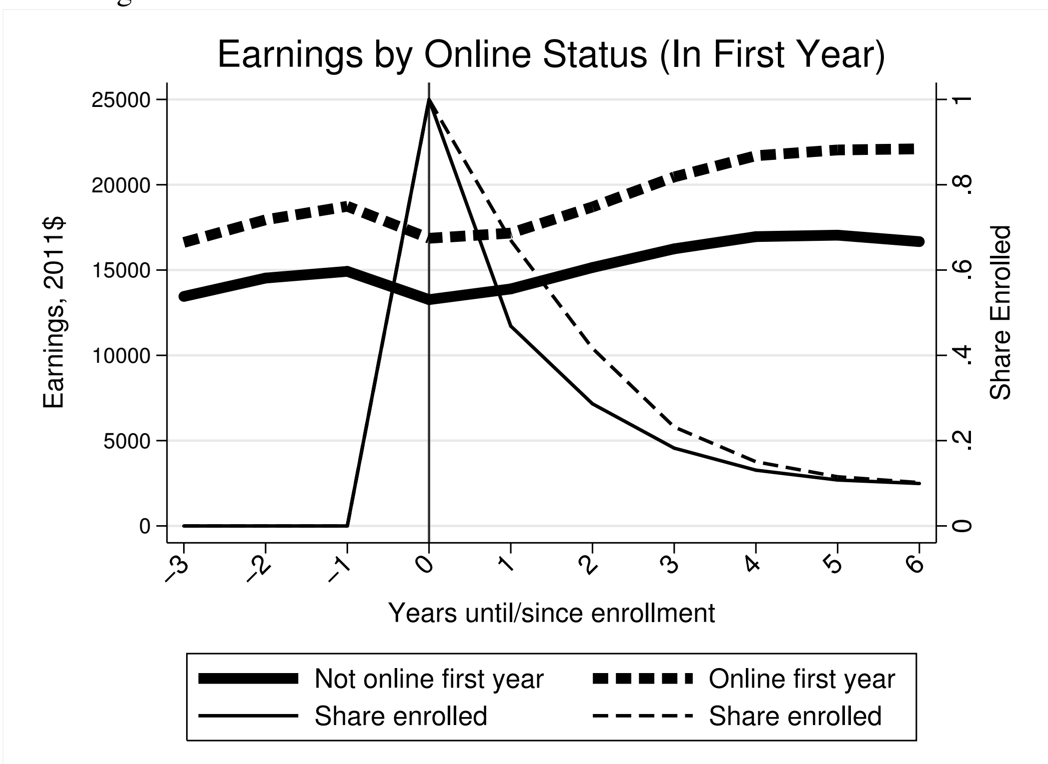


Table 1. Share of Credits Completed Online, by Field

Field	Total Credits	Total Credits Online	Percent of Credits In Field Online	Percent of Online Credits in Field
Liberal Arts, Social/Humanities	851,266	102,181	12.00%	35.40%
Liberal Arts, Quantitative/Science	456,514	25,640	5.62%	8.88%
Health	327,741	13,744	4.19%	4.76%
Business	290,226	49,841	17.17%	17.27%
Computer & Information Science	194,778	42,312	21.72%	14.66%
Engineering & Science Techologies	89,963	2,504	2.78%	0.87%
Education	185,779	24,807	13.35%	8.59%
Other Technical	196,043	519	0.26%	0.18%
Other Professional	329,937	27,120	8.22%	9.39%
Total	2,922,244	288,668	9.88%	

The nine fields are determined based on the two-digit CIP code of the course. Two-digit CIP codes are assigned to each course based on its subject code.

Notes on sample: The sample includes all credits earned by students who first enrolled at one of the state's community colleges during the 2001, 2002, or 2003 academic years. The sample contain individuals aged 17-65 who started school between the ages of 20 and 45. Outliers who earned more than $300,000 per year, who completed more than 200 credits, or more than 50 online credits are dropped from the sample. Students with incomplete demographic (missing race or gender) and transcript information (missing subject or format identifiers) are also dropped from the sample.

Table 2. Credits by Students' Highest Community-College Credential

	No Degree	Certificate	Diploma	Associate's Degree	Total
Avg Total Credits Completed	17.56	29.72	59.93	73.29	25.96
Avg Total Online Credits Completed	1.866	1.833	3.063	7.522	2.564
Percent Fully Online	2.71%	0.40%	0.09%	0.24%	2.18%
# Students	90454	5583	3294	13235	112566

Notes: See notes on Table 1 for sample. The unit of observation in this table is the student. The percent fully online is the percent of students in each award category who completed all of their credits online.

Table 3. Summary Statistics

	1	2	3	4
	All Students	**NOT Online in First Year**	**ONLINE in First Year**	**Difference (Not Online - Online)**
Age at Enrollment	29.53	29.41	30.60	-1.19**
	(7.482)	(7.509)	(7.148)	(-16.14)
Male	0.40	0.41	0.31	0.10***
	(0.489)	(0.491)	(0.461)	(21.05)
White	0.60	0.58	0.74	-0.16***
	(0.490)	(0.493)	(0.437)	(-32.97)
Non White	0.40	0.42	0.26	0.16***
	(0.490)	(0.493)	(0.437)	(32.97)
Remedial Enrollment in First Year	0.36	0.36	0.33	0.03***
	(0.479)	(0.480)	(0.469)	(7.01)
Total Credits Completed	25.06	23.82	36.05	-12.23***
	(26.74)	(26.25)	(28.43)	(-46.80)
Total Online Credits Completed	2.34	1.35	11.07	-9.72***
	(6.085)	(4.436)	(10.29)	(-184.68)
Earnings, 3 Yrs Prior to Enrollment	$13,774	$13,453	$16,619	-3166***
	(17335.3)	(17016.0)	(19712.9)	(-18.53)
N	112,566	101,137	11,429	

Notes on sample: See notes from Table 1.
Notes on table: Columns 1-3 contain means and standard deviations in parentheses. Column 4 contains differences and t-statistics in parentheses. Total Credits Completed and Total Online Credits Completed are measured at 6 years after initial enrollment.

Table 4. Individual Fixed Effects Estimates

	Dependent Variable: Earnings (2011$)			
	1	2	3	4
Total Credits	66.14***	64.46***	48.19***	45.47***
	(2.018)	(2.016)	(2.033)	(2.043)
Total Online Credits	17.68**	26.64***	30.16***	39.00***
	(8.726)	(8.728)	(8.728)	(8.758)
Currently Enrolled		-2,342***	2,105***	1,984***
		(38.90)	(46.33)	(46.03)
Currently Enrolled * Current Credits Attempted			-350.1***	-375.6***
			(3.119)	(3.250)
Currently Enrolled * Current Credits Attempted Online				297.1***
				(9.200)
Demographics	X	X	X	X
Individual Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X
Observations	1,427,488	1,427,488	1,427,488	1,427,488
R-squared	0.659	0.660	0.665	0.666
Students	112566	112566	112566	112566

Notes: Robust standard errors clustered at the level of the student are in parentheses. See Table 1 for sample notes. Demographics is a vector of controls including age, age squared, and the interaction of these two variables with gender and race.

Table 5. Individual Fixed Effects Estimates, Over Time

	Dependent Variable: Earnings (2011$)	
	1	2
Total Credits Completed * 1 Yr Since 1st Enroll	-136.9***	-48.02***
	(3.509)	(3.119)
Total Credits Completed * 2 Yrs Since 1st Enroll	-44.74***	-33.06***
	(2.369)	(2.284)
Total Credits Completed * 3 Yrs Since 1st Enroll	16.10***	2.939
	(2.148)	(2.156)
Total Credits Completed * 4 Yrs Since 1st Enroll	59.60***	38.87***
	(2.267)	(2.284)
Total Credits Completed * 5 Yrs Since 1st Enroll	85.10***	60.81***
	(2.338)	(2.357)
Total Credits Completed * 6 Yrs Since 1st Enroll	96.41***	70.70***
	(2.379)	(2.396)
Total Online Credits Completed * 1 Yr Since 1st Enroll	147.8***	80.07***
	(22.47)	(21.53)
Total Online Credits Completed * 2 Yrs Since 1st Enroll	67.81***	63.01***
	(13.53)	(13.27)
Total Online Credits Completed * 3 Yrs Since 1st Enroll	35.66***	53.30***
	(11.36)	(11.30)
Total Online Credits Completed * 4 Yrs Since 1st Enroll	6.063	29.55***
	(10.51)	(10.50)
Total Online Credits Completed * 5 Yrs Since 1st Enroll	-8.832	18.20*
	(10.43)	(10.46)
Total Online Credits Completed * 6 Yrs Since 1st Enroll	-9.885	19.66*
	(10.12)	(10.13)
Currently Enrolled		1,977***
		(45.75)
Currently Enrolled * Current Credits Attempted		-355.6***
		(3.144)
Currently Enrolled * Current Credits Attempted Online		294.2***
		(8.985)
Demographics	X	X
Individual Fixed Effects	X	X
Year Fixed Effects	X	X
Observations	1,427,488	1,427,488
R-squared	0.661	0.667
Students	112566	112566

Notes: Robust standard errors clustered at the level of the student are in parentheses. See Table 1 for sample notes. Demographics is a vector of controls including age, age squared, and the interaction of these two variables with gender and race. Coefficients on the interactions with 7 and 8 years since 1st enrollment are not shown.

Table 6. Non-Parametric and Semi-Parametric Specification of Credits

	Dependent Variable: Earnings (2011$)			
	1 Year After Initial Enrollment		6 Years After Initial Enrollment	
	1	2	3	4
Total Credits Completed:				
1-3 Credits	1,567***	1,551***	1,351***	1,346***
	(102.8)	(102.6)	(248.2)	(248.3)
4-6 Credits	957.0***	966.6***	1,455***	1,461***
	(100.7)	(100.6)	(236.9)	(236.8)
7-9 Credits	-424.4***	-423.7***	1,202***	1,206***
	(126.7)	(126.6)	(267.4)	(267.3)
10-12 Credits	-475.9***	-462.4***	1,462***	1,468***
	(138.1)	(138.2)	(269.7)	(269.8)
13-15 Credits	-926.8***	-924.6***	1,779***	1,785***
	(163.3)	(163.3)	(291.7)	(291.8)
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
	(149.8)	(149.6)	(257.4)	(257.4)
22-30 Credits	-4,323***	-4,330***	2,051***	2,050***
	(169.1)	(169.5)	(259.5)	(259.5)
31-40 Credits	-4,785***	-4,823***	2,370***	2,364***
	(273.1)	(273.4)	(276.6)	(276.5)
41-50 Credits	-3,802***	-3,900***	2,758***	2,754***
	(646.2)	(644.4)	(293.5)	(293.5)
51-60 Credits	-6,477**	-6,444**	4,074***	4,068***
	(2,652)	(2,645)	(342.4)	(342.4)
61-70 Credits	-5,680***	-5,534***	5,494***	5,490***
	(1,361)	(1,187)	(349.6)	(349.5)
71 Plus Credits			7,877***	7,872***
			(296.7)	(296.6)
Total Online Credits Completed:				
1-3 Credits	496.8**		1,286***	
	(198.7)		(229.2)	
4-6 Credits	1,782***		1,525***	
	(279.9)		(303.4)	
7-9 Credits	2,174***		1,287***	
	(482.5)		(380.8)	
10-12 Credits	3,912***		1,236***	
	(654.3)		(453.3)	
13-15 Credits	3,424***		1,034*	
	(845.0)		(538.1)	
16-21 Credits	4,869***		472.2	
	(1,500)		(449.3)	
22-30 Credits	4,452*		1,122**	
	(2,279)		(514.4)	
31-40 Credits	-12,389		525.6	
	(7,624)		(689.3)	
41 Plus Credits	4,337		542.5	
	(3,802)		(1,115)	
Any Online Credits Completed		-68.80		1,409***
		(251.4)		(202.0)
Total Online Credits Completed		279.9***		-20.70
		(42.70)		(13.83)
Demographics	X	X	X	X
Individual Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X
Observations	757,336	757,336	757,336	757,336
R-squared	0.747	0.747	0.702	0.702
Students	112566	112566	112566	112566

Notes: Robust standard errors clustered at the level of the student are in parentheses. See Table 1 for sample notes. Demographics is a vector of controls including age, age squared, and the interaction of these two variables with gender and race. The specifications in columns 1 and 2 restrict to observations of individuals prior to having any completed credits and one year after their intial enrollment. The specifications in columns 3 and 4 restrict to observations of individuals prior to having any completed credits and six years after their intial enrollment.

Table 7. Semi-Parametric Specification of Online Credits, Over Time

	Dependent Variable: Earnings (2011$)					
	Yr 1	Yr 2	Yr 3	Yr 4	Yr 5	Yr 6
Any Online Credits Completed	-68.80	53.11	632.4***	972.3***	1,148***	1,409***
	(251.4)	(196.2)	(191.0)	(195.6)	(199.5)	(202.0)
Total Online Credits Completed	279.9***	132.3***	52.31***	6.091	-13.38	-20.70
	(42.70)	(21.79)	(16.91)	(15.09)	(14.54)	(13.83)
Total Credits by Bin	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
Individual Fixed Effects	X	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X	X
Observations	757,336	757,336	757,336	757,336	757,336	757,336
R-squared	0.747	0.737	0.727	0.717	0.709	0.702
Students	112566	112566	112566	112566	112566	112566

Notes: Robust standard errors clustered at the level of the student are in parentheses. See Table 1 for sample notes. Demographics is a vector of controls including age, age squared, and the interaction of these two variables with gender and race.

Table 8. Semi-Parametric Specification of Online Credits, Logged Earnings and Employment

Panel A	Dependent Variable: Ln(Earnings (2011$))					
	Yr 1	Yr 2	Yr 3	Yr 4	Yr 5	Yr 6
Any Online Credits Completed	-0.00303	-0.0144	0.0182	0.0252	0.0430**	0.0618***
	(0.0258)	(0.0199)	(0.0177)	(0.0172)	(0.0171)	(0.0172)
Total Online Credits Completed	0.0105**	0.00542**	0.00125	-0.00148	-0.00328***	-0.00350***
	(0.00432)	(0.00217)	(0.00158)	(0.00133)	(0.00122)	(0.00118)
Observations	551,708	551,152	549,615	547,710	545,065	541,926
R-squared	0.610	0.603	0.601	0.599	0.598	0.597
Students	101490	102238	102451	102515	102516	102414

Panel B	Dependent Variable: Employed (0/1)					
	Yr 1	Yr 2	Yr 3	Yr 4	Yr 5	Yr 6
Any Online Credits Completed	0.00219	0.0107**	0.0164***	0.0169***	0.0152***	0.0190***
	(0.00683)	(0.00514)	(0.00473)	(0.00462)	(0.00463)	(0.00468)
Total Online Credits Completed	0.00473***	0.000541	-0.000413	0.000239	0.000544	0.000661**
	(0.00111)	(0.000554)	(0.000412)	(0.000358)	(0.000335)	(0.000315)
Observations	757,336	757,336	757,336	757,336	757,336	757,336
R-squared	0.593	0.588	0.584	0.580	0.576	0.572
Students	112566	112566	112566	112566	112566	112566

Notes: Robust standard errors clustered at the level of the student are in parentheses. See Table 1 for sample notes. All regressions also include conrols for bins of total credits completed, demographics, year fixed effects, and student fixed effects.

Table 9. Semi-Parametric Specification of Online Credits, Control for Field of Credits

	Dependent Variable: Earnings (2011$)					
	Yr 1	Yr 2	Yr 3	Yr 4	Yr 5	Yr 6
Any Online Credits Completed, by Field:						
Liberal Arts, Social/Humanities	1,007***	687.5**	503.4*	523.8*	777.4***	961.8***
	(386.1)	(287.8)	(264.5)	(270.5)	(270.7)	(272.8)
Liberal Arts, Quantitative/Science	1,280	-13.78	212.9	1,063	1,036	988.7
	(1,488)	(836.1)	(738.5)	(675.9)	(658.1)	(629.5)
Health	1,686*	-212.0	796.8	922.8	749.6	-123.7
	(948.5)	(776.2)	(723.4)	(725.1)	(709.5)	(689.4)
Business	419.0	0.329	652.4*	554.2	79.08	414.7
	(610.7)	(416.1)	(394.3)	(375.3)	(378.6)	(387.1)
Computer & Information Science	-1,321**	-816.4*	47.50	501.9	738.5**	659.1*
	(638.1)	(473.5)	(369.7)	(363.3)	(376.0)	(394.1)
Engineering & Science Techologies	-261.5	-792.8	-158.4	-162.9	-2,028	-1,830
	(3,626)	(2,428)	(2,119)	(1,850)	(1,790)	(1,721)
Education	1,211	565.2	456.6	-52.68	81.26	567.0
	(1,010)	(598.7)	(494.3)	(481.5)	(458.8)	(451.9)
Other Technical	-4,553	-2,525	-51.08	-222.0	415.3	-1,110
	(4,535)	(2,041)	(2,391)	(2,919)	(2,939)	(2,918)
Other Professional	850.1	-307.2	693.2	882.4*	500.0	328.6
	(747.4)	(528.1)	(472.5)	(473.9)	(463.5)	(452.9)
Total Online Credits Completed, by Field:						
Liberal Arts, Social/Humanities	133.0	46.42	-3.665	12.29	6.016	23.77
	(83.08)	(47.49)	(37.75)	(35.75)	(34.95)	(33.67)
Liberal Arts, Quantitative/Science	195.7	216.3	234.6	-3.125	-64.50	-91.03
	(381.6)	(184.3)	(158.8)	(135.2)	(130.6)	(123.4)
Health	46.55	200.7	-121.7	-228.9**	-258.0***	-179.5**
	(195.8)	(128.1)	(103.6)	(101.2)	(93.08)	(88.64)
Business	125.5	150.9**	94.05*	66.45	89.81**	34.14
	(121.6)	(58.73)	(50.85)	(43.28)	(44.38)	(40.77)
Computer & Information Science	674.0***	401.5***	141.6*	23.65	-24.89	2.625
	(183.3)	(122.4)	(80.25)	(76.44)	(78.92)	(87.55)
Engineering & Science Techologies	251.7	269.4	477.6	362.2	970.7**	904.0**
	(1,160)	(704.5)	(544.7)	(403.0)	(396.8)	(400.2)
Education	-21.25	-23.74	-42.36	59.76	33.07	-45.22
	(219.1)	(95.03)	(68.75)	(57.43)	(48.73)	(40.91)
Other Technical	984.0	404.9	630.9	628.1	319.9	680.3
	(1,312)	(346.7)	(527.6)	(479.3)	(424.7)	(486.9)
Other Professional	251.9**	258.4***	211.7***	218.2***	264.7***	255.2***
	(123.7)	(73.49)	(54.74)	(53.85)	(53.49)	(51.30)
Observations	757,336	757,336	757,336	757,336	757,336	757,336
R-squared	0.747	0.738	0.729	0.720	0.712	0.706
Students	112566	112566	112566	112566	112566	112566

Weighted Averages:	Yr 1	Yr 2	Yr 3	Yr 4	Yr 5	Yr 6
Any Online Credits Completed	380.4	-9.278	647.1**	962.5***	991.4***	1112***
	(348.2)	(270.6)	(250.9)	(251.2)	(253.6)	(261.0)
Total Online Credits Completed	272.8***	177.7***	78.38***	36.63	32.08	27.90
	(69.58)	(36.47)	(27.23)	(24.09)	(23.35)	(23.09)

Notes: Robust standard errors clustered at the level of the student are in parentheses. See Table 1 for sample notes. All regressions also include conrols for bins of total credits completed by field, demographics, year fixed effects, and student fixed effects.

Table 10. Semi-Parametric Specification of Online Credits, Over Time, Credits Attempted

Panel A.	**Dependent Variable: Earnings (2011$)**					
	Yr 1	Yr 2	Yr 3	Yr 4	Yr 5	Yr 6
Any Online Credits Attempted	397.1*	459.9**	753.2***	1,021***	1,192***	1,450***
	(220.9)	(179.7)	(173.7)	(178.0)	(181.3)	(183.5)
Total Online Credits Attempted	237.7***	105.9***	44.01***	1.009	-17.99	-27.16**
	(36.56)	(19.33)	(14.74)	(13.14)	(12.45)	(11.63)
Total Credits Attempted by Bin	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
Individual Fixed Effects	X	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X	X
Observations	757,336	757,336	757,336	757,336	757,336	757,336
R-squared	0.747	0.737	0.727	0.716	0.708	0.702
Students	112566	112566	112566	112566	112566	112566

Panel B. Weighted Averages of Field-Specific Estimates						
	Yr 1	Yr 2	Yr 3	Yr 4	Yr 5	Yr 6
Any Online Credits Attempted	340.0	176.0	782.5***	998.6***	1156***	1367***
	(312.5)	(266.1)	(234.1)	(231.4)	(232.2)	(234.7)
Total Online Credits Attempted	254.6***	147.7***	58.27**	27.19	14.59	3.478
	(60.59)	(35.28)	(24.58)	(21.38)	(20.25)	(19.28)
Total Credits Attempted by Bin and Field	X	X	X	X	X	X
Demographics	X	X	X	X	X	X
Individual Fixed Effects	X	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X	X
Observations	757,336	757,336	757,336	757,336	757,336	757,336
R-squared	0.748	0.738	0.729	0.720	0.712	0.706
Students	112566	112566	112566	112566	112566	112566

Notes: Robust standard errors clustered at the level of the student are in parentheses.The estimates presented in Panel B are weighted averages of field-specific point estimates similar to those in Table 9. See Table 1 for sample notes. Demographics is a vector of controls including age, age squared, and the interaction of these two variables with gender and race.

Table 11. Heterogeneity in Semi-Parametric Specification of Online Credits, Weighted Averages of Field-Specific Estimates

	Dependent Variable: Earnings (2011$)					
	Yr 1	Yr 2	Yr 3	Yr 4	Yr 5	Yr 6
Panel A. Older Individuals (30 plus at initial enrollment)						
Any Online Credits Completed	671.0	404.0	780.9**	1403***	1158***	1518***
	(465.2)	(388.1)	(372.0)	(371.9)	(380.4)	(391.1)
Total Online Credits Completed	431.1***	217.9***	136.7***	50.20	68.18	48.18
	(84.67)	(46.21)	(37.31)	(32.97)	(32.62)	(31.70)
Observations	360,119	360,119	360,119	360,119	360,119	360,119
R-squared	0.752	0.746	0.739	0.731	0.725	0.719
Students	51233	51233	51233	51233	51233	51233
Panel B. Younger Individuals (20-29 at initial enrollment)						
Any Online Credits Completed	452.1	-318.1	601.7*	576.8*	872.2**	803.7**
	(534.3)	(397.4)	(340.4)	(342.2)	(341.3)	(350.6)
Total Online Credits Completed	110	148.9**	27.74	35.49	5.012	12.22
	(114.9)	(60.65)	(40.12)	(35.51)	(33.74)	(33.65)
Observations	397,217	397,217	397,217	397,217	397,217	397,217
R-squared	0.672	0.655	0.643	0.631	0.623	0.616
Students	61333	61333	61333	61333	61333	61333
Panel C. Males						
Any Online Credits Completed	-763.2	-619.1	568.8	1337**	1556***	1787***
	(844.6)	(650.8)	(580.6)	(566.1)	(593.0)	(591.3)
Total Online Credits Completed	585.5***	336.8***	146.5**	41.08	35.24	40.37
	(176.2)	(94.73)	(68.16)	(59.62)	(62.37)	(60.58)
Observations	298,363	298,363	298,363	298,363	298,363	298,363
R-squared	0.766	0.756	0.748	0.740	0.732	0.725
Students	44764	44764	44764	44764	44764	44764
Panel D. Females						
Any Online Credits Completed	871.3**	388.2	781.3***	947.5***	863.2***	978.5***
	(378.7)	(266.0)	(264.6)	(271.2)	(272.5)	(280.9)
Total Online Credits Completed	158.8**	100.5***	42.16	21.31	18.82	12.53
	(73.50)	(32.80)	(27.33)	(24.67)	(23.37)	(22.82)
Observations	458,973	458,973	458,973	458,973	458,973	458,973
R-squared	0.721	0.712	0.702	0.692	0.685	0.680
Students	67802	67802	67802	67802	67802	67802

Notes: Robust standard errors clustered at the level of the student are in parentheses. See Table 1 for sample notes. The estimates presented here are weighted averages of field-specific point estimates similar to those in Table 9. All regressions also include conrols for bins of total credits completed by field, demographics, year fixed effects, and student fixed effects.

APPENDIX A - Figures

Figure A1. Employment Relative to First Enrollment

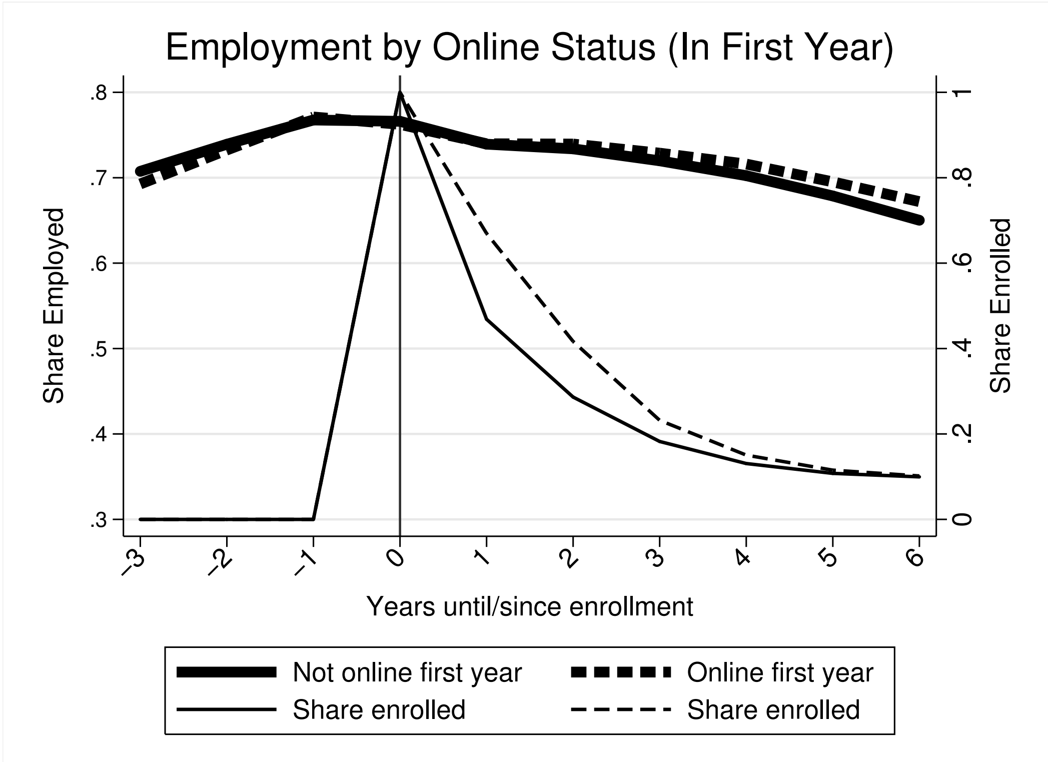


Figure A2. Earnings Relative to First Enrollment, by Ever Online

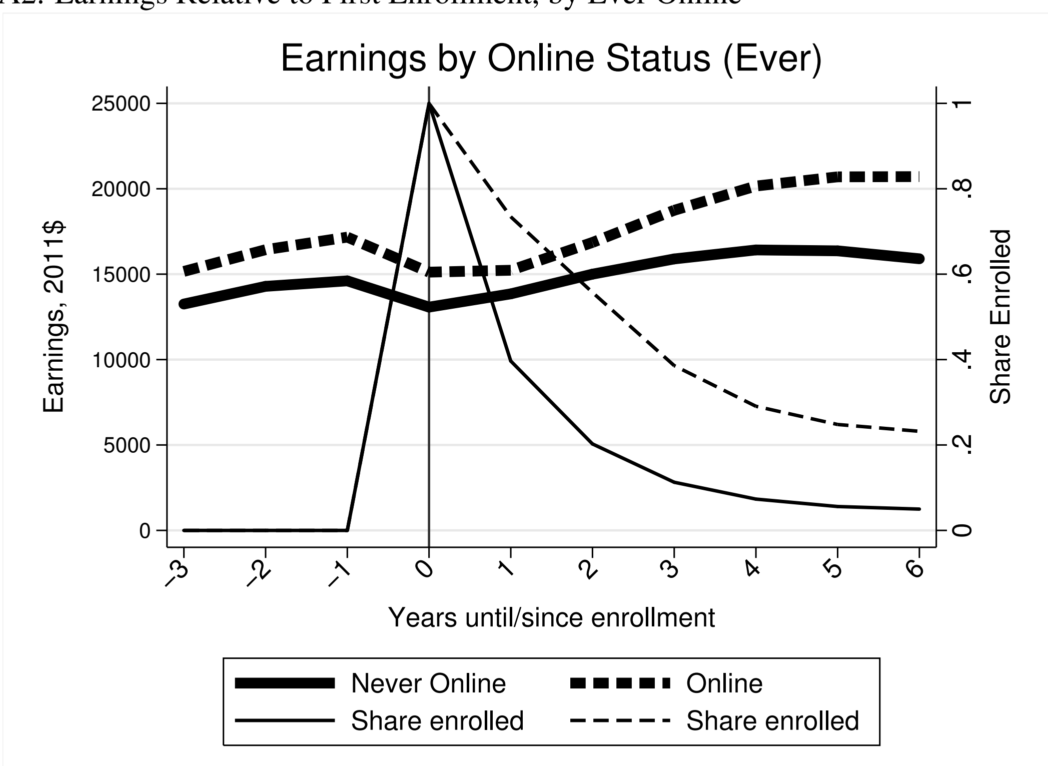
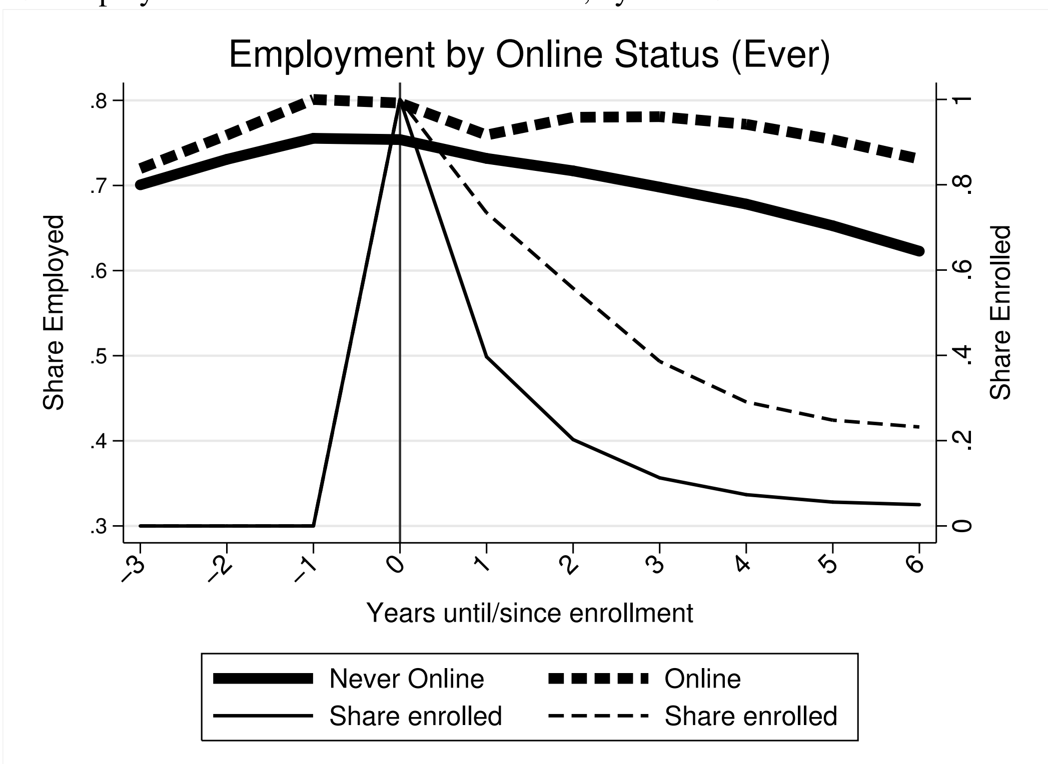


Figure A3. Employment Relative to First Enrollment, by Ever Online



Chapter 2. Online and Hybrid Instruction and Student Success: Evidence from Community-College Students in Two States

Abstract

This study looks at the effect of online and hybrid instruction on students' course-level outcomes. Fixed-effects and instrumental-variables estimation strategies are implemented to remove bias and generate causal estimates. The estimates show that students are 9 - 13 percentage points less likely to pass an online class and 4 - 7 percentage points less likely to pass a hybrid class relative to a face-to-face class. These averages mask substantial heterogeneity. Older, female, and better-prepared students fare better in online and hybrid courses. The estimates also suggest an element of "learning as you go" in online and hybrid education – students with prior experience in these alternative instructional formats fare better than those without prior experience.

1 Introduction

Online education in college is expanding dramatically. In a survey of 2,500 colleges and universities, more than 6.7 million students took an online course in the fall of 2011 (Allen and Seaman, 2013). This represents a 9.3% increase over the number reported in the fall of 2010 and a 319% increase over the number reported in 2002. Another type of distance education, hybrid education, is less common but growing. Hybrid courses, also known as blended courses, use a mix of face-to-face and online instructional methods resulting in less in-class seat time than a traditional face-to-face course, but still having a substantial face-to-face component.

The popularity of online and hybrid instruction is particularly evident among community colleges. In 2007, 96% of public, two-year institutions offered online courses and 66% offered hybrid courses (Parsad and Lewis, 2008), more than any other sector of higher education.[1]

There is little extant research addressing the causal effect of these instructional methods on student outcomes in higher education. An even smaller subset of this research has explored online and/or hybrid instruction in the community-college (public, two-year college) environment – which enrolls the largest share of undergraduate students, 39%.[2] Do online and hybrid instruction improve or inhibit community-college students' performance in their courses?

Answering this question is particularly challenging because outcomes in online and hybrid classes may differ from those in face-to-face classes for reasons other than simply the instructional format. One concern is that students select into online or hybrid classes based on characteristics that are correlated with their academic performance. For example, students may elect to take these courses because they have greater work or family commitments. If these extra-curricular commitments also make it more challenging to successfully complete the coursework, then we may observe worse outcomes among students in online and hybrid courses even if the instructional format was not the cause. Alternatively, students may opt into the online or hybrid format for easier courses that they are more likely to pass. In this scenario, we would observe better outcomes among students in these courses even if the instructional format was not the cause. Furthermore, instructors, coursework, and assessment practices may differ across online, hybrid, and face-to-face formats.

The ideal way to navigate these concerns is to conduct a randomized trial in which students are randomly assigned to online, hybrid, and face-to-face instructional formats of the same course,

[1] 86% of public four-year institutions offered online courses and 49% offered hybrid courses. Public two-year institutions also have the highest participation rate in "distance education." 22% of undergraduates at these institutions participate in such courses (Radford, 2011). "Distance education" is defined here as a course that "[is] not a correspondence course but [is] primarily delivered using live, interactive audio or videoconferencing, pre-recorded instructional videos, webcasts, CD-ROM or DVD, or computer-based systems delivered over the internet." Thus, distance education does not correspond perfectly with the fully online courses I evaluate in this paper, but this fact is still suggestive of the popularity of online courses in the community-college sector.

[2] Author's calculation from statistics in Table 226, U.S. Department of Education, Digest of Education Statistics, 2012.

with the same instructor, the same assignments, and the same assessment practices. The outcomes across the online, hybrid, and face-to-face students can then be compared without concern for systematic differences in student or course characteristics. This approach has been undertaken, but not in the context of community colleges (Figlio et. al, 2013; Bowen et. al, 2012). Short of being able to run such an experiment, I use data on several community colleges in State A as well as the full system of community colleges in State B to answer this question using quasi-experimental techniques that generate plausibly causal estimates of the effect of online and hybrid instruction on students' course-level outcomes.[3] The results are similar across the two states. I find that students are 9 - 13 percentage points less likely to pass an online class and 4 - 7 percentage points less likely to pass a hybrid class, relative to a face-to-face class. These averages mask substantial heterogeneity. Older, female, and better-prepared students fare better in online and hybrid courses. The estimates also suggest an element of "learning as you go" in online and hybrid education – students with prior experience in these instructional formats fare better than those without prior experience.

2 Related Research

There is a growing body of research that looks at the relationship between online and hybrid instruction and student outcomes. A recent meta-analysis by the Department of Education (2010) received a good deal of press regarding its conclusion that online instruction was superior to face-to-face instruction. This study has since received criticism because its broad conclusion is somewhat misleading (Figlio et al., 2013; Jaggars and Bailey, 2010). First, many of the studies included in the meta-analysis looked at outcomes in short training sessions as opposed to full-semester, college-level courses. Second, the study reviewed research that addressed both fully-online instruction and hybrid instruction, and the superior outcomes were driven by the hybrid learning

[3]State A and State B are used to hide the identity of the states as per data use agreements.

conditions while no significant differences were found between students in purely online and face-to-face conditions. This distinction was largely missed by the media (Jaggars and Bailey, 2010).

The evidence on fully-online, full-semester, college-level instruction is mixed. Some studies find that students in the online format perform better in terms of final grades and test scores.[4] The majority of the evidence, however, suggests that students' outcomes are worse in the online format. Withdrawal and dropout rates are typically higher in online courses relative to their face to-face counterparts (Carr, 2000; Carpenter, Brown, and Hickman, 2004; Xu and Jaggars, 2011; Xu and Jaggars, 2013). And, students in online courses score lower on exams, earn lower grades, and are less likely to pass the course (Brown and Liedholm, 2002; Coates et al., 2004; Jaggars and Xu, 2010; Jaggars and Xu, 2011; Xu and Jaggars, 2011; Xu and Jaggars, 2013; Figlio et al., 2013).

The evidence on hybrid, full-semester, college-level instruction is more sparse. The existing research largely suggests that there are no significant differences in student outcomes across hybrid and face-to-face formats (Xu and Jaggars, 2011; Riffell and Sibley, 2005; Brown and Liedholm, 2002; Bowen et al., 2012).

Little of this research establishes strong causal relationships. Xu and Jaggars (2011), Bowen et al. (2012), Xu and Jaggars (2013), and Figlio et al. (2013) are exceptions. Figlio et al. (2013) and Bowen et al. (2012) present experimental estimates of the effect of online and hybrid instruction, respectively. Figlio et al. (2013) focuses on exam scores for students in an introductory economics course at a large research university and finds that live instruction is "modestly superior," but not significantly superior, to online instruction. Bowen et al. conduct a set of experiments at several public universities in which students are randomly assigned to face-to-face and hybrid versions of introductory statistics courses. They find that exam scores and pass rates are higher among the students in hybrid courses, but that the differences are not significant. Xu and Jaggars (2011) and Xu and Jaggars (2013) use quasi-experimental techniques, propensity score matching

[4] To name a few: Navarro and Shoemaker, 2000; Schoenfeld-Tacher et al., 2001; Carpenter, Brown, and Hickman, 2004; Cavus and Ibrahim, 2007; and Washburn, 2012.

and instrumental variables, respectively, to estimate the effect of online instruction on completion and grades.[5] They find that students are significantly less likely to complete online courses and, conditional on completion, earn significantly lower grades.

This study contributes to our understanding of the effect of online and hybrid instruction on students' course-level outcomes, the latter of which is less well understood in the literature. I provide estimates using a variety of techniques to eliminate different sources of bias (course fixed effects, student fixed effects, and instrumental variables (IV)) and generate plausibly causal estimates. I focus on the community-college sector where online and hybrid education are prevalent yet understudied (Xu and Jaggars, 2011).

This study also contributes to our understanding of the heterogeneity in these effects across types of students and types of courses. Prior research suggests that males, younger students, and minority students experience larger performance gaps between face-to-face and online courses (Figlio et al., 2013; Xu and Jaggars, forthcoming; Kaupp, 2012). Xu and Jaggars (forthcoming) also finds larger online performance gaps in courses in the social sciences and applied professions. I provide additional evidence on online performance gaps across different types of students as well as across different levels and fields of courses. Importantly, I provide the first evidence, to my knowledge, of heterogeneity in hybrid performance gaps.

3 Data

This paper employs data from four community colleges in State A and the entire community-college system in State B (58 colleges). The State A sample contains for-credit students who enrolled in the college for the first time between the fall of 2003 and winter of 2011. The State B sample contains for-credit students who enrolled in college for the first time between the fall of

[5] Xu and Jaggars (2013) instruments for participating in the online section of a course with student's travel distance to their college.

2001 and summer of 2010.[6] Administrative records from the State A and State B colleges provide demographic information such as age, gender, and race as well as transcript information that details all the courses in which students enrolled (including the course name, subject, instructional format, and the grades they earned in those courses (including grades of W (withdrawal) and I (incomplete)). Unemployment-insurance records provide information on each student's employment during the time of their enrollment.[7]

3.1 Background on Online and Hybrid Classes in the Two States

There is substantial variety in online education. The phrase "online education" is used to describe all types of courses ranging from Massive Open Online Courses (large-scale, open-access courses offered on the internet) to web-enhanced courses in which the instructors of face-to-face courses simply post readings and assignments for students to retrieve on the course's website. The online and hybrid courses studied here fall in the middle. Both the online and hybrid courses are meant to replicate the face-to-face counterparts, but, in the case of the online courses, the instruction is provided fully online, and, in the case of the hybrid courses, the instruction is split between online and face-to-face delivery.

The online, hybrid, and face-to-face courses cover the same material, and the online and hybrid courses have the same number of students as their face-to-face counterparts. The courses are hosted by a learning management system such as Blackboard or ANGEL. The same professors can be found teaching online, hybrid, and face-to-face courses, and the tuition is the same for

[6]The State B sample was already restricted to first-time college students. The State A sample is students who enrolled at the State A community college for the first time. I also generated several estimates on a restricted sample of first-time ever college students by using National Student Clearinghouse data to restrict the State A sample to students who are not observed to have enrolled or earned degrees from other institutions prior to enrolling in the State A community college. This restriction does not affect the results and the unrestricted sample is used in all the estimates presented herein.

[7]Students are dropped if they are missing information on age or gender. They are also dropped if they first enrolled before the age of 14 or after the age of 70. Students are dropped if they have incomplete transcript information (missing instructional format or course outcome).

online, hybrid, and face-to-face courses.[8] That said, there is still a good deal of heterogeneity in the online and hybrid courses studied here. For example, some of the fully online courses require in-person orientations and proctored exams and others do not.[9] Unfortunately, I do not observe these characteristics in my data and cannot control for them. Most importantly, however, the online and hybrid courses I study are intended to recreate, as closely as possible, the learning that would occur in a comparable face-to-face course.

3.2 Summary Statistics

Figures 3 and 4 plot the number of students (bold line) enrolled by academic year for State A and State B, respectively. Because the samples contain first-time students, the 2003 academic year in Figure 3 only includes students who started in this year whereas the 2004 academic year includes students who started in 2004 plus those who started in 2003 and re-enrolled in 2004. The number of students enrolled appears to drop in State A between 2009 and 2010, but this is simply because data for the summer of of 2011 (the last term in the 2010 academic year) was not provided. The dashed line represents the share of total enrollment that is online, and the dotted line represents the share of total enrollment that is hybrid.[10] Online enrollment has increased markedly over the time period in each sample. Hybrid enrollment has increased markedly in State B but has been relatively constant in State A.

Tables 12 and 13 provide summary statistics for the students in State A and B, respectively. Column 1 of each table summarizes the full sample of students in each state. The average age at initial enrollment is approximately 25 in both samples. The majority of students are female and white. 25-30% of students enroll in a remedial course in their first term. Over 60% of students are working

[8] In State A, there is frequently an additional technology fee ranging from $30 - $100 associated with online courses.

[9] One school in State A uses an accelerated time-frame for their fully online courses. The online courses last 8, 10, or 12 weeks whereas the face-to-face and hybrid courses typically last 16 weeks.

[10] It may be more appropriate to refer to this as the "share of registrations" that is online or hybrid because students are counted each time they enroll in a course.

when they first enroll, and 30-32% enroll full-time.

Tables 12 and 13 also provide a glimpse into the selection patterns into online and hybrid education. Columns 2 and 3 in Table 12 compare State A students who do not and do enroll online in their first term; column 4 provides the difference between these two groups. Columns 5, 6, and 7 replicate this for hybrid enrollment.[11] Table 13 provides the same statistics for State B. Selection patterns are similar across the two samples. Students who enroll in online or hybrid courses are older, more likely to be female, and more likely to be white. Online enrollees are less likely to be remedial students.[12] Hybrid enrollees in State A are also less likely to be remedial students, but hybrid enrollees in State B are more likely to be remedial students. Online enrollees in State A are similarly likely to be working while online enrollees in State B are slightly more likely to be working. Hybrid enrollees in State A are more likely to be working while hybrid enrollees in State B are less likely to be working. Online enrollees in State A are less likely to enroll full time, but hybrid enrollees are less likely. In State B, online and hybrid enrollees are more likely to enroll full time. In general, these summary statistics suggest positive selection (older, female, less remedial) into online and hybrid courses.[13]

Tables 14 and 15 show summary statistics of students' courses in State A and State B, respectively. Column 1 shows total enrollment by the Classification of Instructional Programs (CIP) Code category and level.[14] Columns 2 and 5 show total online and hybrid enrollment by the CIP Code category and level. In both samples, Business, Management, & Marketing courses make up the largest shares of online enrollment (17.5% in State A and 14.3% in State B) while English Language & Literature and Computer & Information Sciences are also popular areas for online study. In State A, Health Professions makes up the largest share of hybrid enrollment (23.65%), while in

[11] I dichotomize students based on online/hybrid enrollment in the first term for a simple exposition of differences between students. Dichotomizing students based on whether they are ever online or ever hybrid shows similar patterns.

[12] Students are designated as remedial if they are observed in a remedial class in their first term. Remediation exam scores are not used because their coverage is incomplete.

[13] Goldin, Katz, and Kuziemko (2006) find that females perform better than males in school on a variety of measures.

[14] Two-digit CIP codes (found here: `http://nces.ed.gov/ipeds/cipcode`) were assigned to each course based on the subject code of the course. Levels were assigned based on the course number and remedial course identifiers.

State B, Computer Information & Sciences makes makes up the largest share of hybrid enrollment (12.06%). In both samples, remedial and pre-college courses are not typically offered in online and hybrid formats, though they are more common in State B. The majority of online and hybrid enrollment in both samples is at the introductory (100s) level.

4 Empirical Strategy

The ideal way to estimate the effect of online and hybrid instruction on student outcomes is to randomly assign students to online, hybrid, and face-to-face courses that are identical in all other respects (same material, same assignments and exams, same instructor, etc.). Then, one can simply compare the average pass rates across students in the two formats to get a causal estimate of the effect. Unfortunately, running such an experiment is not feasible for this study. Fortunately, ample administrative data was made available for many community colleges in State A and State B. Coupling these data with alternative estimation strategies allows me to generate plausibly causal estimates of the effect of online instruction. I employ three alternative estimation strategies: course fixed effects, student fixed effects, and IV. I use a linear probability model for all of the regressions.[15]

First, I generate simple OLS estimates of the bivariate relationship between instructional format and passing the class using the following equation:

$$Pass_{ict} = \beta_1 Online_{ict} + \beta_2 Hybrid_{ict} + \epsilon_{ict} \tag{1}$$

in which *Pass* is an indicator for whether student i passed course c in term t, *Online* is an indicator equal to 1 if the format of the course was online, and *Hybrid* is an indicator equal to 1 if the

[15] I also generate estimates using a probit model (not presented here). Marginal effects for online and hybrid at the means of the other covariates are very similar to the linear probability model estimates.

format of the course was hybrid. A student is considered to have passed a course if they earned a D- or better as opposed to receiving an F, W, or I. In this specification, β_1 simply conveys the raw difference in passing rates between students enrolled in online and face-to-face courses and β_2 conveys the raw difference in passing rates between students enrolled in hybrid and face-to-face courses. I also generate estimates using a richer OLS specification of the following form:

$$Pass_{ict} = \beta_1 Online_{ict} + \beta_2 Hybrid_{ict} + \beta_3 StudentX_{it} + \beta_4 CourseX_c + \gamma_t + \psi_s + \epsilon_{ict} \quad (2)$$

that includes several controls for fixed and time-varying student characteristics (*StudentX*), course characteristics (*CourseX*), school dummies (ψ), and term (γ) dummies.[16]

Within levels or broad subject areas, courses may vary greatly in terms of ease or compatibility with the online or hybrid instructional format. If students select into the online or hybrid format for courses that are easier to pass, the OLS estimates of β_1 and β_2 will be biased up. I account for this type of bias by estimating course-fixed-effects specifications using the following equation:

$$Pass_{ict} = \beta_1 Online_{ict} + \beta_2 Hybrid_{ict} + \beta_3 StudentX_{it} + \lambda_c + \gamma_t + \epsilon_{ict} \quad (3)$$

in which λ_c is a vector of course fixed effects (these are actually course-by-school fixed effects because courses are unique to schools) and the coefficients of interest (β_1 and β_2) are estimated off of variation within courses (e.g. across online, hybrid, and face-to-face sections within Math-100). This method controls for all observed and unobserved differences between courses. It does not, however, account for differences that may still exist across sections within courses (such as instructors). I also estimate this equation with course-by-term fixed effects. Only courses that are

[16]*StudentX* includes age, dummies for race, a dummy for male, dummies for full-time, half-time, or less-than-half-time in term t, dummies for cohort, an indicator for whether the student is working in term t, an indicator for whether the student enrolled in a remedial class in their first term, an indicator for whether the student was receiving any financial aid, and a set of dummies capturing the number of credits the student had previously completed (these dummies include first time student, non-first-time student with 0-14 completed credits, non-first-time student with 15-29 credits completed, non-first-time student with 30-44 credits completed, and non-first-time student with 45 plus credits completed). *CourseX* includes indicators for the level of the course and indicators for the CIP2 category of the course.

offered in multiple formats (i.e. online and face-to-face or hybrid and face-to-face) contribute to the coefficients on *Online* and *Hybrid*, thus these estimates are only generalizable to courses that are offered in multiple formats. 10% of the 4,683 courses in the State A sample have both online and face-to-face enrollment and 4% have both hybrid and face-to-face enrollment. 24% of the 37,332 courses in the State B sample have both online and face-to-face enrollment. 18% have both hybrid and face-to-face enrollment.

Another concern is that students select into online and hybrid formats based on unobserved individual characteristics. I observe basic demographic information, enrollment intensity, financial aid receipt, prior academic experience, and employment information, but if students who are, for example, less motivated or less committed (characteristics we do not observe) are more likely to enroll in online or hybrid courses, the OLS estimates of β_1 and β_2 will be biased down. I account for this type of bias by estimating student-fixed-effects specifications using the following equation:

$$Pass_{ict} = \beta_1 Online_{ict} + \beta_2 Hybrid_{ict} + \beta_3 StudentX_{it} + \beta_4 CourseX_c + \delta_i + \gamma_t + \epsilon_{ict} \quad (4)$$

in which δ_i is a vector of student fixed effects and the coefficients of interest (β_1 and β_2) are estimated off of variation within students (i.e. across online and face-to-face or hybrid and face-to-face courses taken by individual students).[17] This method controls for all fixed observed and unobserved differences between students. If motivation and committedness are fixed characteristics that are correlated with online or hybrid enrollment and likelihood of passing a course, these sources of bias are removed. It does not, however, account for unobserved differences across courses taken within students. Only students who take courses in both the online and face-to-face format or in both the hybrid and face-to-face format during their time in community college con-

[17] *StudentX* only contains time varying student characteristics in this equation such as age, dummies for full-time, half-time, or less-than-half-time in term t, an indicator for whether the student is working in term t, an indicator for whether the student was receiving any financial aid in term t, and dummies capturing the number of credits the student had previously completed. Also, the school dummies are dropped from this equation because students are identified at the school level so school is a fixed characteristic of the student. In the State B sample, however, some students are observed at multiple colleges and thus the school dummies are included.

tribute to the coefficients on *Online* and *Hybrid*, thus one should use caution when generalizing these results to students who only take face-fo-face courses or only take online courses. 14% of the 265,296 students in the State A sample take both online and face-to-face courses and 4.6% take both hybrid and face-to-face courses. 34% of 763,437 students in the State B sample take both online and face-to-face courses. 17% take both hybrid and face-to-face courses.

It is possible that students opt in to the online or hybrid format during terms when they have more extracurricular commitments such as a greater number of hours at work or child care. If students take online or hybrid classes when they are more time constrained, we may observe online and hybrid courses having lower pass rates simply because of this temporal selection. To account for this potential bias, I estimate student-by-term fixed-effects specifications of the following form:

$$Pass_{ict} = \beta_1 Online_{ict} + \beta_2 Hybrid_{ict} + \beta_3 CourseX_c + \theta_{it} + \epsilon_{ict} \tag{5}$$

in which θ_{it} is a vector of student-by-term fixed effects and the coefficients of interest (β_1 and β_2) are estimated off of variation within student-terms (i.e. across online and face-to-face courses or hybrid and face-to-face courses taken by individual students in a given term). Only students who take courses in both the online and face-to-face format or both the hybrid and face-to-face format during a given term contribute to the coefficients on *Online* and *Hybrid*, thus these estimates may not generalize to students who take, for example, one course at a time or only enroll in one type of format per term. 5.5% of the 844,482 student-terms in the State A sample have both online and face-to-face enrollment and 1.5% have hybrid and face-to-face enrollment.[18] 15% of the 2,684,608 student-terms in the State B sample have both online and face-to-face enrollment. 6% have both hybrid and face-to-face enrollment.[19]

Lastly, I use an IV strategy combined with course fixed effects to remove bias stemming from both the types of courses offered/taken in the online and hybrid formats and the selection of students

[18] 3.6% of student-terms have only online enrollment and 0.2% of student-terms have only hybrid enrollment.
[19] 7.9% of student-terms have only online enrollment and 1.2% of student-terms have only hybrid enrollment.

into the online and hybrid formats.[20] To do so, I estimate the following equations:

First Stage for Online :

$$Online_{ict} = \alpha_1 ShareSeatsOnline_{ict} + \alpha_2 ShareSeatsHybrid_{ict} + \alpha_3 StudentX_{it} + \tau_c + \phi_t + \varepsilon_{ict} \tag{6}$$

First Stage for Hybrid :

$$Hybrid_{ict} = \rho_1 ShareSeatsOnline_{ict} + \rho_2 ShareSeatsHybrid_{ict} + \rho_3 StudentX_{it} + \tau_c + \phi_t + \xi_{ict} \tag{7}$$

Second Stage:

$$Pass_{ict} = \beta_1 \widehat{Online}_{ict} + \beta_2 \widehat{Hybrid}_{ict} + \beta_3 StudentX_{it} + \lambda_c + \gamma_t + \epsilon_{ict} \tag{8}$$

in which I instrument for whether the student i enrolled in the online or hybrid format of course c in term t with the share of seats offered for course c in term t in the online or hybrid format. Again, the course fixed effects restrict the identifying variation to be within course over time – the instruments, the share of seats offered online and the share of seats offered hybrid, vary over time within a course but are fixed across students who enroll in that course in a given term.[21] The IV strategy can only be applied to a subset of the State A sample – two schools for which course offering and capacity data was provided.[22] 12.5% of courses offer some seats online at some point in the sample frame. 6.6% of courses offer some seats in the hybrid format at some point in the

[20] One can also include student and course fixed effects in the same equation (but not student-by-course fixed effects). I do so using the Stata command a2reg and find that estimates are very similar to those that simply use student fixed effects.

[21] This method assumes that the student would have enrolled in the face-to-face version of the course if the online/hybrid sections were full.

[22] To construct the share of seats offered online/hybrid for a course, I divide the total seats offered in online/hybrid sections of a course by the total number of seats offered across all sections of the course regardless of instructional format. I include seats that were initially offered in sections that were eventually cancelled because these numbers are the most likely to be exogenous.

sample frame. The within course average share of seats offered online is 0.06 with a standard deviation of 0.19. The within course average share of seats offered in the hybrid format is 0.02 with a standard deviation of 0.11.[23]

For the IV strategy to produce causal estimates, the instruments need to be strong and exogenous. In other words, the share of seats offered in each alternative format needs to be a strong predictor of whether or not student i enrolled in the alternative format of a given course. And, the share of seats offered in the online or hybrid format cannot be correlated with the likelihood of passing the class. The instruments is strong, and this condition is discussed more in the results section. I also argue that the instruments are exogenous. The instruments would not be exogenous if the share of seats offered online or the share of seats offered in the hybrid format for a given course in a given term are systematically correlated with the likelihood of passing the course in that term. If, for example, Math-101 is harder to pass in the fall of 2007 and a larger share of the seats are offered online while it is easier to pass in the winter of 2008 but a smaller share of the seats are offered online, the instruments would fail to meet the exogeneity condition. This scenario is unlikely.

5 Results

5.1 Basic OLS Results

Table 16 contains the basic OLS estimates for the State A sample. Column 1 conveys the raw differences in passing rates between online, hybrid, and face-to-face estimated using equation 1. The passing rate for students in face-to-face courses is 75.7%. Students enrolled in online classes are 4.35 percentage points less likely to pass than their counterparts in face-to-face classes. Students enrolled in hybrid classes are modestly (0.38 percentage points) less likely to pass than their

[23]Basic Statistics (MTH-160), for example, offered no seats online or hybrid at the beginning of my time frame and offered 20% of its seats online and 15% of its seats hybrid by the end.

counterparts in face-to-face classes. Columns 2-4 progressively control for term and school dummies, student characteristics, and course characteristics, respectively. Column 4 is the rich OLS specification described by equation 2. After simply controlling for school and term, by including a set of dummies for each, we see that the gap in online passing rates grows to 6.6 percentage points and the gap in hybrid passing rates grows to 1.6 percentage points. Controlling for student characteristics (column 3) further increases these estimates to 9.5 and 4.2 percentage points respectively – this affirms the positive selection that was suggested in the discussion of the summary statistics. Students who are more likely to pass (older, female, non-remedial) are selecting in to online and hybrid courses, and failing to control for such characteristics leads to underestimates of the differences in passing rates. Controlling for course characteristics (column 4) increases the gaps slightly to 9.6 and 4.4 (though the coefficients are not significantly different from those in column 3).[24]

A similar pattern appears for the State B sample (Table 17) although including additional covariates has less of an effect on the point estimates. The passing rate for students in face-to-face courses is 78.9%. The raw difference in passing rates between face-to-face and online classes is 9.1 percentage points, and the raw difference in passing rates between face-to-face and hybrid classes is 3.6 percentage points. These differences grow to 10.9 and 4.8 percentage points respectively, after including all of the controls, with the largest change in coefficients occurring upon controlling for student characteristics.

The OLS estimates are broadly similar across the two states. They show us that the raw differences in passing rates may understate the effect of online and hybrid instruction on student outcomes – greatly so in the State A sample. Controlling for student characteristics is particularly important – doing so causes substantively and significantly large increases in the gaps in passing rates with online and hybrid classes appearing to be even worse, relative to face-to-face classes, than initially thought.

[24] An overlapping confidence interval test is used here. The coefficients in column 3 are contained in the 95% confidence intervals surrounding the coefficients from column 4.

On one hand, observable characteristics may serve as good proxies for unobservable characteristics, and controlling for the observable differences may allay concerns about bias stemming from unobservable ones. One the other hand, the importance of controlling for observed differences may make one even more concerned about bias that remains unaccounted for from unobservable differences. To account for several of these potential remaining biases, I proceed to the fixed-effects specifications.

5.2 Fixed Effects Results

Table 16, columns 5-8 contain the fixed-effects estimates described in equations 3-5 for the State A sample. Columns 5 and 6 restrict the source of variation to within course and within course-by-term, respectively, removing bias that stems from unobserved differences across courses.[25] The course-fixed-effects estimates (column 5) tell us that, within courses which are observed to be taken in both the face-to-face and online format, students in the online sections of the courses are 9.3 percentage points less likely to pass. Within courses which are observed to be taken in both the face-to-face and hybrid format, students in the hybrid sections of the courses are 5.5 percentage points less likely to pass. The course-by-term fixed-effects estimates tells us that, within courses taken in both the online and face-to-face format in the same term, students in the online sections of the courses are 9.5 percentage points less likely to pass. Within courses taken in both the online and hybrid format in the same term, students in the hybrid sections are 6.5 percentage points less likely to pass. The estimate of the online gap is very similar to, though slightly smaller than, the rich OLS estimate.[26] The estimate of the hybrid gap is larger than the rich OLS estimate suggesting that students are selecting into the hybrid format for courses that may be "easier" to pass, based on unobserved features, and controlling for unobserved course characteristics removes this source of positive bias.

[25]Note that courses are unique to schools, so this is actually course-by-school and course-by-school-by-term.

[26]The coefficient from the rich OLS specification is contained in the confidence interval for both the course and course-by-term fixed-effects specification, thus the estimates are not significantly different.

Columns 7 and 8 restrict the source of variation to within student and within student-by-term, respectively, removing bias that stems from unobserved differences across students. The student-fixed-effects estimate (column 6) tells us that, within students who take courses in both the online and face-to-face format, students are 10.7 percentage points less likely to pass their online classes. Within students who take courses in both the hybrid and face-to-face format, students are 5.2 percentage points less likely to pass their hybrid courses. This estimates highlight, again, the importance of accounting for self-selection of students into specific course formats. Controlling for observed student characteristics in the OLS estimates (column 3) greatly increased the online and hybrid gaps in passing rates. Further controlling for unobserved differences here by including student fixed effects increases the gaps even further suggesting that students were also positively selecting into the online and hybrid formats based on unobserved characteristics.

The student-by-term fixed-effects estimate (column 8) tells us that, within students who take courses in both the online and face-to-face format in the same term, students are 9.7 percentage points less likely to pass their online classes. This estimate is 9.3% smaller than the student-fixed-effects estimate and lies outside of the estimate's 95% confidence interval. The goal of this estimate was to account for potential unobserved temporal selection into the online format, such as students taking online courses during terms when they are more time-constrained, that would cause the gap in passing rates to be negatively biased. The reduction in the gap between columns 7 and 8 suggests that this type of selection was likely occurring. For hybrid courses, however, the estimated gap in passing rates is largely unchanged suggesting that there is less unobserved temporal selection into this instructional format.

Table 17, columns 5-8 contain the same estimates for the State B sample. The course and course-by-term fixed-effects estimates (columns 5 and 6) are larger than the rich OLS estimates in column 4 suggesting that students are selecting into the online and hybrid formats for courses that are "easier" to pass in unobserved ways and controlling for unobserved course characteristics removes this source of positive bias. The student and student-by-term fixed-effects estimates of the online and hybrid gaps are also larger than the rich OLS estimates, suggesting again that students positively select into the online and hybrid format based on unobserved characteristics of themselves. The student-by-term fixed-effects estimate of the online gap is smaller than the student fixed-effects estimate suggesting negative, unobserved temporal selection into online classes (i.e. students take online classes during terms when they are less likely to pass their courses). The student-by-term fixed-effects estimate of the hybrid gap is not significantly or substantially different from the student fixed-effects estimate suggesting that this type of temporal selection in hybrid courses is not happening.

The fixed-effects estimates presented above restrict the estimating variation in ways that remove alternate sources of bias stemming from different types of unobserved characteristics. They do not, however, remove bias from both unobserved course and student characteristics simultaneously. Thus, I proceed to the IV estimation that does exactly that.

5.3 Instrumental Variables Results

Table 18 presents the estimate from the IV specification described in equations 6 through 8 for the restricted State A sample. Column 1 replicates the course-fixed-effects estimate from Table 16, column 5 for the restricted State A sample. Note that this estimate is approximately one percentage point larger than that from the full State A sample. Columns 2 and 3 present the two first stage estimates of the IV estimation. The share of seats offered in course c in term t is a strong predictor of individual i taking course c online in term t. The f-statistic for the first stage for online enrollment is 4711 which easily exceeds the threshold of 10 for strong instruments (Staiger and Stock, 1997). The same applies for the first stage for hybrid enrollment with an f-statistic of 3095. The IV estimates (column 4) tell us that, within courses that are offered both online and face-to-face over time, students who take the course online are 9.1 percentage points less likely to pass. And, within courses that are offered in both the hybrid and face-to-face format over time, students who take the course in the hybrid format are 4.27 percentage points less likely to pass.[27] The IV estimates are smaller than, though not statistically different from, the corresponding course-fixed-effects estimates. Previously noted trends of positive selection into online and hybrid courses suggests that the IV estimates should be larger than the course-fixed-effects estimate (i.e. the relevant "OLS" estimate). But, this is not the case. It may be that, within course, the negative selection dominates. For example, students who are less sure about/committed to taking Statistics-101 will do so in the online environment. If the instrument is actually wiping out this negative selection, then that would explain the decrease in the gap between the course-fixed-effects and IV estimates.[28]

5.4 Separating Completion and "Success"

Some researchers have found that students are less likely to complete an online course, but, conditional on completion, are more successful (in terms of grades) in online courses (Carpenter, Brown, and Hickman, 2004). Hence, some researchers look separately at completion and "success" conditional on completion (Xu and Jaggars, 2011; Xu and Jaggars, 2013).[29] In this section, I replicate the rich OLS and fixed effects specifications using two different outcomes: completion (finishing the class with a grade of A - F as opposed to withdrawing or taking an incomplete) and passing conditional on completion (earning an A - D as opposed to an F conditional on completing the

[27]Note that the number of fixed effects drops in column 4. This is because the Stata command for IV with fixed effects (xtivreg2) drops "singleton" observations. Dropping these observations for the OLS or first stages does not affect those estimates.

[28]Another way to think about this is to reflect on who is "moved" by the instrument. IV estimates are are local average treatment effects because they are estimated off of observations that are moved by the instrument (i.e. students who are moved into the hybrid or online format because it represents a larger share of the seats offered for the course). If students are generally drawn into online or hybrid formats because they are less committed to or motivated for the coursework or because they have larger extra-curricular or family commitments, then those who are simply moved by the instrument are those that are relatively less eager to get into the online or hybrid format. Relative to those students who are eager to get into online or hybrid courses, these students may be more committed to their coursework or have less extra-curricular commitments and hence face a smaller gap in their performance between online/hybrid and face-to-face classes.

[29]This is defined differently across papers. Carpenter et al. (2004) define success as earning a 2.5 (a B) or better, Xu and Jaggars (2011) define success as earning a C or better, and Xu and Jaggars (2013) use a continuous measure of grade point.

course, which I will call "success").

Tables 19 and 20 show these results for State A and State B, respectively. When focusing on completion, Panel A of Tables 19 and 20, we see that the results are largely similar across all of the specifications and similar across the two states. Students are 5 - 6.4 percentage points less likely to complete an online class than a face-to-face class, and they are 2 - 3.8 percentage points less likely to complete a hybrid class than a face-to-face class. When focusing on success, the results are, again, quite similar across specifications within states, but the point estimates are somewhat larger for State B. In State A, students are 6 - 7 percentage points less likely to succeed in an online course and 3 - 4 percentage points less likely to succeed in a hybrid course. In State B, students are 8 - 9 percentage points less likely to succeed in an online course and 3.5 - 5 percentage points less likely to succeed in a hybrid course.

These results show that the online and hybrid formats contribute to reduced likelihoods of passing a class at both the completion and success margins. Unlike some prior research, my results show that, conditional on completion, the format of the course does influence likelihood of success (defined as earning a D or better and earning credit for the course). For the duration of the analysis, I will focus on passing the course as the outcome of interest.

5.5 Heterogeneity

The fixed-effects and IV estimates make it clear that student outcomes (in terms of passing a course and earning credit) are worse in online and hybrid formats. On average, students are 9 - 13 percentage points less likely to pass a class in the online format and 4 - 7 percentage points less likely to pass a class in the hybrid format, depending on the estimation strategy. These averages mask a good deal of heterogeneity across students and courses. In this section, I explore this heterogeneity.

Table 21 presents estimates of the coefficients on *Online* and *Hybrid* for students disaggregated by gender, age, remedial status, employment status, school experience, and prior online/hybrid experience. Only the student-fixed effects estimates are presented for simplicity.[30] Each pair of online and hybrid coefficients are from a separate regression for the subgroup of interest, shown in italics. Panel A presents the coefficients on *Online* and panel B presents the coefficients on *Hybrid*.

The patterns are similar across the State A and State B samples. Females tend to perform relatively better in online and hybrid courses than males. For example, the student fixed effects estimates show that the online gap in passing rates for females is 9.6 percentage points while the gap for males is 13. The hybrid gap in passing rates is 4 percentage points for females and 7.1 percentage points for males. Older students also perform relatively better in online and hybrid courses. Remedial students experience larger online and hybrid gaps in passing rates, especially in the State A sample. Prior research also highlights that underprepared students fare particularly poorly in online courses (Figlio et al., 2013; and Jaggars and Bailey, 2010; Xu and Jaggars, forthcoming). Students who are not employed while taking an online or hybrid class perform relatively better – this finding is concerning in light of flexible online and hybrid education being touted for students with busy work/family lives. Students in later terms of enrollment have smaller online and hybrid gaps in passing rates, and, among those in later terms (i.e. not their first term), students with prior online and hybrid experience have smaller online and hybrid gaps in passing rates. This suggests an element of "learning as you go" in online and hybrid education.

Table 22 explores heterogeneity across courses by level and subject area (6 of the 41 CIP categories are presented). Again, the patterns are similar for the State A and State B samples. The online and hybrid gaps in passing rates are substantially larger for remedial- and pre-college-level courses than for introductory- and advanced-level courses. Furthermore, some subject areas may be more suitable to the online or hybrid format. For example, one might imagine that computing classes or quantitative classes are more amenable to this format than literature or social science classes

[30]Estimates using course-by-term and student-by-term fixed effects show similar patterns.

that may hinge on lectures and in-class discussion. But, we see the online gap in Computer & Information Sciences and Math & Statistics to be larger (though not significantly so) than other fields, such as English Language & Literature and Psychology.[31]

The gap in passing rates is substantially lower for Health courses than for the other categories presented. It is plausible that this low gap is the result of selective enrollment for many of the health courses. For example, students have to apply and be accepted to the nursing program in order to enroll in the nursing classes. Thus, students in many of the health classes have already been screened for their academic ability and have expressed particular commitment to their schooling.

6 Conclusion and Discussion

This study looks at the effect of online and hybrid instructional formats on student success (defined by passing the class and earning credit). Fixed-effects and IV estimation strategies are implemented to remove bias and generate plausibly causal estimates. The fixed-effects estimates suggest that students are 9 - 13 percentage points less likely to pass an online class and 5 - 7 percentage points less likely to pass a hybrid class. The IV estimates are similar at 9.1 and 4.3 percentage points for online and hybrid courses, respectively.

Accounting for self-selection of students into online and hybrid courses in particularly important–controlling for observed and unobserved student characteristics significantly increases the estimates of the gaps in passing rates. These averages mask substantial heterogeneity. Older, female, and better-prepared students fare better in online and hybrid courses. The estimates also suggest an element of "learning as you go" in online and hybrid education–students with prior online and hybrid experience fare better in online and hybrid classes than those without prior experience.

[31] Xu and Jaggars (forthcoming) also find large gaps in online performance in math, but do not find large gaps in computer science.

Similar to prior research, I find that students pay a "penalty" for taking online courses in terms of a reduced likelihood of completing and earning credit in the course. Unlike prior research which finds no significant differences between hybrid and face-to-face outcomes, I find a penalty, though smaller than the online penalty, for taking hybrid courses. One explanation for this difference is that prior research on hybrid outcomes is mostly conducted in the four-year university context (Riffell and Sibley, 2005; Brown and Liedholm, 2002; Bowen et al., 2012). It is possible that the differences in the structure of student schedules or differences in the student populations between four-year university and two-year community college contexts could lead us to find markedly different results across these two contexts. Consequently, it is important to note that one should use caution when generalizing the findings herein to other contexts, such as four-year universities or primary and secondary levels of education.

The primary outcome of interest in this study, passing a course, is not necessarily ideal–passing depends largely on a grade assigned by an instructor. Grades are not always objective nor are they consistent across classes and instructors. Scores from exams that are consistent across sections within a course and measure, to some extent, the actual learning that occurred, as used in Figlio et al. (2013), would be preferred. That said, it is the only course-level outcome that is available in my data for all students and all courses. And, because outcomes are averaged over so many different sections and instructors, some of whom grade more strictly and some more easily, this type of bias is only an issue if online and hybrid instructors systematically grade more or less harshly than face-to-face instructors. In both the State A and State B context, the same instructors can often be found teaching in multiple formats, so this is not likely the case.

Further, the outcome is of particular interest because passing a class is what is required to a) earn credit toward a credential and often to b) qualify to take more advanced courses in the field. Not passing a course (regardless of whether the student did so with an F, I, or W) means that the student forfeited the tuition (plus any time they invested in the coursework) without reaping the benefit of earning the credit. They also occupied a seat in the course that could have been used by another student. Thus, the research herein should be used to inform institutions about their enrollment practices for online and hybrid courses. For example, students who are underprepared for college in general (i.e. those who place into remedial courses) should be cautioned with respect to online and hybrid enrollment. And, the online format may not necessarily be more successful where you expect it to be, such as in the Computer & Information Science field.

This study shows that the online and hybrid formats are associated with reduced likelihood of earning credit in a course (either due to withdrawal or failure). Other research has shown that hitting credit accumulation milestones (i.e. completing 20 by the end of a student's first year or completing 5% of program requirements) is linked to increased likelihood of degree completion (Adelman, 2006; Calcagno et al., 2007). Pairing this evidence suggests that students who enroll in online and hybrid courses may be less likely to complete greater amounts of college credits or complete a degree, both of which are valuable in the labor market (Jacobson et al., 2005; Jepsen et al., 2012). On the other hand, the flexibility of online and hybrid education may help students to remain attached to and persist in college despite the increased likelihood of withdrawal and failure in these courses. Further research is needed in this area to improve our understanding of the effect of these alternative instructional formats on students' likelihood of persistence and degree completion.

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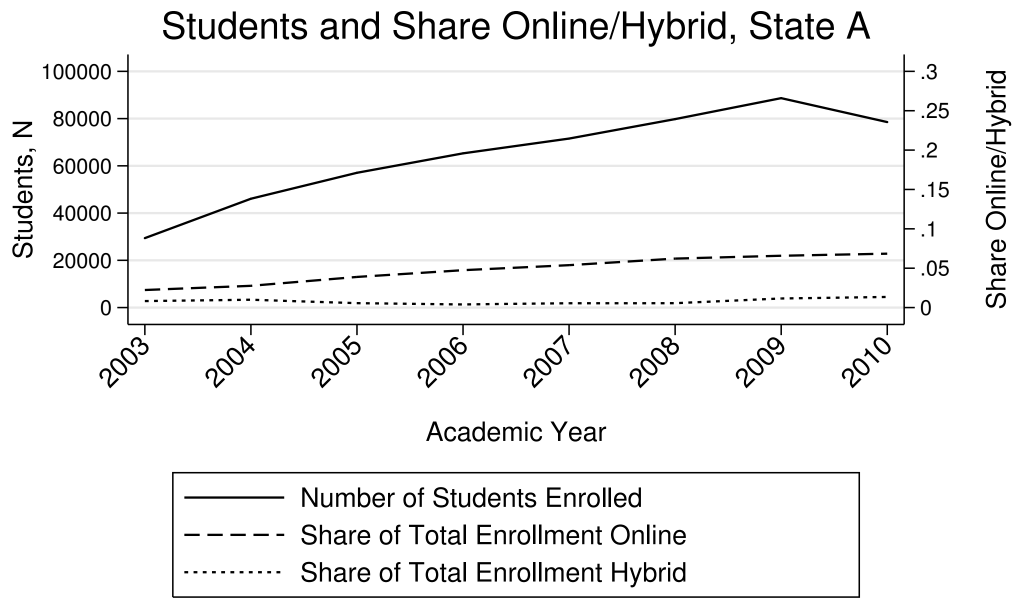
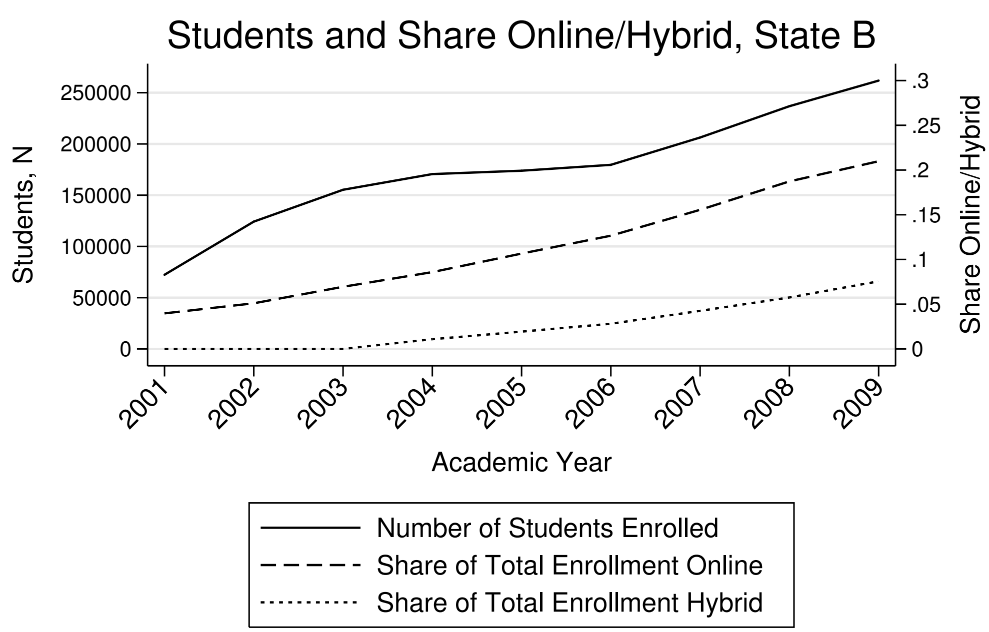
Figure 3.



Share of Total Enrollment Online = Total Enrollment Online/ Total Enrollment where Total Enrollment is the total number of students by courses. Academic Year is defined by the fall term (2003 contains fall 2003, winter 2003, and summer 2003). The apparent drop in enrollment for the 2010 academic year is simply because data is not available for the summer of the 2010 academic year.

Figure 4.



Share of Total Enrollment Online = Total Enrollment Online/ Total Enrollment where Total Enrollment is the total number of students by courses. Academic Year is defined by the fall term (2003 contains fall 2003, winter 2003, and summer 2003).

Table 12. Summary Statistics for Students, State A

		Online Comparison			Hybrid Comparison		
	1	2	3	4	5	6	7
	All Students	**NOT Online in First Term**	**Online in First Term**	**Difference**	**NOT Hybrid in First Term**	**Hybrid in First Term**	**Difference**
Age at enrollment	24.48	24.29	27.11	-2.818***	24.47	24.72	-0.243
	(9.261)	(9.239)	(9.174)	(-38.62)	(9.263)	(9.150)	(-1.67)
Male	0.455	0.464	0.319	0.145***	0.455	0.423	0.0319***
	(0.498)	(0.499)	(0.466)	(37.02)	(0.498)	(0.494)	(4.07)
White	0.675	0.671	0.741	-0.0702***	0.674	0.749	-0.0748***
	(0.468)	(0.470)	(0.438)	(-18.99)	(0.469)	(0.434)	(-10.14)
Black	0.157	0.160	0.125	0.0350***	0.158	0.0989	0.0594***
	(0.364)	(0.366)	(0.330)	(12.16)	(0.365)	(0.299)	(10.36)
Hispanic	0.0209	0.0209	0.0204	0.000527	0.0209	0.0217	-0.000878
	(0.143)	(0.143)	(0.141)	(0.47)	(0.143)	(0.146)	(-0.39)
Remedial in first term	0.250	0.262	0.0767	0.186***	0.251	0.200	0.0516***
	(0.433)	(0.440)	(0.266)	(54.54)	(0.434)	(0.400)	(7.56)
Working in first term	0.670	0.671	0.661	0.00935*	0.669	0.700	-0.0301***
	(0.470)	(0.470)	(0.473)	(2.52)	(0.470)	(0.459)	(-4.07)
Full time in first term	0.321	0.326	0.258	0.0678***	0.318	0.529	-0.211***
	(0.467)	(0.469)	(0.438)	(18.38)	(0.466)	(0.499)	(-28.76)
N	265296	248173	17123		261202	4094	

Notes on State A sample: The sample includes students who enrolled for the first time for credit at one of four State A community colleges between the fall of 2003 and the winter of 2011. It contains individuals who started between the ages of 14 and 70 for whom we have valid age and gender information and for whom transcript data is complete. Other race and missing race categories are not presented in this table.

Notes on table: Columns 1-3 and 5-6 contain means and standard deviations in parentheses. Columns 4 and 7 contains differences and t-statistics in parentheses.

Table 13. Summary Statistics for Students, State B

		Online Comparison			Hybrid Comparison		
	1	2	3	4	6	7	8
	All Students	**NOT Online in First Term**	**Online in First Term**	**Difference**	**NOT Hybrid in First Term**	**Hybrid in First Term**	**Difference**
Age at enrollment	25.37	25.16	26.60	-1.444***	25.34	25.77	-0.423***
	(10.62)	(10.66)	(10.27)	(-41.55)	(10.61)	(10.72)	(-7.55)
Male	0.429	0.440	0.358	0.0825***	0.430	0.410	0.0197***
	(0.495)	(0.496)	(0.479)	(50.95)	(0.495)	(0.492)	(7.56)
White	0.637	0.630	0.679	-0.0488***	0.637	0.643	-0.00545*
	(0.481)	(0.483)	(0.467)	(-30.98)	(0.481)	(0.479)	(-2.15)
Black	0.251	0.259	0.204	0.0548***	0.252	0.241	0.0103***
	(0.434)	(0.438)	(0.403)	(38.62)	(0.434)	(0.428)	(4.51)
Hispanic	0.0393	0.0400	0.0349	0.00513***	0.0396	0.0335	0.00611***
	(0.194)	(0.196)	(0.183)	(8.06)	(0.195)	(0.180)	(5.96)
Remedial in first term	0.301	0.309	0.257	0.0522***	0.296	0.410	-0.114***
	(0.459)	(0.462)	(0.437)	(34.72)	(0.456)	(0.492)	(-47.30)
Working in first term	0.608	0.607	0.617	-0.0108***	0.609	0.586	0.0238***
	(0.488)	(0.488)	(0.486)	(-6.78)	(0.488)	(0.493)	(9.25)
Full time in first term	0.298	0.294	0.320	-0.0258***	0.290	0.450	-0.160***
	(0.457)	(0.456)	(0.466)	(-17.20)	(0.454)	(0.497)	(-66.55)
N							

Notes on State B sample: The sample includes students who enrolled for the first time for credit at one of 58 State B community colleges between the fall of 2001 and the summer of 2010. It contains individuals who started between the ages of 14 and 70 for whom we have valid age and gender information and for whom transcript data is complete. The other race category is not presented in this table.

Notes on table: Columns 1-3 and 5-6 contain means and standard deviations in parentheses. Columns 4 and 7 contains differences and t-statistics in parentheses.

Table 14. Summary Statistics for Courses, State A

	State A						
	1	2	3	4	5	6	7
CIP2	**Total Enrollment**	**Total Online Enrollment**	**Percent of CIP2 Enrollment Online**	**Percent of Online Enrollment**	**Total Hyrid Enrollment**	**Percent of CIP2 Enrollment Hybrid**	**Percent of Hybrid Enrollment**
Agriculture	0	0	0.00%	0.00%	0	0.00%	0.00%
Natural Resources & Conservation	4785	764	15.97%	0.65%	322	6.73%	1.77%
Architecture	4720	0	0.00%	0.00%	0	0.00%	0.00%
Communication & Journalism	60766	2187	3.60%	1.85%	423	0.70%	2.33%
Communications Technology	948	0	0.00%	0.00%	0	0.00%	0.00%
Computer & Information Sciences	103197	11483	11.13%	9.70%	1768	1.71%	9.72%
Personal & Culinary Services	22633	232	1.03%	0.20%	29	0.13%	0.16%
Education	12588	1143	9.08%	0.97%	185	1.47%	1.02%
Engineering	5340	0	0.00%	0.00%	0	0.00%	0.00%
Engineering Technologies	36585	754	2.06%	0.64%	1	0.00%	0.01%
Foreign Languages	51193	0	0.00%	0.00%	83	0.16%	0.46%
Family & Consumer Sciences	3010	0	0.00%	0.00%	0	0.00%	0.00%
Legal Professions & Studies	7047	552	7.83%	0.47%	0	0.00%	0.00%
English Language & Literature	272461	17078	6.27%	14.42%	757	0.28%	4.16%
Liberal Arts, General Studies, & Humanities	58478	3565	6.10%	3.01%	15	0.03%	0.08%
Library Science	587	235	40.03%	0.20%	24	4.09%	0.13%
Biological & Biomedical Sciences	105274	3543	3.37%	2.99%	943	0.90%	5.18%
Mathematics & Statistics	258308	7718	2.99%	6.52%	2682	1.04%	14.74%
Military Science	14	0	0.00%	0.00%	0	0.00%	0.00%
Multi/Interdisciplinary Studies	13117	0	0.00%	0.00%	78	0.59%	0.43%
Parks, Recreation, Leisure, & Fitness Studies	102761	5062	4.93%	4.28%	1135	1.10%	6.24%
Basic Skills	46587	220	0.47%	0.19%	52	0.11%	0.29%
Philosophy & Religious Studies	41292	1589	3.85%	1.34%	9	0.02%	0.05%
Physical Sciences	71082	601	0.85%	0.51%	765	1.08%	4.20%
Science Technologies/Technicians	743	0	0.00%	0.00%	0	0.00%	0.00%
Psychology	135132	8154	6.03%	6.89%	985	0.73%	5.41%
Protective Services	36185	1070	2.96%	0.90%	326	0.90%	1.79%
Public Administration & Social Service	10346	1725	16.67%	1.46%	0	0.00%	0.00%
Social Sciences, Others	7464	309	4.14%	0.26%	0	0.00%	0.00%
Anthropology	15349	1025	6.68%	0.87%	56	0.36%	0.31%
Economics	46309	3355	7.24%	2.83%	55	0.12%	0.30%
Geography	12456	197	1.58%	0.17%	0	0.00%	0.00%
Political Science & Government	55266	1929	3.49%	1.63%	440	0.80%	2.42%
Sociology	50445	2996	5.94%	2.53%	739	1.46%	4.06%
Construction	16882	818	4.85%	0.69%	4	0.02%	0.02%
Mechanic & Repair Technologies/Technicians	33762	63	0.19%	0.05%	18	0.05%	0.10%
Precision Production	12537	0	0.00%	0.00%	0	0.00%	0.00%
Transportation	930	0	0.00%	0.00%	59	6.34%	0.32%
Visual & Performing Arts	116482	924	0.79%	0.78%	162	0.14%	0.89%
Health Professions	130776	14207	10.86%	12.00%	4302	3.29%	23.65%
Business, Management & Marketing	167216	20685	12.37%	17.47%	1776	1.06%	9.76%
History	72092	4224	5.86%	3.57%	0	0.00%	0.00%
Course Level	**Total Enrollment**	**Total Online Enrollment**	**Percent of Level Enrollment Online**	**Percent of Online Enrollment**	**Total Hyrid Enrollment**	**Percent of Level Enrollment Hybrid**	**Percent of Hybrid Enrollment**
Remedial	178961	640	0.36%	0.54%	407	0.23%	2.24%
Pre-college (<100), non-remedial	18241	254	1.39%	0.21%	14	0.08%	0.08%
Introductory (100s)	1506152	85396	5.67%	72.12%	12969	0.86%	71.29%
Advanced (200s +)	499791	32117	6.43%	27.12%	4803	0.96%	26.40%
Total	2203145	118407	5.37%	100.00%	18193	0.83%	100.00%

Notes: The Social Sciences category was disaggregated into Political Science and Government, Sociology, Economics, Anthropolgy, Geography, and Other. Total Enrollment is defined as the number of observations of any student enrolling in a course in a given CIP2 category or level. The CIP2 categories can be found at : http://nces.ed.gov/ipeds/cipcode. Courses are assigned to a CIP2 code based on their subject. Courses are assigned to a level based on their course number.

Table 15. Summary Statistics for Courses, State B

	State B						
	1	2	3	4	5	6	7
CIP2	**Total Enrollment**	**Total Online Enrollment**	**Percent of CIP2 Enrollment Online**	**Percent of Online Enrollment**	**Total Hyrid Enrollment**	**Percent of CIP2 Enrollment Hybrid**	**Percent of Hybrid Enrollment**
Agriculture	47262	1783	3.77%	0.17%	1397	2.96%	0.50%
Natural Resources & Conservation	8686	1011	11.64%	0.09%	251	2.89%	0.09%
Architecture	23271	808	3.47%	0.08%	326	1.40%	0.12%
Communication & Journalism	174297	16395	9.41%	1.54%	7439	4.27%	2.68%
Communications Technology	2243	0	0.00%	0.00%	8	0.36%	0.00%
Computer & Information Sciences	485862	131746	27.12%	12.33%	33534	6.90%	12.10%
Personal & Culinary Services	181039	3546	1.96%	0.33%	1025	0.57%	0.37%
Education	332838	63655	19.12%	5.96%	22818	6.86%	8.23%
Engineering	99731	2984	2.99%	0.28%	4562	4.57%	1.65%
Engineering Technologies	75737	6570	8.67%	0.62%	1923	2.54%	0.69%
Foreign Languages	208227	19238	9.24%	1.80%	12005	5.77%	4.33%
Family & Consumer Sciences	1	0	0.00%	0.00%	0	0.00%	0.00%
Legal Professions & Studies	34307	8244	24.03%	0.77%	3189	9.30%	1.15%
English Language & Literature	1160399	123223	10.62%	11.54%	29809	2.57%	10.75%
Liberal Arts, General Studies, & Humanities	127401	30303	23.79%	2.84%	3795	2.98%	1.37%
Library Science	299	299	100.00%	0.03%	0	0.00%	0.00%
Biological & Biomedical Sciences	323903	21593	6.67%	2.02%	12314	3.80%	4.44%
Mathematics & Statistics	886978	59567	6.72%	5.58%	25103	2.83%	9.06%
Military Science	79	0	0.00%	0.00%	0	0.00%	0.00%
Multi/Interdisciplinary Studies	6559	237	3.61%	0.02%	169	2.58%	0.06%
Parks, Recreation, Leisure, & Fitness Studies	162909	17651	10.83%	1.65%	3433	2.11%	1.24%
Basic Skills	254354	39941	15.70%	3.74%	7448	2.93%	2.69%
Philosophy & Religious Studies	118554	20842	17.58%	1.95%	2438	2.06%	0.88%
Physical Sciences	175098	12581	7.19%	1.18%	6099	3.48%	2.20%
Science Technologies/Technicians	445	2	0.45%	0.00%	0	0.00%	0.00%
Psychology	422697	75775	17.93%	7.09%	7616	1.80%	2.75%
Protective Services	233105	44934	19.28%	4.21%	8138	3.49%	2.94%
Public Administration & Social Service	30657	3590	11.71%	0.34%	2920	9.52%	1.05%
Social Sciences, Others	1347	691	51.30%	0.06%	110	8.17%	0.04%
Anthropology	17322	4676	26.99%	0.44%	222	1.28%	0.08%
Economics	76205	21036	27.60%	1.97%	2241	2.94%	0.81%
Geography	14789	3873	26.19%	0.36%	594	4.02%	0.21%
Political Science & Government	42472	9701	22.84%	0.91%	418	0.98%	0.15%
Sociology	208520	42975	20.61%	4.02%	3789	1.82%	1.37%
Construction	41323	573	1.39%	0.05%	641	1.55%	0.23%
Mechanic & Repair Technologies/Technicians	245737	1617	0.66%	0.15%	3030	1.23%	1.09%
Precision Production	94010	301	0.32%	0.03%	594	0.63%	0.21%
Transportation	11029	552	5.00%	0.05%	0	0.00%	0.00%
Visual & Performing Arts	315224	47256	14.99%	4.42%	8431	2.67%	3.04%
Health Professions	363236	37579	10.35%	3.52%	24421	6.72%	8.81%
Business, Management & Marketing	600025	150564	25.09%	14.10%	31392	5.23%	11.32%
History	266103	40165	15.09%	3.76%	3584	1.35%	1.29%
Course Level	**Total Enrollment**	**Total Online Enrollment**	**Percent of Level Enrollment Online**	**Percent of Online Enrollment**	**Total Hyrid Enrollment**	**Percent of Level Enrollment Hybrid**	**Percent of Hybrid Enrollment**
Remedial	928104	39469	4.25%	3.70%	23801	2.56%	8.59%
Pre-college (<100), non-remedial	79536	3053	3.84%	0.29%	1809	2.27%	0.65%
Introductory (100s)	5562106	799966	14.38%	74.90%	194400	3.50%	70.12%
Advanced (200s +)	1304534	225589	17.29%	21.12%	57216	4.39%	20.64%
Total	7874280	1068077	12.93%	100.00%	277226	3.48%	100.00%

Notes: The Social Sciences category was disaggregated into Political Science and Government, Sociology, Economics, Anthropolgy, Geography, and Other. Total Enrollment is defined as the number of observations of any student enrolling in a course in a given CIP2 category or level. The CIP2 categories can be found at : http://nces.ed.gov/ipeds/cipcode. Courses are assigned to a CIP2 code based on their subject. Courses are assigned to a level based on their course number.

Table 16. OLS and Fixed Effects Estimates, State A
Dependent Variable is Pass Class (0/1); Mean of Dependent Variable for Face-to-face courses: 0.757

	OLS Estimates				**Fixed Effects Estimates**			
					Course Fixed Effects	**Course*Term Fixed Effects**	**Student Fixed Effects**	**Student*Term Fixed Effects**
	1	2	3	4	5	6	7	8
Online	-0.0435***	-0.0664***	-0.0949***	-0.0957***	-0.0933***	-0.0952***	-0.107***	-0.0974***
	(0.00135)	(0.00137)	(0.00134)	(0.00134)	(0.00624)	(0.00277)	(0.00196)	(0.00246)
Hybrid	-0.00388	-0.0157***	-0.0418***	-0.0442***	-0.0548***	-0.0654***	-0.0519***	-0.0526***
	(0.00321)	(0.00319)	(0.00310)	(0.00309)	(0.0106)	(0.00796)	(0.00337)	(0.00424)
Male			-0.0446***	-0.0451***	-0.0452***	-0.0446***		
			(0.000576)	(0.000605)	(0.00173)	(0.000779)		
Black			-0.145***	-0.144***	-0.140***	-0.139***		
			(0.000966)	(0.000961)	(0.00261)	(0.00134)		
Hispanic			-0.0274***	-0.0268***	-0.0258***	-0.0249***		
			(0.00192)	(0.00191)	(0.00220)	(0.00198)		
Age			0.00336***	0.00256***	0.00219***	0.00226***	0.0291***	
			(3.36e-05)	(3.48e-05)	(0.000107)	(5.06e-05)	(0.000813)	
Full Time Term			-0.0273***	-0.0283***	-0.0258***	-0.0233***	0.00390**	
			(0.000980)	(0.000993)	(0.00218)	(0.00128)	(0.00153)	
Half Time Term			-0.0439***	-0.0446***	-0.0422***	-0.0407***	-0.00121	
			(0.000949)	(0.000962)	(0.00191)	(0.00116)	(0.00133)	
Working (0/1)			-0.0218***	-0.0206***	-0.0196***	-0.0199***	-0.0272***	
			(0.000627)	(0.000626)	(0.00110)	(0.000706)	(0.00149)	
Remedial in First Term			-0.0662***	-0.0529***	-0.0489***	-0.0487***		
			(0.000653)	(0.000686)	(0.00233)	(0.000928)		
Receiving Any Fin. Aid			-0.0117***	-0.0118***	-0.0101***	-0.00898***	0.00824***	
			(0.000611)	(0.000607)	(0.00166)	(0.00101)	(0.00170)	
0-14 Completed Credits, Not First Term			-0.0136***	-0.0187***	-0.0219***	-0.0192***	-0.0845***	
			(0.000878)	(0.000877)	(0.00226)	(0.00129)	(0.00111)	
15-29 Completed Credits, Not First Term			0.0844***	0.0769***	0.0712***	0.0736***	-0.152***	
			(0.00101)	(0.00101)	(0.00297)	(0.00147)	(0.00148)	
30-44 Completed Credits, Not First Term			0.141***	0.131***	0.123***	0.125***	-0.185***	
			(0.00120)	(0.00121)	(0.00326)	(0.00173)	(0.00192)	
45 Plus Completed Credits, Not First Term			0.199***	0.185***	0.172***	0.175***	-0.227***	
			(0.00137)	(0.00139)	(0.00372)	(0.00197)	(0.00249)	
Course Level Remedial				-0.0557***			0.0528***	0.0384***
				(0.00160)			(0.00170)	(0.00219)
Course Level Pre-College				-0.0915***			0.0395***	0.0327***
				(0.00347)			(0.00383)	(0.00483)
Course Level Intro				-0.0341***			-0.0103***	-0.00894***
				(0.000707)			(0.000740)	(0.000991)
Term Dummies		X	X	X	X		X	
School Dummies		X	X	X				
CIP2 Dummies				X			X	X
Observations	2,203,145	2,203,145	2,203,145	2,203,145	2,203,145	2,203,145	2,203,145	2,203,145
R-squared	0.001	0.009	0.055	0.070	0.093	0.113	0.426	0.673
# of Fixed Effects					4683	43215	265296	844482

Notes: Robust standard errors are in parentheses. For fixed effects regressions in columns 5-8, standard errors are clustered at the level of the fixed effect. Course fixed effects are actually Course*School FE. White is the omitted race category. Coefficients on other race and missing race are suppressed. Course level advanced is the omitted level category. Less than half time term is the omitted enrollment intensity category. "First Term" student is the omitted category for number of cumulative credits completed. Sample is described in the notes of Table 12.

Table 17. OLS and Fixed Effects Estimates, State B
Dependent Variable is Pass Class (0/1); Mean of Dependent Variable for Face-to-face courses: 0.789

	OLS Estimates				Fixed Effects Estimates			
					Course Fixed Effects	Course*Term Fixed Effects	Student Fixed Effects	Student*Term Fixed Effects
	1	2	3	4	5	6	7	8
Online	-0.0910***	-0.0918***	-0.106***	-0.109***	-0.114***	-0.115***	-0.134***	-0.125***
	(0.000472)	(0.000483)	(0.000472)	(0.000481)	(0.00184)	(0.00108)	(0.000701)	(0.000790)
Hybrid	-0.0363***	-0.0303***	-0.0434***	-0.0477***	-0.0526***	-0.0683***	-0.0551***	-0.0563***
	(0.000835)	(0.000850)	(0.000816)	(0.000815)	(0.00257)	(0.00247)	(0.000919)	(0.00110)
Male			-0.0461***	-0.0522***	-0.0513***	-0.0512***		
			(0.000299)	(0.000328)	(0.000809)	(0.000431)		
Black			-0.0724***	-0.0720***	-0.0742***	-0.0743***		
			(0.000390)	(0.000391)	(0.000917)	(0.000494)		
Hispanic			0.00858***	0.0114***	0.0103***	0.0109***		
			(0.000761)	(0.000758)	(0.00102)	(0.000855)		
Age			0.00383***	0.00338***	0.00354***	0.00367***	0.0344***	
			(1.42e-05)	(1.45e-05)	(4.16e-05)	(2.11e-05)	(0.000461)	
Full Time Term			0.139***	0.144***	0.156***	0.169***	0.162***	
			(0.000510)	(0.000512)	(0.00180)	(0.000921)	(0.000876)	
Half Time Term			-0.00589***	-0.00143***	0.00374***	0.00985***	0.0481***	
			(0.000503)	(0.000504)	(0.00136)	(0.000800)	(0.000749)	
Working (0/1)			-0.0306***	-0.0278***	-0.0250***	-0.0234***	-0.0403***	
			(0.000303)	(0.000301)	(0.000602)	(0.000368)	(0.000761)	
Remedial in First Term			-0.0861***	-0.0616***	-0.0586***	-0.0555***		
			(0.000317)	(0.000330)	(0.000862)	(0.000432)		
Receiving Any Fin. Aid			-0.0429***	-0.0404***	-0.0370***	-0.0333***	-0.0104***	
			(0.000325)	(0.000323)	(0.000723)	(0.000424)	(0.000857)	
0-14 Completed Credits, No			-0.0759***	-0.0788***	-0.0782***	-0.0794***	-0.106***	
			(0.000435)	(0.000435)	(0.00155)	(0.000692)	(0.000602)	
15-29 Completed Credits, N			-0.00911***	-0.0216***	-0.0225***	-0.0240***	-0.155***	
			(0.000499)	(0.000505)	(0.00175)	(0.000779)	(0.000785)	
30-44 Completed Credits, N			0.0282***	0.00787***	0.00435**	0.00226**	-0.178***	
			(0.000581)	(0.000592)	(0.00196)	(0.000897)	(0.000992)	
45 Plus Completed Credits,]			0.0751***	0.0464***	0.0391***	0.0360***	-0.195***	
			(0.000636)	(0.000658)	(0.00212)	(0.000991)	(0.00124)	
Course Level Remedial				-0.0766***			0.00648***	0.00108
				(0.000769)			(0.000972)	(0.00112)
Course Level Pre-College				-0.0959***			-0.0528***	-0.0592***
				(0.00164)			(0.00186)	(0.00232)
Course Level Intro				-0.0266***			-0.00967***	-0.00709***
				(0.000418)			(0.000439)	(0.000536)
Term Dummies		X	X	X	X		X	
School Dummies		X	X	X				
CIP2 Dummies				X			X	X
Observations	7,874,280	7,874,280	7,874,280	7,874,280	7,874,280	7,874,280	7,874,280	7,874,280
R-squared	0.006	0.019	0.088	0.102	0.141	0.200	0.404	0.677
# of Fixed Effects					37332	325706	763437	2.685e+06

Notes: Robust standard errors are in parentheses. For fixed effects regressions in columns 5-8, standard errors are clustered at the level of the fixed effect. Course fixed effects are actually Course*School FE. White is the omitted race category. Coefficient on other race is suppressed. Course level advanced is the omitted level category. Less than half time term is the omitted enrollment intensity category. "First Term" student is the omitted category for number of cumulative credits completed. Sample is described in the notes of Table 13.

Table 18. IV Estimates, State A Restricted Sample
Dependent Variable is Pass Class (0/1)

	1	2	3	4
	Course Fixed Effects	**First Stage for Online**	**First Stage for Hybrid**	**2SLS**
Online	-0.105***			-0.0909***
	(0.0110)			(0.0129)
Hybrid	-0.0545***			-0.0427***
	(0.0111)			(0.0160)
Share of Seats Offered Online		0.912***	-0.00353	
		(0.0133)	(0.00492)	
Share of Seats Offered Hybrid		0.000149	0.936***	
		(0.00931)	(0.0168)	
Observations	676,502	676,502	676,502	676,428
F-test of Instruments		4711	3095	2431
P-value		0	0	0
Number of Fixed Effects	1,844	1,844	1,844	1,770

Notes: Robust standard errors are in parentheses. Standard errors are clustered at the level of the fixed effect. Course fixed effects are actually Course*School FE. The sample is described in the notes of Table 12, but, for the IV specifications, the sample is restricted to two schools that provided course capacity data and to the courses for which capacity data was complete. The controls in these regressions are identical to those included in the course fixed effects specification in Column 5 of Table 16.

Table 19. OLS and Fixed Effects Estimates, State A

Dependent Variable is Complete Course (0/1) or Pass Conditional on Completion (0/1)

	1	2	3	4	5
Panel A			**Outcome: Complete Course**		
Mean of Dependent Variable for Face-to-face courses: 0.847					
	Rich OLS	Course FE	Course*Term FE	Student FE	Student*Term FE
Online	-0.0539***	-0.0523***	-0.0529***	-0.0568***	-0.0547***
	(0.00111)	(0.00431)	(0.00216)	(0.00166)	(0.00228)
Hybrid	-0.0207***	-0.0289***	-0.0379***	-0.0280***	-0.0316***
	(0.00254)	(0.00781)	(0.00594)	(0.00294)	(0.00388)
Observations	2,203,145	2,203,145	2,203,145	2,203,145	2,203,145
R-squared	0.047	0.065	0.085	0.327	0.596
# of Fixed Effects		4683	43215	265296	844482
Panel B			**Outcome: Pass Conditional on Complete**		
Mean of Dependent Variable for Face-to-face courses: 0.894					
	Rich OLS	Course FE	Course*Term FE	Student FE	Student*Term FE
Online	-0.0573***	-0.0574***	-0.0591***	-0.0693***	-0.0581***
	(0.00115)	(0.00507)	(0.00229)	(0.00165)	(0.00206)
Hybrid	-0.0310***	-0.0353***	-0.0396***	-0.0340***	-0.0280***
	(0.00258)	(0.00821)	(0.00627)	(0.00280)	(0.00367)
Observations	1,864,749	1,864,749	1,864,749	1,864,749	1,864,749
R-squared	0.054	0.076	0.099	0.438	0.701
# of Fixed Effects		4658	42895	252220	783539

Notes: Robust standard errors in parentheses, clustered at the level of the fixed effect. The sample is described in the notes to Table 12. The controls in these regressions are identical to those included in corresponding estimates from Table 16.

Table 20. OLS and Fixed Effects Estimates, State B
Dependent Variable is Complete Course (0/1) or Pass Conditional on Completion (0/1)

	1	2	3	4	5
Panel A			**Outcome: Complete Course**		
Mean of Dependent Variable for Face-to-face courses: 0.885					
	Rich OLS	Course FE	Course*Term FE	Student FE	Student*Term FE
Online	-0.0498***	-0.0526***	-0.0552***	-0.0642***	-0.0641***
	(0.000385)	(0.00160)	(0.000919)	(0.000570)	(0.000704)
Hybrid	-0.0192***	-0.0253***	-0.0334***	-0.0229***	-0.0247***
	(0.000639)	(0.00204)	(0.00194)	(0.000760)	(0.000973)
Observations	7,874,280	7,874,280	7,874,280	7,874,280	7,874,280
R-squared	0.093	0.121	0.176	0.315	0.606
# of Fixed Effects		37332	325706	763437	2.685e+06
Panel B			**Outcome: Pass Conditional on Complete**		
Mean of Dependent Variable for Face-to-face courses: 0.892					
	Rich OLS	Course FE	Course*Term FE	Student FE	Student*Term FE
Online	-0.0775***	-0.0808***	-0.0797***	-0.0935***	-0.0798***
	(0.000429)	(0.00156)	(0.000962)	(0.000619)	(0.000704)
Hybrid	-0.0354***	-0.0355***	-0.0465***	-0.0417***	-0.0394***
	(0.000708)	(0.00235)	(0.00215)	(0.000797)	(0.000977)
Observations	6,914,428	6,914,428	6,914,428	6,914,428	6,914,428
R-squared	0.057	0.111	0.184	0.389	0.682
# of Fixed Effects		37296	324836	761725	2.551e+06

Notes: Robust standard errors in parentheses, clustered at the level of the fixed effect. The sample is described in the notes to Table 13. The controls in these regressions are identical to those included in corresponding estimates from Table 17.

Table 21. Heterogeneity in Student Characteristics
Specification is Student FE; Dependent variable is Pass Class (0/1)

Panel A — **Online Coefficient**

	State A			State A	State B			State B
				Significant Difference Between Groups				Significant Difference Between Groups
Gender	*Male*	*Female*			*Male*	*Female*		
	-0.129***	-0.0957***		Y	-0.157***	-0.123***		Y
	(0.00354)	(0.00235)			(0.00129)	(0.000832)		
Age	*Age < 20 at start*	*Age >=20 & <30 at start*	*Age >=30 at start*		*Age < 20 at start*	*Age >=20 & <30 at start*	*Age >=30 at start*	
	-0.127***	-0.0947***	-0.0773***	Y,Y	-0.159***	-0.137***	-0.0867***	Y,Y
	(0.00292)	(0.00348)	(0.00398)		(0.00108)	(0.00131)	(0.00124)	
Remedial Status	*Non Remedial*	*Remedial*			*Non Remedial*	*Remedial*		
	-0.0975***	-0.134***		Y	-0.130***	-0.138***		Y
	(0.00222)	(0.00417)			(0.000881)	(0.00115)		
Employment Status	*Not Working*	*Working*			*Not Working*	*Working*		
	-0.0989***	-0.108***		N	-0.103***	-0.115***		Y
	(0.00365)	(0.00234)			(0.00115)	(0.000881)		
School Experience	*First Term*	*Not First Term*			*First Term*	*Not First Term*		
	-0.112***	-0.105***		N	-0.156***	-0.130***		Y
	(0.00635)	(0.00213)			(0.00214)	(0.000758)		
Online Experience	*Prior Online Experience*	*No Prior Online Experience*			*Prior Online Experience*	*No Prior Online Experience*		
	-0.0809***	-0.123***		Y	-0.111***	-0.155***		Y
	(0.00312)	(0.00289)			(0.000973)	(0.00115)		
Hybrid Experience	*Prior Hybrid Experience*	*No Prior Hybrid Experience*			*Prior Hybrid Experience*	*No Prior Hybrid Experience*		
	-0.0937***	-0.105***		N	-0.116***	-0.133***		Y
	(0.00742)	(0.00223)			(0.00158)	(0.000859)		

Panel B — **Hybrid Coefficient**

	State A			State A	State B			State B
				Significant Difference Between Groups				Significant Difference Between Groups
Gender	*Male*	*Female*			*Male*	*Female*		
	-0.0713***	-0.0396***		Y	-0.0693***	-0.0474***		Y
	(0.00574)	(0.00411)			(0.00163)	(0.00111)		
Age	*Age < 20 at start*	*Age >=20 & <30 at start*	*Age >=30 at start*		*Age < 20 at start*	*Age >=20 & <30 at start*	*Age >=30 at start*	
	-0.0654***	-0.0418***	-0.0293***	Y,N	-0.0646***	-0.0610***	-0.0295***	N,Y
	(0.00487)	(0.00620)	(0.00690)		(0.00138)	(0.00183)	(0.00159)	
Remedial Status	*Non Remedial*	*Remedial*			*Non Remedial*	*Remedial*		
	-0.0480***	-0.0607***		N	-0.0541***	-0.0561***		N
	(0.00394)	(0.00637)			(0.00118)	(0.00146)		
Employment Status	*Not Working*	*Working*			*Not Working*	*Working*		
	-0.0438***	-0.0544***		N	-0.0426***	-0.0519***		Y
	(0.00590)	(0.00414)			(0.00153)	(0.00124)		
School Experience	*First Term*	*Not First Term*			*First Term*	*Not First Term*		
	-0.0719***	-0.0456***		Y	-0.0698***	-0.0530***		Y
	(0.00933)	(0.00375)			(0.00281)	(0.000998)		
Online Experience	*Prior Online Experience*	*No Prior Online Experience*			*Prior Online Experience*	*No Prior Online Experience*		
	-0.0282***	-0.0529***		Y	-0.0427***	-0.0617***		Y
	(0.00677)	(0.00450)			(0.00135)	(0.00150)		
Hybrid Experience	*Prior Hybrid Experience*	*No Prior Hybrid Experience*			*Prior Hybrid Experience*	*No Prior Hybrid Experience*		
	-0.0284***	-0.0520***		N	-0.0339***	-0.0677***		Y
	(0.00770)	(0.00434)			(0.00156)	(0.00132)		

Notes: To assess heterogeneity, separate regressions were run for each separate subsample (listed in italics). The coefficients on "Online" and "Hybrid" from these regressions are presented in panels A and B, respectively. Robust standard errors in parentheses, clustered at the level of the fixed effect. The samples are described in the notes to Tables 12 and 13. Remedial students are those who were observed enrolled in a remedial class in their first term. The samples used in the prior online experience specifications are restricted to students observed in 2nd or later terms of enrollment.The controls in these regressions are identical to those included in corresponding fixed effects estimates from Tables 16 and 17. Differences between groups were tested using an overlapping confidence interval test. For the test of differences in coefficients between age groups, the first Y/N applies to the test of Age<20 with Age>=20 &<30 while the second Y/N applies to the test of Age>=20 &<30 with Age>=30.

Table 22. Heterogeneity in Course Characteristics
Specification is Student FE; Dependent variable is Pass Class (0/1)

Panel A	Online Coefficient													
	State A							**State B**						
							Significant Difference Between Groups							Significant Difference Between Groups
Course Level	*Remedial and Pre-College*	*Introductory*	*Advanced*					*Remedial and Pre-College*	*Introductory*	*Advanced*				
	-0.206***	-0.102***	-0.112***				Y,N	-0.207***	-0.134***	-0.118***				Y,Y
	(0.0332)	(0.00235)	(0.00404)					(0.00431)	(0.000792)	(0.00168)				
Subject	*Math & Statistics (CIP2 ==27)*	*English Language & Literature (CIP==23)*	*Business, Management & Marketing (CIP2 == 52)*	*Computer & Information Sciences (CIP2 == 11)*	*Health Professions (CIP2 ==51)*	*Psychology (CIP2 == 42)*		*Math & Statistics (CIP2 ==27)*	*English Language & Literature (CIP==23)*	*Business, Management & Marketing (CIP2 == 52)*	*Computer & Information Sciences (CIP2 == 11)*	*Health Professions (CIP2 ==51)*	*Psychology (CIP2 == 42)*	
	-0.129***	-0.124***	-0.119***	-0.148***	-0.0586***	-0.0780***	--	-0.178***	-0.163***	-0.117***	-0.192***	-0.0227***	-0.146***	--
	(0.0130)	(0.00800)	(0.00650)	(0.0116)	(0.00620)	(0.0179)		(0.00411)	(0.00278)	(0.00225)	(0.00457)	(0.00345)	(0.00630)	

Panel A	Hybrid Coefficient													
	State A							**State B**						
							Significant Difference Between Groups							Significant Difference Between Groups
Course Level	*Remedial and Pre-College*	*Introductory*	*Advanced*					*Remedial and Pre-College*	*Introductory*	*Advanced*				
	-0.220***	-0.0435***	-0.0470***				Y,N	-0.0718***	-0.0579***	-0.0395***				Y,Y
	(0.0467)	(0.00414)	(0.00725)					(0.00463)	(0.00112)	(0.00212)				
Subject	*Math & Statistics (CIP2 ==27)*	*English Language & Literature (CIP==23)*	*Business, Management & Marketing (CIP2 == 52)*	*Computer & Information Sciences (CIP2 == 11)*	*Health Professions (CIP2 ==51)*	*Psychology (CIP2 == 42)*		*Math & Statistics (CIP2 ==27)*	*English Language & Literature (CIP==23)*	*Business, Management & Marketing (CIP2 == 52)*	*Computer & Information Sciences (CIP2 == 11)*	*Health Professions (CIP2 ==51)*	*Psychology (CIP2 == 42)*	
	-0.0478***	-0.118***	-0.0493***	-0.0127	-0.0160***	-0.0211	--	-0.0888***	-0.0524***	-0.0575***	-0.0718***	-0.0114***	-0.0935***	--
	(0.0181)	(0.0295)	(0.0143)	(0.0313)	(0.00616)	(0.0423)		(0.00638)	(0.00459)	(0.00296)	(0.00628)	(0.00294)	(0.0151)	

Notes: To assess heterogeneity, separate regressions were run for each separate subsample (listed in italics). The coefficients on "Online" and "Hybrid" from these regressions are presented in panels A and B, respectively. Robust standard errors in parentheses, clustered at the level of the fixed effect. The samples are described in the notes to Tables 12 & 13. Remedial and Pre-College level courses are combined into one category. The CIP2 subsamples represent 6 of the top 10 subject areas of study in both samples. The controls in these regressions are identical to those included in corresponding fixed effects estimates from Tables 16 and 17. Differences between groups were tested using an overlapping confidence interval test. For the test of differences in coefficients between course levels, the first Y/N applies to the test of Remedial and Pre-College with Introductory while the second Y/N applies to the test of Introductory with Advanced. Tests were not conducted for the subject categories.

Chapter 3. The Expansion of Online Education and the Composition of Community-College Enrollees

Abstract

This study looks at the effect of expanding online offerings on the composition of enrollees in community-college courses. A course-fixed effects analysis is employed to generate estimates of the effect of offering any online seats and the percent of seats offered in the online format on the percent of enrollees in courses that have the following characteristics: working, adult, female, remedial, and financial aid recipient. I find that expanding online offerings significantly increases the percent of enrollees that are working, adult, and financial aid recipients, with the largest effects being found for working adults. I attempt to disentangle the extent to which the changing composition of students is due to new enrollments versus shifting enrollments and find that shifting enrollment does not appear to be driving the results.

1 Introduction

Since the late 1800s, many educational innovations have expanded access to post-secondary education. For example, the Morrill Land-Grant Act of 1862 brought a rapid increase in the availability of public universities that serviced the broader community by offering a wider range of programs, including agriculture and mechanics (Goldin, 1999). Furthermore, junior colleges, increasingly referred to as community colleges, grew rapidly during the 1970s to meet the vocational needs of local communities (Cohen, 2003). More recently, distance education has evolved over time to expand access to those who would be without due to geographic and socio-economic factors. Initially distance education was conducted via mail correspondence, then progressed through radio, television, and email (Casey, 2008). Online education, the newest manifestation of distance education, is the most recent educational innovation geared at at expanding access to higher education.

This recent innovation of online education in college is expanding dramatically. In a survey of over 2,800 colleges and universities, more than 6.7 million students attempted an online course in the fall of 2011 (Allen and Seaman, 2013). This represents a 9.3% increase over the number reported in the fall of 2010 and a 319% increase over the number reported in 2002. Community colleges, in particular, are leaders in offering online education. In 2007, 97% of public, two-year institutions offered online courses, more than any other sector of higher education (Parsad and Lewis, 2008).[1]

The most popular reasons colleges offer distance education, which, of late, is primarily composed of online education, are (i) to meet student demand for flexible schedules and (ii) to provide access to college for students who would otherwise be without because of family, work-related, or geographic reasons (Parsad and Lewis, 2008). These reasons may resonate especially strongly in the community-college sector, where colleges are viewed as an "access point" for populations that might otherwise be poorly served by traditional post-secondary institutions (e.g non-traditional students)(AACC, 2012).[2] The expansion of online offerings among community colleges may help them to further their mission of access by providing a flexible educational opportunity for these students.

Existing research does not shed light on whether online offerings have in fact affected the enrollment decisions of those who may be poorly served by traditional post-secondary institutions. In this paper, I look at how the composition of students enrolling in community-college classes changes as courses expand their online offerings paying particular attention to populations that community colleges may be especially interested in serving such as older, working individuals. I use administrative data from two community colleges in a midwestern state and employ a course-fixed-effects estimation strategy that allows me to look at within-course changes in the composition

[1] 86% of four-year public institutions offered online courses.

[2] http://www.aacc.nche.edu/AboutCC/Trends/Pages/studentsatcommunitycolleges.aspx

of enrolled students that are associated with changes in online offerings. I focus on several populations of interest–adults, working students, females, remedial students, and financial aid recipients. I find that expanding online offerings significantly increases the percent of enrollees that are working, adult, and financial aid recipients, with the largest effects being found for working adults.

These reduced-form estimates capture changes in enrollment composition due to new enrollments (i.e. an older, working student choosing to enroll in course a when they otherwise would not have enrolled in any course) and changes in enrollment composition due to shifting enrollments (i.e. an older working student choosing to take course A instead of course B because course A is offered online). I attempt to assess how important the shifting margin is by comparing the effect of online offerings between more and less substitutable courses. The analysis suggests that shifting of enrollment is not driving these findings.

All in all, my findings show that expanding online education in community colleges is affecting the enrollment decisions of students. Online offerings have a particularly large effect on the enrollment decisions of working adults – a population that community colleges are driven to serve because they may be poorly served by traditional post-secondary institutions. Expanding online offerings may in fact be helping community colleges fulfill their mission.

2 Related Research

Prior research shows, descriptively, that students who enroll in online courses are different from those who enroll in face-to-face courses. Online students are more likely to be older, female, have dependents, and have larger work responsibilities (Halsne and Gatta, 2002; Coates et al., 2004; Carpenter, Brown, and Hickman, 2004; Jaggars and Xu, 2010; Jaggars and Xu, 2011; Xu and Jaggars, 2011; Xu and Jaggars, 2013). It could be, however, that the types of courses offered online are more preferable or appropriate for these groups of students. These patterns alone do not tell us whether offering the online format is responsible for the higher rates of participation among these groups.[3] Jaggars and Bailey (2010) note that a "primary assumption underpinning the increase in online course offerings is that they increase educational access...presumably for those who are traditionally underserved." Jaggars and Bailey explicitly focus on low-income and underprepared students and state that they know of no research which examines whether post-secondary enrollment of these students has grown because of rapidly expanding online education opportunities. I know of no studies looking at any populations that attempt to understand how online opportunities affect college enrollment, and my study is the first to attempt to tackle this question.

The implication of observing, for example, adult students enrolling in online courses is not necessarily that the online offering induced these adult students to enroll at all. It may be that, as online offerings expand, students sort themselves into the format that is most convenient. So, adult students may simply be shifting into courses that are offered online even though would have enrolled in a face-to-face course had the online option not been available. The "democratization or diversion" literature discusses a similar phenomenon in attempt to understand the effect of students' two-year versus four-year college enrollment decisions (Rouse, 1995; Leigh and Gill, 2004; Mykerezi et al., 2009). Community colleges likely increase educational attainment by drawing students into college who otherwise would not have attended (the democratization effect), but they may divert some students from a four-year degree to a terminal two-year degree (the diversion effect). This concept can also be applied to the effect of online education within community colleges. Online education may draw students in to college who otherwise would not have enrolled (or into greater numbers of credits among students who would not have been able to participate as extensively) or it may divert students from traditional face-to-face courses within the college.

[3]Some of these descriptive statistics have been shown within specific courses. Carpenter, Brown, and Hickman (2004) look specifically at a developmental writing course in a Michigan community college and find that females, older students, and white students are represented in higher rates in the online sections. Xu and Jaggars (2011) look specifically at an introductory English course and an introductory math course in the Virginia community-college system and also find that females, older students, and white students are represented in higher rates in the online sections. This within-course evidence is much more suggestive of a causal relationship between the online offering and the composition of the enrolled students.

There is some evidence that online education is of lower quality in that students are less likely to complete and pass the course, score lower on exams, and earn lower grades (Figlio et al., 2013; Xu and Jaggars, 2011; Xu and Jaggars, 2013). Given this evidence we might worry that shifting behavior will have a negative effect on students' academic outcomes. On the other hand, my own ongoing research looks at the effect of online education on earnings and shows that long-run labor market outcomes for students who complete online courses are similar to (if not better than) their peers who complete face-to-face courses. These findings make us less concerned about shifting behavior.

My primary estimates combine these democratization and diversion effects, but supplemental analysis attempts to disentangle the two.

3 Background on Online Courses

There is substantial variety in online education. The phrase "online education" is used to describe all types of courses ranging from Massive Open Online Courses (large-scale, open-access courses offered on the internet) to web-enhanced courses in which the instructors of face-to-face courses simply post readings and assignments for students to retrieve on the course's website. The online education studied here falls in the middle. Online courses are meant to replicate their face-to-face counterparts, but the instruction is provided fully online.

Online courses are meant to replicate the face-to-face counterparts, but, in the case of the online courses, the instruction is provided fully online. The online and face-to-face courses cover the same material, and the online courses have the same number of students as their face-to-face counterparts (i.e. these are not MOOCs which can enroll tens of thousands of students, but instead, for example, Business 101 online has 25 students and one professor while Business 101 face-to-face has 25 students and one professor). The courses are hosted by a learning management system such as

Blackboard. The same professors can be found teaching online and face-to-face courses, and the tuition is the same across instructional formats.[4] That said, there is still a good deal of heterogeneity in the online courses studied here. For example, some of the fully online courses require in-person orientations and proctored exams and others do not. Unfortunately, I do not observe these characteristics in my data and cannot control for them. Most importantly, however, the online courses I study are intended to recreate, as closely as possible, the learning that would occur in a comparable face-to-face course but are fully online and inherently more flexible.

It is important to note that these schools also offer other flexible formats for their courses. Some sections of courses are offered in a hybrid format (also known as blended) where students receive part of their instruction online and part of their instruction face-to-face but have reduced in-class seat-time relative to fully face-to-face courses. Community colleges also offer evening and weekend courses to accommodate the schedules of their students.

My analysis attempts to assess whether changes in online offerings within courses over time cause changes in the composition of the enrolled students in these courses. One might imagine, however, that schools make several efforts to enhance flexibility for the students they attempt to serve and might be expanding hybrid, evening, and weekend offerings in tandem with expanding online offerings. I will control for the supply of seats in these alternative formats in my analysis to rule out that the composition of students is changing in response to changes in the offerings of these alternative formats.

4 Data and Methods

To answer a question about whether/how much expanded online offering induce students to enroll in college, one would ideally like to randomly assign community colleges to start offering online

[4] There is, however, an additional technology fee ranging from \$30 - \$100 associated with some online courses.

courses and then compare enrollment rates among the populations of interest in the schools' service areas. Unfortunately, such a randomization is not feasible and most community colleges are already offering online education.

Within schools, one could randomly assign certain classes to begin offering a portion of their seats in the online format. Even with this type of randomization, one would not be able to disentangle the effect of online offerings on new enrollments as opposed to shifting enrollments by simply comparing, for example, the percent of adults enrolled in the courses with online offerings to the percent of adults enrolled in courses without. Thus, the course-fixed effects analysis that I apply to existing data on online offerings and enrollment compositions is a good way to approximate the scenario in which one could randomize online offerings across courses. The course fixed effects restrict identification to be within courses thus controlling for observed and unobserved differences across courses that are fixed over time.

I use data from two community colleges in a Midwestern state for which I observe the full schedule of course offerings. The schedule data details which courses are offered in each term from the fall of 2003 through the winter of 2011. It contains information on each section of the course that is being offered including the number of seats available (capacity), the instructional format (face-to-face, online, or hybrid), and the times and days the sections meet (if the sections are not fully online). I use these data to track whether or not any seats in given courses are offered online as well as the percent of seats that are offered online–the key independent variables. I also use this data to track other changes in the supply of seats for each course such as whether any (or what percent of) seats in a course are offered in the hybrid format, during the evening, or during the weekend. These features also offer added flexibility to students and may be introduced at similar times as online seats. Without controlling for these changes in course offerings the estimates may suffer from omitted variables bias.

I pair the schedule data with administrative records that contain demographic (race, gender, and age), financial aid (receipt of Pell grants), and transcript information for students who enrolled during the fall of 2003 through the winter of 2011. I supplement these administrative records with unemployment insurance records that tell us whether or not students were working while they were enrolled. These data allow me to calculate the percent of enrollees in each class who have a variety of characteristics (such as the percent of enrollees that are working, 30 years or older, etc.)–the key dependent variables.

The schedule data is important because it tells us about the exogenous supply of seats offered in each course each term (including seats that are never filled and seats in sections of courses that are eventually cancelled).[5] Thus, I can calculate the percent of seats in course C that are offered in the online format. The data from the transcript records only allow us to calculate the percent of all enrollments in course C that are online (i.e. we do not observed unfilled/cancelled seats in the transcript data). Anecdotal evidence from community-college administrators suggests that online sections fill up first. If face-to-face sections are more likely to be cancelled or have empty seats, this will cause the percent of enrollments that are online in course C to be inflated relative to the percent of seats that are offered online in course C. The appendix offers an example of this (Appendix Table 1) and a bit of additional description. Among all courses in the analytic dataset, however, the average amount of inflation of the transcript-based measure is only 0.04 percentage points.

In light of the course-fixed-effects estimation strategy, which I discuss shortly, using the inflated measure will only bias the estimates if, within courses, the magnitude of the inflation varies systematically with the composition of the enrollees. I explicitly test whether using the potentially inflated measures of online offerings impacts the estimates by replicating the primary analysis using the inflated measures and comparing the results. If I find that measures of online "supply" from the transcript data produce similar estimates as the true measures of online supply from the sched-

[5] I use the term exogenous here to mean that it is the supply of seats that is planned and has not yet been interacted with demand in any given term. The supply may not be truly exogenous. Schools may try to offer more sections of courses for which they observed excess demand in prior terms. They may also try to offer more online seats in courses where the online sections filled up quickly in prior terms.

ule data, which I do, the analysis could be extended in future research to include other institutions for which schedule data is not available.[6]

The analytic dataset is a course-by-term-level panel that tracks a) whether any seats in course c are offered online in term t, b) the percent of seats in course c that are offered online in term t, and c) the percent of enrollees in course c that have certain characteristics. The characteristics of interest are age, employment status, gender, academic preparedness (proxied by remedial status), and financial need (proxied by receipt of Pell grants), as well as the interaction of age and employment status. Adult (30+) and working students who likely have numerous other commitments and responsibilities in life might not be served well by traditional college courses that require them to be on campus multiple times per week during the traditional work day. I would expect that the enhanced flexibility of online courses would attract them. Females are currently over-represented in higher education and have been shown to be over-represented in online courses. It is not clear, however, whether they are drawn to online courses because of the flexible instructional format or because of the type/content of courses that tend to be offered online. My analysis will help to disentangle this. Community colleges commonly serve underprepared students who need to complete remedial coursework before they are deemed prepared for college-level courses. Anecdotally, colleges tend to counsel underprepared students away from the online instructional format thus it will be interesting to note whether the expansion of online offerings does in fact result in smaller shares of remedial students observed in these courses.[7] Lastly, financially needy students may be enticed by the online format because of its flexibility and the reduced costs associated with commuting to campus on a regular basis, but may also find it challenging to equip themselves with the technology (e.g., computer and internet) that is necessary to complete the course. I will look at Pell Grant recipients as a proxy for financial need.[8] I employ this panel of data to estimate the

[6]In such future analyses, one would not be able to control for evening and weekend courses because that information is identified in the schedule data.

[7]Remedial students are designated in this analysis based on whether or not they enrolled in a remedial course in their first term.

[8]I do not know whether an individual is a parent. This would be another interested population to look at, but unfortunately I cannot identify parents in the data.

following course-fixed-effects model:

$$PercentChar_{ct} = \beta AnyOnline_{ct} + \gamma_c + \delta_t + \epsilon_{ct} \quad (1)$$

in which *PercentChar* measures the percent of people who enroll in the course who have certain characteristics (e.g. the percent of enrollees who are working, the percent of enrollees who are 30 or older, etc.). *AnyOnline* is an indicator for whether course c offered any online seats in term t. I also run models that use a continuous measure of the percent of seats offered online (*PercentOnline*) in course c in term t. γ is a vector of course fixed effects that capture fixed characteristics of courses such as field (science, math, humanities, etc.) and level (intro, advanced).[9] These fixed characteristics may be correlated with both the composition of enrolled students and the online offerings. δ is a vector of term fixed effects that capture any time-specific variation in the composition of enrolled students that may be confounded with changes in online offering.[10]

The estimates from this course-fixed-effects model can be interpreted as difference-in-difference estimates. In other words, they compare changes in the composition of enrollees in courses that begin to offer online opportunities (or change their offering of online opportunities) with changes in the composition of enrollees in courses that do not. This estimation strategy allays concerns about bias stemming from a) fixed characteristics of courses that are correlated with both whether/how much online options are offered in the course and the composition of the enrolled students (eg. humanities courses are more likely to be offered online and also attract a higher percent of females regardless of the format) and b) the expansion of online offerings happening along with a secular change in the composition of students (eg. online offerings expand in tandem with a secular growth in the percent of female enrollees across all courses).

[9] Note that the course fixed effects are actually course-by-school fixed effects because courses are unique to schools.

[10] I have also estimated models with course-by-season fixed effects (e.g., BUS101Fall, BUS101Winter, BUS101SpringSummer) and term fixed effects. These models produce somewhat smaller point estimates but the results are largely similar.

The only possible remaining sources of bias stem from factors that are changing at the same time as online offerings and are also correlated with changes in the composition of enrolled students within courses. If, for example, courses that start offering online sections also begin offering hybrid sections (both of which may increase scheduling flexibility for non-traditional students) at the same time, this may bias up the estimated effect of the expansion of online offerings if the expansion of hybrid offerings also affects the enrollment make up within the course. To address this potential bias, I also include controls for hybrid, evening, and weekend offerings.[11]

One concern about these estimates is that reverse causality is at play and that demand for online offerings might actually be leading to higher supply of online offerings. I address this in two ways. First, I include leads of the key independent variables (*AnyOnline* and *PercentOnline*) to make sure that, for example, the percent of course enrollees that are older and working is not increasing prior to the change in the supply of online seats. In other words, coefficients on lead measures of the variables should not be significant (e.g., $PercentOnline_{t+1}$, the percent of seats offered online *next* term, should not be significantly associated with the share of enrollees that are working adults this term). I also include lagged measures of these variables to see if there is a delay in enrollment changes among the populations of interest after changes in online offerings. I include two leads and two lags in each specification.

Second, I implement an event-study analysis that illustrates how the composition of enrollees in classes changes around the event in which a class is first offered online.[12] To implement this analysis, I restrict to the set of courses for which I observe the first term during which online seats were offered. I estimate the following equation:

$$PercentChar_{ct} = \sum_{j=-9}^{9} D_{ct}^{j}\beta_j + \gamma_c + \delta_t + \epsilon_{ct} \tag{2}$$

[11]Evening classes are defined as those that have a start-time of 5 pm or later.

[12]I define the term in which a class is first offered online as the term in which I first observe the class to offer any online seats as long as this term is not equal to the first term of the panel (i.e., 20037).

where *PercentChar* measures the percent of people who enroll in the course that have certain characteristics (e.g. the percent of enrollees who are working, the percent of enrollees who are 30 or older, etc.), γ is a vector of course fixed effects, and δ is a vector of term fixed effects. D^j_{ct} are a set of dummies, one for each term from eight terms before to eight terms after a course is first offered online, omitting the dummy for the term immediately prior to the course's first term online. For terms greater than eight terms prior, there is a single dummy that is set to one; for terms greater than eight terms after, there is a single dummy that is set to one.

There are 19 dummies in total, but the dummy representing the term immediately prior to initial online implementation is omitted. Together, the coefficients on the dummies map out differences in the shares of enrollees with certain characteristics relative to the term prior to initial online implementation. Thus, β_0 reflects the difference in the share of enrollees who are, for example, adults, in the first term of online implementation relative the term immediately proceeding this. β_1 reflects the difference in the share of enrollees who are, for example, adults, one term after initial online implementation relative to the term prior to initial implementation. The results of the event-study analysis will be presented in graphical form.

It is important to note that the parameter of interest here, β, contains the effect of expanded online offerings on whether, for example, working adults enroll in the course at all (the "democratization" effect) and the effect of expanded online offerings on, for example, working adults shifting between courses because one is offered online while another is not (the "diversion" effect). In attempt to understand how important these two effects are, I define a set of courses that I think should be highly "substitutable." To do this, I reviewed course catalogs for each institution and designated courses that are one of multiple options to fulfill a general education requirement as "substitutable." Approximately 15% of the course observations are deemed substitutable using this method. Then, I interact *AnyOnline* and *PercentOnline* with *Substitutable*. If the coefficient on this interaction is insignificant, this suggests that the effect of online offerings is similar between more and less substitutable courses and that online education induces additional enrollment rather than simply shifting enrollment. It is important to note that this test is merely suggestive. If, for example, the courses that I characterized as "substitutable" have smaller effects of online expansion for other reasons (potentially the subject matter or level of the course) then this would lead to a negative coefficient on the interaction of *Substitutable* with the measures of online offerings. It will be important for future research to explore other approaches to addressing the margins on which online offerings influence enrollment decisions.[13]

5 Summary Statistics

Table 23 displays summary statistics. The data employed in the analysis is a course-by-term-level panel spanning the fall of 2003 through the winter of 2010.[14] For simplicity's sake, summary statistics are only presented for the first six and last five terms observed in the data. In the fall of 2003, 9.3% of the 885 courses offered had at least one seat offered online. By the fall of 2010, nearly 14% of the 899 courses offered had at least one seat offered online. The average percent of seats offered online (including 0s for courses with no online offerings) increased from 4.86% to 6.75% over the same time frame. Hybrid offerings are also expanding over the time frame in the sample. At the beginning, no seats were offered in the hybrid format whereas at the end approximately three percent of seats were offered in the hybrid format. Evening offerings have

[13] Another method that I employed to try to test for substitution was to look for "cross-course" effects. Consider a simple scenario, for example, where there are only two classes, Business 101 and Psychology 101. Psychology 101 begins to offer online courses. If expanded online offerings only induce new enrollments of, for example, working adults, then we should see that the percent of seats online in PSY101 is positively associated with the percent of working adults in PSY101 but not associated with the percent of working adults in BUS101. If, however, expanded online offerings cause working adults who would have enrolled in BUS101 to enroll in PSY101 instead, then the percent of seats online in PSY101 should be negatively associated with the percent of working adult enrollees in BUS101. With nearly 2000 courses observed in my panel of data, it was not feasible to look at all the "cross-course" effects for these courses. I collapsed the data to a panel of 16 subject ares and used this collapsed data to estimate a system of equations that would allow me to look at cross-subject effects. Unfortunately, this analysis suffered from a lack of precision and was largely uninformative. Furthermore, the definition of subject categories may not have been reflective of the margins on which students may shift enrollment.

[14] Courses are unique to schools and the panel is unbalanced. Overall, the panel contains data on 1928 separate courses, not all of which are observed in each term. In a given term, the largest number of courses I observe is 959 (winter, 2009).

been large throughout the entire time frame with approximately 30% of seats being offered in the evening, on average. Weekend offerings are much less common – a little over one percent of seats are offered on the weekend, on average.

To get a better sense of the introduction of online offerings, Figure 5 shows a histogram of the first term in which a course is observed to have online offerings. 82 courses already had online offerings in the fall of 2003 (20037). These courses may be offering online seats for the first time during this term, or they may have offered online seats in prior terms that are not observed. 25 courses began offering online seats in the following winter (20043). Later in the sample time frame, a handful of courses begin offering online seats each fall (6-10) with fewer initial online offerings happening in the winter and spring/summer terms. Table 24 provides a bit of additional detail about which courses were being introduced online. Table 24 shows the subject area of the classes offered online for three terms of initial online offering. In the fall of 2003, the first term of the sample, courses across a variety of subjects were offering online seats (e.g. accounting, dental assisting, english, nursing, and sociology). In a couple of later terms, additional accounting, business, and dental assisting courses moved online.

When courses come online, they typically offer 30 to 50% of all available seats online. Once courses are offered online, however, they do not always continue to be offered online for all future terms. For courses that are initially observed to offer seats online, about 60% of these courses continue to offer some seats in the online format for all future terms in which the course is offered at all.

6 Baseline Estimates

Table 25 contains estimates of equation (1) using both an indicator for any online offerings and a continuous measure of the percent of seats that are offered online. In Panel A, however, fixed effects are omitted from the equation. Panel B includes both the course and term fixed effects. Six different populations of interest are addressed: adults (age 30+), working individuals, working adults, females, remedial students, and Pell recipients. Comparing Panel A and B, we see that including the fixed effects has a non-negligible effect on the point estimates. In fact, some of the point estimates change sign (e.g., working and remedial) and some of the magnitudes change drastically (e.g., female).

Focusing on Panel B, estimates in column 1 show that when a course offers any online seats, it increases the percent of enrollees that are adults (age 30+) by 4.87 percentage points. Column 2 shows that a one-percentage point increase in the percent of seats offered online increases the percent of enrollees that are adults by 0.087 percentage points. Increasing the online offerings is also shown to have significant positive effects on the share of enrollees in a course that are working, working adults, female, and Pell recipients with the largest effect being seen for working adults.[15] On average, a course's enrollees are 17% working adults. When a course begins offering any online seats, the percent of working adults increases by 5.2 percentage points – a 31% increase over the average. Increasing online offerings has a significant negative effect on the share of enrollees in the class that are remedial. Anecdotal evidence suggests that underprepared students are cautioned with respect to online course-taking as it requires good time-management skills and self-motivation. This counseling may be reflected in the shrinking share of remedial students observed in classes that increase their online offerings.

One threat to a causal interpretation of these estimates is that there may be other characteristics of the courses that are changing in tandem with the changes in online offerings that are responsible for the changing composition of the enrollees. Table 26 presents estimates that control for hybrid, evening, and weekend offerings (either "any" or the percent of seats, depending on the specification). The point estimates on online offerings do not change substantially upon inclusion of these other controls. In fact, if anything, it appears that the coefficients on online offerings increase upon

[15]Increasing online offerings only has a significant positive effect on the share of enrollees in a course that are receiving Pell grants for the *AnyOnline* measure.

inclusion of these controls suggesting that courses with expanding online offerings are less likely to be expanding hybrid, evening, or weekend offerings. It is possible that these alternative formats are seen as substitutes and that courses offer some flexible format but not multiple.

Furthermore, the coefficients on the other alternative formats are interesting in and of themselves. While expanded online offerings appear to have the biggest impact on the share of enrollees who are, for example, working adults, expanded evening offerings also increase the share of enrollees who are working adults. Evening classes also offer a degree of flexibility (in terms of scheduling school around a traditional day job) that traditional on-campus, day-time classes do not. They are not as flexible as online classes, but are clearly enticing to students that who may need/desire an alternative format. Interestingly, expanded weekend offerings do not appear to entice these students.

7 Measures of "Supply" from Enrollment Data

As discussed earlier, these data are unique because they contain information on the course schedules that shows the exogenous supply of seats offered in each course in each term, including seats that are never filled and seats in sections of courses that are eventually cancelled. Thus, I know the percent of seats in course C that are offered in the online format as opposed to the percent of enrollments (i.e. seats filled) in the online format which can be gleaned from the transcript data. As discussed in the Data and Methods section as well as the Appendix, calculations of the share of enrollments in the online format are, on average, similar to the share of seats offered in the online format. Thus, I expect that using measures of "any online enrollment" and "percent of enrollments online" in place of "any online seats offered" and "percent of seats offered online" should generate very similar estimates. Furthermore, the schedule data is what provides the information regarding evening and weekend sections that could not be gleaned from the transcript data allowing me to control for changes in the supply of seats in these two alternative formats. Table 26 showed, however that controlling for the supply of seats alternative hybrid, evening, and weekend formats did not substantially affect the estimates of interest on the supply of online seats. Thus, I still expect that estimates will be similar when I cannot control for evening and weekend offerings (I can still control for hybrid offerings because these are identified in the transcript data).

Table 27 shows that this is in fact true. The estimates generated using measures of "supply" from the transcript data are very similar to those generated using the actual capacity measures from the supplemental schedule data. As a result, this analysis could be extended to include other institutions for which schedule data is not available with a good deal of confidence that the estimates will not suffer from substantial bias.

8 Lead Measures of Online Offerings and Event Study Analysis

One concern about these estimates is that reverse causality is at play and that demand for online offerings among these populations of interest might actually be leading to higher supply of online offerings. I address this in two ways–first with lead measures of the key independent variables and second with an event-study analysis.

First, I include leads of the key independent variables (*AnyOnline* and *PercentOnline*) to make sure that, for example, the percent of course enrollees that are working adults is not increasing prior to the change in the supply of online seats. If, for example, college administrators observe that the share of enrollees that are working adults in some courses increased substantially one fall, and they decide to increase the share of seats offered online in these classes during the following winter term in order to address what they think is a changing pattern of demand among students who would desire flexibility, we might see that the share of seats offered online in t+1is significantly related to the share of enrollees that are working adults in period t. The coefficients on the lead measures of the variables should not be significant. I also include lagged measures of these variables to see if there is a delay in enrollment changes among the populations of interest after changes in online offerings. I include two leads and two lags in each specification. These results are presented in Table 28. Note that the sample size falls substantially because many courses are not offered for five consecutive terms which is the requirement to have all non-missing values for all of the lead and lag measures.

For the most part, the lead measures of the supply of online seats are not significant. Three of the specifications, columns 1, 5, and 11, have modestly significant one-period leads, but the signs are actually in the wrong direction suggesting that demand for these courses among the populations of interest might have been falling leading up to the expansion of online offerings. For example, column 5 shows that if a course will change to offering any online courses next term (t+1), the share of enrollees who are working adults in this term falls by 1.5 percentage points.[16] All in all, these results suggest that a significant increase in the share enrollees who have these characteristics does not precede the implementation of online offerings.

Additionally, contemporaneous measures of online offerings are similar to the estimates in Table 28 and the the lag measures are, for the most part, insignificant suggesting that changes in online offerings have a contemporaneous effect on the composition of the students taking courses but no delayed effect.

The second way that I address this is to implement an event-study analysis, as described in the Data and Methods section. This analysis focuses on the time path of the share of enrollees who have different characteristics surrounding the event in which a course begins to offer online seats.[17]

[16]These negative leads could be occurring because students are aware that online seats will be offered next term, so they shift their enrollment temporally.

[17]Courses do not always consistently offer online seats following the initial observed offering of online seats. For this analysis, however, I focus on the event in which I first observe a course switching from not offering online seats to offering online seats.

The results of this analysis for each of the six groups of interest are presented in Figures 6 through 11. The figures plot β_j from equation (2) and the relevant 95% confidence intervals. The omitted group is the term immediately preceding the term in which online seats were initially offered. Thus, the plotted coefficients represent differences in the share of enrollees who have the specified characteristics relative to the term preceding online implementation.

Figure 6 shows changes in the share of enrollees who are adult, relative to the term just prior to online implementation. This plot shows that, in terms leading up to online implementation, the share of enrollees who are adult was not significantly different from the period just prior to implementation. In the period of initial implementation, however, the share of enrollees who are adult increased by over five percentage points. Although this fluctuates over time, the periods subsequent to initial online implementation mostly show a significantly elevated share of enrollees who are adult relative to the period just prior to implementation. The jump in the share of enrollees who are adults is actually larger than the estimated effect in the simple fixed effects analysis in Table 26.

Figures 8 and 11 indicate that the share of enrollees who are working adults and Pell recipients, respectively, jump significantly in the period in which online seats are first offered. The share of enrollees who have these characteristics remains elevated, though not always significantly so, in all terms after implementation, relative to the term prior to online implementation.

Figures 7, 9, and 10 indicate that the share of enrollees who are working, female, and remedial, respectively, do not experience significant changes at the onset of online offerings, relative to the term prior to online offerings.

It is important to note that the event-study analysis focuses on the event in which a course was first observed to switch from not offering online seats to offering any online seats. Not all courses continue to offer online seats in every term following initial online implementation (approximately 40% do not). The fixed effects estimates of the effect of "any online" offerings presented earlier may differ from these event-study estimates, in part, because they are estimated off of all changes within a course in which it switches from not offering to offering online seats, even if it is not the first occasion.

9 Diversion versus Democratization

It is important to note that the parameter of interest here, β, contains the effect of expanded online offerings on whether, for example, working adults enroll in a course at all (the "democratization" effect) and the effect of expanded online offerings on, for example, working adults shifting between courses because one is offered online while another is not (the "diversion" effect). In attempt to understand how important these two effects are, I look at the interaction between online offerings and the substitutability of a course. If the coefficient on this interaction is insignificant, this suggests that the effect of online offerings is similar between more and less substitutable courses and that online education induces additional enrollment rather than simply shifting enrollment.

The results of this analysis are in Table 29. Most of the interaction terms are insignificant meaning that online offerings affect the share of enrollees who have these characteristics similarly in more substitutable and less substitutable courses. This suggests that, in general, online offerings induce new enrollments rather than simply shifting enrollments from other courses. A few of the interactions are significant, however. For example, the interaction of *PercentOnline* with *Substitutable* for the share of enrollees who are female is significant at 0.05. This means that a one-percentage point increase in the percent of seats offered online increases the share of enrollees who are female by 0.05 percentage-points more in classes that are more substitutable. This suggests that women are being pulled from other courses into those that offer online seats.

Interestingly, many of the interactions of interest, although mostly insignificant, are negative. This actually suggests that online offerings in more substitutable courses do not increase the shares of enrollees with these characteristics as much as online offerings in less substitutable courses. As discussed in the section Data and Methods, this test is merely suggestive. The courses that I characterized as "substitutable" may have smaller effects of online expansion for other reasons (potentially the subject matter or level of the course) which could be driving these negative coefficients on the interaction terms. It will be important for future research to explore other approaches to addressing the margins on which online offerings influence enrollment decisions.

10 Discussion

The analysis herein assesses how the expansion of online offerings in community-college courses alters the composition of students enrolling in these courses. I employ a course-fixed-effects analysis that compares changes in the composition of enrollees in courses that begin to offer online opportunities or expand their offering of online opportunities with changes in the composition of enrollees in courses that do not. I also implement an event-study analysis that looks at the time path of the effect around initial offerings of online seats. I find that expanding online offerings increases the percent of enrollees that are adult (30+), working, and financially needy (i.e. Pell recipients). The largest effect is on working adults, for which course-fixed-effects analysis shows that a one percentage point increase in the percent of seats offered online increases the percent of enrollees that are working adults by 0.11 percentage points.

I attempt to look at the importance of the diversion effect, separate from the democratization effect, by comparing the effect of online offerings between more and less substitutable courses. Overall, this analysis suggests that diversion effects are not driving the results. Future research is needed, however, to better understand the how online offerings might influence students decisions in terms of their enrollment and course selection.[18]

[18]One potential avenue for future research is to structure the analysis around a discrete choice framework and use a conditional logit model to estimate the effect of online offerings on, for example, a working, adult student's decision to enroll in one course as opposed to another. There are some details regarding the question and data at hand that

Overall, my analysis indicates that expanded online offerings affect the enrollment decisions of community-college enrollees–especially those that community colleges may be most interested in serving, working adults. President Obama recently stated a goal of having the highest proportion of college graduates in the world by 2020. Attaining the this goal will require more than helping traditional college students transition from high school to college and succeed; it will require helping non-traditional students gain access to and be successful in college (Lane, 2012). My findings suggest that online education may help us to reach this goal by showing that the enrollment decisions of non-traditional students (working and 30+ years old) are being affected by expanded online offerings.

More work is needed, however, to disentangle the margins on which expanded online offerings affect the enrollment decisions of the populations of interest. It would be very useful to understand, for example, exactly how much the expansion of online education has induced non-traditional students to enroll in college at all. Furthermore, more work is needed to precisely understand whether online education affects persistence and degree completion. Existing research shows that students are less likely to complete online courses and earn lower grades in online courses (Xu and Jaggars, 2011; Xu and Jaggars, 2013; Figlio et al., 2013), but we do not know whether it impedes or supports persistence to a degree or transfer to a four-year college. Much research is still needed in this area.

make this strategy challenging to implement. First, these data do not contain information on non-enrollees. Second, students who enroll do not simply choose one course out of all of their options. Conditional logit models are used in the literature to estimate, for example, the effect of certain college characteristics on students' decision of which college to attend (Long, 2004; Jacob, McCall, & Stange, 2013). Unlike these examples, students enrolling in community college may choose 1, 2, 3 or even more courses in a given term. These and other details would need to be considered before proceeding with this type of analysis.

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Data Appendix

Inflated measures from transcript data

Appendix Table 1 shows capacity and enrollment data for a Principles of Management course at one of the schools. Columns 4 and 5 show the total capacity of seats offered in this course and the number of these seats that are in online sections. Columns 6 and 7 show the total enrollment in this course and the total enrollment in online sections. Columns 8 and 9 show the share of seats that are offered online (calculated by dividing the numbers in columns 5 by the numbers in columns 4 and multiplying by 100) and the share of enrollments that are online (calculated by dividing the numbers in columns 7 by the numbers in columns 6 and multiplying by 100). In this example, the percent of enrollments that are online is inflated relative to the percent of seats that are offered online in most terms. In the fall of 2005, for example, 25 of the 95 seats offered were in the online format. 22 of the online seats were filled but online 72 of all seats were filled. This means that 3 online seats were not used (12% of online seats offered) while 20 non-online seats were not filled (25% of non-online seats offered). More of the non-online seats were not used meaning the percent of enrollments in the online format ($30.56\% = 22/72 * 100$) is greater than the percent of seats offered online ($26.32\% = 25/95 * 100$) by 4.24 percentage points. Among all course observations, however, the average amount of inflation in the measure from the transcript data is only .04 percentage points.

Appendix Table 1. Example of "Share Online" Being Inflated in the Transcript Data Relative to Schedule Data

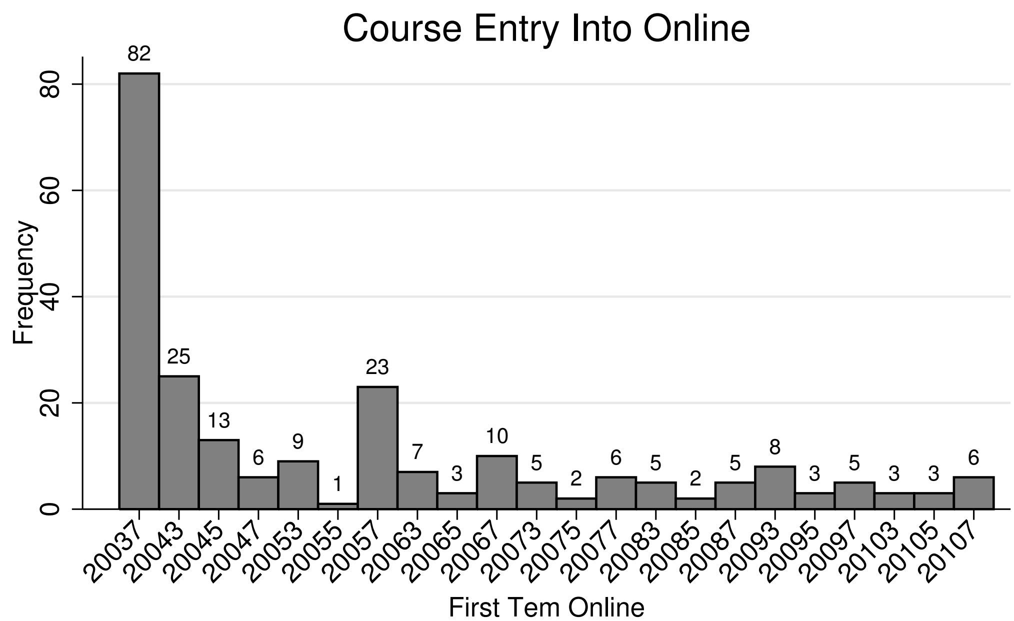
1	2	3	4	5	6	7	8	9	10
Academic Year	**Term**	**Course**	**Total Capacity (Measured in Schedule Data)**	**Online Capacity (Measured in Schedule Data)**	**Total Enrollment (Measured in Trascript Data)**	**Total Online Enrollment (Measured in Trascript Data)**	**Percent of Seats Offered Online**	**Percent of Enrollments Online**	**Amount Percent of Enrollments is Inflated Relative to Share of Seats Offered**
2005	Fall	Principles of Management	95	25	72	22	26.32%	30.56%	4.24%
2005	Winter	Principles of Management	194	50	143	24	25.77%	16.78%	-8.99%
2006	Fall	Principles of Management	192	50	113	33	26.04%	29.20%	3.16%
2006	Winter	Principles of Management	204	50	96	34	24.51%	35.42%	10.91%
2007	Fall	Principles of Management	178	50	93	26	28.09%	27.96%	-0.13%
2007	Winter	Principles of Management	182	50	118	41	27.47%	34.75%	7.27%
2008	Fall	Principles of Management	200	50	90	27	25.00%	30.00%	5.00%
2008	Winter	Principles of Management	168	50	107	26	29.76%	24.30%	-5.46%
2009	Fall	Principles of Management	196	50	135	45	25.51%	33.33%	7.82%
2009	Winter	Principles of Management	141	40	139	53	28.37%	38.13%	9.76%
2010	Fall	Principles of Management	220	60	180	55	27.27%	30.56%	3.28%
2010	Winter	Principles of Management	145	30	131	30	20.69%	22.90%	2.21%
						All Course Observations:	6.58%	6.62%	0.04%

Table 23. Summary Statistics

	2003			2004			…	2009			2010		
	Fall	Winter	Spring/ Summer	Fall	Winter	Spring/ Summer		Fall	Winter	Spring/ Summer	Fall	Winter	Total
Any Online	0.0927	0.0847	0.133	0.0876	0.0731	0.134		0.128	0.124	0.206	0.138	0.132	0.121
	(0.290)	(0.279)	(0.340)	(0.283)	(0.260)	(0.341)		(0.334)	(0.330)	(0.405)	(0.345)	(0.339)	(0.326)
Any Hybrid	0	0	0	0	0.0221	0.0100		0.0624	0.0626	0.0458	0.0601	0.0569	0.0259
	(0)	(0)	(0)	(0)	(0.147)	(0.0997)		(0.242)	(0.242)	(0.209)	(0.238)	(0.232)	(0.159)
Any Evening	0.550	0.563	0.524	0.557	0.560	0.503		0.542	0.515	0.454	0.538	0.516	0.529
	(0.498)	(0.496)	(0.500)	(0.497)	(0.497)	(0.500)		(0.498)	(0.500)	(0.498)	(0.499)	(0.500)	(0.499)
Any Weekend	0.0395	0.0440	0.00605	0.0580	0.0554	0.0180		0.0612	0.0636	0.0246	0.0634	0.0590	0.0422
	(0.195)	(0.205)	(0.0776)	(0.234)	(0.229)	(0.133)		(0.240)	(0.244)	(0.155)	(0.244)	(0.236)	(0.201)
Percent of Seats Offered Online	4.861	5.029	8.432	4.951	4.053	9.151		6.433	6.240	12.62	7.143	6.747	0.0658
	(18.32)	(19.74)	(24.65)	(18.88)	(17.19)	(25.93)		(20.53)	(19.98)	(28.46)	(21.63)	(20.67)	(0.213)
Percent of Seats Offered Hybrid	0	0	0	0	1.495	0.591		3.161	3.341	2.583	2.709	2.935	0.0147
	(0)	(0)	(0)	(0)	(11.23)	(6.291)		(15.87)	(16.61)	(14.55)	(14.68)	(15.41)	(0.112)
Percent of Seats Offered Evening	32.83	34.81	34.87	32.61	32.79	32.45		30.69	29.63	26.98	28.95	28.86	30.96
	(37.81)	(38.64)	(39.75)	(37.48)	(37.59)	(38.93)		(37.46)	(37.51)	(36.88)	(36.07)	(36.72)	(37.40)
Percent of Seats Offered Weekend	0.898	1.015	0.108	1.409	1.842	1.153		1.514	1.777	0.902	1.735	1.628	1.206
	(7.447)	(8.028)	(1.633)	(8.908)	(11.59)	(9.281)		(9.176)	(10.53)	(7.403)	(10.18)	(9.498)	(8.608)
Percent of Enrollees Age 30+	29.18	29.51	36.11	27.46	29.49	34.35		30.17	32.17	39.81	32.01	32.40	0.305
	(23.69)	(22.99)	(24.97)	(22.58)	(23.18)	(25.20)		(21.55)	(22.35)	(23.72)	(21.61)	(20.87)	(0.226)
Percent of Enrollees Working	73.16	72.98	69.80	73.73	72.41	70.31		61.43	61.34	56.11	62.34	61.46	0.683
	(19.74)	(17.82)	(21.06)	(18.05)	(18.30)	(19.95)		(17.64)	(16.40)	(19.16)	(17.24)	(16.07)	(0.187)
Percent of Enrollees Working and 30+	18.44	18.62	22.66	16.70	17.70	21.18		14.20	15.25	17.28	15.79	16.46	17.33
	(19.13)	(18.85)	(21.44)	(18.07)	(18.21)	(20.65)		(13.10)	(13.93)	(14.42)	(14.36)	(14.36)	(17.02)
Percent of Enrollees Female	52.83	52.02	55.56	50.73	51.04	57.20		48.79	47.95	50.75	48.45	49.31	0.508
	(31.75)	(32.06)	(32.23)	(31.59)	(31.37)	(30.30)		(30.17)	(30.14)	(29.73)	(30.44)	(30.51)	(0.312)
Percent of Enrollees Remedial	4.856	7.414	9.894	11.70	14.01	14.32		26.89	26.74	28.97	27.12	28.31	19.69
	(11.93)	(12.61)	(15.62)	(14.48)	(15.22)	(17.31)		(18.95)	(18.75)	(20.47)	(18.49)	(19.20)	(18.70)
Percent of Enrollees Receiving Pell	22.51	23.04	26.05	22.24	23.72	25.06		36.74	38.63	46.61	44.47	46.78	29.22
	(18.27)	(16.97)	(21.03)	(16.76)	(17.17)	(19.06)		(18.70)	(18.63)	(21.36)	(19.03)	(19.88)	(19.55)
N	885	909	496	879	903	499		898	959	568	899	932	18067

Notes: Standard deviations are in parentheses. The dataset is a panel of courses that were offered between the fall of 2003 and the winter of 2010 at two community colleges in a Midwestern state. Any online is an indicator for whether a course had any online seats offered. Any hybrid, any evening, and any weekend are defined similarly.

Figure 5.



Notes: Terms are identified by the calendar year and the season (YYYYS) where the season can be fall (7), winter (3), or spring/summer (5). The winter of 2004 is 20043. Fall of 2003 (20037) is the first term in which online courses are identified in the data, so the 82 courses whose first online term is 20037 may have been online in prior terms as well.

Table 24. Subject of Courses Coming Online By Term

Term: 20037

Subject	#	%
Accounting	1	1.22
Art	1	1.22
Biology	1	1.22
Business	10	12.20
Communication	2	2.44
Computer Information Systems	9	10.98
Dental Assisting	8	9.76
Diagnostic Medical Sonography	9	10.98
Economics	2	2.44
English	8	9.76
Health Science	4	4.88
History	2	2.44
Humanities	1	1.22
Internet Professional	2	2.44
Licensed Practical Nursing	1	1.22
Mathematics	4	4.88
Medical Assistant	2	2.44
Nursing	3	3.66
Philosophy	1	1.22
Political Science	2	2.44
Psychology	5	6.10
Sociology	3	3.66
Speech	1	1.22
Total	82	100

Term: 20067

Subject	#	%
Accounting	2	20
Business	3	30
Dental Assisting	2	20
Geology	1	10
Mathematics	1	10
Political Science	1	10
Total	10	100

Term: 20097

Subject	#	%
Business	2	40
Computer Networking and Security	2	40
English	1	20
Total	5	100

Notes: Terms are identified by the calendar year and the season (YYYYS) where the season can be fall (7), winter (3), or spring/summer (5). The winter of 2004 is 20043. Fall of 2003 (20037) is the first term in which online courses are identified in the data, so the 82 courses whose first online term is 20037 may have been online in prior terms as well.

Table 25. Baseline Estimates

Panel A: No Fixed Effects

	1	2	3	4	5	6	7	8	9	10	11	12
	Dependent Variable: Percent of Enrollees with Following Characteristic											
	Age 30+	**Age 30+**	**Working**	**Working**	**30+ & Working**	**30+ & Working**	**Female**	**Female**	**Remedial**	**Remedial**	**Receive Pell**	**Receive Pell**
Any Online	4.168***		-1.950***		1.901***		18.08***		1.274***		5.885***	
	(0.435)		(0.347)		(0.322)		(0.459)		(0.362)		(0.383)	
Percent Online		0.132***		-0.0450***		0.0683***		0.295***		-0.0411***		0.0672***
		(0.00693)		(0.00644)		(0.00606)		(0.00708)		(0.00503)		(0.00656)
Observations	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067
R-squared	0.004	0.015	0.001	0.003	0.001	0.007	0.036	0.041	0.000	0.002	0.010	0.005

Panel B: Course and Term Fixed Effects

	1	2	3	4	5	6	7	8	9	10	11	12
	Dependent Variable: Percent of Enrollees with Following Characteristic											
	Age 30+	**Age 30+**	**Working**	**Working**	**30+ & Working**	**30+ & Working**	**Female**	**Female**	**Remedial**	**Remedial**	**Receive Pell**	**Receive Pell**
Any Online	4.870***		1.488**		5.233***		2.541***		-1.884***		1.832***	
	(0.863)		(0.665)		(0.840)		(0.715)		(0.594)		(0.703)	
Percent Online		0.0865***		0.0292***		0.0902***		0.0479***		-0.0287***		0.0140
		(0.0139)		(0.0106)		(0.0137)		(0.00987)		(0.00984)		(0.0117)
Observations	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067
R-squared	0.602	0.602	0.448	0.448	0.499	0.500	0.859	0.860	0.657	0.657	0.545	0.545
Courses	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the course level for estimates in Panel B. Course (by school) and term fixed effects are included in each specification in Panel B.

Table 26. Course Fixed Effects, Control for Hybrid, Evening, and Weekend Offerings

	1	2	3	4	5	6	7	8	9	10	11	12
	Dependent Variable: Percent of Enrollees with Following Characteristic											
	Age 30+	**Age 30+**	**Working**	**Working**	**30+ & Working**	**30+ & Working**	**Female**	**Female**	**Remedial**	**Remedial**	**Receive Pell**	**Receive Pell**
Any Online	5.272***		2.108***		5.906***		2.585***		-1.952***		1.626**	
	(0.847)		(0.664)		(0.818)		(0.726)		(0.600)		(0.690)	
Any Hybrid	-0.671		-0.414		-0.356		0.752		1.364*		1.614*	
	(0.933)		(0.772)		(1.016)		(0.680)		(0.757)		(0.851)	
Any Evening	2.315***		3.358***		3.633***		-0.0224		-0.810***		-1.574***	
	(0.423)		(0.400)		(0.345)		(0.340)		(0.306)		(0.369)	
Any Weekend	0.516		1.038		0.424		0.618		0.548		-1.251*	
	(0.842)		(0.719)		(0.660)		(0.528)		(0.581)		(0.673)	
Percent Online		0.101***		0.0451***		0.110***		0.0553***		-0.0309***		0.00889
		(0.0135)		(0.0106)		(0.0128)		(0.0101)		(0.0101)		(0.0125)
Percent Hybrid		0.0141		-0.00246		0.0116		0.0448***		0.0126		0.00921
		(0.0220)		(0.0173)		(0.0245)		(0.0113)		(0.0184)		(0.0181)
Percent Evening		0.0349***		0.0476***		0.0519***		-0.00280		-0.0136***		-0.0203***
		(0.00638)		(0.00558)		(0.00531)		(0.00486)		(0.00426)		(0.00509)
Percent Weekend		0.0159		0.0410*		0.0217		0.0104		0.00315		-0.00720
		(0.0242)		(0.0220)		(0.0207)		(0.0168)		(0.0167)		(0.0194)
Observations	18067	18067	18067	18067	18067	18067	18067	18067	18,067	18,067	18,067	18,067
R-squared	0.602	0.602	0.448	0.448	0.499	0.5	0.859	0.86	0.657	0.657	0.546	0.546
Courses	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the course level. Course and term fixed effects are included in each specification.

Table 27. Course Fixed Effects, Using Enrollment-Based Supply Measures

	1	2	3	4	5	6	7	8	9	10	11	12
	Dependent Variable: Percent of Enrollees with Following Characteristic											
	Age 30+	**Age 30+**	**Working**	**Working**	**30+ & Working**	**30+ & Working**	**Female**	**Female**	**Remedial**	**Remedial**	**Received Pell**	**Received Pell**
Any Online	5.292***		1.386**		5.339***		2.851***		-1.884***		2.197***	
	(0.879)		(0.651)		(0.821)		(0.723)		(0.580)		(0.715)	
Any Hybrid	0.280		0.196		0.668		1.357**		1.128		2.071**	
	(0.929)		(0.785)		(1.022)		(0.679)		(0.742)		(0.863)	
Percent Online		0.0883***		0.0256**		0.0883***		0.0588***		-0.0282***		0.0174
		(0.0132)		(0.0109)		(0.0122)		(0.0103)		(0.00956)		(0.0124)
Percent Hybrid		0.0109		-0.00769		0.00749		0.0494***		0.00922		0.00831
		(0.0213)		(0.0174)		(0.0239)		(0.0116)		(0.0180)		(0.0174)
Observations	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067	18,067
R-squared	0.602	0.603	0.448	0.448	0.499	0.500	0.859	0.860	0.657	0.657	0.546	0.545
Courses	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the course level. Course and term fixed effects are included in each specification.

Table 28. Course Fixed Effects, Lags and Leads

	1	2	3	4	5	6	7	8	9	10	11	12
VARIABLES	**Age 30+**	**Age 30+**	**Working**	**Working**	**30+ & Working**	**30+ & Working**	**Female**	**Female**	**Remedial**	**Remedial**	**Received Pell**	**Received Pell**
Any Online (t+2)	-0.559		0.658		-0.764		-0.816		1.129		1.919	
	(1.189)		(0.788)		(0.976)		(0.823)		(1.008)		(1.265)	
Any Online (t+1)	-2.006*		-0.908		-1.524*		-0.636		-0.601		-2.273*	
	(1.116)		(1.136)		(0.888)		(1.070)		(1.043)		(1.326)	
Any Online (current t)	6.013***		1.625*		6.112***		4.250***		-3.228***		1.536	
	(1.583)		(0.861)		(1.476)		(1.222)		(0.998)		(1.090)	
Any Online (t-1)	-1.010		-1.257		-1.255		-2.512**		0.897		-0.119	
	(1.275)		(0.867)		(1.048)		(0.987)		(1.016)		(1.258)	
Any Online (t-2)	-1.133		0.211		-0.807		-0.778		0.244		0.691	
	(1.199)		(0.747)		(1.002)		(0.881)		(0.823)		(1.096)	
Percent Online (t+2)		-0.0186		-0.00347		-0.0277*		-0.0160		0.0225		0.0241
		(0.0179)		(0.0116)		(0.0152)		(0.0138)		(0.0144)		(0.0162)
Percent Online (t+1)		-0.0167		-0.0109		-0.0141		-0.00839		0.00767		-0.0227
		(0.0179)		(0.0138)		(0.0150)		(0.0136)		(0.0122)		(0.0168)
Percent Online (current t)		0.103***		0.0363***		0.112***		0.0644***		-0.0473***		-0.00454
		(0.0224)		(0.0137)		(0.0193)		(0.0148)		(0.0142)		(0.0176)
Percent Online (t-1)		-0.00349		0.00170		0.00266		-0.0221**		-0.00226		-0.00118
		(0.0211)		(0.0121)		(0.0190)		(0.0112)		(0.0148)		(0.0156)
Percent Online (t-2)		-0.0146		0.0148		0.00163		0.000876		0.0161		0.0238*
		(0.0248)		(0.0111)		(0.0226)		(0.0121)		(0.0122)		(0.0141)
Observations	6,619	6,619	6,619	6,619	6,619	6,619	6,619	6,619	6,619	6,619	6,619	6,619
R-squared	0.628	0.629	0.486	0.487	0.477	0.482	0.861	0.861	0.738	0.738	0.570	0.570
Courses	632	632	632	632	632	632	632	632	632	632	632	632

Notes: Robust standard errors in parentheses. Standard errors are clustered at the course level. *** p<0.01, ** p<0.05, * p<0.1 Course and term fixed effects are included in each specification. Controls for hybrid, evening, and weekend offers are also included in each specification.

Figure 6.

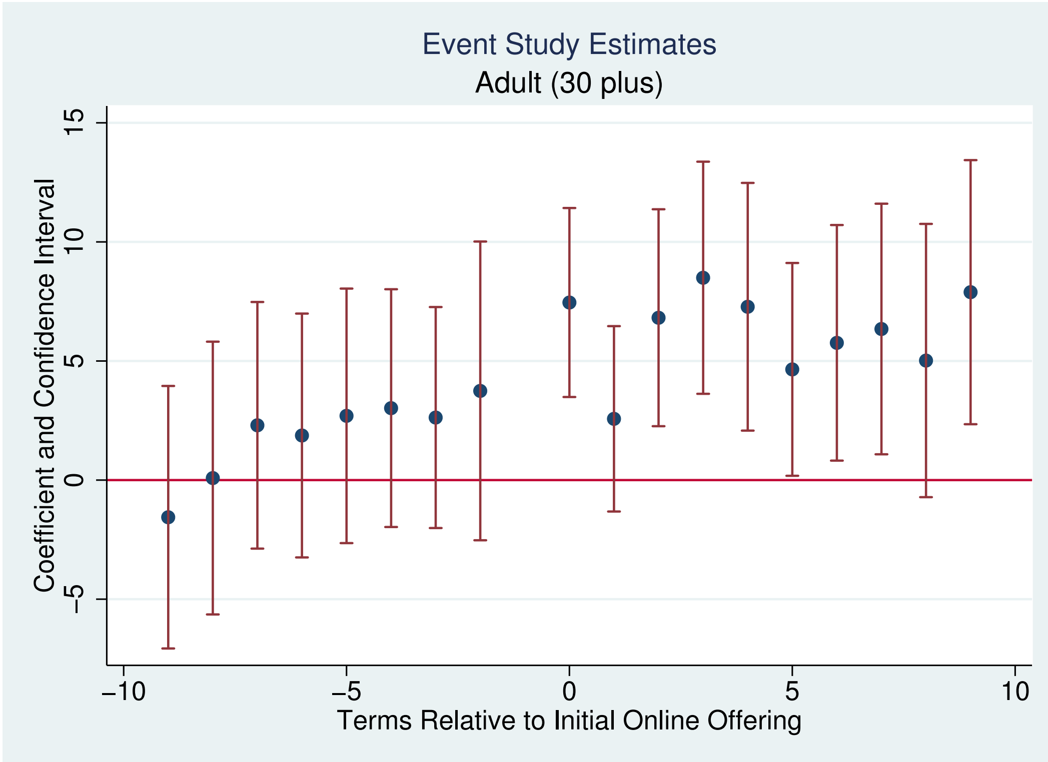


Figure 7.

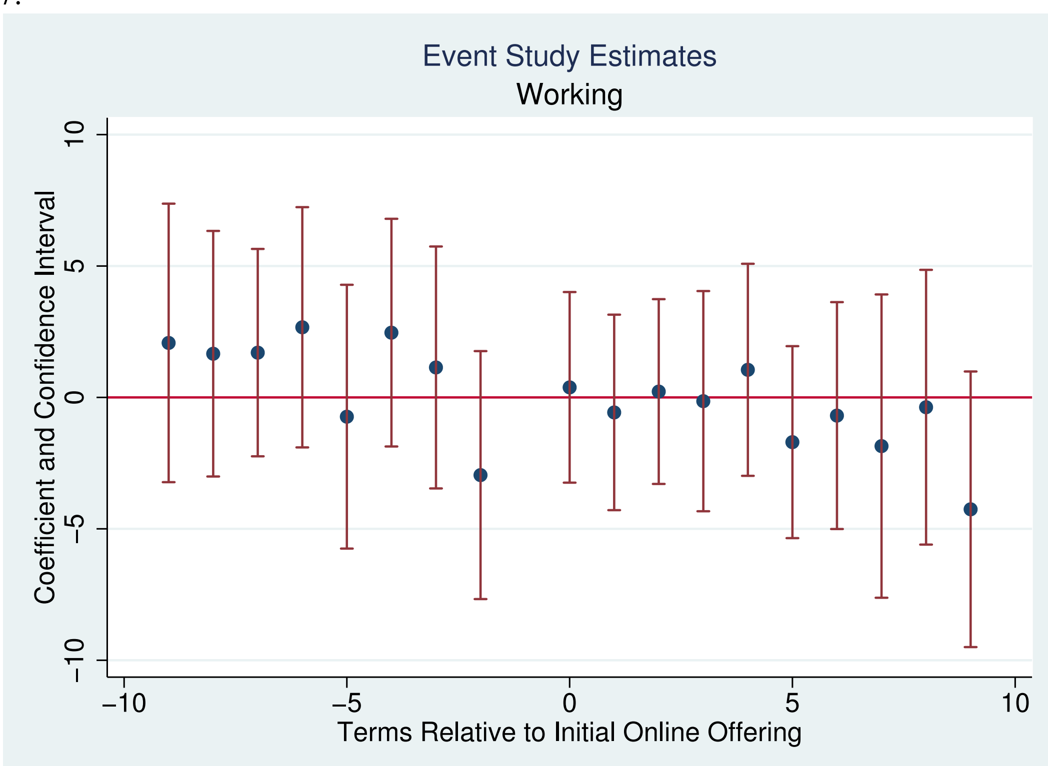


Figure 8.

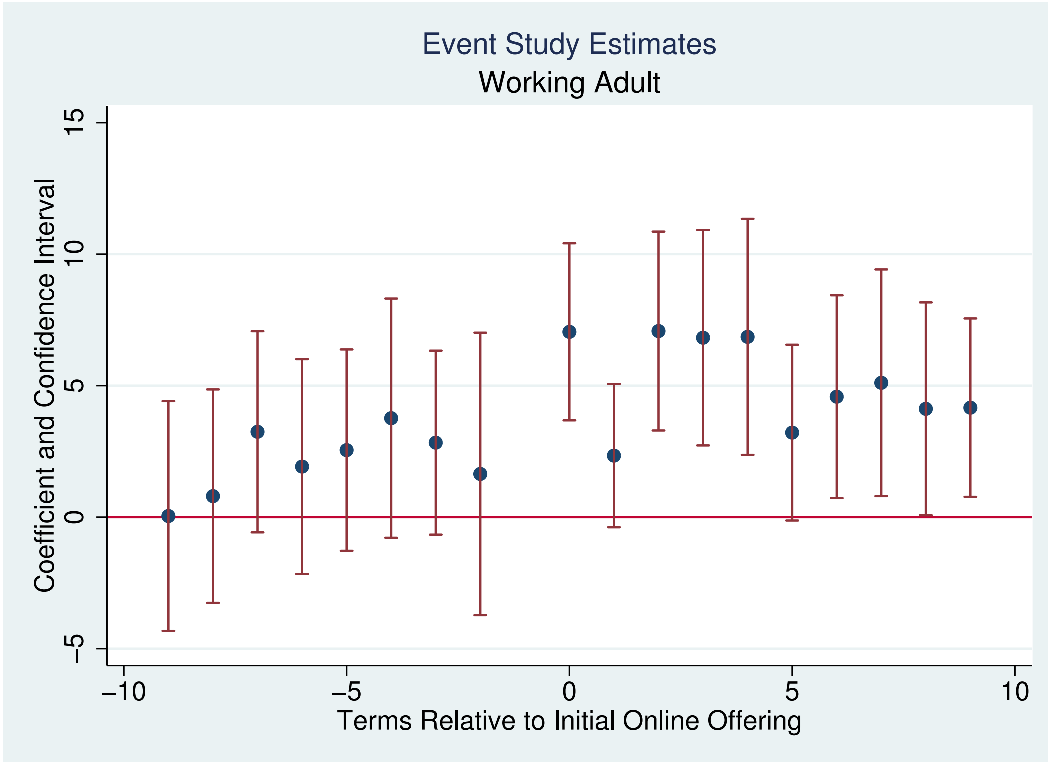


Figure 9.

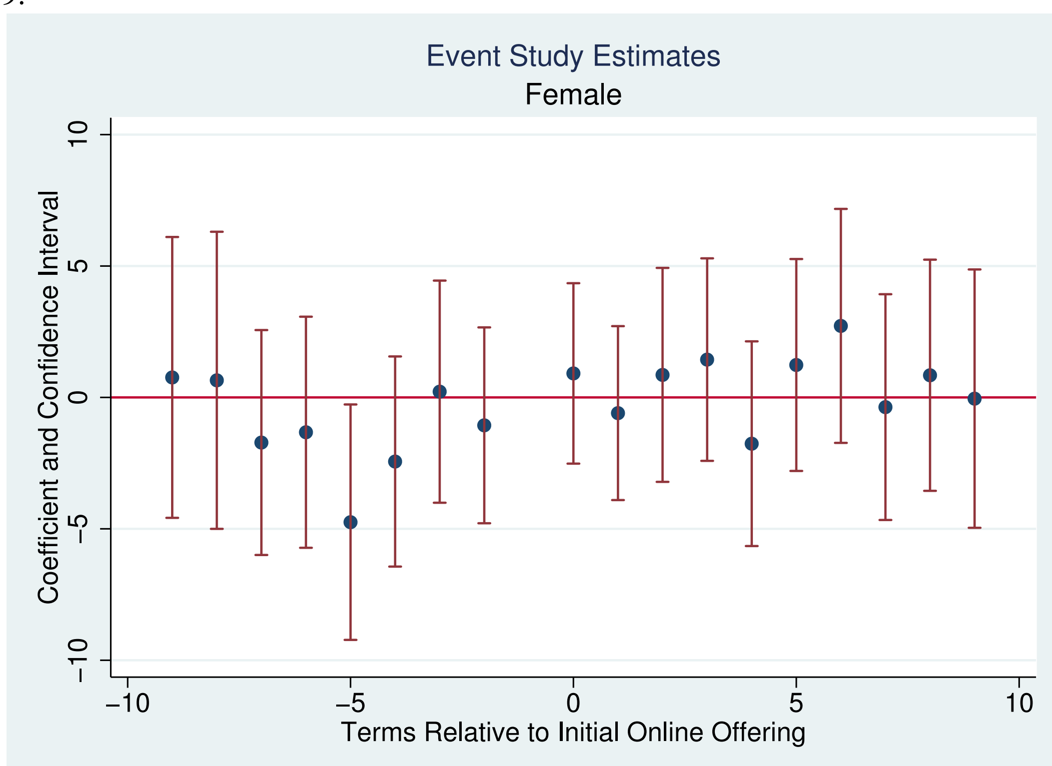


Figure 10.

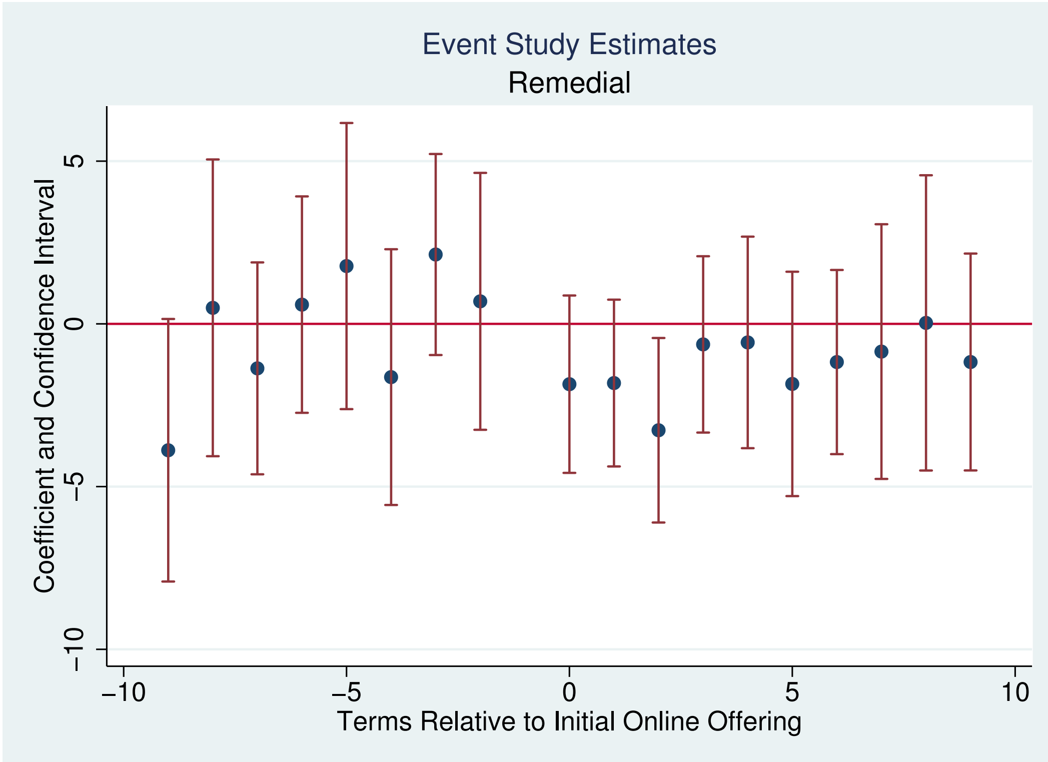


Figure 11.

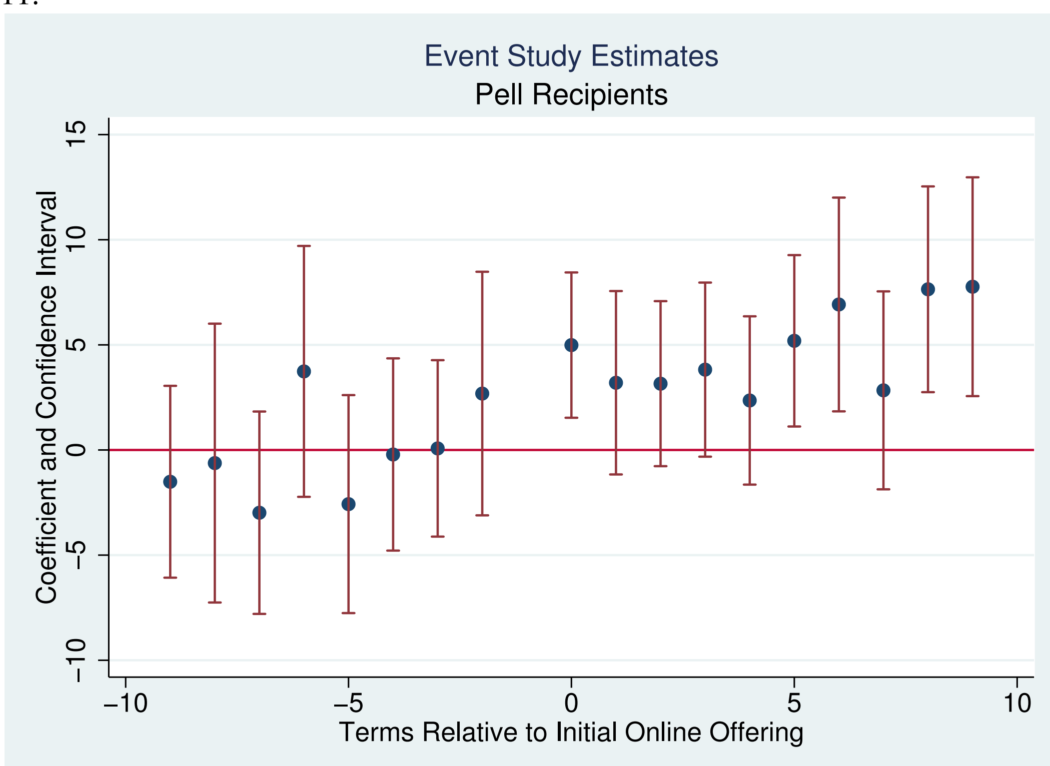


Table 29. Course Fixed Effects, Substitutable Interaction

	1	2	3	4	5	6	7	8	9	10	11	12
	Dependent Variable: Percent of Enrollees with Following Characteristic											
	Age 30+	**Age 30+**	**Working**	**Working**	**30+ & Working**	**30+ & Working**	**Female**	**Female**	**Remedial**	**Remedial**	**Receive Pell**	**Receive Pell**
Any Online	5.742***		2.796***		6.461***		2.389***		-1.534**		1.705**	
	(1.077)		(0.800)		(1.072)		(0.869)		(0.763)		(0.857)	
Any Online X Substitutable	-1.663		-2.435**		-1.963		0.695		-1.481		-0.277	
	(1.481)		(1.213)		(1.253)		(1.521)		(1.078)		(1.339)	
Percent Online		0.106***		0.0509***		0.116***		0.0438***		-0.0208*		0.00946
		(0.0157)		(0.0123)		(0.0156)		(0.0113)		(0.0119)		(0.0151)
Percent Online X Substitutable		-0.0257		-0.0265		-0.0271		0.0528**		-0.0466***		-0.00260
		(0.0267)		(0.0210)		(0.0199)		(0.0209)		(0.0171)		(0.0219)
Observations	18067	18067	18067	18067	18067	18067	18067	18067	18067	18067	18067	18067
R-squared	0.603	0.604	0.451	0.452	0.503	0.505	0.859	0.86	0.657	0.657	0.546	0.546
Courses	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928	1928

Notes: Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). Standard errors are clustered at the course level. Course and term fixed effects are included in each specification. Controls for hybrid, evening, and weekend offerings are also included in each specification.