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The effects of computers and acquired skills on earnings, employment and college enrollment: Evidence from a field experiment and California UI earnings records☆

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ABSTRACT

This paper provides the first evidence on the earnings, employment and college enrollment effects of computers and acquired skills from a randomized controlled trial providing computers to entering college students. We matched confidential administrative data from California Employment Development Department (EDD)/ Unemployment Insurance (UI) system earnings records, the California Community College system, and the National Student Clearinghouse to all study participants for seven years after the random provision of computers. The experiment does not provide evidence that computer skills have short- or medium-run effects on earnings. These null effects are found along both the extensive and intensive margins of earnings (although the estimates are not precise). We also do not find evidence of positive or negative effects on college enrollment. A non-experimental analysis of CPS data reveals large, positive and statistically significant relationships between home computers, and earnings, employment and college enrollment, raising concerns about selection bias in non-experimental studies.

1. Introduction

Although the returns to education have been studied extensively, the labor market returns to computers and the skills acquired in using them are not as well understood. A few recent studies find higher wages among workers with computer skills, but the evidence is not as clear as the evidence of positive returns to formal schooling (Card 1999; Dickerson & Green, 2004; Falck, Heimisch, & Wiederhold, 2016; Hanushek et al. 2015; OECD 2015).[1] Similar to concerns regarding estimating the returns to education, identifying the causal effects of computer skills on labor market outcomes is difficult because of unobserved heterogeneity.

This study takes a novel approach to estimate the labor market returns to computers and the acquired skills from using them by exploiting a randomized controlled trial (RCT) providing free personal computers for home use. The field experiment was conducted with entering community college students in Fall 2006, following them through their educational and early career labor market experiences. Previous findings from the experiment indicate that the treatment group receiving home computers had substantially better computer skills than the control group (Fairlie, 2012), and that the randomly provided home computers were found to have small, positive, short-run

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[1] Earlier research found that computer users at work had higher wages than non-computer users, arguably due to their computer skills (e.g. Krueger, 1993). But, whether this estimated computer-wage premium captures the returns to computer skills or simply unobserved worker, job, or employer heterogeneity has been questioned (e.g. DiNardo & Pischke, 1997).

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(1.5 year) effects on educational outcomes (Fairlie & London, 2012).[2] In this study, we collect new administrative data from three sources for all study participants, and build on these findings by examining short- to medium-term effects on earnings, employment and college enrollment. To analyze earnings and employment effects, we obtained confidential administrative earnings data collected by the California State Employment Development Department (EDD) through the Unemployment Insurance (UI) system for all study participants.[3] We also obtained restricted-access administrative data on college enrollment from the California Community College System and the National Student Clearinghouse for all study participants. The data on earnings, employment and college enrollment cover nearly a decade after the computers were randomly distributed, allowing for a rare analysis of medium-term experimental effects in addition to short-term effects. Furthermore, the use of administrative data eliminates concerns over follow-up survey attrition and item non-response, which are often problematic in RCTs and especially problematic for capturing longer-term outcome effects.

From the experiment and administrative data on earnings and college enrollment, we do not find evidence that computer skills have a positive effect on earnings. We do not find evidence of positive effects on the extensive or intensive margins of labor supply. We also do not find evidence that computers have a positive effect on college enrollment, which could explain the null effects on earnings if students delayed entry into the labor market. The findings across many different specifications, measures and subgroups are consistent in finding null effects. Although the results consistently show null effects, one limitation is that the estimates are not precisely estimated. In contrast, a supplementary, non-experimental analysis of CPS data suggest large, positive, and statistically significant non-experimental relationships between home computers and earnings, employment and college enrollment. These findings raise concerns about positive selection bias in non-experimental studies even including those using nearest neighbor and propensity score matching models.

The remainder of the paper is organized as follows. The next section describes the random experiment in detail. Section 2 reports estimates of treatment effects on earnings, employment, and college enrollment. Section 3 reports non-experimental estimates from the CPS. Section 4 concludes.

2. The field experiment

To study the earnings, employment and college enrollment effects of computers, we randomly assigned free computers to entering community college students who were receiving financial aid (see Fairlie & London, 2012 for more details on the experiment).[4] All of the students attended Butte College full-time in fall 2006 and were followed through 2013, capturing work while attending college and in the first several years of their careers. Butte College is a community college located in Northern California and is part of the California Community College system – the largest postsecondary system in the United States, comprised of 113 colleges, enrolling more than 2.1 million students, and serving one out of every five community college students in the United States (Chancellor's Office, 2016). In 2006, Butte College had a total enrollment of 15,709 students (Butte College, 2006).

The focus on workers who attended community colleges is important for examining computer returns for the middle- to high-end of the skill distribution. Community colleges provide a wide range of educational pathways, including workforce training and serving as a gateway to four-year colleges and universities (Bahr & Gross, 2016). Community colleges enroll about half of all students in public postsecondary institutions in the United States (Bahr & Gross, 2016).[5] Likewise, nearly half of students who complete a baccalaureate degree attended a community college at some point (National Student Clearinghouse, 2015). For community college students who do not transfer to a four-year institution, the returns to a community college education in many fields are high (see Bahr, 2014; Jepsen, Troske, & Coomes, 2014; Kane & Rouse, 1995, 1999; Leigh & Gill, 2007; Stevens, Kurlaender, & Grosz, 2015 for example). Thus, community colleges are an important educational environment in which to examine the returns to computers.

In addition, unlike many four-year institutions, community college students frequently live off-campus, commuting to school (Bahr & Gross, 2016). This limits their access to large computer labs and other on-campus computing resources, making personally owned computers potentially important for acquiring computer skills and knowledge.

The computers used in the study were provided by Computers for Classrooms, Inc., a company in Chico, California, that refurbishes computers.[6] To implement the study, we first obtained a list of all entering students in the fall of 2006 who received financial aid. In the fall 2006, there were 1042 financial aid students who were enrolled full-time. The Office of Financial Aid (OFA) at Butte College advertised the program by mailing letters to all of these full-time students on financial aid, and all subsequent correspondence with them was conducted through the OFA.

Participation in the experiment involved returning a baseline questionnaire and consent form releasing future academic records from the college for use in the study. Students who already owned computers were not excluded from participating in the lottery because their computers may have been very old, not fully functional, or lacking the latest software and hardware. The estimates of treatment effects on earnings, employment and education that we present below are not sensitive to the exclusion of these students, who represent 29% of the sample.

We received 286 responses with valid consent forms and completed questionnaires, and received enough funding to provide free computers to a randomly selected subset of 141 of these students.[7] Eligible students

[2] A growing literature examines the educational effects of home computers and generally finds mixed results (see Fuchs & Woessmann, 2004; Schmitt & Wadsworth, 2006; Fiorini, 2010; Malamud & Pop-Eleches, 2011; Vigdor, Ladd, & Martinez, 2014; Beuermann, Cristia, Cueto, Malamud, & Cruz-Aguayo, 2015; Fairlie & Robinson, 2013; Hull & Duch, 2017 for a few examples, and Bulman & Fairlie, 2016 for a review of the literature).

[3] Employment effects rarely have been examined in the literature because of the focus on computer use at work as a proxy for computer skills. One exception is Blanco and Boo (2010) who examine the effects of randomly listing ICT skills on a resume in two Latin American cities and find that it increases the probability of receiving a call back by roughly 1%.

[4] We did not provide Internet service as part of the experiment but found at the end of the study that more than 90% of the treatment group had Internet service. Estimates from the U.S. Census Bureau (2013) indicate similar level Internet subscription rates among computer owners in the United States (89–95% from 2007 to 2012).

[5] In California, the percentage is even higher, representing more than 70% of all public higher education enrollments in the state (Sengupta & Jepsen, 2006).

[6] The computers were refurbished Pentium III 450 MHz machines with 256 MB RAM, 10 GB hard drives, 17" monitors, modems, ethernet cards, CD drives, and Windows 2000 Pro Open Office (with Word, Excel and PowerPoint). Each system also came with a 128 MB flash drive for printing student papers on campus and a two-year warranty on hardware and software. Computers for Classrooms offered to replace any computer not functioning properly during the first two years after students received them.

[7] We compared administrative data for students who applied to the computer giveaway program to all students at the college who received financial aid and to all students enrolled at the college. We do not find large differences in racial composition or whether students' primary language was English. We do find gender differences, with women overrepresented among applicants to the computer giveaway program. The distributions of reported goal at college entry are very similar across groups. In sum, although study participants are a self-selected group of all students receiving financial aid, they do not appear to be very different in terms of observable characteristics from all students who received financial aid or the entire student body. Nevertheless, they may differ, however, along dimensions directly related to participation in the study, and these differences may have implications for our ability to generalize the results based on study participants to all community college students receiving financial aid. But, students with limited access to computers and financial resources are the population of most interest for any policy intervention involving the provision of free or subsidized computers. See Fairlie and London (2012) for more discussion.

were notified by mail and instructed to pick up their computers at the Computers for Classrooms warehouse. More than 90% of eligible students picked up their free computers by the end of November 2006.

Butte College provided detailed administrative data on students' course-taking and outcomes, receipt of financial aid, assessment test scores, degree completion, and other outcomes through July of 2008. Additional information about study participants, was collected in a follow-up survey in the late spring and summer of 2008.[8]

2.1. Butte college programs

Butte college offers a wide range of programs and courses. Table A.1 reports the total number of course enrollments by program type over the 2006/07–2013/14 academic years for Butte college and the California community college system. The data on course enrollments are from the California Community College Chancellor's Office, Management Information Systems Data Mart.[9] Butte college, similar to other community colleges, provides a broad range of educational opportunities for students.

2.2. Earnings and employment data

To measure earnings and employment, study participants were matched with confidential earnings data from the state's unemployment insurance (UI) system. Quarterly UI earnings data are collected by the California Employment Development Department (EDD).[10] These data cover all workers in California except those who are self-employed, civilian employees of the federal government, military, railroad employees, and a small selection of others. The data also do not address earnings garnered in other states (Feldbaum & Harmon, 2012).

In this study, we found that only 2 of the 286 participants had no earnings records in the system. Nevertheless, to explore the extent to which the exclusions from UI system data collection may result in noncoverage in our study, we examined microdata from the 2009–13 American Community Survey (ACS). We focused specifically on ACS data for individuals living in California who were between the ages of 18 and 34 years, which captures 75% of our study participants' ages over the study period. We also further restrict the sample to individuals who have AA degrees. We estimate the percentage of individuals in the ACS sample who were: (i) self-employed, (ii) federal government employees, and (iii) military employees. The largest group is self-employed workers, but they represent only 4.1% of individuals in this sample (4.2% in the subsample with AA degrees). Combining all three categories, we find that only 6.7% of individuals are in one of these three uncovered classifications (7.8% in the AA degree subsample. Furthermore, average self-employment earnings were less than $1000 per year, and average wage/salary earnings for federal and military work were roughly $1000 per year, which is expected given that these groups represent small percentages of the total workforce for this age group.

Out-migration is another concern with missing information in the administrative earnings data. If a community college student moves out of California then we cannot observe their earnings information. Using the ACS, we find that roughly 2.5% of our sample of California residents do not live in California one year later.

In sum, concerns over noncoverage appear to be low, but nevertheless we caution that earnings, as defined in this study, refer to earnings in covered jobs in California. Likewise, employment (i.e. positive earnings) refers to employment in covered jobs in California. Examining the data for our cohort of community college students, we find earnings and employment numbers that seem somewhat low (an average employment rate of 54%, quarterly earnings of $2600 for the full sample, and quarterly earnings for workers of $4900). We do not have an explanation for why these numbers seem low. It is important to note, however, that unless the treatment has a large effect on self-employment, military work, and/or mobility then noncoverage cannot have a large effect on the estimates of treatment effects on earnings and employment presented below.

2.3. College enrollment data

College enrollment in a given quarter was constructed by combining information collected in the administrative database of the California Community College (CCC) system and the database maintained by the National Student Clearinghouse (NSC), and then matching this information to study participants. The CCC system administrative database addresses all 113 community colleges in California, while the NSC database adds public and private four-year, two-year, and less-than-two-year institutions both inside and outside of California. In this analysis, we treat college enrollment as a time-varying dichotomous indicator.

2.4. Treatment and control group balance check

Table 1 reports a comparison of background characteristics for the treatment and control groups prior to the experiment. All study participants were given a baseline survey that included questions on gender, race/ethnicity, age, high school grades, household income, parents' education, and other characteristics. The average age of study participants was 25 years. More than half of the students had a parent with at least some college education, and about one-third of students reported receiving mostly *A*'s and *B*'s in high school. A little over one-quarter of study participants have children, and one-third live with their parents. As would be expected among students receiving financial aid, study participants had relatively low income at the beginning of the study, with only 17% having household incomes of $40,000 or more. The majority of study participants had household incomes below $20,000, and more than half were employed. Although not reported, the treatment and control groups were also similar in terms of educational goals reported at the time of college application.

The similarity on these baseline characteristics confirms that randomization created comparable treatment and control groups for the experiment. We do not find large differences for any of the characteristics, and none of the differences are statistically significant.

2.5. Computer skill effects

Home computers improve computer skills possibly through increased use time, flexibility, autonomy, experimentation, and learning by doing. Previous findings from the field experiment provide evidence of positive effects of home computers on computer skills (Fairlie, 2012). These findings are described in detail in Fairlie (2012), but the highlights are noted here. Information on self-reported computer skills are provided by students' responses to the follow-up survey at the end of the second academic year of the experiment. The treatment group of students receiving free computers to use at home was found to have better computer skills than did the control group of students not receiving free computers.[11] In particular, two-thirds of the treatment

[8] The response rates to the follow-up survey were 65% overall, 61% for the control group, and 69% for the treatment group. The difference in response rates is not statistically significant. The baseline characteristics of students who responded to the follow-up survey are roughly similar to those of the full sample (see Fairlie & London, 2012).

[9] The data were downloaded from http://datamart.cccco.edu/Outcomes/Program_Awards.aspx.

[10] The earnings data are also used in Bahr (2014) and Stevens, Kurlaender, and Grosz (2015) to estimate the returns to various degrees, certificates and programs in California community colleges.

[11] Students were asked ``How would you rate your computer skills?," and were given the possible responses of ``excellent," ``very good," ``good," ``satisfactory," and ``inadequate." This self-reported, five-point scale is similar to previously used measures of technology skills. Hargittai (2005) finds that self-reported measures of skill in Internet use have good predictive power for actual Internet skills.

Table 1
Baseline characteristics of study participants and balance check.

	All study participants	Treatment group	Control group	P-value for difference
Female	63.3%	64.5%	62.1%	0.666
Minority	35.7%	36.9%	34.5%	0.674
Age	25.0	24.9	25.0	0.894
Parent some college	37.8%	41.8%	33.8%	0.161
Parent college graduate	22.0%	18.4%	25.5%	0.150
High school grades Bs and Cs	56.6%	55.3%	57.9%	0.657
High school grades As and Bs	30.4%	32.6%	28.3%	0.426
Live with own children	27.3%	27.7%	26.9%	0.885
Live with parents	34.6%	31.2%	37.9%	0.234
Household income: \$10,000–19,999	31.5%	30.5%	32.4%	0.728
Household income: \$20,000–39,999	25.9%	27.7%	24.1%	0.498
Household income: \$40,000 or more	16.8%	14.9%	18.6%	0.401
Sample size	286	141	145	286

Notes: Based on baseline survey administered to all study participants or from California EDD UI administrative records.

group reported having high-level computer skills compared with only half of the control group.[12] Regression estimates controlling for baseline demographic characteristics indicated a similar treatment-control difference in high-level computer skills (the coefficient estimate is 0.17).

The finding of positive effects of home computers on computers skills also was robust to using the full range of categorical skill levels. Results from ordered probit models indicate a large, positive effect of receiving free computers on computer skills throughout the distribution.[13]

Taken together, these findings are consistent with home computers improving computer skills. These findings are also consistent with previous work using data containing information on both computer ownership and detailed computer skills. Although this evidence may be subject to selection bias, it is illustrative. For example, Atasoy, Banker, and Pavlou (2013) find that computer owners have substantially higher basic, medium and advanced computer skills than non-owners. They also find from a battery of survey questions on skill acquisition that the two most common methods of acquiring computer skills are ``Individually with experience/trial and error'' and ``With the help of your friends and family.'' Both of these methods are facilitated by having access to a computer at home.

Using survey data from the Programme for the International Assessment of Adult Competencies (PIAAC), the OECD (2015) finds a strong positive relationship between computer skills and having Internet access at home across countries. Using microdata for the United States from the same underlying survey, we estimate the correlation between computer skills and computer use at home and other non-work locations.[14] We find a strong, positive relationship between skills and home computer use.

It is important to note, however, that the estimates of treatment effects on computer skills were measured at the end of the second year of the experiment. Over time, it is likely that an increasing percentage of the control group purchased computers and improved their computer skills, allowing them to catch up with the treatment group. At the same time, with prolonged exposure the treatment group would also experience a greater improvement in computer skills over time. Unfortunately, we do not have data on computer ownership and skills over each of the subsequent years due to the prohibitive expense of collecting such data. Thus, the results of the study presented here, focusing on labor market outcomes, should be viewed as the effects of access to computers on earnings while enrolled in college and in the early career period.[15]

3. Empirical models and results

To examine the effects of computers on earnings, we estimate several regressions. The initial specification is straightforward in the context of the random experiment:

$$Y_{it} = \alpha + \beta X_i + \delta T_i + \lambda_s + u_i + \varepsilon_{it}, \tag{2.1}$$

where Y_{it} is the earnings of student i in quarter t, measured in CPI-adjusted (2013Q4) dollars. The use of earnings avoids problems with overly influential zero earnings observations using logs. Including all observations of zero earnings is essential for estimating the full treatment effect. The term X_i represents a set of time-invariant pre-treatment student characteristics, including gender, race/ethnicity, age, parents' highest education level, high school grades, presence of own children, living with parents, and family income. These controls were collected in the baseline survey administered to all study participants or extracted from administrative data provided by the college. T_i is the treatment indicator, λ_s are year fixed effects, and u + ε_{it} is the composite error term. The computers were distributed in 2006Q4, when all students were full-time entering students at the community college. The sample period covers 7 years (28 quarters) following the treatment, from 2007Q1 through 2013Q4. The effect of becoming eligible for a free computer (the ``intent-to-treat'' estimate of the program) is captured by δ.[16] In this specification, δ describes a permanent shift effect of computers on earnings; however, it is likely that computers have differential effects on earnings over time. This may be especially true when students are still enrolled in college immediately following treatment compared to a several years later when many students have completed formal schooling.

To allow for a more flexible earnings equation, in alternative Eq. (2.2) we do not restrict δ to be a one-time permanent shift in earnings. Rather, we allow the treatment effect to differ each year following the treatment.

$$Y_{it} = \alpha + \beta X_i + \sum_{s=1}^{7} \delta_s T_i + \lambda_s + u_i + \varepsilon_{it} \tag{2.2}$$

This specification allows for flexibility of computer impacts on earnings over time (i.e. a separate treatment effect for each year, $\delta_1 \ldots \delta_7$). For example, it allows for the possibility that earnings might be depressed in the first two years post treatment if there is a positive effect of computers on college enrollment.

Table 2 reports treatment effect estimates of Eqs. (2.1) and (2.2). Both equations are estimated with ordinary least squares (OLS). Robust

[12] High-level skills are defined as reporting ``excellent'' or ``very good'' computer skills.

[13] Given the categorical nature of the computer skills measure we do not estimate the labor market returns using this measure and treatment as an IV for it (which would ultimately result in a scaled up version of the treatment estimate).

[14] See also Hanushek, Schwerdt, Wiederhold, and Woessmann (2015) and Falck, Heimisch, and Wiederhold (2016) for use of the PIAAC.

[15] As shown below, employment rates are high among community college students.

[16] LATE (or IV) estimates would be larger. We do not report these estimates, however, because we cannot technically scale up the coefficients with the IV estimator due to differential and unknown timing of purchasing computers by the control group. In the initial study period from fall 2006 to spring 2008, it was found that 8% of the treatment group did not pick up their free computers from the experiment, and 28% of the control group reported obtaining a new computer on the follow-up survey collected in the summer of 2008. Fairlie and London (2012) thus report ``lower'' and ``upper'' bounds on their IV estimates for educational outcomes in the 1.5 year study period, and these were approximately 9–36% larger than the OLS estimates. Another issue for the current study is that we also would have to adjust IV estimates for each year of treatment because we are covering a much longer follow-up period than that considered by Fairlie and London. For these reasons, we focus on ITT estimates.

Table 2
Treatment effect estimates for quarterly earnings.

	Earnings (1)	Earnings (2)	Earnings (No covariates) (3)	Earnings (Top censored) (4)
Treatment (entire period)	−254.0			
	(348.3)			
1 year since treatment		357.6	443.0	345.0
		(293.5)	(304.9)	(288.7)
2 years since treatment		116.8	202.2	126.4
		(318.5)	(340.5)	(316.6)
3 years since treatment		−547.8	−462.3	−520.7
		(377.0)	(391.6)	(369.3)
4 years since treatment		−817.7*	−732.3	−756.8*
		(440.1)	(447.8)	(423.7)
5 years since treatment		−581.0	−495.6	−510.0
		(473.1)	(483.4)	(452.1)
6 years since treatment		−113.8	−28.4	−46.5
		(499.5)	(516.4)	(483.3)
7 years since treatment		−191.7	−106.2	−119.5
		(587.2)	(603.5)	(547.0)
Time dummy variable (2 years later)	205.1*	323.8**	323.8**	317.2**
	(110.3)	(151.5)	(151.4)	(148.8)
Time dummy variable (3 years later)	306.2*	752.6***	752.6***	728.4***
	(165.0)	(251.1)	(250.9)	(245.2)
Time dummy variable (4 years later)	611.8***	1191.2***	1191.2***	1129.1***
	(194.0)	(314.4)	(314.1)	(292.1)
Time dummy variable (5 years later)	693.1***	1155.8***	1155.8***	1081.1***
	(228.5)	(364.9)	(364.6)	(342.1)
Time dummy variable (6 years later)	1056.2***	1288.6***	1288.6***	1217.2***
	(252.6)	(371.6)	(371.3)	(352.2)
Time dummy variable (7 years later)	1433.8***	1704.6***	1704.6***	1567.2***
	(293.5)	(422.3)	(422.0)	(387.7)
Control mean for D.V.	2808	2808	2808	2752
Sample size	8008	8008	8008	8008

Notes: The dependent variable is quarterly earnings from California EDD UI records. Robust standard errors are reported and adjusted for multiple quarterly observations for study participants. Baseline controls include gender, race, age, parents' highest education level, high school grades, presence of own children, live with parents, and family income.

*** Significant at the 1% level.

** Significant at the 5% level.

* Significant at the 10% level.

standard errors are reported with adjustments for multiple observations per student (i.e., clustered by student). For reference, average earnings across all years for the control group is $2808. Average earnings across all years for the treatment group is similar at $2640. The difference of $168 is not statistically significant. Controlling for baseline characteristics does not change the results. Estimates from Eq. (2.1) reported in Specification 1 indicate that the point estimate on the treatment effect variable is small in magnitude and not statistically significant. These estimates do not provide evidence of an earnings differential between the control and treatment groups when averaged over the entire sample period. Furthermore, a 95% confidence interval around the point estimate rules out large positive effects. The 95% confidence interval is [−937, 429] relative to a control group mean of 2808. Unfortunately, however, the estimates are not precise enough to rule out even more effect sizes. The upper bound on the confidence interval represents 15% of the control group mean, which is not a small effect.

Specification 2 reports estimates from Eq. (2.2) that includes flexibility to earnings effects over time. The control group experienced steady growth in average earnings from $1891 in the first year since treatment to $3596 in the seventh year after treatment. Most importantly, the treatment group has similar earnings and experienced similar earnings growth over that time. None of the estimates of the treatment effects are positive and statistically significant. In fact, one of the point estimates is negative (but significant at only the $p < 0.10$ level). Thus, we do not find evidence that the computers increased earnings in any of the seven years following their distribution to students.

These results are robust to the exclusion of controls. In Specification 3 in Table 2, we remove all baseline controls. The treatment effect estimates are thus differences in means between the treatment and control groups for each year. We find very similar results, mainly that there is no evidence of positive treatment effects on earnings.

We examined different functional forms to place more structure on the time-series patterns. In both quadratic and cubic specifications, we find no differences between treatment and control groups.

The results are also not due to a few very large earnings outliers. We find that quarterly earnings exceeded $25,000 ($100,000 annualized) in only 12 person-quarters with the maximum quarterly earnings of $32,084. In Specification 4, we report estimates from Eq. (2.2) in which we censor (or top-code) the highest earnings observations to $20,000 per quarter. The treatment effect estimates are similar to those from the main specification without censoring.

Finally, we also estimate log earnings specifications (reported in Specifications 1 and 2 of Table A.2). For all zero earnings values and earnings values less than 100 we censor at log(100) to lessen the influence of zero and very small earnings observations. We find no evidence of treatment effects using the log specifications.

We also estimate quantile treatment effects. Table A.3 reports treatment effect estimates for the 50th, 60th, 70th, 80th and 90th percentiles. We do not report estimates for lower percentiles such as the 10th through 40th percentiles because earnings are equal to zero at those levels for both groups. The quantile regression estimates do not reveal treatment effects at other parts of the distribution. We do not find, for example, that computer skills have large, positive returns for workers at the high end of the earnings distribution.

We also do not find evidence of treatment effects for subgroups of the participant population. Our finding of null effects for the total sample might be masking positive effects for specific subgroups. In particular, we examine treatment effects for minorities, non-minorities, women, men, younger students, and older students. Focusing on these particular subgroups is motivated by theoretical reasons. For example, the returns to computers on earnings may differ between men and women because of different career life cycles especially for the ages contained in our sample. Minority workers might face discrimination in the labor market altering job opportunities and the trajectory of earnings. Also, differential rates of overall access to computers (i.e. the digital divide) could lead to different experiences with computers and thus returns to computers in the labor market. Younger students are likely to have less prior work experience altering their returns to computer skills. For all subgroups, we do not find clear evidence of treatment effects on earnings or employment.[17]

3.1. Net present value of earnings stream

We also estimate a discounted net present value (NPV) model for earnings in order to combine the computer effects on earnings in all follow-up years in the data. To do so, we calculate the NPV for each participant i as follows:

$$NPV_i = \sum_{q=1}^{28} \frac{1}{(1+r)^q} Y_{iq} \tag{2.3}$$

We then estimate model 2.4:

$$NPV_i = \alpha + \beta X_i + \delta T_i + u_i \tag{2.4}$$

We estimate separate models for three different annualized discount rates (r), including 0.03, 0.05, and 0.07. We use the same baseline controls as used in Eq. (2.2), but we use nominal earnings for each quarter in Eq. (2.3) instead of the inflation-adjusted earnings used in Eqs. (2.1) and (2.2) to ensure a constant a priori discount rate.

Table 3 reports estimates of the NPV regressions. The point estimates indicate lower NPV earnings among the treatment group, as compared with the control group, but the estimated differences are relatively small (roughly $5000–$6000 over a seven-year period) and are not statistically significant. Thus, focusing on NPV estimates does not change our conclusions: we do not find evidence that the computers increased earnings.

3.2. Employment and extensive margin

Focusing on the extensive margin of labor supply, we also examine computer effects on employment. Computer skills may be more important for finding employment than for obtaining higher wages or more work hours, implying that these skills work more on the extensive margin than on the intensive margin. This may be especially true while students are still enrolled in community college. For many jobs available to students, wages might be relatively fixed.

We estimate linear probability models of the dependent variable *employment*, defined as having any positive earnings in quarter q. These models are comparable to Eq. (2.2), earlier. Table 4 reports estimates for treatment effects on employment. The average employment rate over the period for the control group is 54%. The regression estimates do not indicate any differences between the treatment and control groups in employment probabilities. Marginal effects for probit and logit models are similar. Computer skills do not appear to have an effect on the extensive margin for labor supply.

3.3. Intensive margin and decomposition

For exploratory purposes, we also investigate treatment and control differences in earnings conditional on employment which sheds light on potential computer effects on the intensive margin of earnings. It is important to note, however, that we cannot interpret these estimates as causal because there is the possibility of selection into employment. Furthermore, the interpretation of estimates might be unintuitive because we could, for example, find a negative treatment effect on average conditional earnings even with positive treatment effects on average employment and earnings. This could happen if the positive effect is concentrated among new marginal workers finding employment. Fig. 1 displays treatment/control difference in conditional earnings as well as the treatment/control difference in total earnings. The patterns are similar, and we do not find evidence of a positive relationship between computers and earnings, conditional on employment (regression estimates are reported for conditional earnings in Table A.4 and log conditional earnings in Specifications 3 and 4 of Table A.2). This is also consistent with not finding treatment effects on employment, which also suggests that selection in focusing on conditional earnings was not likely to be a problem here.

To further examine the roles played by treatment-control differences in the intensive and extensive margins, we perform a decomposition. Specifically, we decompose the treatment-control difference in earnings into the part that is due to the treatment-control difference in the extensive (employment) margin and the part that is due to the treatment-control difference in the intensive (conditional earnings) margin. The decomposition in the treatment-control difference in average earnings can be expressed as:

$$\overline{Y}^T - \overline{Y}^C = [(\overline{E}^T - \overline{E}^C)\overline{Y}|E^T] + [\overline{E}^C(\overline{Y}|E^T - \overline{Y}|E^C)] \tag{2.5}$$

[17] Results are available upon request from the authors.

Table 3
Net present value of earnings stream.

	Discount rate 3% (1)	Discount rate 5% (2)	Discount rate 7% (3)
Treatment	−5900.4	−5371.8	−4894.7
	(8391.2)	(7744.4)	(7169.9)
Control mean for D.V.	66,215	61,372	57,044
Sample size	286	286	286

Notes: The dependent variable is the net present value of earnings from 2007Q1 to 2013Q4 from California EDD UI records. Robust standard errors are reported. Baseline controls include gender, race, age, parents' highest education level, high school grades, presence of own children, live with parents, and family income.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.

Table 4
Treatment effect estimates for quarterly employment.

	Employment (1)	Employment (No covariates) (2)
1 year since treatment	0.018	0.020
	(0.048)	(0.050)
2 years since treatment	−0.003	−0.001
	(0.049)	(0.051)
3 years since treatment	−0.031	−0.029
	(0.051)	(0.052)
4 years since treatment	−0.059	−0.057
	(0.051)	(0.053)
5 years since treatment	0.011	0.013
	(0.051)	(0.053)
6 years since treatment	0.023	0.025
	(0.052)	(0.054)
7 years since treatment	−0.004	−0.001
	(0.052)	(0.054)
Control mean for D.V.	0.542	0.542
Sample size	8008	8008

Notes: The dependent variable is quarterly employment, defined as having positive earnings, from California EDD UI records. Robust standard errors are reported and adjusted for multiple quarterly observations for study participants. Year dummies are included. Baseline controls include gender, race, age, parents' highest education level, high school grades, presence of own children, live with parents, and family income.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.

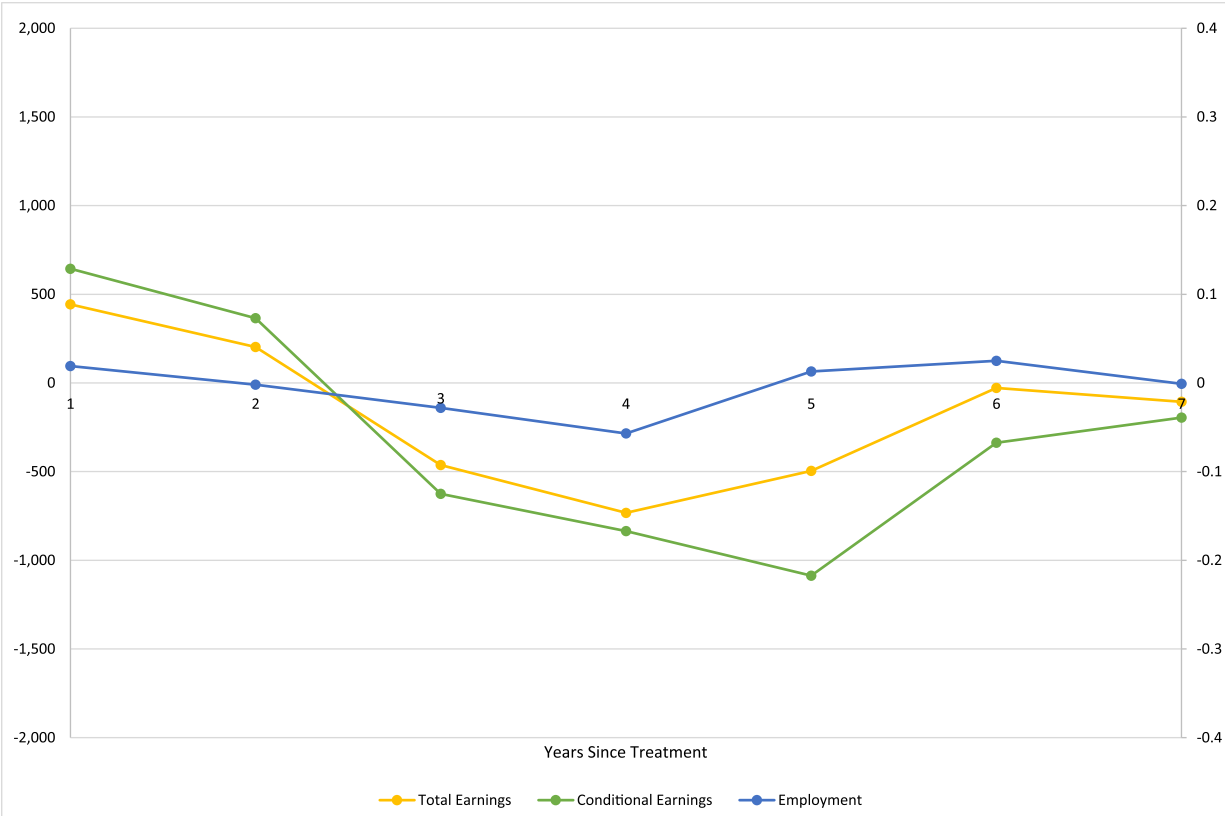

Fig. 1. Treatment/Control differences in total earnings, earnings conditional on employment, and employment.

Table 5
Decomposition of treatment-control group earnings difference.

Years since treatment	Treatment-control difference in earnings	Decomposition (4.5)		Decomposition (4.6)	
		Employment contribution	Conditional earnings contribution	Employment contribution	Conditional earnings contribution
1	443	63	380	75	368
2	202	−8	210	−8	211
3	−462	−134	−328	−116	−346
4	−732	−318	−415	−270	−462
5	−496	77	−573	63	−559
6	−28	154	−182	146	−174
7	−106	−7	−99	−7	−99

Notes: Earnings and emplloyment data are from California EDD UI records. See text for more details on decomposition.

where $\overline{E}^T$ and $\overline{E}^C$ are employment rates for the treatment and control groups, respectively, and $\overline{Y}|E^T$ and $\overline{Y}|E^C$ are the conditional earnings for the treatment and control groups, respectively. The decomposition is not unique, however, and an equally valid representation of the decomposition can be expressed as:

$$\overline{Y}^T - \overline{Y}^C = [(\overline{E}^T - \overline{E}^C)\overline{Y}|E^C] + [\overline{E}^T(\overline{Y}|E^T - \overline{Y}|E^C)] \tag{2.6}$$

In both Eqs. (2.5) and (2.6), the first term in brackets represents the part of the treatment-control earnings difference that is due to differences in employment rates, while the second term in brackets represents the part that is due to differences in average conditional earnings.

Table 5 reports the results of the decomposition. The treatment-control earnings difference is also reported for each follow-up year. The contributions from differences in conditional earnings often represent 100% of the total difference in earnings across years, but all of these differences are small. This is consistent with the finding of similar patterns in Fig. 1 for the treatment/control differences in conditional and total earnings. Given that we are finding null treatment effects on earnings, employment, and earnings conditional on employment, the decomposition technique is not overly revealing for this analysis, but nevertheless could be useful in other settings.

3.4. College enrollment

Computers and the skills acquired in using them might have a positive effect on college enrollment, which could explain the null effects on earnings and employment.[18] For example, the computers may have increased the number of terms in which students enrolled in college and thereby depressed their short-run earnings. To check for this possibility, we first estimate a linear probability model (again comparable to Eq. (2.2)) in which college enrollment is the dependent variable. The variable *college enrollment* includes all types of postsecondary institutions and was constructed by combining administrative data from the California

[18] Another possibility is that the computers change students' areas of concentration. We do not find evidence of treatment/control differences in the distribution of courses taken across departments at the community college.

Table 6
Treatment effect estimates for quarterly college enrollment and controlling for college enrollment.

	College enrollment (1)	Col. enrollment (No covariates) (2)	Earnings (3)	Employment (4)
1 year since treatment	−0.033	−0.036	330.1	0.018
	(0.035)	(0.035)	(296.5)	(0.048)
2 years since treatment	0.002	−0.002	118.3	−0.003
	(0.050)	(0.051)	(318.0)	(0.049)
3 years since treatment	−0.048	−0.052	−588.2	−0.030
	(0.051)	(0.053)	(379.9)	(0.051)
4 years since treatment	0.003	0.000	−815.0*	−0.059
	(0.051)	(0.053)	(437.0)	(0.051)
5 years since treatment	0.021	0.017	−563.7	0.010
	(0.051)	(0.052)	(467.1)	(0.051)
6 years since treatment	0.035	0.031	−84.4	0.023
	(0.046)	(0.047)	(493.0)	(0.052)
7 years since treatment	0.041	0.038	−157.0	−0.004
	(0.045)	(0.044)	(582.7)	(0.051)
College enrollment			−837.0***	0.011
			(268.8)	(0.029)
Control mean for D.V.	0.470	0.470	2808	0.542
Sample size	8008	8008	8008	8008

Notes: The dependent variable is quarterly college enrollment from administrative data from the California Community College (CCC) system and the National Student Clearinghouse (NSC) in Spefications 1 and 2. The dependent variables are quarterly earnings and employment from CA UI records in Specifications 3 and 4, respectively. Robust standard errors are reported and adjusted for multiple quarterly observations for study participants. Year dummies are included. Baseline controls include quarter dummies, gender, race, age, parents' highest education level, high school grades, presence of own children, live with parents, and family income.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.

Community College (CCC) system and data from the National Student Clearinghouse (NSC), both matched to participants in the experiment.

Table 6 reports estimates of treatment effects on college enrollment. Specification 1 reports estimates with baseline controls, and Specification 2 reports estimates without baseline controls. The average quarterly college enrollment is 47% for the control group, but this average over the seven-year time period masks a steadily declining enrollment rate from 100% in the treatment quarter (2006Q4) to 92.4% one quarter later (2007Q1) to 15.9% at the end of the sample period (2013Q4). The coefficient estimates do not reveal a pattern of higher college enrollment among the treatment group relative to the control group over the study period. None of the point estimates are statistically significant, nor are they consistently positive or negative.[19] One problem, however, is that the standard errors are not small (ranging from 0.045 to 0.051).

Examining a shorter horizon of 1.5 years after treatment, previous findings from the experiment indicate that the treatment group receiving home computers had slightly higher educational outcomes than the control group (Fairlie & London, 2012). Although we find some evidence that the treatment group achieved better educational outcomes than the control group, the estimated effects are not large and for a few measures are imprecise and cannot rule out zero effects. The estimates presented here for longer term effects on college enrollment are more clearly indicating null effects. The short run effects may have just been too small or short-lived to have any lasting effects on enrollment.

Another approach to addressing this question is to control for college enrollment directly in the earnings and employment regressions. Although controlling for contemporaneous college enrollment in the earnings regression is endogenous (because it also is potentially affected by treatment), the resulting coefficient estimates on the treatment effects are illustrative. If we were to find that the treatment effect on earnings changes dramatically with the inclusion of this control, it would be suggestive that a treatment effect on college enrollment suppresses earnings.

Specification 3 of Table 6 reports estimates for earnings. The coefficient on the college enrollment variable is negative, large, and statistically significant, as one would expect. Contemporaneous enrollment in school is associated with lower quarterly earnings. More importantly, however, the estimates of treatment effects do not change with the inclusion of this variable. The treatment effect estimates are similar when including or excluding contemporaneous college enrollment in the earnings equation.

We also estimate a model for employment with contemporaneous college enrollment included (Specification 4 in Table 6). The inclusion of contemporaneous college enrollment in the employment regression does not change the treatment effect estimates.

Collectively, these results suggest that the absence of an effect of computers on earnings or employment is not due to increased college enrollment delaying labor market entry. We do not find treatment effects on college enrollment, and controlling for contemporaneous college enrollment does not alter conclusions regarding treatment effects for earnings or employment.

4. Non-experimental estimates

Although we find null treatment effects on earnings, employment and college enrollment from the experiment, the previous literature tends to find positive estimates (Bulman & Fairlie, 2016). In this section, we investigate these differences by estimating several non-experimental earnings, employment and college enrollment regressions that include access to a home computer as an independent variable.

We estimate non-experimental earnings regressions using the 2011 Computer and Internet Supplement from the Current Population Survey.[20] Weekly earnings information is available for individuals in the outgoing rotations in the CPS, and information on home computers is available in the Computer and Internet Supplement. We start by estimating an earnings regression that includes a dummy variable for having a home computer for the full working-age population. Panel I of Table 7 reports estimates. All specifications incorporate a set of detailed

[19] We also estimate separate models for 4-year college enrollment and enrollment in other than 4-year colleges. The estimates do not provide evidence of consistent treatment effects for either type of college enrollment.

[20] The CPS, conducted by the U.S. Census Bureau for the Bureau of Labor Statistics, is representative of the entire U.S. civilian non-institutional population and interviews approximately 50,000 households. The Computer and Internet Supplements are the primary source of information collected by the Census Bureau on computer ownership.

Table 7
Non-experimental regression results for quarterly earnings.

	OLS (1)	OLS removing new computers (2)	Propensity score match (3)	Nearest neighbor (3)
Sample: Ages 18–64				
Home computer	1207.8***	1150.3***	2325.6***	1402.7***
	(148.3)	(147.6)	(181.8)	(325.6)
Sample size	20,547	17,109	20,547	20,547
Sample: Ages 18–34				
Home computer	864.5***	834.4***	1739.0***	1347.7***
	(190.7)	(191.4)	(204.0)	(320.8)
Sample size	7052	5813	7052	7052
Sample: AA degree or some college				
Home computer	747.5**	693.0**	958.8**	955.1**
	(325.0)	(326.1)	(453.0)	(460.2)
Sample size	2360	1934	2360	2360

Notes: The sample is ages 18–64, 18–34, or ages 18–34 with AA degrees or some college (no degree) from the 2011 Computer and Internet Supplement to the Current Population Survey. The dependent variable is quarterly earnings from weekly earnings in the CPS ORGs. Controls include state dummies, central city status, gender, race, age, age squared, marital status, live with parents, home ownership, detailed educational levels, and school enrollment. Specification 2 removes observations in which the newest computer is purchased in 2011.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.

controls, including state fixed effects, central city status, gender, race, age, age squared, marital status, living with parents, home ownership, detailed education level (up to 16 different codes), and school enrollment. The inclusion of detailed education levels and school enrollment raises endogeneity concerns, but it is useful for generating a conservative non-experimental estimate of the returns. The base estimates, which are reported in Specification 1, indicate that quarterly earnings (based on weekly earnings) are $1208 higher among computer owners than they are among those who do not own a computer, all else equal.

A concern about the estimated relationship using cross-sectional data is that the computers were purchased contemporaneously with earnings. To rule out this concern, we take advantage of information available in the CPS on when the newest computer was purchased. Specification 2 removes all observations in which the newest computer was purchased in the year of the survey. Thus, all computers in the new sample were purchased prior to when earnings were measured.[21] Removing these observations has little effect on the estimates.

To further investigate the question and control for unobserved heterogeneity, we estimate the relationship using nearest-neighbor and propensity score estimators (reported in Specifications 3 and 4, respectively). These models include a large number of variables to match on because of the detailed controls available in the CPS. In both cases, we find large, positive estimates of the relationship between computer ownership and earnings. These estimates are larger than those from the OLS specifications.

Establishing that there is a strong positive correlation between earnings and home computers using the full working-age population, we now turn to focused populations that more closely match our experimental population. In Panel II of Table 7 we report estimates for a sample of individuals ages 18–34, which is a range of ages that captures 75% of the experimental sample during the sample period. In Panel III, we limit this sample to only individuals reporting having an associate degree or some college, which is even more restrictive than our experimental sample. In both cases, we find large, positive and statistically significant estimates on the home computer variable. Computer owners have quarterly earnings that are roughly $700–$1700 higher than non-computer owners, all else equal.

A similar analysis for employment (reported in Table 8) also provides large, positive, and statistically significant estimates of the relationship between computer ownership and employment across all of the different samples. These estimates indicate that weekly employment rates are 7–12 percentage points higher, on average, among computer owners than they are among individuals who do not own a computer.

Table 9 reports regressions for college enrollment. College enrollment in the CPS is only defined for the age 18–24 population. Again, we find large, positive, and statistically significant estimates of the relationship between computer ownership and college enrollment across all of the specifications. These estimates indicate that college enrollment rates are 12–16 percentage points higher, on average, among computer owners than they are among individuals who do not own a computer.

These estimates of the effect of home computers on earnings, employment and college enrollment using the CPS are large, positive, and statistically significant, contrasting sharply with the estimates of null effects found in our experiment.[22] Also, although the experimental estimates reflect Intent-to-treat (ITT) estimates ``scaling them up'' will not change the null effects finding. This discrepancy raises concerns about positive selection into computer ownership resulting in an overstatement of the non-experimental estimates of the effects of home computers on various outcomes. Furthermore, controlling for a long list of independent variables, a few somewhat endogenous variables, and techniques such as nearest neighbor matching and propensity score matching to address selection does not change the conclusion. In all cases, we find large, positive and statistically significant estimates.

5. Conclusions

We provide new evidence on whether computers and the skills acquired in using them have effects on earnings, employment and college enrollment by performing a field experiment in which community college students were randomly given computers to use at home and were followed for 7 years after treatment. Restricted-access administrative data on earnings were obtained from the California State Employment Development Department (EDD) UI system records and matched to all study participants. These data allow us to study employment and earnings among covered wage/salary jobs in the state (i.e. non-federal and non-military). We do not find evidence of treatment effects (either positive or negative) on earnings. We also do not find evidence of effects on the extensive or intensive margins of labor supply. The findings of null effects are consistent across many different specifications, measures, and subgroups. One caveat, however is that although the results consistently show null effects the estimates are not precise.

Using matched restricted-access administrative data on college enrollment from the California Community College system and National Student Clearinghouse, we also do not find that computers and the skills acquired in using them increase college enrollment. This is important not only because it contributes to estimates of the effects of computers on educational outcomes, but because it suggests that the null effects of computer skills on earnings do not appear to be due to increased college enrollment. We do not find evidence of treatment effects on college enrollment in the short or medium run, and controlling for ``endogenous'' college enrollment in the earnings and employment regressions has little effect on the treatment effect estimates.

Importantly, our null effect estimates from the random experiment differ substantially from those found from an analysis of CPS data,

[21] Note that computers purchased in 2011 could be a replacement or additional computer for computers purchased earlier.

[22] We find similar results using microdata for the United States from the PIAAC. For both earnings and employment, we find large, positive and statistically significant coefficient estimates on home computer use (or other non-work use) even after controlling for detailed levels of education and numerous other variables.

Table 8
Non-experimental regression results for weekly employment.

	OLS (1)	OLS removing new computers (2)	Propensity score match (3)	Nearest neighbor (3)
Sample: Ages 18–64				
Home computer	0.091***	0.088***	0.101***	0.086***
	(0.010)	(0.010)	(0.014)	(0.018)
Sample size	20,547	17,109	20,547	20,547
Sample: Ages 18–34				
Home computer	0.090***	0.091***	0.103***	0.092**
	(0.016)	(0.017)	(0.023)	(0.046)
Sample size	7052	5813	7052	7052
Sample: AA degree or some college				
Home computer	0.074**	0.070**	0.106**	0.120***
	(0.031)	(0.032)	(0.047)	(0.046)
Sample size	2360	1934	2360	2360

Notes: The sample is ages 18–64, 18–34, or ages 18–34 with AA degrees or some college (no degree) from the 2011 Computer and Internet Supplement to the Current Population Survey. The dependent variable is positive weekly earnings (employment). Controls include state dummies, central city status, gender, race, age, age squared, marital status, live with parents, home ownership, detailed educational levels, and school enrollment. Specification 2 removes observations in which the newest computer is purchased in 2011.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.

Table 9
Non-experimental regression results for college enrollment.

	OLS (1)	OLS removing new computers (2)	Propensity score match (3)	Nearest neighbor (3)
Sample: Ages 18–24				
Home computer	0.156***	0.120***	0.130***	0.131***
	(0.022)	(0.023)	(0.031)	(0.027)
Sample size	2823	2278	2823	2823

Notes: The sample is ages 18–24 from the 2011 Computer and Internet Supplement to the Current Population Survey. The dependent variable is college enrollment. Controls include central city status, gender, race, age, age squared, marital status, live with parents, and home ownership. Specification 2 removes observations in which the newest computer is purchased in 2011.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.

raising concerns about the potential for selection bias in non-experimental estimates of returns. Estimates from regressions with detailed controls, nearest-neighbor models, and propensity score models all indicate large, positive, and statistically significant relationships between computer ownership and earnings and employment, in sharp contrast to the null effects of our experiment. It may be that non-experimental estimates overstate the labor market returns to computer skills.

Our focus in this study was on the labor market returns to computer skills among community college students. Of course, the returns to computer skills may differ for other groups, but community college students are an interesting group in their own right. They represent roughly half of all public college students in the United States and a much larger share in some states, such as California. Community colleges provide training for a wide range of jobs of which a large percentage require the use of computers at work (e.g. Table A.1). Among workers with community college degrees, 85% use a computer at work (OECD 2012).[23] On the other hand, community college students may have more limited computer skills than do four-year university students because they have less exposure to computer labs on campus and rarely have the opportunity to live on campus.[24] We might expect the labor market returns to computer skills to be higher when those skills are more limited in supply. Thus, the finding of a null effect for community college students, among whom we might expect larger effects, provides a useful test of the hypothesis. The null effects might be due to employers training workers with the technology skills required for jobs, or other skills acquired in community colleges as being much more important for employment. Alternatively, we cannot rule out the possibility that the experiment failed to detect an effect because it is difficult to perfectly alter the computer skills of workers.[25]

Still, the labor market effects of computer skills likely differ across groups and the experimental results presented here make one contribution to this body of evidence. More experimental research is needed using different groups, especially from different parts of the educational distribution.

[23] For comparison, 94% of workers with a 4-year university degree use computers at work, and 59% of workers with a high school or lower education (OECD 2012).

[24] Site visits to the campus revealed that the college has only a few very crowded computer labs. On the follow-up survey, one quarter of students reported experiencing wait times when using computers at the college.

[25] Similar to concerns regarding educational effects, the entertainment value of home computers and the Internet might also provide a distraction for users negatively affecting labor market outcomes thus offsetting potential positive computer skill effects (see Malamud & Pop-Eleches, 2011 and Bulman & Fairlie, 2016 for example).

Appendix

Tables A.1–A.4.

Table A.1
Total course enrollments by most common program types for Butte college and California community college system (2006–2014 AY).

Program type	Butte college	California system
Mathematics, General	61,225	6,750,987
English	56,528	5,783,755
Physical education	54,409	3,138,910
Psychology, General	25,281	2,592,161
Office technology/Office computer appl.	24,265	864,388
History	22,709	2,488,690
Speech communication	21,612	1,778,678
Anthropology	18,150	1,013,240
Reading	15,868	872,244
Political science	15,288	1,353,331
Nutrition, Foods, and Culinary arts	14,879	408,659
Child development/Early care and education	14,860	1,369,649
Philosophy	13,808	1,110,125
Sociology	13,570	1,400,127
Music	13,121	1,893,898
Registered nursing	12,318	634,680
Fine arts, General	11,537	578,066
Health education	11,123	1,061,805
Geography	10,217	590,159
Economics	10,095	913,615
Accounting	10,081	1,017,766
Administration of justice	9948	1,437,122
Chemistry, General	9127	1,091,433
Anatomy and physiology	8200	682,059
Spanish	8096	1,033,057
Welding technology	7645	173,561
Physical sciences, General	7196	133,079
Automotive technology	6984	383,193
Biology, General	6605	1,567,479
Cosmetology and barbering	5915	169,384
Painting and drawing	5714	397,783
Business and commerce, General	5709	594,568
Fire technology	5294	568,894
Dramatic arts	5127	592,965
Family and consumer sciences, General	4163	71,613
Agriculture technology and sciences, Gen.	4025	33,179
Physics, General	3707	441,550
Intercollegiate athletics	3528	372,338
Academic guidance	3489	352,475
Ceramics	3454	128,290
Photography	3435	161,947
Education, General	3316	61,273
Medical office technology	2959	43,974
Licensed vocational nursing	2879	118,587
Alcohol and controlled substances	2734	127,192
Film studies	2678	121,709
Plant science	2668	46,792
Business management	2646	430,541
Geology	2568	313,074
Respiratory care/Therapy	2546	63,506
Fire academy	2393	338,048
Agricultural power equipment technology	2360	15,880
Real estate	2003	265,584
Information technology, General	1982	629,209
Drafting technology	1954	164,878
Agriculture business, sales and service	1931	24,954
Computer programming	1797	350,002
Job seeking/Changing skills	1760	142,979
Other interdisciplinary studies	1729	66,857
Natural resources	1582	33,024
Creative writing	1523	60,220
Radio and television	1503	101,900
Other program types at Butte (113)	57,260	10,508,914
Other program types not at Butte	–	6,779,949
Total	673,076	68,809,948

Notes: Total course enrollments by program type are from 2006/07 to 2013/14 academic years. Only program types with 1500 or more total course enrollments at Butte College are reported. Data are from the California Community College Chancellor's Office, Management Information Systems Data Mart.

Table A.2
Treatment effect estimates for log earnings.

	Log Earnings (1)	Log earnings (No covariates) (2)	Conditional log earnings (3)	Conditional log earnings (No covariates) (4)
1 year since treatment	0.157	0.180	0.193*	0.200*
	(0.177)	(0.185)	(0.109)	(0.116)
2 years since treatment	0.073	0.095	0.155	0.173
	(0.184)	(0.193)	(0.110)	(0.118)
3 years since treatment	−0.188	−0.166	−0.141	−0.128
	(0.196)	(0.204)	(0.133)	(0.136)
4 years since treatment	−0.291	−0.269	−0.135	−0.127
	(0.203)	(0.209)	(0.132)	(0.135)
5 years since treatment	−0.019	0.003	−0.097	−0.081
	(0.206)	(0.212)	(0.139)	(0.142)
6 years since treatment	0.013	0.035	−0.139	−0.108
	(0.213)	(0.221)	(0.131)	(0.137)
7 years since treatment	−0.080	−0.058	−0.122	−0.103
	(0.219)	(0.229)	(0.130)	(0.136)
Control mean for D.V.	6.503	6.503	8.107	8.107
Sample size	8008	8008	4322	4322

Notes: The dependent variable is log quarterly earnings from California EDD UI records in Specifications 1 and 2, and log quarterly earnings conditional on employment (i.e. positive earnings) in Specifications 3 and 4. Earnings values less than 100 are censured at log(100). Robust standard errors are reported and adjusted for multiple quarterly observations for study participants. Baseline controls include gender, race, age, parents' highest education level, high school grades, presence of own children, live with parents, and family income.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.

Table A.3
Quantile treatment effect estimates for quarterly earnings.

	Earnings 50th percentile (1)	Earnings 60th percentile (2)	Earnings 70th percentile (3)	Earnings 80th percentile (4)	Earnings 90th percentile (5)
1 year since treatment	13.6	397.9	476.7	379.0	890.6
	(263.2)	(378.8)	(416.7)	(406.7)	(669.6)
2 years since treatment	73.1	404.3	330.6	391.8	465.8
	(263.2)	(378.8)	(416.7)	(406.7)	(669.6)
3 years since treatment	−91.5	−217.7	−143.7	−238.4	−568.3
	(263.2)	(378.8)	(416.7)	(406.7)	(669.6)
4 years since treatment	−88.7	−450.4	−837.2**	−698.8*	−1199.1*
	(263.2)	(378.8)	(416.7)	(406.7)	(669.6)
5 years since treatment	−20.7	64.9	−226.5	−958.1**	−939.3
	(263.2)	(378.8)	(416.7)	(406.7)	(669.6)
6 years since treatment	−28.9	397.5	399.3	−121.0	−81.3
	(263.2)	(378.8)	(416.7)	(406.7)	(669.6)
7 years since treatment	−43.4	101.7	−161.4	−393.8	63.0
	(263.2)	(378.8)	(416.7)	(406.7)	(669.6)
Sample size	8008	8008	8008	8008	8008

Notes: Quantile treatment effects are not reported for lower percentiles because earnings are zero at these percentiles. The dependent variable is quarterly earnings from California EDD UI records. Robust standard errors are reported and adjusted for multiple quarterly observations for study participants. Baseline controls include gender, race, age, parents' highest education level, high school grades, presence of own children, live with parents, and family income.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.

Table A.4
Treatment effect estimates for conditional earnings.

	Conditional earnings (1)	Conditional earnings (No covariates) (2)
1 year since treatment	572.9	639.6*
	(350.3)	(386.7)
2 years since treatment	198.9	361.4
	(377.1)	(429.4)
3 years since treatment	−792.1	−620.4
	(519.2)	(533.5)
4 years since treatment	−872.8	−835.6
	(599.9)	(630.2)
5 years since treatment	−1151.1*	−1085.3
	(656.4)	(702.4)
6 years since treatment	−534.7	−340.6
	(661.1)	(709.5)
7 years since treatment	−314.7	−188.2
	(820.0)	(860.0)
Control mean for D.V.	5182	5182
Sample size	4322	4322

Notes: The dependent variable is quarterly earnings conditional on employment (i.e. positive earnings). Robust standard errors are reported and adjusted for multiple quarterly observations for study participants. Baseline controls include gender, race, age, parents' highest education level, high school grades, presence of own children, live with parents, and family income.
***Significant at the 1% level.
**Significant at the 5% level.
*Significant at the 10% level.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.econedurev.2018.01.004.

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