Economics of Education Review 57 (2017) 66–79

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

Computers and productivity: Evidence from laptop use in the college classroom

Richard W. Patterson[a,1,*], Robert M. Patterson[b]

[a] Department of Social Sciences, United States Military Academy, United States
[b] Department of Finance, Gore School of Business, Westminster College, United States

ARTICLE INFO

Article history:
Received 26 December 2016
Revised 17 February 2017
Accepted 24 February 2017
Available online 4 March 2017

ABSTRACT

This paper evaluates the effect of classroom computer use on academic performance. Using a quasi-experimental design and administrative data, we find that computer use in college classrooms has a negative impact on course grades. Our study exploits institutional policies that generate plausibly random variation in laptop use within the classroom. Compared to students who are not affected by computer policies, students who are induced to use computers in class perform significantly worse and students who are influenced *not* to use computers perform significantly better. We find that the negative effects of computer use are concentrated among males and low-performing students and more prominent in quantitative courses.

1. Introduction

Computers have the potential to drastically improve productivity in education. Computers enable students to engage with educational software, take better notes, complete tasks more quickly, stay more organized, and instantly access a broad range of learning resources. However, as the number of Internet and computer-based distractions increase, so do concerns about student cyber-slacking and non-productive computer use. If students are sufficiently susceptible to distractions or other unproductive behaviors associated with computer use, then the costs of computer use may outweigh the benefits. As computer use in the classroom becomes pervasive, it is increasingly important to identify whether computers help or hinder student performance and what policies may lead to improved student outcomes. Despite the significant role computers play in the classroom and the low costs of implementing classroom computer policies, relatively little is known about how computer use impacts student productivity.

Several recent trends highlight why understanding the impact of computer use on student outcomes is becoming increasingly important. First, the prevalence of computer use in the classroom has increased dramatically in recent years; in 2011 57% of recent college graduates reported using a smartphone, laptop, or tablet in class at least some of the time (Parker, Lenhart, & Moore, 2011). In more recent studies, computer use is much higher. In our study, we find that 72% of students use laptops in the classroom and Carter, Greenberg, and Walker (2017) find that 79% of students use laptops.[2] Additionally, the efficacy of classroom learning is becoming increasingly important as the ratio of class to study time has increased to the point where the average college student spends more time in the classroom than studying outside of it. Babcock and Marks (2011) find that although the time college students spend in the classroom each week has remained roughly constant at about 16 h a week over the last 50 years, the time students spend outside the classroom studying each week has fallen precipitously from 24 h in 1961 to just 11 h in 2004. Also, concerns about equity motivate a study of the impact of computer use in the college classroom. African American and Hispanic students, as well as those from economically disadvantaged backgrounds, are less likely to own laptops (Lenhart, Purcell, Smith, & Zickuhr, 2010). These patterns of computer ownership may widen or shrink gaps in student outcomes depending on the effect of computers inside and outside the classroom. Finally, identifying the effects of computer use in a college classroom environment informs the potential impact of computers on productivity in settings where straightfor-

* Corresponding author.
E-mail addresses: richard.patterson@usma.edu (R.W. Patterson), rpatterson@westminstercollege.edu (R.M. Patterson).

[1] All opinions expressed in this manuscript are those of the authors and do not represent the opinions of the United States Military Academy, Department of Defense, or the United States Army. All errors are our own.

[2] Carter et al. (2017) study principles of economics classes at the United States Military Academy that prohibit and allow computer use. The 79% figure is the average computer use among classes that allow computer use.

http://dx.doi.org/10.1016/j.econedurev.2017.02.004
0272-7757/Published by Elsevier Ltd.

ward measures of productivity are unavailable, such as the workplace.

In this paper we present quasi-experimental evidence of the impact of computer use in college classrooms on academic outcomes. This study takes advantage of a college policy that requires all students to own a laptop computer, but allows individual instructors to require, allow, or ban computers in their classrooms. We conduct surveys among a subset of students (n=229) and find that students who are required to bring computers to any of their classes on a certain day are 21% more likely to use computers in laptop-optional classes than students who are not required to bring computers to any of their classes. Similarly, we find that students who are prohibited from bringing computers to at least one class on a certain day are 49% less likely to use computers in laptop-optional classes. We treat computer-use policies in same-day classes as an instrument for computer use in laptop-optional classes and compare the grades of students (n=5571, obs=32,959) who are and who are not influenced to bring computers to class by their course schedules. This approach yields reduced-form evidence that computers have a significant negative impact on course grades. We find that a requirement to bring a computer to school decreases a student's course grades in classes that allow laptops by between 0.04 and 0.05 grade points while a laptop prohibition improves a student's course grades in laptop-optional courses by between 0.05 and 0.09 grade points. When scaled by our first-stage survey results,[3] our estimates suggest that computer use decreases course grades by between 0.14 and 0.37 grade points or 0.17 and 0.46 standard deviations. Our results are robust to multiple specifications and are consistent across multiple identification strategies. Additional heterogeneity analysis suggests that the negative effect of computer use is strongest among male students and driven by weaker students as identified by their cumulative GPA. We also find that computer use has more negative effects in quantitative courses and in courses within a student's major.

Our results are compelling in light of other recent efforts to determine what improves academic performance in higher education. Need-based financial aid has a small or insignificant effect on student grade point averages (GPAs) (Denning, 2016; Goldrick-Rab, Kelchen, Harris, & Benson, 2016); performance-based financial aid may improve academic performance (e.g. Scott-Clayton, 2011), but is expensive and can generate unintended behavioral responses (Cornwell, Lee, & Mustard, 2005); and counseling services alone do not appear to significantly improve academic performance (Angrist, Lang, & Oreopoulos, 2009). In contrast, policies that limit computer use in the classroom are inexpensive and may significantly improve student academic performance.

2. Background

Our study builds on a growing literature examining the causal role of computer technology in human capital formation. This research has investigated the effects of increasing access to computer technology in schools, increasing computer access at home, and using computer instructional technology in the classroom. With regards to in-school access, Angrist and Lavy (2002) find that increasing access to computers in elementary and middle school has no measurable effect on student outcomes. Similarly, Goolsbee and Guryan (2006) find that increasing Internet access in schools has no measurable effect on student outcomes, Leuven, Lindahl, Oosterbeek, and Webbink (2007) find that increasing information communication technology (ICT) budgets has no significant effect on student outcomes, and a number of studies investigating “one laptop per student” policies find no effects on performance in k-12 classrooms (e.g. Cristia, Ibarrarán, Cueto, Santiago, & Severín, 2012; Suhr, Hernandez, Grimes, & Warschauer, 2010; Shapley, Sheehan, Maloney, & Caranikas-Walker, 2009). Regarding home computers, Fairlie and London (2012) find no significant effects of randomly providing home computers to community college students, Fairlie and Robinson (2013) conduct a large scale experiment and find no evidence of academic or behavioral effects of providing 6–10th graders with home computers, and Malamud and Pop-Eleches (2011) use regression discontinuity evidence to show that providing home computers lowered grades of 7–19 year-old Romanian students but improved their computer skills. Research investigating the efficacy of computer instructional technology (CIT) also finds mixed results. Barrow, Markman, and Rouse (2009) and Banerjee, Cole, Duflo, and Linden (2007) both find that primary school students randomly assigned to use CIT performed significantly better on post-treatment assessments, but Rouse and Krueger (2004) find that middle school students randomly assigned to use English learning CIT had no effect on reading skills.

Regarding the effect of personal computer use in the classroom in higher education, there is little consensus on how computers are likely to affect student outcomes. Proponents of computers in the classroom argue that computers improve access to information, adoption of active learning strategies, collaboration, computer literacy, and overall course performance (e.g. Gulek & Demirtas, 2005; Barak, Lipson, & Lerman, 2006). Critics of computer use in the classroom argue that computers not only generate distractions for students using the laptops but also for nearby students (e.g. Sana, Weston, & Cepeda, 2013). The potential for distraction may be particularly large for weak students, given evidence that lower cognitive ability is positively correlated with greater impatience (Dohmen, Falk, Huffman, & Sunde, 2010). Furthermore, Mueller and Oppenheimer (2014) find that the process of taking notes by hand generates better recall than by computer. These diverging opinions highlight the need to identify whether computers generally help or hinder academic performance and the degree to which the effect of computers on performance varies by student characteristics.

The existing evidence on the impact of computers in the college classroom can be broadly grouped into two categories: (1) studies that examine the correlation between computer use and academic outcomes and (2) studies that experimentally manipulate whether and how students are able to use computers in the classroom. In general, the studies that examine the correlation between computer use and academic performance in college find that students who use computers in classrooms perform worse than students who do not (e.g. Fried, 2008; Grace-Martin & Gay, 2001; Kraushaar & Novak, 2010). However, these correlational studies likely suffer from selection issues, as students who choose to use computers are likely to differ from students who choose not to use computers in important ways.

The experimental studies examining the impact of computer use in the classroom vary significantly in their purpose and scope. Several laboratory experiments have sought to identify how certain components of computer use may impact academic performance. For example, Mueller and Oppenheimer (2014) experimentally test whether the medium used in note-taking impacts recall and find that students who are randomly assigned to take notes via notepad instead of computer have significantly better recall of the information taught. Sana et al. (2013) randomly assign study participants to take lecture notes on a computer with some students randomly assigned to multitask (or complete non lecture-related web activities during the lecture) and find that multitasking reduces the academic performance of both the multitasker and those students who are able to see the multitasker's computer

[3] Survey results are collected from a subsample of 229 students in 14 classes.

screen.[4] Other studies examine the impact of providing college students free laptops. Although these studies cannot distinguish between use inside and outside the classroom, Wurst, Smarkola, and Gaffney (2008) and Fairlie (2012) find that laptops have no significant impact on academic performance and a positive impact on academic performance for minority students, respectively.

Most closely related to our study is a recent paper by Carter et al. (2017) that experimentally tests the effects of computers in actual classrooms.[5] Carter et al. (2017) randomly assign different sections of a principles of economics course at the United States Military Academy to either allow or prohibit computer use in the classroom. They find that students in sections that allow computers in the classroom perform roughly 0.2 standard deviations worse than students in sections that prohibit computer use.

While Carter et al. (2017) generate convincing causal estimates of the effect of prohibiting classroom computer use, our study compliments their study in two important ways. First, because Carter et al. (2017) randomize at the classroom level, they are unable to distinguish whether their outcomes are driven by the effects of individual computer use or by classroom-level changes induced by the treatment. For example, it is possible that laptops have no negative effects on the individuals that use them, but that the presence of laptops in the classroom creates significant distractions for students who are not using laptops, fosters an unproductive learning environment, or makes it more difficult for the instructor to engage with the class.[6] Because our study takes advantage of variation in computer use at the individual level within a classroom, we are able to isolate the individual effects of laptop use from factors that vary at the classroom level. Second, Carter et al. (2017) conduct their study within a single course in a unique learning environment. For example, a majority of students in their study are taught by active duty Army Officers and students are required to work up to 10 h for missing a single class. By studying the effects of classroom computer use across a broad range of courses in a different college environment, our paper helps address concerns about the external validity of Carter et al. (2017).

Our study contributes to the literature in the following ways: First, our study is among the first to provide causal evidence of the impact of classroom computer use on academic performance in higher education. Second, our study uses variation that applies to all class subjects and types of students, which allows for broader generalizability than previous studies and enables us to examine heterogeneous effects by course characteristics. Third, our study is unique in that the identifying variation in computer use comes from *within* the classroom. In our study, students who are and who are not influenced to use computers share the same classroom, same peers, same teacher, and experience the exact same lecture. This allows us to isolate computer use from other potential factors that may contribute to differences found in between-class designs such as peer effects, distractions generated by other student computer use, changes in teacher behavior, and other differences between classrooms unrelated to computer use.

[4] Evidence for computer spillover effects is mixed. Aguilar-Roca, Williams, and O'Dowd (2012) randomly assign certain classes to have laptop-free zones and find no impact on student performance.

[5] Hembrooke and Gay (2003) also experimentally test the impact of laptops in a college classroom and finds negative effects on student performance. However their study is limited by a very small sample (n=44) from a single class on a single day over 10 years ago.

[6] These class-level factors could potentially explain a significant portion of the effects observed in Carter et al. (2017). A number of papers find evidence of peer effects in college environments (e.g. Carrell, Fullerton, and West, 2009; Lyle, 2007). Also, Lavy and Schlosser (2011) find that two driving mechanisms of classroom peer effects are disruptions created by other students and the influence certain students have on how teachers interact with a class.

3. Research design and sample

In this study, we take advantage of a natural experiment where the probability that students at a private liberal arts college bring a laptop computer to class depends on the order of their class schedules. At this college, all students are required to have access to a laptop, but teachers may decide to (a) require laptops, (b) allow laptops, or (c) prohibit laptops in their classes.[7] Our research design is based on the hypothesis that a student's laptop use in laptop-optional classes is significantly impacted by the laptop policies in the student's other classes. Specifically, we hypothesize that students who are required to bring laptops to at least one of their classes on a certain day are more likely to bring and use laptops in laptop-optional courses that same day and students who are prohibited from bringing laptops to at least one of their classes are less likely to bring and use laptops in laptop-optional classes. For example, if a student is required to bring a laptop to her history class at 10:00 AM on Monday, she is more likely to have her laptop in her bag and use that laptop in her 11:30 AM biology class that same day. On the other hand, if the history class at 10:00 AM prohibits laptop use, then she is less likely to have the laptop in her bag and therefore less likely to use her laptop in her biology class. We also hypothesize that, after controlling for a few basic covariates, laptop policies in other classes are uncorrelated with course performance in laptop-optional courses *except* through the change in laptop use.[8] If our hypotheses hold, then (1) our natural experiment generates similar variation to a formal experiment where individual students within a classroom are randomly assigned to use computers and (2) our estimates of the impact of external course laptop policies on grades in laptop-optional courses generate unbiased estimates of the directional impact of laptop use on course performance.

3.1. Empirical design

3.1.1. Primary analysis

In our empirical design, we estimate the reduced form impact of laptop use on student academic performance using the laptop policies surrounding a student's laptop-optional classes as an instrument for laptop use.[9] This estimation strategy involves estimating the impact of two different types of policies on overall course-grades[10]: policies that require students to bring laptops to class and policies that prohibit students from bringing laptops to class. If our identifying assumptions are met, then we can infer that a positive correlation between laptop requirements and course grades (in laptop optional classes) indicates that laptops have a positive impact on student performance and that a negative correlation between laptop requirements and course grades indicates that laptops have a negative impact on student performance. For laptop prohibitions we would infer that a positive correlation be-

[7] The laptop policy states "All incoming students must have access to a laptop computer with at least Windows 7 or Snow Leopard (10.6.8)."

[8] The unique policy requirement for all students to have access to a laptop is an important factor in this assumption. If students were not required to have access to a laptop, then students would be likely to select into laptop-required and laptop-prohibited classes based on whether they had access to a laptop. Our policy environment eliminates the possibility that differential access to laptops could be driving or biasing our results.

[9] Ideally, we would observe individual student laptop use in each of the laptop optional courses in our data (32,946 student-course observations), which would allow us to estimate the effect of laptop use in a two-stage least-squares design. Because we are unable to collect laptop use at the individual course level, we instead estimate reduced form effects of laptop use using faculty policy.

[10] We also explore the effect of laptop policies on course failures. However, given that course failures only occur 3% of the time, we do not have the precision to accurately estimate the effect on course failures. Estimates of the effects of course policies on failures are presented in appendix Table A8.

tween prohibitions and course grades indicates a *negative* impact of laptop use and a negative correlation between laptop prohibitions and course grades indicate a *positive* impact of laptops on student performance.[11] With the above approach in mind, we estimate the following two equations in our main specification:

$$y_{ict} = \beta_0 + \beta_{1a} * LaptopReq_{ict} + \gamma * X_i + \lambda_{ct} + \epsilon_{ict} | LaptopAllowed = 1 \quad (1)$$

$$y_{ict} = \beta_0 + \beta_{1b} * LaptopBan_{ict} + \gamma * X_i + \lambda_{ct} + \epsilon_{ict} | LaptopAllowed = 1 \quad (2)$$

where y_{ict} is the grade received in a laptop-optional course by an individual in a specific semester, *Laptop* Req_{ict} and *Laptop* Ban_{ict} are indicators for whether a laptop was required or prohibited on at least one of the class days of the laptop-optional class,[12] X_i is a vector of demographic characteristics including controls for race, gender, age, course load, course schedule difficulty,[13] major,[14] and lagged GPA,[15] and λ_{ct} is a vector of class-term fixed effects. With the inclusion of class-term fixed effects, our estimates only compare students who are exposed to identical lectures, peers, and other classroom-specific variation. Our primary estimators of interest are β_{1a} and β_{1b}, which provide reduced-form evidence of the impact of laptop use on course grades. In each specification we cluster our standard errors at the individual level. A positive β_{1a} in Eq. ((1)) and a negative β_{1b} in Eq. (2) indicates that laptops have a positive impact on students, as students who were influenced to bring laptops to class do better and those who are dissuaded from using laptops do worse. Conversely, a negative β_{1a} and positive β_{1b} indicates that laptops have a negative impact on student outcomes.[16]

3.1.2. Heterogeneity analysis

In addition to measuring the main effects of laptop policies on academic performance, we also examine whether laptop use has a heterogeneous impact on various subgroups of students and in different types of courses. For student characteristics, we examine whether laptops have larger impact on males or females, white or non-white students, and high-performing or low-performing students as defined by their cumulative GPA.[17] For course characteristics, we explore whether laptops have a differential effect on students in quantitative or non-quantitative courses, lower or upper level courses, and courses inside or outside the student's major. Our estimates follow the structure of Eqs. ((1)) and (2) above but include interactions between student/course characteristics and laptop policies:

$$y_{ict} = \beta_0 + \beta_{1a} * LaptopReq_{ict} + \beta_{2a} * (LaptopReq_{ict} * x_i) + \gamma * X_i + \lambda_{ct} + \epsilon_{ict} | LaptopAllowed = 1 \quad (3)$$

$$y_{ict} = \beta_0 + \beta_{1b} * LaptopBan_{ict} + \beta_{2b} * (LaptopBan_{ict} * x_i) + \gamma * X_i + \lambda_{ct} + \epsilon_{ict} | LaptopAllowed = 1 \quad (4)$$

where *Laptop* $Req_{ict}{}^{*}x_i$ and *Laptop* $Ban_{ict}{}^{*}x_i$ represent interaction variables between student/course characteristics and laptop policies, and β_{2a} and β_{2b} are estimates of the differential effects of laptop policies on each subgroup of students or type of course.

3.1.3. Falsification and robustness specifications

The fidelity of our results depends on the scheduling of laptop-required and laptop-prohibited courses being unrelated to unobservable student characteristics that might influence grades. The two primary threats to our design are non-random selection into laptop-required and prohibited courses and courses with laptop policies impacting course grades in laptop optional classes through channels other than changes in laptop use. In addition to providing survey evidence that students do not select into courses based on laptop policies and showing that our estimates are robust to the inclusion of potentially confounding controls in our main specification, we also include a falsification test to determine whether our results could be driven by selection or factors other than laptop use. This falsification test takes advantage of the fact that most course observations (57%) are from Monday/Wednesday or Tuesday/Thursday courses taken by students who have both Monday/Wednesday and Tuesday/Thursday courses. We hypothesize that it is unlikely that having a laptop-required or prohibited class in a Monday/Wednesday class will affect laptop use in a Tuesday/Thursday class (and vice versa). However if our results are primarily driven by unobserved selection into laptop-required or prohibited classes, we would likely see opposite-day policies impact course grades. Therefore we run the following falsification specifications for both laptop-required and laptop-prohibited classes:

$$y_{ict} = \beta_0 + \beta_{1a} * OppositeDayLaptopReq_{ict} + \gamma * X_i + \lambda_{ct} + \epsilon_{ict} | LaptopAllowed = 1 \quad (5)$$

$$y_{ict} = \beta_0 + \beta_{1b} * OppositeDayLaptopBan_{ict} + \gamma * X_i + \lambda_{ct} + \epsilon_{ict} | LaptopAllowed = 1 \quad (6)$$

where *Opposite Day Laptop* Req_{ict} and *Opposite Day Laptop* Ban_{ict} are indicators for whether a laptop was required or prohibited on the days opposite the scheduled class and all other variables are as previously specified.[18] These specifications provide additional evidence of the validity of our primary estimation strategy.

In our final robustness exercise, we include individual fixed effects to examine the impact of laptop policies on performance in laptop-optional classes within individuals. In this setting, identification comes from individuals who have multiple non-overlapping laptop-optional courses with varying policies between the days of those classes. For example, a student who is required to bring a

[11] While our reduced form approach limits our ability to precisely identify the magnitude of effects, our first stage estimates for a subsample of students can provide a general sense of the magnitude of effect sizes.

[12] In 38 student-course observations, a student in a laptop-optional course has both same-day prohibited and required courses. While we include these observations in our analysis, our estimates are robust to omitting these observations, treating these observations as having only laptop-prohibited courses, and treating these observations as only having laptop-required courses.

[13] Course schedule difficulty is a measure of the difficulty of the courses a student takes on the same day as the laptop-optional class of interest. Difficulty for each class is determined by taking the average distance between GPA and course grade for all students enrolled in the class other than the student. Course schedule difficulty is the average across these courses.

[14] A majority of students do not have a declared major in the college's administrative system, so major is inferred from the modal course major topic area taken by a student.

[15] We are missing lagged GPA for 34% of observations. We include an indicator for missing lagged GPA in all specifications that include lagged GPA. Given the large fraction of students missing lagged GPA and the significant predictive power of lagged GPA on current grades, we want to ensure our results are not driven by how these missing values are characterized. Appendix Table A7 generates bounds for the influence of missing lagged GPAs by replacing missing lagged GPA with values from the 10th and 90th percentile of the lagged GPA distribution. Our results are robust to both upper and lower bound designations.

[16] If β_{1a} and β_{1b} move in the same direction, our estimates from Eqs. ((1)) and (2) imply contradictory impacts of laptop use, suggesting that our identification assumptions are invalid.

[17] We define high-performing students as having a cumulative GPA in the upper half of the GPA distribution.

[18] We additionally condition on *Laptop Required* = 0 in Eq. (5) and *Laptop Ban* = 0 in Eq. (6) to ensure that students are untreated on the day of the laptop-optional course. Our results, however, remain unchanged if we do not include these conditions.

Table 1
Instructor laptop policies.

Laptops optional	0.67
Laptops required	0.20
Laptops prohibited	0.04
Opinion: laptops increase learning	0.57
Opinion: laptops decrease learning	0.26
Opinion: laptops increase participation	0.31
Opinion: laptops decrease participation	0.42
Number of instructors	163

90 Faculty responded to the impact of laptop policies.

laptop to one of her Monday/Wednesday classes, has at least one laptop-optional Monday/Wednesday class, has at least one laptop-optional Tuesday/Thursday class, and is not required to bring laptops to any class on Tuesday or Thursday would provide identifying variation for this estimation. To estimate the within-student reduced form impact of laptop use on course performance, we run the following within-student specifications:

$$y_{ict} = \beta_0 + \beta_1 * LaptopReq_{ict} + \delta * X_{ict} + \lambda_c + \zeta_i + \epsilon_{ict} | LaptopAllowed = 1 \tag{7}$$

$$y_{ict} = \beta_0 + \beta_2 * LaptopBan_{ict} + \delta * X_{ict} + \lambda_c + \zeta_i + \epsilon_{ict} | LaptopAllowed = 1 \tag{8}$$

where X_{ict} is a vector of characteristics that vary by individual, course, and term including the number of same-day courses and course schedule difficulty, λ_c is a vector of course fixed effects, and ζ_i is a vector of individual effects. While this specification no longer has the attractive feature of examining variation within a classroom and relies on a much more limited source of variation than our primary analysis, it completely eliminates the possibility of our results being driven by selection bias across individuals. Therefore, this exercise generates a powerful test of one our primary identifying assumptions.

3.2. Faculty survey

Our identification strategy requires the collection of instructor classroom laptop policies. To determine the laptop polices in each class, we sent a short survey via email to each full-time faculty member. This survey asked them about their classroom laptop policy and their opinions about how computers in the classroom impacted teaching and learning.[19] Table 1 indicates that among the 72% of full-time faculty that responded to the survey, 20% require laptops, 67% allow laptops and 4% prohibit laptops.[20] Additionally, faculty indicated that classroom laptop use is prevalent – 73% of faculty reported that half or more of their students used laptops in class. In general, the faculty held positive opinions about the impact of classroom laptop use on learning. In our survey, 57% of faculty believed that laptop use in class increased learning compared to just 26% of faculty that believed that classroom laptop use decreased student learning.[21]

3.3. Missing instructor policies

Although our faculty survey identified the laptop policies of 72% of full-time instructors, we are unable to observe the laptop policies for the remaining 28% of full-time instructors and the entire population of part-time and adjunct professors. These missing data complicate our analysis as we are unable to claim with certainty that students who have no identified laptop-required or prohibited courses, but also have courses without reported policies, have all laptop-optional courses. To increase the coverage of faculty laptop policies, we use the 241 responses to our student survey to help us identify the laptop policies of instructors who did not respond to our faculty survey.

The student survey questions that we use to identify missing policies include a question that asks the number of classes a student has that ban and allow laptops on the day of the survey and another question that asks whether they have courses that ban or allow laptops on each day of the week. Given these survey answers, we use a series of logical arguments to predict laptop policies in classes with missing instructor policies.[22] We aggregate responses by instructor to ensure that the policies are consistent within an instructor. In total, this simple algorithm consistently identifies the policies of 81 additional instructors leading to a total coverage of 73% of all student-class observations. In the cases where discrepancies arise, we apply the policy with the greatest share of responses.[23]

In our primary analysis we omit observations with missing instructor policies. This approach omits a total of 15,730 student-class observations or 27% of observations. While we believe this conservative approach is the most appropriate treatment of the missing instructor policies, we want to ensure that our approach does not influence our results. The primary concern with omitting these observations is that students in classes with missing instructor policies may be systematically different from other students, and that excluding these observations may bias our results. To ensure that omitting observations with missing laptop policies is not driving our results, we compare the characteristics of students in courses with and without instructor laptop policies in appendix Table A1 and conduct our main analyses with missing courses redesignated as "laptop optional."[24] While we find that students in missing policy classes are slightly younger, have slightly lower GPAs, and take slightly fewer courses than students with laptop policies, our primary results are robust to including the redesignated missing observations and remain consistent when omitting observations from students that have courses with missing laptop policies.[25]

19 A copy of the survey is included in Appendix B. The survey was created on the Survey Monkey platform and initially distributed via email in March 2014. A follow-up survey request was sent in April 2014, and an abbreviated email simply asking for faculty to report their laptop policies was sent in March 2015.

20 The remaining 9% of faculty indicated that the laptop policy varied by class or by day.

21 The remaining 17% indicated that laptop use had no effect on learning.

22 For example, if a student reports having no laptop-required or laptop-prohibited courses, then all her courses would be categorized as laptop-optional. We aggregate all responses by instructor to determine if the categorization is consistent within instructor. If a student reports having a laptop-prohibited course on a certain day, is only missing a laptop-policy for one class, and all of her other classes on that day are either laptop-optional or laptop-required, then the missing class is categorized as laptop-prohibited. We continue with similar patterns to identify as many instructor policies as possible. The code used to identify these laptop policies will be posted in our online appendix.

23 Ties are broken by categorizing a course as "laptop optional."

24 This approach has the advantage of using all of the data, but has the disadvantage of attenuating our results toward zero if classes are miss-specified. This is because laptop policies in adjacent classes should not affect laptop use in laptop-required or laptop-prohibited courses.

25 Results including the redesignated observations can be found in appendix Table A5. A secondary concern is that some students who are "treated" with laptop policies are designated incorrectly as "untreated." Our current approach treats courses that are missing a laptop policy as "untreated." However, potential misdesignations affect how our results should be scaled. To address this possible concern, we re-run our analysis dropping observations from students that have missing courses on the same day as their laptop-allowed courses and find that our results

Table 2
Student characteristics.

Master's student	0.230
Female	0.547
Asian	0.034
African American	0.018
Hispanic or Latino	0.105
White	0.799
Other race or ethnicity	0.044
Age	24.605
	(7.379)
Number of courses	3.844
	(1.082)
Cumulative GPA	3.409
	(0.614)
Laptops allowed	0.834
Laptops required	0.147
Laptops prohibited	0.019
Missing laptop policy	0.265
Ever laptop required	0.523
Ever laptop prohibited	0.141
Grade: laptops allowed	3.386
	(0.757)
Grade: laptops required	3.453
	(0.766)
Grade: laptops prohibited	3.410
	(0.806)
Grade: missing policy	3.488
	(0.744)
Observations	5571

Standard deviations in parentheses. Observations from students over the course of 6 semesters.

3.4. Student population

The student population, described in Table 2, includes 5571 students enrolled in a private liberal arts college over the course of six semesters between 2013 and 2015.[26] This population consists of both undergraduate (77%) and master's degree students (23%). Students enrolled in this college are demographically similar to other liberal arts students, with 55% female enrollment, 80% white student enrollment, 62% receiving student loans, and 58% of students graduating within six years.[27] Students take courses with a mix of laptop policies. Among classes with recorded laptop policies, 83% of each student's courses allow laptops, 15% require laptops, and only 2% ban laptops. In our sample, 52% of students ever take a laptop-required course and 14% ever take a laptop-prohibited course.[28]

One potential confound in our study is that students who have laptop-required and laptop-prohibited courses may systematically differ from students who only have laptop-optional classes. To investigate whether student characteristics vary across laptop policies, in Table 3 we compare students who (1) have all laptop-optional classes, (2) have at least one laptop-required course and (3) have at least one laptop-prohibited course. In this table, we take students from the first semester we observe them in the course and make comparisons separately for students with Monday/Wednesday courses and Tuesday/Thursday courses.[29] We use this approach instead of aggregating policies to the individual level because students often switch from having laptop-optional or laptop-required classes to only having laptop-optional classes from day to day and term to term.[30]

With 10 out of 54 pairwise comparisons varying at the 5% level, we see more imbalance than we would expect from a randomized controlled trial. However we do not believe that this imbalance could generate systematically biased results in our study. Our unique study context requires opposing selection patterns into laptop-required and laptop-prohibited courses for selection to generate same-direction biases across estimation strategies.[31] However, we find no instances where students with a characteristic are positively and significantly selecting into laptop-required courses and negatively-selecting into laptop-prohibited courses, or vice versa. Furthermore, 2 of the 10 statistically significant differences arise from students with laptop-required or laptop-prohibited courses taking more courses than students with all laptop-optional courses. We would expect this difference to occur mechanically in our data even if laptop polices are randomly assigned, as students who take more classes are more likely to have a laptop-required courses and laptop-prohibited courses in their schedules. Finally, of the remaining eight statistically significant pairwise comparisons only one characteristic (GPA in laptop-prohibited vs. laptop-optional classroom) is significant in both Monday/Wednesday and Tuesday/Thursday samples. Given that we find reasonable balance in student characteristics across laptop policies and do not see evidence of the type of variation in our balance test that would generate systematically biased estimates of the effects of laptop use, we are confident that our estimates accurately represent the directional effect of laptop use on student performance.

3.5. Student survey

Our identification strategy relies on our hypothesis that laptop requirements and prohibitions impact computer use in laptop-optional courses. To test whether laptop use in laptop-optional classes is influenced by laptop policies in other classes, we surveyed laptop use in 14 laptop-optional classes that had significant variation in the laptop policies students were exposed to in other classes that same day. These surveyed courses are broadly representative of the courses available to students, representing nine different subject areas and every level of undergraduate course.[32] In total, we surveyed 229 students[33] and found that 73% use lap-

remain consistent when excluding these observations. The results of this analysis are reported in appendix Table A6.

[26] Semesters include: Spring 2013, Fall 2013, Spring 2014, Fall 2014, Spring 2015, and Fall 2015. Students are enrolled in an average of 3.7 semesters over this time period, with the modal number of 5 semesters.

[27] Sources: http://nces.ed.gov/programs/coe/indicator_csb.asp (accessed 26.05.16) and https://collegescorecard.ed.gov (accessed 23.11.16).

[28] A list of required and prohibited courses are provided in Appendix B.

[29] Monday/Wednesday and Tuesday/Thursday courses are the most common schedules representing a combined 59.6% of all student-course observations.

[30] This approach also minimizes mechanical differences in students who have different laptop policies. Comparing students who ever have laptop requirements and prohibitions to those who are only ever in laptop-optional classes can generate large mechanical differences between students even if assignment to laptop-optional classes is completely random. This is because persistence, which is correlated with a number of student characteristics, mechanically increases the probability that a student will ever take a laptop-required or laptop-prohibited course.

[31] If high-ability students are sorting into *both* laptop-required and laptop-prohibited courses, this would bias us towards finding a positive impact of laptop use when using laptop-required courses as an instrument for laptop use, but bias us towards finding a negative impact when using laptop-prohibited courses. Conversely, if low-ability students are sorting into both laptop-required and laptop-prohibited courses, this would bias us towards finding a negative impact of laptops when using a laptop requirements as an instrument for laptop use but bias us towards finding a positive impact of laptops when using laptop prohibitions as an instrument for laptop use.

[32] Courses include: Abstract Algebra, Calculus, Cognitive Psychology, Discrete Math, Environmental Studies, Global Health, History of Math, International Finance, Introduction to Public Health, Speech, Nutrition, Principles of Chemistry, Public Health Capstone, and Science as Knowledge. In total, we surveyed three 100-level courses, six 200-level courses, two 300-level courses, and three 400-level courses. Surveys were conducted in the middle of April 2014, towards the end of the January–May Semester.

[33] 11 students were in two surveyed classes and responded twice, yielding a total sample size of 241 responses.

Table 3
Student characteristics by laptop policies.

	Panel A: Monday/Wednesday courses						
	Optional	Required	Prohibited	1–2	1–3	2–3	Joint test
Female	0.540	0.535	0.621	0.85	0.11	0.13	0.29
Asian	0.030	0.032	0.012	0.83	0.13	0.18	0.60
African American	0.019	0.010	0.012	0.12	0.52	0.88	0.43
Hispanic or Latino	0.112	0.106	0.081	0.75	0.32	0.48	0.66
White	0.791	0.801	0.849	0.66	0.14	0.29	0.40
Other race or ethnicity	0.049	0.051	0.047	0.84	0.93	0.85	0.97
Age	21.409	22.125	21.579	0.03**	0.68	0.28	0.04**
	(4.827)	(5.891)	(3.883)				
Cumulative GPA	3.258	3.339	3.382	0.03**	0.02**	0.50	0.03**
	(0.693)	(0.638)	(0.517)				
Number of courses	4.078	4.416	4.274	0.00***	0.07	0.24	0.00***
	(1.033)	(0.980)	(1.056)				
Observations	2946	344	95	–	–	–	–
	Panel B: Tuesday/Thursday courses						
	Optional	Required	Prohibited	1–2	1–3	2–3	Joint Test
Female	0.524	0.583	0.512	0.02**	0.84	0.24	0.06*
Asian	0.025	0.045	0.031	0.07*	0.79	0.58	0.10*
African American	0.017	0.015	0.016	0.75	0.93	0.96	0.95
Hispanic or Latino	0.110	0.121	0.078	0.52	0.35	0.25	0.56
White	0.797	0.782	0.859	0.50	0.16	0.11	0.35
Other race or ethnicity	0.050	0.037	0.016	0.20	0.03**	0.24	0.24
Age	21.596	20.562	21.439	0.00***	0.68	0.03**	0.00***
	(5.008)	(3.058)	(3.341)				
Cumulative GPA	3.276	3.219	3.464	0.12	0.00***	0.00***	0.01***
	(0.671)	(0.735)	(0.416)				
Number of courses	4.062	4.333	4.220	0.00***	0.15	0.33	0.00***
	(1.041)	(0.828)	(0.994)				
Observations	2806	463	82	–	–	–	–

Standard deviations in parentheses. Stars indicate whether values are statistically significantly different from laptop allowed category levels as follows: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$. Joint Test *p*-Values are result of an F-test across treatments. Observations are at the indidual level for Monday/Wednesday and Tuesday/Thursday courses, the two most common class schedules. Categories are mutually exclusive as the very small of students have both laptop-required and laptop-prohibited classes are omitted.

Table 4
Impact of same-day laptop policies on laptop use in laptop-optional classes.

	(1)	(2)	(3)	(4)	(5)	(6)
Laptops required	0.133**	0.142**	–	–	0.112**	0.116**
	(0.054)	(0.055)	–	–	(0.056)	(0.057)
Laptops prohibited	–	–	−0.320**	−0.367**	−0.284*	−0.329**
	–	–	(0.147)	(0.158)	(0.148)	(0.160)
Constant	0.680***	–	0.749***	–	0.705***	–
	(0.038)	–	(0.030)	–	(0.042)	–
R-squared	0.017	0.146	0.024	0.161	0.035	0.172
Class FE	No	Yes	No	Yes	No	Yes
Sample size	241	241	241	241	241	241

Robust standard errors in parentheses. Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$. Observations are clustered at the individual level (229 individuals) and come from 14 classes that were surveyed about their laptop use. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day. The overall fraction of students using laptops in the classroom from our sample is 0.730.

tops in laptop-optional courses, which is consistent with faculty reports of widespread laptop use. Particularly important to our study design are the effects of same-day laptop policies on computer use in laptop-optional courses. The estimates of these effects are reported in Table 4. We find that having a laptop-required class on the same day increased the probability that a student used a laptop in class by 20.6% or 14.2 percentage points (significant at the 1% level) and having a class that prohibited laptop use on the same day decreased the probability of using a laptop by 48.9% or 36.7 percentage points (significant at the 5% level) as reported in Table 4. In columns 1, 3, and 5 of Table 4 we report raw differences and in columns 2, 4, and 6 we control for class fixed effects.[34] Including class fixed effects has no significant impact on our estimates and actually increases the absolute magnitude of our estimates. These first-stage estimates provide consistent evidence that laptop policies influence laptop-use in optional classrooms.

In addition to identifying the impact of laptop requirements on whether students use laptops in the classroom, our identification strategy also requires that the laptop policies in students' schedules are only correlated with student outcomes in optional classes

[34] We do not include additional controls because we were unable to link our survey data to administrative records that included demographic and grade information.

Table 5
Impact of laptop policies on GPA in laptop-optional courses.

	Panel A: laptops required			
	(1)	(2)	(3)	(4)
Laptops required same day	−0.039*	−0.044**	−0.049**	−0.055***
	(0.022)	(0.018)	(0.019)	(0.019)
R-squared	0.216	0.418	0.421	0.426
	Panel B: laptops prohibited			
	(1)	(2)	(3)	(4)
Laptops prohibited same day	0.092***	0.053*	0.061**	0.054*
	(0.033)	(0.028)	(0.028)	(0.028)
R-squared	0.216	0.418	0.421	0.426
Sample size	32,959	32,959	32,959	32,959
Demographic vars	No	Yes	Yes	Yes
Schedule vars	No	No	Yes	Yes
Major FE	No	No	No	Yes
Class/semester FE	Yes	Yes	Yes	Yes

Estimates in terms of grade points. "Laptop required same day" variable is an indicator for having one or more courses on the same day that requires laptop use. "Laptop prohibited same day" variable is an indicator for having one or more courses on the same day that prohibits laptop use. Robust standard errors in parentheses, clustered by individual (5571 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day. Demographic variables include sex, race, age, and lagged GPA. Schedule variables include number of same-day courses per student, course schedule difficulty, and course grade level.

through the channel of laptop use. A number of factors could lead to the laptop policies in student schedules to be correlated with outcomes through alternate channels. Some of these channels can be directly controlled. For example, while it is possible that the number of classes a student takes is related to the expected grade in a course and to the probability that a student has laptop requirements or prohibitions in their schedule, we can control for the number of courses taken. Similarly, it is possible that certain majors are more or less difficult and more or less likely to be required to use a laptop. In this case, we are able to look at variation within a class and control for previous performance and majors. However, if students are making decisions about which classes to take based on the class laptop policies, this would make it particularly challenging to ascribe our results to random variation in laptop use. To address this potential concern, we surveyed students about whether they were aware of the laptop policies in their classes prior to enrollment and whether they selected classes based on the classroom computer policies. In our survey, we found that only 22% of students were aware of the laptop policies in any of their classes prior to enrollment and that only 4% of students were both aware of any laptop polices and indicated that laptop policies had any influence on their class decisions.[35] This finding increases our confidence that selection into laptop classes is unrelated to unobserved differences among students.

4. Results

4.1. Primary analysis

Our primary estimates of the reduced-form effects of laptop policies on course grades are presented in Table 5.[36] The impact of having at least one laptop-required course on grades in laptop-optional courses, reported in Panel A, is consistently negative and significant across specifications. In column 1, where only class fixed effects are controlled, we find that having a laptop-required class is correlated with 0.04 grade point drop in laptop-optional classes (significant at the 10% level). If this negative relationship were due to selection we might expect it to dissipate when additional controls are added. However, when demographic controls,[37] schedule controls,[38] and major fixed effects are included, the estimates remain stable (between −0.04 and −0.05) and become more precisely estimated (significant at the 5%, 1% and 1%, respectively). Because laptop-required courses increase the probability of laptop use, these results suggest that laptop use significantly worsens academic performance. Scaling the results by the first-stage survey results reported in Table 4 suggests that laptop use decreases course grades by between 0.27 and 0.38 grade points, or between 0.32 and 0.46 standard deviations.

The impact of having at least one laptop-prohibited course on course grades laptop-optional courses, reported in Panel B of Table 5, suggests a similar impact of laptop use on GPA. When only course fixed effects are controlled in column 1, having a laptop-prohibited course is associated with a 0.09 grade point improvement (significant at the 1% level). When demographic, schedule, and major controls are included, the point estimates drop to between 0.05 and 0.06 grade points, but remain statistically significant around the 5% level.[39] Because having laptop-prohibited courses decrease the probability of laptop use, these estimates also suggest that laptops have a significant negative impact on grades. When we scale the laptop-prohibited results by the first stage reported in Table 4, our results suggest that laptops decrease course grades by between 0.14 and 0.25 grade points, or 0.17 and 0.30 standard deviations. In context of other studies, these effects are quite large; for example, Scott-Clayton (2011) finds that students who just qualify to receive a full-tuition scholarship at West Virginia University that requires maintaining a 3.0 GPA attain GPAs between 0.04 and 0.13 grade points higher than students who just miss the qualification cutoff.

That both laptop-required and laptop-prohibited courses predict that laptops significantly worsen academic performance and both approaches are robust to a series of controls provides compelling evidence that laptops, in fact, worsen academic performance. In addition to generating multiple points of evidence of a negative impact of laptop policies, the consistent estimates generated these opposing policies help rule out any sources of selection that are positively correlated or uncorrelated across having laptop-required and laptop-prohibited courses.

4.2. Heterogeneity analysis

Because we would like to identify whether laptops are more helpful or harmful to some populations, we test whether the impact of laptop use appears to differ by student and course characteristics. In Table 6 we explore whether treatment effects vary by student characteristics including gender, race/ethnicity, and academic ability. In columns 1 and 2 we investigate whether female students are differentially impacted by laptop use. While the coefficient on *Female*Policy* in column 1 is imprecisely estimated, it is consistent with the -0.10 point estimate in column 2 (significant at the 5% level). When this interaction effect is compared to main effect of 0.11 grade points, this suggests that the impact of laptops on course grades is largely driven by male students. This finding is consistent with Carter et al. (2017) and may be consistent with

[35] 3% indicated that laptop policies were somewhat important and 1% indicated that laptops were very important to their class decisions.

[36] These estimates are expanded to report additional covariates in appendix Tables A2 and A3.

[37] Demographic controls include sex, race, age, and lagged GPA of student.

[38] Schedule controls include the number of same-day courses taken by the student, and the average difficulty of other same-day courses.

[39] The *p*-Values on *Laptop prohibited* in columns 2 and 4 are 0.053 and 0.050, respectively.

Table 6
Heterogeneous impact of laptop policies on GPA in laptop-optional courses.

	Sex		Race/Ethnicity		GPA	
	Required	Prohibited	Required	Prohibited	Required	Prohibited
Laptop Policy	−0.062**	0.114***	−0.047**	0.066**	−0.092***	0.088*
	(0.026)	(0.045)	(0.020)	0.028	(0.028)	(0.046)
Female	0.039***	0.043***	–	–	–	–
	(0.010)	(0.010)	–	–	–	–
Female*Policy	0.013	−0.105**	–	–	–	–
	(0.032)	(0.054)	–	–	–	–
Non-white	–	–	−0.041***	−0.043***	–	–
	–	–	(0.013)	(0.013)	–	–
Non-white*Policy	–	–	−0.038	−0.057	–	–
	–	–	(0.084)	(0.084)	–	–
High GPA	–	–	–	–	0.349***	0.360***
	–	–	–	–	(0.013)	(0.013)
High GPA*Policy	–	–	–	–	0.092***	−0.091*
	–	–	–	–	(0.030)	(0.052)
R-squared	0.426	0.426	0.425	0.425	0.450	0.450
Class/semester FE	Yes	Yes	Yes	Yes	Yes	Yes
Class difficulty	Yes	Yes	Yes	Yes	Yes	Yes
Major FE	Yes	Yes	Yes	Yes	Yes	Yes
Grade level FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	32,959	32,959	32,959	32,959	32,959	32,959

Estimates in terms of grade points. "Laptop Policy" variable in "Required" columns is an indicator for having one or more courses on the same day that requires laptop use. "Laptop Policy" variable in "Prohibited" columns is an indicator for having one or more courses on the same day that prohibits laptop use. Robust standard errors in parentheses, clustered by individual (5571 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day.

research that finds young males tend to have weaker noncognitive skills (including attentiveness) than females (e.g. Cornwell, Mustard, & Van Parys, 2013; Jacob, 2002). Columns 3 and 4 show inconsistent and imprecise estimates of a differential impact of laptops by white and non-white racial/ethnic categorization, suggesting that race is not a strong predictor of response to laptop use.

Our heterogeneity analysis across student characteristics finds evidence that weaker students are most negatively affected by laptop use. In both columns 5 and 6 of Table 6, the coefficients on *High GPA*Policy* (0.09 and −0.09 respectively) are statistically significant and completely negate the coefficients on *Laptop Policy* (−0.09 and 0.09 respectively), suggesting that only weak students, as predicted by their GPAs, are negatively impact by laptop use. This result contrasts with Carter et al. (2017), who find larger impacts on strong as opposed to weak students but is consistent with Beland and Murphy (2016) who find that classroom cell-phone bans in K-12 grades benefit weak students and have no effect on stronger students.

In Table 7 we examine whether laptop policies have a heterogeneous effect across course characteristics. In columns 1 and 2 we explore whether students in quantitative or non-quantitative courses are more aversely affected by computer use in the classroom. In column 1 we find that the coefficient on the interaction between *Quantitative Course* and *Laptop Required* of −0.10 (significant at the 5% level) suggests that the negative effect of laptop is significantly larger in quantitative courses. While the coefficient on *Quantitative Course*Laptop Prohibited* of 0.03 is smaller in magnitude and not statistically significant, it is also consistent with the negative effect of laptop use being larger in quantitative courses. One potential reason for this finding is that quantitative courses may build more directly on previous concepts than non-quantitative courses and distractions might have a longer-run penalty in quantitative courses than in non-quantitative courses. Another potential explanation is that computers have a larger negative effect due to computers providing less note-taking value in quantitative courses due to the difficulty of transcribing mathematical notation.

In columns 3 and 4 of Table 7 we find no evidence of laptops having a differential effect in lower-, upper-, or master's-level courses. In columns 5 and 6, however, we find some suggestive evidence that laptops have a larger negative effect in courses within a student's major. In column 5 the coefficient on *Course in Major*Policy* of −0.06 grade points is marginally significant (at the 10% level) and while the coefficient on *Course in Major*Policy* of 0.04 in column 6 is not statistically significant it is also consistent with laptops having a more negative effect in courses within a student's major. While it may seem counterintuitive for students to be more easily distracted in major courses, one explanation for this finding is that students have a higher baseline level of attention in major courses and may therefore face a larger penalty for being distracted.

4.3. Falsification and robustness tests

Although our balance tests, survey results, and the consistency of our primary estimates all increase our confidence that our results are estimating a causal relationship between laptop use and course grades, we also corroborate these results with the falsification test described in our empirical strategy. Table 8 shows the impact of opposite day policies on course grades in laptop optional courses. If our identification assumptions hold, we expect there to be no impact of opposite day policies on course grades. This is, in fact, what we find. In columns 1 and 2, the point estimates for the impact of opposite day required courses on course grades are very close to zero (−0.006 and 0.000 grade points, respectively) and statistically insignificant. Columns 3 and 4 report the estimated impact of opposite day prohibited courses on grades and show similar patterns. The estimated impact of opposite day prohibited courses on course grades in column 3 of 0.01 is small and insignificant and the estimate with controls of −0.03 is both insignificant and in the opposite direction of our primary results. This balance

Table 7
Heterogeneous impact of laptop policies on GPA in laptop-optional courses.

	Course type		Course level		Major course	
	Required	Prohibited	Required	Prohibited	Required	Prohibited
Laptop Policy	−0.022	0.069**	−0.035	0.039	−0.034	0.119***
	(0.021)	(0.035)	(0.023)	0.041	(0.024)	(0.048)
Quantitative Course*Policy	−0.098**	0.028	–	–	–	–
	(0.041)	(0.070)	–	–	–	–
Upper Level Course*Policy	–	–	−0.062	−0.094	–	–
	–	–	(0.040)	(0.061)		
Master's Level Course*Policy	–	–	−0.018	0.060	–	–
	–	–	(0.042)	(0.121)	–	–
Course in Major	–	–	–	–	0.034***	0.028**
	–	–	–	–	(0.013)	(0.013)
Course in Major*Policy	–	–	–	–	−0.056*	0.039
	–	–	–	–	(0.034)	(0.049)
R-squared	0.426	0.242	0.425	0.232	0.425	0.425
Class/semester FE	Yes	Yes	Yes	Yes	Yes	Yes
Class difficulty	Yes	Yes	Yes	Yes	Yes	Yes
Major FE	Yes	Yes	Yes	Yes	Yes	Yes
Grade level FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample size	32,959	32,959	32,959	32,959	32,959	32,959

Estimates in terms of grade points. "Policy" variables in "Required" columns are indicators for having one or more courses on the same day that requires laptop use. "Policy" variables in "Prohibited" columns are indicators for having one or more courses on the same day that prohibits laptop use. Robust standard errors in parentheses, clustered by individual (5571 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day.

Table 8
Falsification test.

Impact of opposite day laptop policies on grades in laptop-optional courses				
	(1)	(2)	(3)	(4)
Opposite day required	−0.007	−0.000	–	–
	(0.017)	(0.017)	–	–
Opposite day prohibited	–	–	0.014	−0.033
	–	–	(0.035)	(0.024)
R-squared	0.155	0.556	0.154	0.556
Demographic controls	No	Yes	No	Yes
Class/semester FE	Yes	Yes	Yes	Yes
Major FE	No	Yes	No	Yes
Grade level FE	No	Yes	No	Yes
Sample size	17,756	17,756	19,011	19,011

Estimates in terms of grade points. Robust standard errors in parentheses, clustered by individual (3689 in columns 1 and 2 and 3836 clusters in columns 3 and 4). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The Sample in columns 1 and 2 consists of all student-class observations from students in laptop-allowed classes that do not have any laptop-required classes on class days. The reduction in sample size from 32,952 in our primary results to 17,759 in this sample is primarily due to restricting our sample to observations from students with both Monday/Wednesday and Tuesday/Thursday courses (a reduction of 13,938 observations). The remaining reduction in observations (1255) are due to omitting observations from student with laptop-required courses on the same day. The sample in columns 3 and 4 consists of all student-class observations from students in laptop-allowed classes that do not have any laptop-banned classes on class days. Opposite day required variable is an indicator for the student having at least one class on an alternate day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day.

Table 9
Impact of laptop policies on grades in laptop-optional courses.

Within student estimates			
	Panel A: laptops required		
	(1)	(2)	(3)
Laptops required same day	−0.027*	−0.010	−0.020
	(0.016)	(0.017)	(0.017)
R-squared	0.437	0.442	0.569
	Panel B: laptops prohibited		
	(1)	(2)	(3)
Laptops prohibited same day	0.016	0.036	0.053*
	(0.032)	(0.032)	(0.029)
R-squared	0.437	0.442	0.569
Sample size	32,959	32,959	32,959
Schedule vars	No	Yes	Yes
Class FE	No	No	Yes
Student FE	Yes	Yes	Yes

Estimates in terms of grade points. Robust standard errors in parentheses. Standard errors are clustered at the individual level. Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day. Schedule variables include term fixed effects, same-day course schedule difficulty, and same-day number of courses.

provides further evidence that laptops do, in fact, have a negative impact on academic performance.

Additionally, we estimate the impact of laptop policies using within-student fixed effects in Table 9, as outlined in our empirical strategy.[40] While these estimates rely on significantly less variation than our primary estimates and are less imprecisely estimated, they generally corroborate our primary results. In Panel A columns 1–3, we estimate the within-student impact of having a laptop-required class on performance in a laptop-optional classes with no controls, controls for schedule variables (term fixed effects, num-

[40] Our identifying variation in the laptop-required specifications come from the 1024 students (18% of all students) who have one or more semesters with at least one M/W or T/W laptop-optional course on the same day as a laptop-required course and at least one laptop-optional course and no laptop-required courses on the alternate day schedule. Likewise, our identification in our laptop-prohibited specifications comes from 349 students (6% of all students) who have one or more semesters with at least one M/W or T/W laptop-optional course on the same day as a laptop-prohibited course and at least one laptop-optional course and no laptop-prohibited courses on the alternate day schedule.

Table 10
Heterogeneous impact of laptop policies on grades in laptop-optional courses.

Within student estimates						
	Panel A: laptops required					
	Female	Male	Non-white	White	Low GPA	High GPA
Laptops required same day	−0.013	−0.025	−0.013	−0.022	−0.041	−0.001
	(0.024)	(0.025)	(0.040)	(0.019)	(0.030)	(0.015)
R-squared	0.571	0.563	0.611	0.553	0.500	0.305
	Panel B: laptops prohibited					
	Female	Male	Non-white	White	Low GPA	High GPA
Laptops prohibited same day	0.042	0.078	−0.068	0.070**	0.113**	−0.013
	(0.035)	(0.051)	(0.093)	(0.030)	(0.054)	(0.026)
R-squared	0.571	0.563	0.611	0.554	0.500	0.305
Sample size	17,945	15,014	5716	27,243	15,628	17,331
Schedule vars	Yes	Yes	Yes	Yes	Yes	Yes
Class FE	Yes	Yes	Yes	Yes	Yes	Yes
Student FE	Yes	Yes	Yes	Yes	Yes	Yes

Estimates in terms of grade points. Robust standard errors in parentheses. Standard errors are clustered at the individual level. Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day. Schedule variables include term fixed effects, same-day course schedule difficulty, and same-day number of courses.

ber of same-day courses, and difficulty of same-day courses), and course fixed effects, respectively. In Panel A we find consistently negative but either a marginally significant or statistically insignificant impact of laptop requirements in laptop-optional courses (between −0.01 and −0.03 grade points). These results are both consistent with laptops having a negative impact on student performance and with our primary estimates.

In Panel B of Table 9 we estimate the impact of having laptop-prohibited classes on grades in laptop-optional classes within student. While these results in Table 9 are not statistically significant, the results are consistent with our primary results. Columns 1–3 each generate positive point estimates of 0.02–0.05 grade points with the estimate of 0.05 grade points in column 3 sharing the same magnitude our primary estimate in column 4 of Table 5 and is marginally significant at the 10% level.[41] If we scale our individual fixed-effects models by our first estimates in Table 4, our laptop-required estimates suggest that laptops reduce grades by between 0.08 and 0.20 grade points and our laptop-prohibited estimates suggest laptops reduce grades by between 0.04 and 0.14 grade points. These estimates fall within the bounds of our primary estimates.

Finally, in Table 10 we examine whether the within-student effects of laptops on student performance differ by race, gender, or GPA. Our estimates of the effects across different student characteristics, while imprecisely estimated, are also consistent with our previous results. In Panel A, we find that the point estimates of laptop-required courses are more negative for male than female students (−0.03 vs. −0.01), white than non-white students (−0.02 vs. 0.01), and low-GPA than high-GPA students (−0.04 vs 0.00). These estimates, while statistically insignificant, are consistent with our heterogeneous results that suggested that the effects of laptops are most negative for male and low GPA students. Similarly, in Panel B we find that point estimates of laptop-prohibited courses are more positive for male than female students (0.08 vs. 0.04), white than non-white students (0.7 vs. −0.07), and low GPA students than high GPA students (0.11 vs. −0.01, significant at 05% level). Taken altogether, the consistency of the student fixed-effect results with our primary results furthers our confidence that laptops have a deleterious impact on student grades and that these effects are largest among male students and students with low GPAs.

5. Discussion

In this paper we present quasi-experimental evidence of the impact of classroom computer use on productivity. We leverage differences in student schedules and laptop policies to generate plausibly exogenous variation in computer use in laptop-optional courses. Our results suggest that computer use has a significant negative impact on course performance, on the scale of 0.14–0.37 grade points or 0.17–0.46 standard deviations. To put these results in context, we estimate that the impact of eliminating classroom computers on academic performance is similar to providing full-tuition incentives to maintain a 3.0 GPA (Scott-Clayton, 2011). Additionally, we find evidence that computers have the most negative impact on male and low-performing students and in quantitative and major courses. Throughout our study we find evidence consistent with a causal interpretation of our results. First, we obtain survey evidence that suggests students are unlikely to select into courses based on their laptop policies. Second, our balance test in Table 3 does not show any evidence of the type of selection that could drive systematically biased results. Third, we find consistent evidence in Table 5 that laptops reduce student performance from two different instruments–having laptop-required and laptop-prohibited courses in one's schedule. Fourth, our falsification test in Table 8, which examines the impact of opposite-day policies on course outcomes, shows no evidence of selection bias. Fifth and finally, we find that the within-student estimates reported in Tables 9 and 10 are uniformly consistent with our primary results. Thus we are confident that our results indicate that computer use worsens student academic outcomes.

Nevertheless, our results should be interpreted with some care. Our study design precludes us from directly measuring the impact of laptop use on academic performance, so we must rely on our survey results from 229 students to estimate the scale of our results. Also, as with any instrumental variable approach, our study isolates the impact of laptop use on the students who are on the

[41] As additional robustness checks, we re-run our primary analysis excluding all students who do not have any laptop-required or laptop-prohibited courses in their schedules and also re-categorizing courses with missing laptop policies as "laptop-optional" courses. Our results are robust to these alternate samples. The alternate sample results are reported in appendix Tables A4 and A5.

margin of using a laptop in class. It is possible that students who always use laptops in class could still benefit from use while those on the margin suffer. In addition, our study focuses on the effects of laptops in laptop-optional courses where instructors are unlikely to incorporate computer-based learning in the classroom. Therefore, our results may not generalize to classroom settings where instructors actively integrate computer exercises and activities. Finally, because our results are driven by variation within the classroom, we are cautious in interpreting our results from a class-level policy perspective. While our results suggest that prohibiting laptops in the classroom would benefit students who are on the margin of using laptops, we are unable to observe how classroom dynamics might change when moved to a laptop-free environment.

While our within-classroom variation is a weakness in one sense, it is a strength in another. Because treated and untreated students in our study are in the same classroom, being exposed to the exact same course with the exact same peers, we are able to directly attribute our results to personal laptop use. Our results suggest that laptop use directly worsens academic outcomes for students who choose to use them.

Our finding that students choose to use laptops in spite of significant negative academic consequences prompts a number of questions. Why are students making choices in the classroom that seem inconsistent with their long-run interests? Also, what factors are driving the negative effects of laptop use? Are the negative effects of laptop use driven by the distracting nature of computers (Sana et al., 2013), students having worse recollection when taking notes on a computer versus on paper (Mueller & Oppenheimer, 2014), or other factors such as instructors favoring students who do not use computers? The policy implications of our findings are likely to depend on the channels through which laptops effect performance. The optimal policy response to the negative impact of laptops may include prohibiting laptops in the classroom, eliminating student access to distracting programs and websites during class, or simply making both students and instructors aware of the negative effects of laptops in the classroom. With the near ubiquity of computers in the college classroom and professional workplace, research investigating how to help students and workers avoid productivity losses associated with computer use may be particularly impactful.

Appendix A. Tables

Table A1
Student characteristics in classes with and without laptop policies.

	Has policies	Missing policies	p-Value
Master's student	0.184	0.180	0.00
Female	0.543	0.551	0.35
Asian	0.032	0.031	0.51
African American	0.015	0.014	0.18
Hispanic or Latino	0.110	0.106	0.43
White	0.801	0.807	0.20
Other race or ethnicity	0.043	0.043	0.97
Age	23.670	23.539	0.00
	(6.635)	(6.763)	
Number of courses	4.117	4.212	0.00
	(1.206)	(1.237)	
Cumulative GPA	3.452	3.423	0.00
	(0.510)	(0.539)	
Observations	43,284	15,730	–

Standard deviations in parentheses. Observations from students over the course of 6 semesters.

Table A2
Impact of laptop requirements on GPA in laptop-optional courses.

	(1)	(2)	(3)	(4)
Laptops required same day	−0.039*	−0.044**	−0.049**	−0.055***
	(0.022)	(0.018)	(0.019)	(0.019)
Asian	–	−0.021	−0.022	−0.034
	–	(0.028)	(0.028)	(0.028)
African American	–	−0.112**	−0.107**	−0.108*
	–	(0.052)	(0.053)	(0.056)
Hispanic or Latino	–	−0.041**	−0.041**	−0.042**
	–	(0.017)	(0.017)	(0.017)
	–	−0.044*	−0.045*	−0.035
	–	(0.026)	(0.026)	(0.027)
Age	–	−0.002**	−0.003***	−0.003***
	–	(0.001)	(0.001)	(0.001)
Lagged GPA	–	1.086***	1.080***	1.071***
	–	(0.016)	(0.016)	(0.016)
Number of same-day courses	–	–	−0.008	−0.003
	–	–	(0.009)	(0.009)
Course difficulty	–	–	0.136***	0.097***
	–	–	(0.020)	(0.021)
R-squared	0.216	0.418	0.421	0.426
Class/semester FE	Yes	Yes	Yes	Yes
Grade level FE	No	No	Yes	Yes
Major FE	No	No	No	Yes
Sample size	32,959	32,959	32,959	32,959

Estimates in terms of grade points. Robust standard errors in parentheses, clustered by individual (5571 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p<$ 0.01, ** $p<$ 0.05, * $p<$ 0.1. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day.

Table A3
Impact of laptop prohibitions on GPA in laptop-optional courses.

	(1)	(2)	(3)	(4)
Laptops prohibited same day	0.092***	0.053*	0.061**	0.054*
	(0.033)	(0.028)	(0.028)	(0.028)
Asian	–	−0.020	−0.022	−0.034
	–	(0.028)	(0.028)	(0.028)
African American	–	−0.111**	−0.106**	−0.106*
	–	(0.052)	(0.053)	(0.056)
Hispanic or Latino	–	−0.041**	−0.042**	−0.043**
	–	(0.017)	(0.017)	(0.017)
	–	−0.045*	−0.046*	−0.036
	–	(0.026)	(0.026)	(0.027)
Age	–	−0.002**	−0.003***	−0.003***
	–	(0.001)	(0.001)	(0.001)
Lagged GPA	–	1.085***	1.080***	1.071***
	–	(0.016)	(0.016)	(0.016)
Number of same-day courses	–	–	−0.012	−0.007
	–	–	(0.009)	(0.009)
Course difficulty	–	–	0.138***	0.099***
	–	–	(0.020)	(0.021)
R-squared	0.216	0.418	0.421	0.426
Class/semester FE	Yes	Yes	Yes	Yes
Grade level FE	No	No	Yes	Yes
Major FE	No	No	No	Yes
Sample size	32,959	32,959	32,959	32,959

Estimates in terms of grade points. Robust standard errors in parentheses, clustered by individual (5571 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p<$ 0.01, ** $p<$ 0.05, * $p<$ 0.1. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day.

Table A4
Estimates of laptop policies on GPA in laptop-optional courses.

Omitting never-treated students				
	Panel A: laptops required			
	(1)	(2)	(3)	(4)
Laptops required same day	−0.068***	−0.061***	−0.060***	−0.058***
	(0.022)	(0.019)	(0.019)	(0.019)
R-squared	0.220	0.434	0.436	0.443
	Panel B: laptops prohibited			
	(1)	(2)	(3)	(4)
Laptops prohibited same day	0.073**	0.041	0.054*	0.051*
	(0.034)	(0.029)	(0.029)	(0.028)
R-squared	0.216	0.418	0.421	0.426
Sample size	21,943	21,943	21,943	21,943
Demographic vars	No	Yes	Yes	Yes
Schedule vars	No	No	Yes	Yes
Major FE	No	No	No	Yes
Class/semester FE	Yes	Yes	Yes	Yes

Estimates in terms of grade points. Robust standard errors in parentheses, clustered by individual (3232 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day. Demographic variables include sex, race, and lagged GPA. Schedule variables include number of same-day courses per student, course schedule difficulty, and course grade level.

Table A5
Estimates of laptop policies on GPA in laptop-optional courses.

Redesignating courses without laptop policies as laptop-optional				
	Panel A: laptops required			
	(1)	(2)	(3)	(4)
Laptops required same day	−0.027	−0.043***	−0.049***	−0.058***
	(0.019)	(0.016)	(0.016)	(0.016)
R-squared	0.197	0.386	0.389	0.395
	Panel B: laptops prohibited			
	(1)	(2)	(3)	(4)
Laptops prohibited same day	0.092***	0.067***	0.072***	0.068***
	(0.029)	(0.025)	(0.025)	(0.025)
R-squared	0.197	0.386	0.389	0.395
Sample size	48,679	48,679	48,679	48,679
Demographic vars	No	Yes	Yes	Yes
Schedule vars	No	No	Yes	Yes
Major FE	No	No	No	Yes
Class/semester FE	Yes	Yes	Yes	Yes

Robust Standard errors in parentheses, clustered by individual (5571 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day. Demographic variables include sex, race, and lagged GPA. Schedule variables include number of same-day courses per student, course schedule difficulty, and course grade level.

S

Table A6
Estimates of laptop policies on GPA in laptop-optional courses.

Omitting observations with missing same-day laptop policies				
	Panel A: laptops required			
	(1)	(2)	(3)	(4)
Laptops required same day	−0.030	−0.029	−0.034*	−0.032
	(0.024)	(0.020)	(0.020)	(0.021)
R-squared	0.221	0.423	0.426	0.432
	Panel B: laptops prohibited			
	(1)	(2)	(3)	(4)
Laptops prohibited same day	0.110***	0.063**	0.071**	0.066**
	(0.036)	(0.030)	(0.030)	(0.029)
R-squared	0.221	0.423	0.426	0.432
Sample size	24,527	24,527	24,527	24,527
Demographic vars	No	Yes	Yes	Yes
Schedule vars	No	No	Yes	Yes
Major FE	No	No	No	Yes
Class/semester FE	Yes	Yes	Yes	Yes

Estimates in terms of grade points. Robust Standard errors in parentheses, clustered by individual (5034 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day. Demographic variables include sex, race, age, and lagged GPA. Schedule variables include number of same-day courses per student, course schedule difficulty, and course grade level.

Table A7
Estimates of laptop policies on GPA in laptop optional courses.

Panel A: laptops required-lower bound GPA				
	(1)	(2)	(3)	(4)
Laptops required same day	−0.039*	−0.036*	−0.037*	−0.040**
	(0.022)	(0.019)	(0.019)	(0.019)
R-squared	0.216	0.391	0.393	0.399
Panel B: laptops required-upper bound GPA				
	(1)	(2)	(3)	(4)
Laptops required same day	−0.039*	−0.051***	−0.063***	−0.070***
	(0.022)	(0.019)	(0.019)	(0.019)
R-squared	0.216	0.387	0.393	0.400
Panel C: laptops prohibited-lower bound GPA				
	(1)	(2)	(3)	(4)
Laptops prohibited same day	0.092***	0.049*	0.055*	0.048*
	(0.033)	(0.028)	(0.028)	(0.028)
R-squared	0.216	0.391	0.393	0.399
Panel D: laptops prohibited-upper bound GPA				
	(1)	(2)	(3)	(4)
Laptops prohibited same day	0.092***	0.067**	0.076***	0.067**
	(0.033)	(0.028)	(0.029)	(0.028)
R-squared	0.216	0.387	0.393	0.400
Sample size	32,959	32,959	32,959	32,959
Demographic vars	No	Yes	Yes	Yes
Schedule vars	No	No	Yes	Yes
Major FE	No	No	No	Yes
Class/semester FE	Yes	Yes	Yes	Yes

Estimates in terms of grade points. Lower bound lagged GPA replaces missing lagged GPA with 10th percentile lagged GPA – 2.86. Upper bound lagged GPA replaces lagged GPA with 90th percentile GPA – 3.93. Robust standard errors in parentheses, clustered by individual (5571 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day. Demographic variables include sex, race, age, and lagged GPA. Schedule variables include number of same-day courses per student, course schedule difficulty, and course grade level.

Table A8
Estimates of laptop policies on course failure.

	Panel A: laptops required			
	(1)	(2)	(3)	(4)
Laptops required same day	−0.003	−0.002	0.006	0.007
	(0.005)	(0.005)	(0.005)	(0.005)
R-squared	0.018	0.085	0.124	0.130
	Panel B: laptops prohibited			
	(1)	(2)	(3)	(4)
Laptops prohibited same day	−0.015**	−0.010	−0.007	−0.005
	(0.007)	(0.007)	(0.007)	(0.007)
R-squared	0.018	0.085	0.124	0.130
Sample size	34362	34362	34362	34,362
Demographic vars	No	Yes	Yes	Yes
Schedule vars	No	No	Yes	Yes
Major FE	No	No	No	Yes
Class/semester FE	Yes	Yes	Yes	Yes

Estimates in terms of course failure where course failure is defined as failing, withdrawing, or receiving an incomplete grade. Robust Standard errors in parentheses, clustered by individual (5699 clusters). Stars indicate whether coefficients are statistically significantly different from zero at conventional levels as follows: *** $p<$ 0.01, ** $p<$ 0.05, * $p<$ 0.1. Laptop required variable is an indicator for the student having at least one class on the same day that requires laptop use. Laptop prohibited variable is an indicator for the student having at least one class that bans laptop use on the same day. Demographic variables include sex, race, age, and lagged GPA. Schedule variables include number of same-day courses per student, course schedule difficulty, and course grade level.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.econedurev.2017.02.004.

References

Aguilar-Roca, N. M., Williams, A. E., & O'Dowd, D. K. (2012). The impact of laptop-free zones on student performance and attitudes in large lectures. *Computers & Education, 59*(4), 1300–1308.

Angrist, J., Lang, D., & Oreopoulos, P. (2009). Incentives and services for college achievement: Evidence from a randomized trial. *American Economic Journal: Applied Economics, 1*(1), 136–163.

Angrist, J., & Lavy, V. (2002). New evidence on classroom computers and pupil learning. *The Economic Journal, 112*(482), 735–765.

Babcock, P., & Marks, M. (2011). The falling time cost of college: Evidence from half a century of time use data. *Review of Economics and Statistics, 93*(2), 468–478.

Banerjee, A. V., Cole, S., Duflo, E., & Linden, L. (2007). Remedying education: Evidence from two randomized experiments in india. *The Quarterly Journal of Economics, 122*(3), 1235–1264. doi:10.1162/qjec.122.3.1235.

Barak, M., Lipson, A., & Lerman, S. (2006). Wireless laptops as means for promoting active learning in large lecture halls. *Journal of Research on Technology in Education, 38*(3), 245–263.

Barrow, L., Markman, L., & Rouse, C. E. (2009). Technology's edge: The educational benefits of computer-aided instruction. *American Economic Journal: Economic Policy, 1*(1), 52–74. doi:10.1257/pol.1.1.52.

Beland, L.-P., & Murphy, R. (2016). Ill communication: Technology, distraction, and student performance. *Labour Economics, 41*, 61–76. http://dx.doi.org/10.1016/j.labeco.2016.04.004. SOLE/EALE conference issue 2015.

Carrell, S. E., Fullerton, R. L., & West, J. E. (2009). Does your cohort matter? Measuring peer effects in college achievement. *Journal of Labor Economics, 27*(3), 439–464.

Carter, S. P., Greenberg, K., & Walker, M. S. (2017). The impact of computer usage on academic performance: Evidence from a randomized trial at the united states military academy. *Economics of Education Review, 56*, 118–132. http://dx.doi.org/10.1016/j.econedurev.2016.12.005.

Cornwell, C., Mustard, D. B., & Van Parys, J. (2013). Noncognitive skills and the gender disparities in test scores and teacher assessments: Evidence from primary school. *Journal of Human Resources, 48*(1), 236–264.

Cornwell, C. M., Lee, K. H., & Mustard, D. B. (2005). Student responses to merit scholarship retention rules. *Journal of Human Resources, 40*(4), 895–917.

Cristia, J., Ibarrarán, P., Cueto, S., Santiago, A., & Severín, E. (2012). Technology and child development: Evidence from the one laptop per child program.

Denning, J. T. (2016). Born under a lucky star: Financial aid, college completion, labor supply, and credit constraints.

Dohmen, T., Falk, A., Huffman, D., & Sunde, U. (2010). Are risk aversion and impatience related to cognitive ability? *The American Economic Review, 100*(3), 1238–1260.

Fairlie, R. W. (2012). Academic achievement, technology and race: Experimental evidence. *Economics of Education Review, 31*(5), 663–679.

Fairlie, R. W., & London, R. A. (2012). The effects of home computers on educational outcomes: Evidence from a field experiment with community college students. *The Economic Journal, 122*(561), 727–753.

Fairlie, R. W., & Robinson, J. (2013). Experimental evidence on the effects of home computers on academic achievement among schoolchildren. *American Economic Journal: Applied Economics, 5*(3), 211–240.

Fried, C. B. (2008). In-class laptop use and its effects on student learning. *Computers & Education, 50*(3), 906–914.

Goldrick-Rab, S., Kelchen, R., Harris, D. N., & Benson, J. (2016). Reducing income inequality in educational attainment: Experimental evidence on the impact of financial aid on college completion. *American Journal of Sociology, 121*(6), 1762–1817.

Goolsbee, A., & Guryan, J. (2006). The impact of internet subsidies in public schools. *The Review of Economics and Statistics, 88*(2), 336–347.

Grace-Martin, M., & Gay, G. (2001). Web browsing, mobile computing and academic performance. *Educational Technology & Society, 4*(3), 95–107.

Gulek, J. C., & Demirtas, H. (2005). Learning with technology: The impact of laptop use on student achievement. *The Journal of Technology, Learning and Assessment, 3*(2).

Hembrooke, H., & Gay, G. (2003). The laptop and the lecture: The effects of multitasking in learning environments. *Journal of Computing in Higher Education, 15*(1), 46–64.

Jacob, B. A. (2002). Where the boys aren't: Non-cognitive skills, returns to school and the gender gap in higher education. *Economics of Education review, 21*(6), 589–598.

Kraushaar, J. M., & Novak, D. C. (2010). Examining the effects of student multitasking with laptops during the lecture. *Journal of Information Systems Education, 21*(2), 241.

Lavy, V., & Schlosser, A. (2011). Mechanisms and impacts of gender peer effects at school. *American Economic Journal: Applied Economics, 3*(2), 1–33.

Lenhart, A., Purcell, K., Smith, A., & Zickuhr, K. (2010). Social media & mobile internet use among teens and young adults. Millennials. Pew Internet & American Life Project.

Leuven, E., Lindahl, M., Oosterbeek, H., & Webbink, D. (2007). The effect of extra funding for disadvantaged pupils on achievement. *The Review of Economics and Statistics, 89*(4), 721–736.

Lyle, D. S. (2007). Estimating and interpreting peer and role model effects from randomly assigned social groups at west point. *The Review of Economics and Statistics, 89*(2), 289–299.

Malamud, O., & Pop-Eleches, C. (2011). Home computer use and the development of human capital. *The Quarterly Journal of Economics, 126*(2), 987–1027. doi:10.1093/qje/qjr008.

Mueller, P. A., & Oppenheimer, D. M. (2014). The pen is mightier than the keyboard: Advantages of longhand over laptop note taking. *Psychological Science, 25*(6), 1159–1168.

Parker, K., Lenhart, A., & Moore, K. (2011). The digital revolution and higher education: College presidents, public differ on value of online learning. Washington, D.C.: Pew Research Center.

Rouse, C. E., & Krueger, A. B. (2004). Putting computerized instruction to the test: A randomized evaluation of a "scientifically based" reading program. *Economics of Education Review, 23*(4), 323–338.

Sana, F., Weston, T., & Cepeda, N. J. (2013). Laptop multitasking hinders classroom learning for both users and nearby peers. *Computers & Education, 62*, 24–31.

Scott-Clayton, J. (2011). On money and motivation a quasi-experimental analysis of financial incentives for college achievement. *Journal of Human Resources, 46*(3), 614–646.

Shapley, K., Sheehan, D., Maloney, C., & Caranikas-Walker, F. (2009). *Evaluation of the Texas technology immersion pilot: Final outcomes for a four-year study (2004–05 to 2007–08).*. Texas Center for Educational Research.

Suhr, K. A., Hernandez, D. A., Grimes, D., & Warschauer, M. (2010). Laptops and fourth grade literacy: Assisting the jump over the fourth-grade slump. *The Journal of Technology, Learning and Assessment, 9*(5).

Wurst, C., Smarkola, C., & Gaffney, M. A. (2008). Ubiquitous laptop usage in higher education: Effects on student achievement, student satisfaction, and constructivist measures in honors and traditional classrooms. *Computers & Education, 51*(4), 1766–1783.