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DISCUSSION PAPER SERIES

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Valerie Bostwick
The Ohio State University

Stefanie Fischer
Cal Poly State University, San Luis Obispo and IZA

Matthew Lang
University of California, Riverside

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Semesters or Quarters? The Effect of the Academic Calendar on Postsecondary Student Outcomes*

We examine the impact of US colleges and universities switching from an academic quarter calendar to a semester calendar on student outcomes. Using panel data on the near universe of four-year nonprofit institutions and leveraging quasi-experimental variation in calendars across institutions and years, we show that switching from quarters to semesters negatively impacts on-time graduation rates. Event study analyses show that these negative effects persist well beyond the transition. Using detailed administrative transcript data from one large state system, we replicate this analysis at the student-level and investigate several possible mechanisms. We find shifting to a semester: (1) lowers first-year grades; (2) decreases the probability of enrolling in a full course load; and (3) delays the timing of major choice. By linking transcript data with the Unemployment Insurance system, we find minimal evidence that a semester calendar leads to increases in summer internship-type employment.

JEL Classification: I2

Keywords: education policy, academic calendar, postsecondary graduation rates, postsecondary retention

Corresponding author:
Stefanie Fischer
Department of Economics
Orfalea College of Business
California Polytechnic State University
1 Grand Avenue
San Luis Obispo, CA 93407
USA

E-mail: sjfische@calpoly.edu

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1 Introduction

Fewer than half of students seeking to obtain a bachelor's degree do so within four years of initial enrollment. In the 2010 entering cohort of college freshmen, only 60% had completed a bachelor's degree by the end of their sixth year. These low completion rates and the high average time-to-degree impose both direct and indirect costs on students and have thus compelled interest among researchers and policy makers. As such, there is a growing body of literature aimed at better understanding the causes of these less-than-ideal graduation outcomes. One hypothesis is that student-level factors such as socioeconomic status and preparation are key contributors (e.g., Bailey and Dynarski, 2011; Belley and Lochner, 2007; Carneiro and Heckman, 2002). Another line of inquiry investigates whether institution-level characteristics such as financial aid availability and resources per student play an important role (e.g., Denning et al., 2019a; Deming and Walters, 2017; Cohodes and Goodman, 2014; Bound et al., 2012, 2010; Bound and Turner, 2007; Singell, 2004).[1] We contribute to this line of research by considering how an institution's academic calendar affects graduation rates, and investigate the underlying mechanisms. We further explore the impact of the calendar change on summer internship employment.

Semesters and quarters comprise the two predominant academic calendars among postsecondary institutions in the US. Recently, a large number of institutions have converted from quarters to semesters, making the semester calendar more common and the quarter calendar increasingly rare. Such conversions have been widespread, directly affecting nearly 2 million students at 132 colleges and universities since 1987.[2] Many of these calendar adoptions were the result of state-level mandates, whereby all schools within a state system are required to convert to semesters within a specified time frame. State and university officials often assert that the reasons for adopting semesters are to improve students' academic outcomes and to increase their odds of securing summer internships; yet surprisingly little evidence exists on the effects of the academic calendar

[1] Denning et al. (2019b) show that the recent upward trend in graduation rates is correlated with standards for degree receipt. They rule out student and institutional characteristics as explanations.

[2] These statistics are the authors' calculations and are generated from the Integrated Postsecondary Education Data System (IPEDS).

on these postsecondary student outcomes.

A priori, the effects of the calendar system on student outcomes are ambiguous. A semester calendar has longer terms, requires one to take more courses per term to remain a full-time student, and operates over a different set of months than a quarter calendar. As such, semesters may be more conducive to learning and/or degree attainment as there is a longer time horizon to master complex material. They may also provide more summer internship opportunities due to their earlier end dates in the spring term. On the other hand, it is possible that the longer terms unique to semesters may allow one to become complacent or procrastinate between exams, leading to poorer performance. Moreover, the greater number of simultaneous courses in a semester term may be difficult to juggle and/or pose scheduling challenges (Section 2 provides a more complete discussion of the costs and benefits associated with each calendar).

In this paper we leverage quasi-experimental variation in the timing of the adoption of semesters across institutions to causally examine the effects of switching from a quarter calendar to a semester calendar. We implement this strategy for the near universe of nonprofit, four-year, US colleges and universities, which come from the Integrated Postsecondary Education Data System (IPEDS), and find that switching to semesters reduces on-time graduation rates by 3.7 percentage points (pp). An additional event study analysis reveals that the negative effect of a semester calendar on four-year graduation rates begins to emerge in the partially treated cohorts – those students who were in their second, third, or fourth year of enrollment when the semester calendar was adopted – and grows larger and remains negative for many years thereafter among fully treated cohorts of students. This suggests that the negative impact on graduation rates is not a temporary consequence of the transition between calendars, but is rather due to some fixed characteristic of the semester calendar itself.

We further explore the potential mechanisms for this negative effect using detailed administrative transcript data from the Ohio Longitudinal Data Archive (OLDA).[3] The public university

[3]The Ohio Longitudinal Data Archive is a project of the Ohio Education Research Center (oerc.osu.edu) and provides researchers with centralized access to administrative data. The OLDA is managed by The Ohio State University's Center for Human Resource Research (chrr.osu.edu) in collaboration with Ohio's state workforce and education agencies (ohioanalytics.gov), with those agencies providing oversight and funding. For information on OLDA sponsors,

system in Ohio is one of the largest comprehensive postsecondary systems in the nation – serving over 300,000 students annually at 13 four-year universities and 24 regional four-year branch campuses – and provides the ideal context for this study as nearly half of the system has made the conversion from quarters to semesters since 1999. The student-level analysis of these transcript data confirms that switching from quarters to semesters decreases the probability of on-time graduation. The mechanism analysis reveals that students on a semester calendar are more likely to earn a GPA that is below the 2.0 threshold for academic probation, are less likely to enroll in the recommended number of credits per year, and that these students are delaying the timing of major choice. These findings suggest that the longer terms and higher number of courses per term associated with a semester calendar are likely driving the estimated decline in on-time graduation.

Finally, we investigate the possibility that these negative academic outcomes are offset by an increase in summer internship employment – a benefit of semesters often touted by university administrators – by linking state unemployment records to the administrative transcript files. This analysis provides only limited evidence that the switch to a semester calendar improves summer employment in internship-type jobs. On the other hand, we find that student employment during the school year declines substantially following the switch to semesters.

To the best of our knowledge, this is the first paper to examine the effects of quasi-experimental changes in academic calendars on postsecondary students' outcomes, to analyze the longer-term effects of these changes, and the first to address this question at a large scale using the near universe of institutions in the US. The few existing case studies on university calendar changes focus on a small subset of schools and compare outcomes at those schools in the 1-2 year window before and after a calendar switch (Day, 1987; Matzelle et al., 1995). Gibbens et al. (2015) show that student performance in Biology coursework fell after the University of Minnesota changed from quarters to semesters in the fall of 1999. Coleman et al. (1984) find that students on semesters take fewer credit hours and are more likely to withdraw from courses, but this analysis is limited to ten universities and only three years of data. These studies provide some preliminary evidence

see http://chrr.osu.edu/projects/ohio-longitudinal-data-archive.

that the conversion from quarters to semesters might be harmful academically to certain subsets of students. We add to these findings by providing a well-identified analysis of the short- and longer-term effects of a calendar conversion on student outcomes at a national scale, as well as a detailed, student-level view of the potential underlying mechanisms.

Moreover, this study relates to a literature aimed at understanding the optimal way to structure schools and academic calendars. In recent years, economists have documented the effects of several education calendar reforms on student outcomes including: the year-round academic calendar (Depro and Rouse, 2015; McMullen and Rouse, 2012; Graves, 2010); the four-day school week (Fischer and Argyle, 2018; Anderson and Walker, 2015); and adjusting school start times (Bostwick, 2018; Cortes et al., 2012; Hinrichs, 2011; Edwards, 2012; Carrell et al., 2011).[4] For the most part, these reforms have been adopted at the elementary and secondary level in response to rapid enrollment growth and overcrowded schools. Higher education institutions face similar issues but much less is known about the optimal way to structure universities and postsecondary academic calendars.

The findings in this paper are particularly timely and policy relevant as entire university systems are currently considering switching from quarters to semesters. Contrary to the hopes of the many universities that have made the calendar shift, we find that this change leads to significantly worse academic outcomes – implying substantial economic costs for the affected students – on top of the considerable costs to the universities of enacting the policy. While a solution to the negative impact of semesters requires much further study, our analysis of the underlying mechanisms suggests that policies aimed at increasing scheduling flexibility and easing the transition of freshmen into the demands of college study may prove effective.

The remainder of the paper is organized as follows. Section 2 provides a background of the two academic calendars and includes a discussion of the potential costs and benefits associated with each. Section 3 presents the institution-level analysis: the data; the empirical framework including a discussion of the identifying assumption; and the results. Section 4 presents the individual-

[4] For more details on these reforms see Jacob and Rockoff (2011).

level analysis: a replication of the main results; a mechanism analysis; and employment findings. Section 5 concludes.

2 Background

While semesters have always been more common, quarters were first introduced to the US in 1891 at the University of Chicago. When the school was founded, the organizers decided to make it operational year round and divide it into four terms instead of the then-traditional two terms (Malone, 1946). In 1930, 75% of US institutions reported being on a semester calendar and 22% on quarters. During the 1960s several large statewide educational systems switched from semesters to quarters to accommodate enrollment booms caused by the baby boomers; i.e., most notably the University of California system. In 1970, 70% of schools operated on semesters (Day, 1987). By 1990, the share increased to 87%. Many of the recent calendar shifts occurred in the late 1990s, but the University System of Ohio began the conversion decades ago completing it recently in 2012, and many schools in the California State University system and University of California system are considering switching in the near future (Gordon, 2016). Today, about 95% of four-year institutions operate on a semester calendar.

There are at least two main differences between the two calendars that may affect students' academic performance: the length of terms and the number of courses required per term for on-track full-time enrollment. Typically, a semester academic year comprises two 15-week terms where the average full time student takes five courses per term and a quarter schedule includes three 10-week terms where students take three to four courses per term. Quarter systems also allow for a full 10-week summer term.

The most common reason institutions cite for making the switch to semesters is to synchronize schedules with other schools in the state including other colleges and universities, and community colleges (Smith, 2012). School administrators believe there are many benefits of a common schedule. Because a majority of schools operate on a semester calendar, institutions on quarters feel their students are disadvantaged when it comes to securing summer internships and studying abroad. A semester school year typically begins in late August and concludes in early May, whereas a quarter academic year runs from late September through the beginning of June. If firms center internship program dates around a semester schedule because they are more common, students who attend schools on quarters may be ineligible. Similarly, quarter system students often have to forgo a term abroad because most study abroad programs align with the semester calendar. It is also more straightforward to transfer community college credits to four-year institutions, and fewer credits are lost, when they operate on a common academic calendar.

The longer terms and more concurrent courses per term distinct to semesters may pose a cost to students in the way of scheduling flexibility. Courses may be offered less frequently and many courses are offered at less desirable times, both earlier and later class times are used by universities to accommodate the larger number of concurrent courses being offered under semesters.[5] Generally, there are fewer courses to choose from in a semester calendar and students are exposed to fewer professors.[6] This lack of flexibility could lead students to take longer to complete their degree if they are unable to schedule the appropriate courses required for graduation within a four-year window.

These attributes of semesters may also make switching majors more costly. To highlight the added cost, consider a full-time semester student who wishes to switch majors midway through her freshman year. She spends 1/8 of her four years taking prerequisites for a major she is no longer pursuing whereas had she been on a quarter schedule, she would have only given up 1/12 of her total time. Since approximately one-half of students report switching majors at least once during their undergraduate education, this might be an important channel through which a semester calendar increases time-to-graduation (Sklar, 2015).

In terms of learning, it is unclear whether the quarter or semester calendar is preferable. Students on semesters have to juggle more courses and the associated materials and deadlines at any

[5]This information comes from an interview with an administrator from Ohio State University.

[6]Although descriptive in nature, a comparison of course offerings from UCLA, which is on quarter schedule, and UC Berkeley (semester schedule) in Psychology, English and Political Science shows that UCLA offers substantially more courses in each department; 61%, 37% and 43% more, respectively (Ramzanali, Accessed: 2016-11-9).

given time. On the other hand, students on semesters have more time with instructors and more time to master complex materials. In a similar vein, because the term is longer, it is easier for a student to 'turn-it-around' if she finds herself performing poorly in the first half of the course. This may be particularly beneficial to first year students who are adjusting to college life. However, upon receiving grades at the end of a term, if a student performs poorly, it is harder for her to improve her grade point average going forward because each term carries a larger weight compared with quarter terms.

Lastly, one must consider the direct cost of switching. Switching academic calendars is often a multi-year process and can take up to four years. It is administratively costly to convert course credits from quarters to semesters and faculty have to redesign curriculum and courses to fit within the longer term. Guidance and scheduling counselors must also be re-trained to adequately advise students in the new system. Prior to their recent conversion to semesters, administrators at California State University, Los Angeles estimated that the change would cost about $7 million. This included the cost of revamped computer systems and student records, increased counseling, and changes in faculty assignments (Gordon, 2016). Sinclair Community College budgeted $1.8 million for their conversion to semesters and the switch from quarters to semesters cost Ohio State University $12.6 million (Pant, 2012).

In summary, there are a multitude of costs and benefits associated with switching from a quarter to a semester academic calendar that could affect student outcomes. Ultimately it is unclear which of these effects will dominate, ex ante, and thus, we are presented with an empirical question.

3 Institution-Level Analysis

We begin our analysis at the institution level by employing a nationally representative dataset. This approach is ideal because it allows us to document the causal impact of switching from quarters to semesters on student outcomes more broadly compared to the existing case studies.

3.1 Institution-Level Data

All data for the institution-level analysis come from the Integrated Postsecondary Education Data System (IPEDS), a branch of the National Center for Education Statistics (NCES), and comprise a school-level panel that covers the near universe of four-year, nonprofit higher education institutions within the US. Completion of the IPEDS surveys is mandatory for all postsecondary institutions that participate in Federal financial assistance programs; consequently, there is nearly full compliance. Because we are interested in four-year and six-year graduation rates, we keep only nonprofit colleges and universities that offer comparable, traditional, four-year bachelor's degrees. This includes all schools in IPEDS defined as bachelor's, master's or doctoral degree granting institutions by the Carnegie Classification system.

The final school-level dataset includes 19 cohorts of students that entered a four-year college or university between 1991 and 2010.[7] We exclude 1994 from the analysis since IPEDS did not collect four-year graduation rates for this cohort. Finally, to construct a balanced panel, we keep only institutions that report graduation rates in all 19 years (1991-2010, excluding the missing cohort of 1994).[8] The final dataset includes 731 institutions over 19 years for a total of 13,889 observations.

The two primary variables used in our analysis are the academic calendar system variable and graduation rates. The academic calendar variable which comes from the institution files of IPDES includes seven different mutually exclusive categories: (1) two 15 to 16 week semesters, (2) three 10 to 12 week quarters plus a summer quarter, (3) three 12 to 13 week trimesters without a summer term, (4) a 4-1-4 system consisting of two four month (semester) blocks with a one month, one course block, (5) nontraditional calendar systems used often for online courses, (6) calendar systems that differ by program, commonly used by vocational and occupational programs, and (7) a continuous academic calendar system that allows students to enroll at any time during the year.

[7] The most recent graduation file reported by IPEDS is for 2016, which corresponds to the 2010 entering cohort. The lag allows one to observe both four and six year graduation rates.

[8] In Appendix Table A1 we report results using the unbalanced panel and obtain similar results. In this sample, there are 1,253 institutions for a total of 22,089 observations.

We restrict our sample to include schools that are on semesters, quarters, trimesters or 4-1-4 academic calendar systems, and drop the small share that move from semesters to quarters as there are not enough of these types of calendar conversions over the sample frame to draw meaningful conclusions. Furthermore, 4-1-4 systems are recoded as semesters in our analysis as they are equivalent to two traditional semesters surrounding a single, one-month course. Trimesters and quarters are closely related in many cases and trimesters are recoded as quarters. Less than 1% of the institutions in our sample are on trimesters and 8% of the institutions are on a 4-1-4 schedule. Our results below are not sensitive to the recoding of semesters and quarters.

The main dependent variables in our analysis are four-year and six-year graduation rates. The IPEDS provides information on the incoming cohort size at each school and the number of students in the cohort that graduate within four and six years allowing us to construct four-year and six-year graduation rates for every incoming cohort since 1991. Graduation rates only include full-time students who enrolled at the institution as a freshman, and thus exclude transfer students.

Figure 1 visually displays the policy variation that we exploit; in 1991 about 87% of schools operated on a semester calendar and this increased to 95% by 2010. Table 1 reports summary statistics for the main sample. The first column of Table 1 shows that the four-year graduation rate for all students is 36%, with women having a significantly higher rate, 40%, than men, 30%. Underrepresented minority graduation rates are just below male rates at 29%. As expected, the average six-year graduation rate is much higher, 58%. We also observe several other institution-level characteristics including cohort size, in-state tuition, the number of faculty at an institution and total annual operation expenditures. The average number of full-time faculty at a university is 340, in-state tuition (without room and board) averages $11,088 and the average cohort size is 1,099 students.

The second and third columns of Table 1 report summary statistics disaggregated by school calendar, those that do not change their calendar system between 1991 and 2010 and those that change to semesters during the time period. The most striking difference between the two groups is the share of public institutions; 71% of switchers are public compared to 42% of never-switchers.

This difference also drives differences in the average cohort size (1,376 vs. 1,066) and the average in-state tuition ($7,240 vs. $11,555) between switchers and never-switchers, as public institutions typically have larger average cohorts and lower in-state tuition. The disaggregated summary statistics highlight the fact that the effect of switching is, for the most part, identified off of large public universities. The differences in means between switchers and never-switchers do not threaten the internal validity of the estimates presented in Section 3.3, as the causal interpretation of the results rely on the parallel trends assumption (see event study presented in Figure 2).

3.2 Empirical Framework: Institution-Level

We leverage quasi-experimental variation in academic calendars across institutions and years to identify the causal relationship between semester systems and graduation rates. We first employ an event study design and estimate the following equation:

$$Y_{st} = \sum_{k=-10}^{10} \theta_k G_{stk} + X'_{st}\alpha + \gamma_s + \phi_t + \rho_s * t + \varepsilon_{st} \tag{1}$$

where Y_{st} is either the four-year or six-year graduation rate for the cohort of full-time, first-time students enrolling at school s in year t. G_{stk} is an indicator for k years from the adoption of a semester system for school s in the year t (e.g., $G_{st0} = 1$ if school s converted to semesters in year t). The first fully treated cohort (those who enrolled as freshmen in the same year that a semester calendar was first adopted) is $k = 0$. The cohorts who enrolled in years $k = \{-1, -2, -3\}$ are the partially treated cohorts (i.e., those students who were already at the institution enrolled in their second, third, or fourth year when the semester calendar was first adopted). The omitted category is the last untreated cohort, $k = -4$.

We restrict the effect of treatment on all cohorts who enrolled more than 10 years before or after the calendar switch to semesters to be unchanging, so that θ_{-10} and θ_{10} represent the average effect 10 or more years prior to or after the calendar switch, respectively.[9] There are a total of 25

[9]For schools that are "always treated", we do not observe the year of adopting a semester calendar (or if the school was ever on a quarter calendar). We include these schools in the $k = 10$ group for all years. However, this might lead to classification errors if these schools switched to semesters less than 10 years before the start of our sample. In Appendix Figure A1, we show that our results are robust to dropping the first 10 years of IPEDS data where these classification errors might occur.

pre-policy years and 22 post years in the sample. The vector X_{st} includes time-varying university level controls such as in-state tuition, number of full time equivalent faculty, and annual operation costs.[10] The variables γ_s and ϕ_t are university and year fixed effects, respectively. The model also includes university-specific linear time trends, $\rho_s * t$. All regressions are weighted by average cohort size and standard errors are clustered by institution.[11]

We also employ a difference-in-differences approach and estimate a model similar to Eq. (1), but which groups cohorts into 3 categories. This strategy provides more power to detect average treatment effects. We estimate the following equation:

$$Y_{st} = \beta_1 G1_{st} + \beta_2 G2_{st} + X_{st}'\alpha + \gamma_s + \phi_t + \rho_s * t + \varepsilon_{st}. \tag{2}$$

In this model, $G1_{st}$ is an indicator for the partially treated cohorts. In specifications where the outcome variable is four-year graduation rates, this includes cohorts who enrolled at university s, one to three years before the adoption of semesters ($G1_{st} = \bigcup_{k=-3}^{-1} G_{stk}$). In specifications where the outcome variable is six-year graduation rates, this includes cohorts who enrolled 1-5 years before the switch to semesters ($G1_{st} = \bigcup_{k=-5}^{-1} G_{stk}$). The indicator $G2_{st}$ is equal to one for fully treated cohorts ($G2_{st} = \bigcup_{k=0}^{22} G_{stk}$); that is, if university s is using a semester calendar when the cohort first enrolls in year t. The omitted category includes all untreated cohorts. All other variables are the same as in Eq. (1).

The identifying assumption for estimating the effect of a semester calendar is that the adoption of the semester calendar is uncorrelated with other unobserved time-varying determinants of four-year and six-year graduation rates. The inclusion of institution and year fixed effects controls for time-invariant institution-level variables and overall time trends that might affect graduation rates. Moreover, by including institution-specific linear time trends, we control for differential trends in graduation rates across institutions over time.

While the identifying assumption is not directly testable, several indirect tests support its plausibility. Suppose institutions enact policies such as a calendar change, aimed at improving student

[10] One could be concerned with the inclusion of time-varying controls, particularly if they are affected by the calendar switch. We show in Table 3 that the results are robust to the exclusion of these controls.

[11] Table A2 shows that the results are not sensitive to the inclusion of weights.

outcomes, in response to falling graduation rates. A pre-existing trend of this nature would undermine the causal interpretation of the treatment, as it will be impossible to distinguish the effect of a semester calendar from the pre-trend. The event study model provides a natural test for this type of pre-trend. We discuss these results in detail in Section 3.3 and show that they provide no empirical evidence of confounding patterns in graduation rates in the years leading up to a calendar switch (see Figure 2). In a related vein, it is also possible that schools enact initiatives to increase on-time graduation at the same time as an academic calendar change. If this is the case, our estimated negative effect will be a lower bound – a smaller negative effect than the true negative effect – as such initiatives would likely improve graduation rates (i.e., work against our findings).

A second indirect test of the identifying assumption is to examine whether adoption of the semester calendar is correlated with other observed time-varying characteristics of universities. In Table 2, we regress institution and student body characteristics (full-time equivalent faculty, total operation expenditures, in-state tuition, cohort size, percent of student body white, percent URM, percent male, and percent female) on a semester calendar indicator and year and institution fixed effects. For the most part, Table 2 shows no sign of a relationship between observable institution or student characteristics and the adoption of a semester calendar. Importantly, semester adoption does not appear to change the racial or gender composition of a cohort, enrollment, or the total operation expenditures. The one exception is that in-state tuition appears to increase with the calendar change. To further examine this issue we estimate an event study model with in-state tuition as the dependent variable (see Figure A2) and find that institutions are raising tuition by about 3% on average in the year prior to semester calendar adoption. This could be to help finance the calendar conversion which can be expensive, as mentioned at the end of Section 2. Regardless, this small increase in tuition likely cannot account for the sizable decline in on-time graduation that we find in Section 3.3.[12] Furthermore, Deming and Walters (2017) find no impact of tuition increases on degree completion providing further support that the small increase in tuition is likely not driving our main results.

[12]We also find that school year employment declines as a result of the calendar shift, which indicates that students are not responding to the small increase in tuition by working more; see Section 4.6

A final concern is that institutions that change to a semester system may be inherently different from those who do not. If this is the case, it would not jeopardize the internal validity of our analysis – we include institution fixed effects to estimate a local average treatment effect – rather it would call into question the external validity of our results. That is, do our results extend to those institutions who we do not observe switching if they were to switch? First, we show in Table 1 that switchers are predominantly public institutions. Since a majority of students attend public institutions – the average cohort size at a public institution is 1,724 compared to 570 at private schools and nearly half of institutions in the dataset are public – our results are relevant to a majority of students in the US. Second, in a heterogeneity analysis, we find similar results among the subset of private schools, again suggesting that our results extend widely.

3.3 Institution-Level Results

The main results are represented in the event study in Figure 2 and come from estimating Eq. (1). Figure 2a reports the effect of policy adoption on four-year graduation rates (on-time graduation), and Figure 2b for six-year graduation rates. The pre-treatment region, $k < -3$, includes untreated cohorts. All estimates are relative to the left out group, $k = -4$, which is the last untreated cohort before policy adoption. The partially treated region includes $k \in [-3, 0)$. These cohorts were fourth, third, and second year students when semesters were implemented and, as such, were treated for one, two, or three years, respectively. Year 0 represents the first fully treated cohort because this is the group of students who were incoming freshmen in the fall that the institution adopted a semester calendar. The post-treatment region, $k \geq 0$, includes cohorts who are fully treated.

The pre-treatment regions in both panels of Figure 2 reveal that prior to semester adoption, there is no statistically significant difference in graduation rates between institutions that switch and those that switched at different times or not at all. This helps assuage the concern that pre-existing trends in graduation rates might be driving the decision to switch calendars and, as such, bolsters confidence in the validity of the identifying assumption.[13]

Figure 2a shows that on-time graduation rates fall as a result of semester calendar implementation. The negative effect begins to emerge in the partially treated cohorts and grows larger as cohorts become more fully treated (i.e., as they are exposed to more years of a semester calendar). This impact levels out as all entering cohorts become fully treated. These results indicate that the first fully treated cohort – those students who first enrolled as freshmen in the same year that the semester calendar was adopted – experienced a significant reduction in on-time graduation rates of approximately 5 pp. Furthermore, this negative effect is not isolated to this cohort or the students enrolling in the first few years following the calendar switch. Looking out to year 5 in Figure 2a, we find that the cohort enrolling five years after the adoption of semesters experience a similar reduction in on-time graduation of approximately 5 pp. This indicates that the negative impact on student outcomes is not merely a short-term consequence of the calendar switch, but a longer-term effect of some characteristic of the semester calendar.

Figure 2b repeats this exercise for six-year graduation rates. Similar to four-year graduation rates, we find no evidence of differential trends in six-year graduation rates prior to the adoption of a semester calendar. After adoption, there is no statistically significant impact on six-year rates. This smaller and statistically insignificant effect of the calendar change on six-year graduation rates implies that students on a semester calendar are increasing their time-to-degree.[14]

Table 3 presents results from Eq. (2) which leverages the difference-in-differences approach. Panel A presents estimates of the mean effect of switching to semesters on four-year graduation rates for the partially treated and fully treated cohorts. Following Goodman-Bacon (2018), we separately estimate the effects of the partially treated cohorts from the fully treated cohorts rather

[13]Figure A3a and Figure A3b report event studies without institution-specific linear time trends. Comparing these figures to those with institution-specific linear time trends (Figure 2) highlights the importance of the inclusion of such trends in the regression analysis. Without the time trends, it is visually apparent that both four-year and six-year graduation rates are differentially trending *upward* before and after the policy adoption. However, the treatment effect is still quite apparent: at the time of policy adoption there is an immediate change in graduation rates in the opposite direction of the trend. Once the policy is fully adopted, the upward trend resumes.

[14]Ideally, we would like information on retention, however, because these data are not available in IPEDS for our sample period, we use six-year graduation rates as a proxy for whether students ever graduate. In Section 4, we will be able to more fully address the question of whether there is an effect on student retention.

than estimating a single post period indicator because, as evident in Figure 2a, the treatment effects are heterogeneous over these cohorts. Each column within panel A represents a separate regression. The results from the main specification (column 3) indicate that switching from a quarter system to a semester system reduces four-year graduation rates by 3.7 pp on average for the fully treated cohorts. For context, the average four-year graduation rate is 36%, thus a 3.7 pp reduction is equivalent to a 10% drop at the mean. The partially treated cohorts experience a smaller negative effect of 2.4 pp. We divide the data into several subgroups: males, females, underrepresented minorities, non-underrepresented minorities, public institutions, and private institutions. We estimate the model separately for each subgroup and report these results in columns 4-9. Strikingly, there is no evidence of heterogeneity on these dimensions. The results show, across the board, declining four-year graduation rates as a result of the adoption of a semester calendar.

Panel B of Table 3 presents estimates of the mean effect of switching to semesters on six-year graduation rates. Consistent with the event study results, we find no strong evidence that the calendar switch affects six-year graduation rates, as the estimates are small in magnitude and indistinguishable from zero.

To provide context for the magnitudes of our estimated effects, we compare to estimates of the effects of financial aid policies on college completion rates. In a study of the West Virginia Promise program, Scott-Clayton (2011) finds that the large merit-based scholarship increased four-year graduation rates by 4-7 pp (from a baseline of just 27%). Using regression discontinuity analyses, Denning et al. (2019a) find that eligibility for the maximum Pell Grant award leads to a 10% increase in the probability of graduating on-time and Castleman and Long (2016) find that an additional $1,300 in need-based aid eligibility increased the probability of earning a bachelor's degree within six years by 22%.

4 Individual-Level Analysis

We next turn to a student-level analysis using detailed transcript data from all of the public bachelor's degree-granting universities in Ohio. This will allow us to explore the mechanisms underlying the drop in four-year graduation rates presented in Section 3.3. With these more nuanced data, we are able to observe term-by-term outcomes including whether or not a student drops out, what courses are taken, cumulative grade point average (GPA), and major choice. We also link these data to employment files from Ohio's Unemployment Insurance system to study the effects of the calendar conversion on student employment.

4.1 Individual-Level Data

The student-level data are provided by the Ohio Longitudinal Data Archive (OLDA) and include administrative transcript records for all students attending public colleges in Ohio between Summer 1999 and Spring 2017. These data provide student demographics, major subject identifiers, degree completions, and course-level data on enrollment and grades. The full sample is limited to all students who enroll as first-time freshmen at a bachelor's degree-granting institution in the fall term of the years 1999-2015.[15] The full sample covers 709,404 students enrolled at 37 campuses.

These data provide an ideal context in which to explore the effects of a change in the academic calendar because Ohio has one of the largest comprehensive public college systems in the US and because more than half of the schools in Ohio switched from quarters to semesters in the sample time period.[16] There are 16 campuses in the data that were already on a semester calendar at the start of the sample in 1999. Four campuses switched from a quarter calendar to semesters over the course of the following decade. All of the remaining campuses in the state switched to a semester calendar in the Fall of 2012 by mandate of the Ohio Department of Higher Education.[17] In total,

[15] Students who transfer into a four-year Ohio public institution are excluded from the sample. If a student enrolls as a first-time freshman in a fall term at a four-year Ohio public institution and then transfers to another institution in this system, we will only observe them at the first institution.

[16] Appendix Table A3 details the variation in academic calendars within this sample.

[17] A driving motivator for this policy mandate was to facilitate credit transfer between institutions within the state. Additional information on the policy can be found at https://www.ohiohighered.org/calendar-conversion

64% of students in the full sample first enrolled under a semester calendar while 36% first enrolled under a quarter system.

The term-by-term transcript data allow us to construct several dependent variables of interest. For each student, we create indicator variables for: (1) graduate; (2) drop out; and (3) transfer to another school (within the dataset) in year $y \in [1,5]$ of enrollment.[18] We can also aggregate these variables to create indicators for each outcome occurring anytime within four years or within five years of initial enrollment. For each student in each term we also observe cumulative GPA, the number of credits attempted, and the student's declared major.

Panel A of Table 4 displays the demographic characteristics of all students in the full sample. These summary statistics show that the sample is 53% female, predominantly white (78%) and almost entirely US-born (98%). Panel B of Table 4 shows summary statistics for the individual outcome variables in this sample. The four-year graduation rate is 23% (this is lower than the national average of 36% shown in Table 1) and the five-year graduation rate is 40%. Panel C of Table 4 displays statistics for outcomes measured at the end of each student's first year of enrollment. This panel shows that 20% of students drop out in their first year, while 8% transfer to another public Ohio college, and only 54% of students enroll in a full-time course load.[19] Note that while graduation rates increase significantly from year 4 to year 5 of enrollment (shown in panel B), drop out rates and transfer rates are largely determined in the first and second years of enrollment. This pattern is depicted in Figure 3, which plots the enrollment status measured $y \in [1,6]$ years after initial enrollment for the subset of students in the 1999-2011 cohorts (those for whom we observe 6 years of data). This figure shows that most students who graduate do so in years 4 or 5 of enrollment and very few students in this sample take 6 years to graduate (only 4.7%).

In Section 4.3, we report estimates for separate subgroups of students who we define to be from high- or low-income backgrounds. In order to identify students in the sample who are high- or low-

[18]We do not attempt to analyze the effect of calendar switching on six-year outcomes in this sample because we only observe 5 years of post-treatment data for the large group of schools that switch to semesters in Fall 2012.

[19]A full course load is 15 credits per term, which totals 45 credits per year under quarters or 30 credits per year under semesters.

income, we link the OLDA transcript data to the 2006-2010 American Community Survey (ACS). In the transcript data, we observe the zip code where each student's high school was located[20] and map this to the mean household income reported for that zip code in the ACS. We then split the estimation sample in two and designate students who attended high schools in zip codes with mean household income above the median value to be higher income students.

In order to estimate the effect of the switch to semesters on student employment in Subsection 4.6, we link the transcript data to quarterly wage data from the Ohio Department of Job and Family Services (ODJFS). The ODJFS collects quarterly earnings data through the Unemployment Insurance (UI) system for all individuals working in Ohio who are not: (1) self-employed; or (2) employed by the Federal government. All records in the UI data include a linkage identifier that enables deterministic matching to students in the transcript data.[21] The data also include the industry in which each student was employed, as categorized by the North American Industry Classification System (NAICS) code. If a student was employed by more than one employer in a given quarter, we assign that student to the employer from which the student received the most income.

4.2 Empirical Framework: Individual-Level

We leverage the same identification strategy as in Section 3.2 and estimate the following model:

$$Y_{ist} = \sum_{k=-10}^{10} \theta_k G_{stk} + X_i'\alpha + \gamma_s + \phi_t + \rho_s * t + \varepsilon_{ist} \tag{3}$$

where Y_{ist} is an indicator that individual i enrolled at school s completes a bachelor's degree within 4 years of first enrolling in cohort t. The vector X_i includes individual characteristics: age, age^2, sex, a foreign-born indicator, and indicators for race/ethnicity. Campus and cohort fixed-effects are captured by γ_s and ϕ_t, and $\rho_s * t$ are campus-specific linear time trends. As in Eq. (1), G_{stk} is an indicator for k years from the adoption of a semester system.

We also employ a difference-in-differences approach that is analogous to Eq. (2) for the individual-

[20] Note that this variable is missing for approximately 7% of students in the main estimation sample.

[21] Note that this linking identifier is unavailable for approximately 7% of our full estimation sample and for nearly all foreign-born students.

level data:

$$Y_{ist} = \beta_1 G1_{ist} + \beta_2 G2_{ist} + X_i'\alpha + \gamma_s + \phi_t + \rho_s * t + \varepsilon_{ist} \tag{4}$$

where $G1_{ist}$ is an indicator for students who are in partially treated cohorts. That is, students who first enroll at a university 1 to 3 years prior to the adoption of a semester calendar (if the outcome variable is the probability of graduating in four years) or students who enroll 1-4 years prior to the calendar change (if the outcome variable is the probability of graduation in five years). The indicator $G2_{ist}$ is equal to one if student i enrolls as a first-time freshman at a university that is currently using a semester system. The omitted category is students who are untreated. All other variables are the same as in Eq. (3).

We estimate both of the above models using Ordinary Least Squares (OLS).[22] In order to estimate standard errors and conduct valid inference, we implement several methods to best suit the structure of the individual-level data. The full sample includes 709,404 students enrolled at 37 campuses comprising 555 school-by-year cohorts. Clustering at the level of the treatment variable, the school-by-year cohort level, assumes that there is no serial correlation in the error term that might impact two students who enroll in the same university in consecutive years. Bertrand et al. (2004) show that this approach can lead to under-estimated standard errors and over-rejection of standard hypothesis tests. Alternatively, clustering at the campus-level can provide broadly conservative estimates of the standard errors. We report these campus-level clustered standard error estimates in all results tables throughout Section 4.3. However, in this particular setting, campus-level clustering relies on relatively few, very large and unbalanced clusters. To account for the small number of clusters, we also estimate and report wild cluster bootstrap p-values (Cameron et al., 2008).

Finally, we note that these methods that cluster at the campus-level are most likely overly conservative in our context. This is due to the assumption that any two students who attend the same university – no matter how many years apart – may have correlated error terms. For this reason, we also estimate standard errors using multiway clustering (Cameron et al., 2011) along 5

[22]Estimates using a Probit Maximum-Likelihood estimator are very similar and are available upon request.

dimensions. These dimensions correspond to the 5 overlapping peer groups that a student might be exposed to over the course of a five-year enrollment at a given school. The errors are then assumed to have the property that for all $i \neq j$: $\mathbb{E}\left[\varepsilon_{ist}\varepsilon_{jsr}|x_{ist},x_{jsr}\right] = 0$ unless (1) $t \in [r-4,r]$; (2) $t \in [r-3,r+1]$; (3) $t \in [r-2,r+2]$; (4) $t \in [r-1,r+3]$; or (5) $t \in [r,r+4]$. This multiway clustering structure allows for arbitrary correlation between the errors of any two students who enroll at the same campus within 4 years of each other and assumes a zero correlation between students who either attend the same university five or more years apart or who attend different universities. This creates an error covariance structure akin to Newey-West standard errors, which account for temporal autocorrelation by assuming a decay in the correlation between two observations as the time lag between them grows larger. Using the multiway standard errors, we do not impose any structure on the decay rate and allow for arbitrary correlation between students as long as they enroll at the same university within four years of each other.

Each of these 3 methods of inference offer advantages and disadvantages and without knowledge of the true nature of the underlying error structure it is impossible to say which is best. Thus, in all of the following tables in Section 4.3 we report: (1) standard error estimates using multiway clustering; (2) standard error estimates using campus-level clustering; and (3) p-values for a Wald test estimated using wild cluster bootstrapping.

4.3 Individual-Level Results

We first focus on replicating the results shown in Figure 2 and Table 3 from the institution-level analysis. The individual-level event study is estimated from Eq. (3) and is shown in Figure 4. Each point on the figure represents an estimate of θ_k while the dashed lines plot the 95% confidence intervals estimated using multiway clustered standard errors. These results show that there were no significant trends in the graduation probabilities for cohorts who attended universities in the pre-treatment period (more than 3 years before the switch to semesters).[23] As in the institution-level

[23]To further rule out threats to our identifying assumption, we show in Table A4 that there is no evidence that the calendar switch changed the composition of students who enroll. This table reports results from a series of balance regressions where the outcomes are female, age, foreign-born, and whether a student is an underrepresented minority.

event study analysis, the negative effect of the semester calendar begins to emerge in the partially treated cohorts and grows larger and statistically significant in the fully treated cohorts.

In Table 5 we replicate the results from Table 3 by estimating Eq. (4) with the individual-level data. Broadly, the results from this analysis confirm the findings from the institution-level analysis, albeit less precisely estimated. The switch to semesters leads to a reduction in the probability of graduating in four years (panel A, column 1) and the effect is consistent across various sub-populations (panel A, columns 2-7). In panel B we report estimates of the effect of the switch to semesters on the probability of graduating in five years.[24] Here we find imprecisely estimated negative effects that are smaller in magnitude.[25]

4.4 Mechanism Exploration

Next we seek to better understand *why* a semester schedule leads to reduced on-time graduation. In this section we investigate drop out and transfer out behavior, course taking, grades, and major switching to help shed light on the underlying mechanisms. It is possible the reduction in on-time graduation is because students are more likely to leave a university on semesters, either as a dropout or to transfer to a different institution. It may also be due to an increase in time-to-degree. Students may be under-enrolling on the semester calendar – that is, taking fewer credits per term than what constitutes a full load – and thus extending their time-to-degree. If students on semesters earn lower grades, this could also lead to repeated course-taking and delayed graduation, or worse, dismissal. Furthermore, the calendar shift may change major switching behavior. If students take longer to settle on a major or if they are less likely to switch majors due to the lack of flexibility offered on a semester calendar, time-to-degree could increase.

We investigate each of these possibilities in turn and begin by analyzing the impact of the policy on first year outcomes. Isolating first year students allows us to abstract from the selection

[24] Unfortunately, we are unable to estimate a model for six-year graduation rates because we only observe five years of data for the large group of universities that switched to semesters in 2012.

[25] Note that panel A includes student who enter as first-time freshmen in the F1999-F2013 cohorts and panel B is limited to the F1999-F2012 cohorts (for whom we can observe 5 years of data).

issues present in 2nd - 4th year students as all follow-on years are only observed conditional on continued enrollment.[26] First year students may also be the most vulnerable. More than half of all dropout and transferring out occurs in a student's freshman year (see Figure 3).

Panel A of Table 6 reports the mean effects of the calendar switch for these first-year outcomes. Each column of this panel represents a separate regression, each estimated with a different dependent variable. Note that for outcomes measured in students' freshman year, there does not exist a partially treated group. Students who enroll at a university one or more years before the semester calendar is adopted necessarily do not experience any effect of the policy during their freshman year. Thus, when estimating Eq. (4) in Table 6, we do not include the $G1_{ist}$ variable and those students are absorbed into the omitted category of untreated students.

The estimates in column 1 of Table 6 show that switching to a semester calendar increases freshman year dropouts by 2.0 pp. Evaluated at the mean, this is equivalent to a 10% increase in first year dropouts. Because the OLDA data are limited to enrollment at public universities within Ohio, a dropout could include a student who is no longer enrolled in any higher education institution or it could include students who transfer to a private institution, a public institution in another state or a non-bachelor's degree-granting institution. These potential misclassifications should be kept in mind when interpreting the dropout results. Column 2 reveals that there is no evidence of an effect of the calendar change on transfer behavior, where a transfer out is defined as a student who moves to another public four-year institution in Ohio.

The first two columns suggest that some of the reduction in on-time graduation resulting from the calendar switch stems from students leaving the institution. In the IPEDS analysis, we do not find evidence of the policy changing behavior in this way, although we are not able to explicitly analyze dropouts in this dataset. We do, however, show the policy had little effect on six-year graduation rates; a proxy for ever graduating. The difference in findings across these datasets could be due to a variety of factors. The OLDA data is slightly less female and more white than the full population of college students in the IPEDS data, and has a four-year graduation rate that is

[26]If the switch to semesters has an effect on the probability of remaining enrolled past the first year, then outcomes measured in enrollment years 2-4 will necessarily suffer from selection.

13 percentage points lower than the national average (23% compared with 36%). It is also possible that we do not have the granularity to detect dropping out behavior in the IPEDS data. Regardless of these differences, we find the same general pattern of poorer graduation outcomes in response to the calendar switch across the two datasets. As such, it is likely that there are similar underlying mechanisms at play.

In columns 3-5 of Table 6, we investigate three additional first year outcomes that may explain why students are dropping out or delayed in their completion as a result of the calendar switch. Column 3 reports results for course taking behavior. Taking fewer credits than the recommended course load in the first year will create a deficit for students from the onset in terms of degree progression. As such, the dependent variable in column 3 is an indicator for attempting the recommended number of credits for a full-time student to graduate in four years. This equates to 45 credits per year for students on a quarter calendar and 30 credits per year for those on a semester calendar. The results presented in column 3 are imprecisely measured, but the point estimates suggest that students on semesters may be under-rolling in their first year.

Column 4 of Table 6, presents the estimates of the effect of a calendar switch on the probability of earning a cumulative grade point average less than 2.0 (measured at the conclusion of the spring term of a student's first year of enrollment). This threshold is meaningful because it is typically the cutoff used to place students on academic probation. Students who fail to raise their cumulative GPA above a 2.0 in subsequent terms become eligible for academic dismissal.[27] We estimate that the switch to a semester calendar increases the probability of earning a GPA below the 2.0 threshold by 4.9 pp. This is equivalent to a substantial increase of 20% evaluated at the mean and is very likely a driving mechanism behind the increase in freshman year dropouts and, among those who persist, the reduction in on-time graduation.[28]

In column 5 of Table 6, we assess the effect of switching to a semester calendar on the prob-

[27] For example, see https://advising.osu.edu/academic-status-0 for detailed information on the academic probation policy at Ohio State University.

[28] We cannot identify whether the lower grades are a result of poorer performance by students or a change in the way instructors grade in the new calendar system, but regardless students are receiving lower grades which threatens degree progress.

ability of switching one's declared major in the first year of enrollment.[29] These estimates reveal that first-year students are 7.1 pp less likely to switch majors under a semester calendar. Two possibilities emerge, (1) students are overall less likely to switch majors or (2) they delay major switching to a later year. We investigate this at the end of the section in Table 7.

In panel B of Table 6, we probe the persistence of the estimated effects on first year outcomes by decomposing the $G2_{ist}$ variable in Eq. (4) into 3 indicator variables for each of the first 3 fully treated cohorts and a 4th indicator for students who enrolled 4 or more years after the adoption of a semester calendar. The estimates indicate that for first year students, dropping out, under-enrolling in credits, the increased probability of earning a cumulative GPA less than 2.0, and the decreased probability of switching majors are lasting effects – affecting cohorts enrolling 4 or more years after the switch – and are not merely a temporary response to the calendar change.

Next, we restrict the sample to students who remain enrolled for at least four years and, as such, are very unlikely to ever drop out to further home in on the mechanisms underlying the increase in time-to-degree. Table 7 includes estimates of the effects of the calendar conversion on cumulative course-taking, cumulative grades, and cumulative major-switching behavior in years 1-4 of students' enrollment.[30] Recall that, for first year outcome variables, there is no partially treated group. For outcomes measured in students' 2nd year, the partially treated group (represented by $G1_{ist}$ in Eq. (4)) is defined to be students who first enrolled at a given university 1 year prior to the adoption of a semester calendar. For outcomes measured in students' 3rd year, the partially treated group also includes students who enrolled 2 years prior to the switch to semesters. And for outcomes measured in students' 4th year, the partially treated group includes students who enrolled 1-3 years prior to the adoption of a semester calendar.

In panel A of Table 7, the outcome variable measures whether the student had attempted the recommended number of credits by the end of each enrollment year.[31] These estimates show that,

[29] This excludes the switch to first declared major for those students who enroll as undeclared. However, the estimates are very similar if we include those initial declarations in the dependent variable.

[30] We estimate these regressions using the F1999-F2013 cohorts (those for which we observe at least 4 years of data).

[31] The outcome measures cumulative course taking at the end of each year such that a student under semesters who takes 29 credits in year 1 and 31 credits in year 2 is observed to be "on-track" (total credits $\geq$ 60) and will have an

among students who do not drop out and instead persist through at least 4 years of enrollment, those in their 4th year on a semester calendar are 11.5 pp less likely to have attempted an on-track course load than their quarter calendar counterparts. These results suggest that students on a semester calendar fall behind in taking the recommended number of credits early on in their college careers, and then are unable to catch-up in subsequent years.

In panel B of Table 7, we estimate the effect of the switch to a semester calendar on the probability of earning a cumulative GPA below a 2.0 in years 1-4 of enrollment. These estimates show that even among students who do not drop out, the change to a semester calendar increases the probability of being below this academic probation threshold in each of the first 4 years of enrollment. Finally, in panel C of Table 7, we estimate the effect of calendar switching on the probability of ever having switched majors measured at the end of each year of enrollment. These estimates show that students are less likely to have switched majors by the end of their first and second year of enrollment under semesters, but no less likely to have switched majors by the end of their fourth year - supporting the hypothesis that students on a semester calendar are no less likely to switch majors overall, but are merely doing so later into their college careers.

4.5 Mechanism Discussion

The switch to a semester calendar changes a student's academic experience in a number of ways, and guided by empirical evidence, we can only speculate as to which channels underlie our main findings. We posit that there are two characteristics of semesters that are particularly relevant and discuss these factors in turn.

First, the *higher number of courses per term* may produce several of our findings. Students may find it difficult to balance more courses and topics simultaneously. This could explain the increase in the probability of falling below the 2.0 GPA cutoff. At the same time, some students may simply enroll in fewer credits per term (i.e., four courses instead of five) to avoid taking too many different courses at once. These possibilities are consistent with Buser and Peter (2012) who

outcome value equal to 1 in column 2.

show in an experimental setting that individuals perform relatively worse when assigned to a multi-tasking treatment and conclude that scheduling is a significant determinant of productivity. It is also possible that the higher number of courses in a term presents more of a scheduling challenge, particularly if a student wishes to avoid class times outside of the standard 9-5 school day. For instance, a student may prefer to enroll in fewer courses to avoid an 8 a.m. start time, especially since the cost of under-enrolling is not realized until a future period.

Second, the *increased length of the term* may be at play. Longer terms could incentivize procrastination. There are longer periods between exams and more time to put off studying. It is possible that this type of behavior leads to lower grades and an increased probability of earning a GPA below a 2.0. For instance, Ariely and Wertenbroch (2002) show in an experimental setting that externally imposed deadlines, such as an exam date, enhance performance more than self-imposed deadlines. In a related vein, Beattie et al. (2018) provides descriptive evidence showing that low performing first year college students are more likely to cram for exams and wait longer before starting assignments compared to higher performing first year students. The increased term length may be particularly harmful to those lower down in the ability distribution.

Additionally, longer/fewer terms mean that experimenting with a major takes more time. If, for instance, there are a set number of courses needed to learn about the match between one's skills/interests and major, then this learning is more costly in a semester calendar as one must commit to at least half a year in a major compared to only a third of the year in a quarter system. Our findings on the timing of major switching are consistent with this proposed mechanism: students are no less likely to switch majors overall but they are doing so later on in their college careers. Delayed major switching likely results in needing more time to complete major requirements and, thus, delayed graduation.

4.6 Employment Analysis

Beyond academic outcomes, employment may also be affected by the switch to semesters. In fact, one reason university administrators cite for making the calendar switch is to give students their best shot at obtaining a summer internship. The belief is that the majority of summer internship programs are geared towards a semester calendar – the most prevalent academic calendar – such that students at institutions on quarters are disadvantaged. To investigate this hypothesis we link the transcript data to employment information from the Ohio Department of Job and Family Services (ODJFS). These data include quarterly earnings from all employers in Ohio, and allow us to construct quarterly employment indicators for each student given that they are not self-employed, employed by the Federal government, or employed outside of Ohio.

We use these data to estimate the effects of the change to a semester calendar on the probability of employment in the summer following a student's 1st-3rd year of enrollment. Summer employment is coded as one if the student earned positive earnings in the state of Ohio in Quarter 2 or 3, i.e., April-September. Ideally, we would observe summer internship employment separately from other types of summer employment (i.e., a server or barista job). In lieu of this type of data, we analyze employment in the retail and food service industries separately from all other industries, where all other industries serve as a proxy for summer internship employment.[32]

We begin with an event study analysis by separately estimating Eq. (3) for all types of employment in each of the three summers and report the findings in Figure 5.[33] There are three key takeaways: (1) there is no evidence of pre-treatment trends in summer employment, (2) there is a dip in summer employment right before policy adoption due to the fact that the transition summer is one month shorter than any other summer, and (3) there is suggestive evidence of a modest increase in summer employment after a student's first year as a result of semester calendar adoption.

Next, we estimate a model similar to Eq. (4) for the summer employment outcome variables. These results are shown in Table 8. In column 1 we report the effect of the calendar change on the share of summers employed, where we allow a student to be employed in the summer after their first, second, or third year. We do not include summer employment after one's fourth year because a substantial subset of those students will have graduated. Columns 2-4 report the effect

[32]We define retail and food service industries as those classified under NAICS codes: 451, 452, 453, 454, and 722.

[33]We do not include an institution-specific linear time trend in any of the employment models since there is no evidence or reason to think that employment is trending differentially across institutions in Ohio.

of the switch to semesters on summer employment separately for each of those three summers.[34] As in Table 7, we restrict the sample to students who remain enrolled for at least four years in the F1999-F2013 cohorts.

The dependent variables in panel A indicate summer employment *excluding* the retail and food service industries. Overall the results confirm the findings from Figure 5; that is, there is limited evidence that the calendar adoption improves summer internship employment. Using the campus clustered standard errors and focusing on column 1, we can rule out effects larger than 10% relative to the mean. Panel B reports results for retail and food service employment only. All coefficients are small in magnitude and largely indistinguishable from zero indicating that the policy also has little effect on non-internship type summer employment.

While the impetus of the employment analysis was to investigate whether semesters change summer internship employment, the calendar adoption may also affect school year employment, particularly if semesters affect scheduling flexibility. Table 9 reports results for student employment during the school year – Quarters 1 and 4 (October-March) – and Figure 6 presents the corresponding event studies. It is clear that school year employment, particularly employment in the retail and food service industries, declines substantially due to the calendar change. The calendar switch decreases the share of school years a student is employed in a retail or food service job by 4.8 pp, which is equivalent to a 23% reduction at the mean (Table 9, column 1). This result is consistent with the mechanism hypothesis that the higher number of courses per term in a semester system is overwhelming for students. Students on semesters may also have more difficulty in scheduling a part-time job if they are required to take more courses in a term.

In conclusion, these results provide little evidence that the calendar switch produces meaningful positive improvements for summer internship employment. Furthermore, following the calendar switch, we show that students are substantially less likely to be employed during the school year in

[34] Note that the partially treated group, $G1$, is defined separately for each year/column and, in all cases, includes the short summer preceding the calendar switch. In column 2, $G1$ includes students who enrolled 1 year before the switch to semesters. Column 3 includes students enrolled 1 and 2 years before, and in column 4 (and column 1) it includes those enrolled 1-3 years before. In all specifications, the fully treated group, $G2$, includes all students who first enroll at a university that is on a semester calendar.

all types of jobs. This decline in school year employment might be seen as a positive outcome if it leads to improved academic outcomes; however, we find that this is not the case. It is important to keep in mind that this analysis only includes students who are employed in the state of Ohio and earning positive wages. Our estimates will not capture any effect the calendar switch may have on out-of-state employment opportunities or the propensity to hold unpaid internships.

5 Discussion and Conclusion

The documented negative relationship between the semester calendar and on-time graduation is unexpected. Colleges and universities that have switched to semesters often cite better academic outcomes as a reason for making the shift (Burns, 2013), but we show that it is costly to students academically and does not appear to improve summer employment in a meaningful way. We find that students are 3.7-4.4 pp less likely to graduate on time.

The cost to students of this increase in time-to-degree is substantial and includes both the added tuition and the lost earnings from the additional time spent enrolled. We provide an estimate of this cost using a back-of-the-envelope calculation. Based on a National Center for Education Statistics report, the cost of one year of tuition at a four-year public institution in 2014 was \$18,110 and the average starting salary for 2014 graduates was \$26,217.[35] Thus, the total cost of an additional year of schooling for a public university student is approximately \$44,327.43.[36] To put the total cost of the policy into context, consider that the average cohort size in our sample is 1,237 students. If we assume that the 3.7 pp decline in on-time graduation is fully due to a one-year delay in graduation for 46 students per cohort, then a lower bound estimate of the cost of the policy to students would be \$2 million per year at an average-sized university.

Our mechanism analysis suggests that the longer terms and higher number of courses per term associated with a semester calendar are likely driving the estimated increase in time-to-degree. We

[35] This salary was calculated using the 2014 March Current Population Survey. It includes all individuals who are age 22-24, with a four-year degree, who are not in school, and includes those with a zero wage.

[36] This is a rough approximation. We acknowledge that there other costs associated with delayed graduation including the year of forgone experience in the labor market. As such, our estimated cost is a lower bound.

speculate that these features produce less scheduling flexibility, increase the cost of learning about one's optimal major match, and potentially create a suboptimal learning environment, particularly in a student's first year. As such, to combat these negative effects, higher education institutions that operate on a semester calendar might consider policies that improve scheduling flexibility and providing added academic support to first-year students in order to ease them into the demands of college.

In summary, we view this study as an important step in better understanding the optimal way to design a higher education institution. While this paper provides a thorough analysis of the effect of the conversion to a semester calendar on students' academic behaviors and outcomes, there remain many other potential effects of this policy to be considered in future work. There may be longer-term labor market effects associated with semesters. A longer post-period in administrative data is needed to investigate this possibility. The effects of switching to semesters on faculty research productivity is another factor worth considering. This paper also only addresses traditional, first-time college students and the effects on community college transfer students might differ significantly. Finally, because over the past 30 years schools are almost exclusively switching from quarters to semesters, our results do not allow one to learn about the effects of switching from semesters to quarters.

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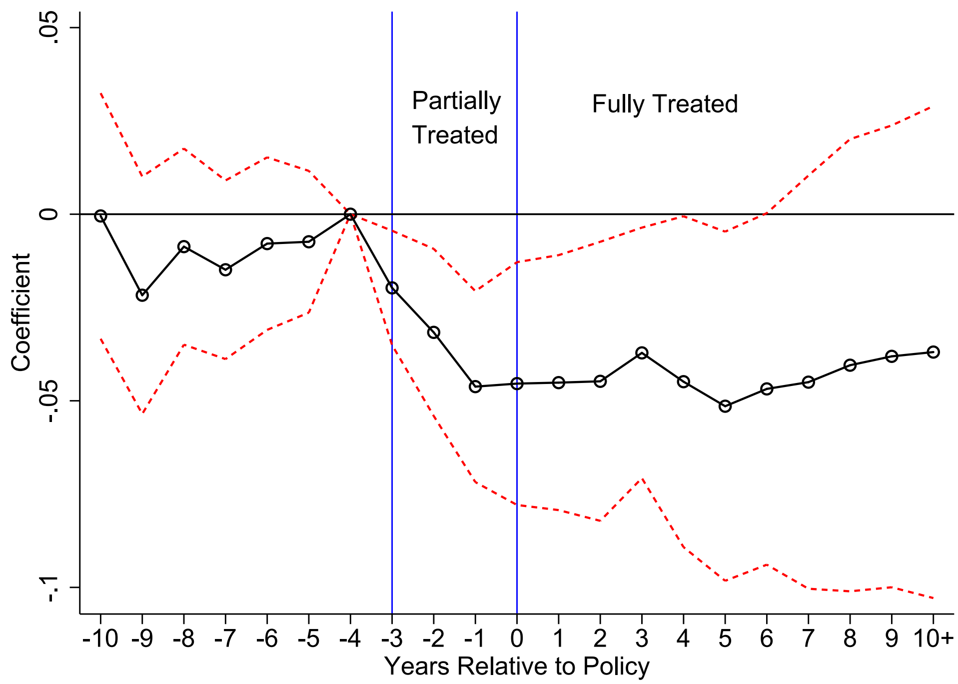
Figure 1: Fraction of Schools on Semesters and Four-Year Graduation Rates

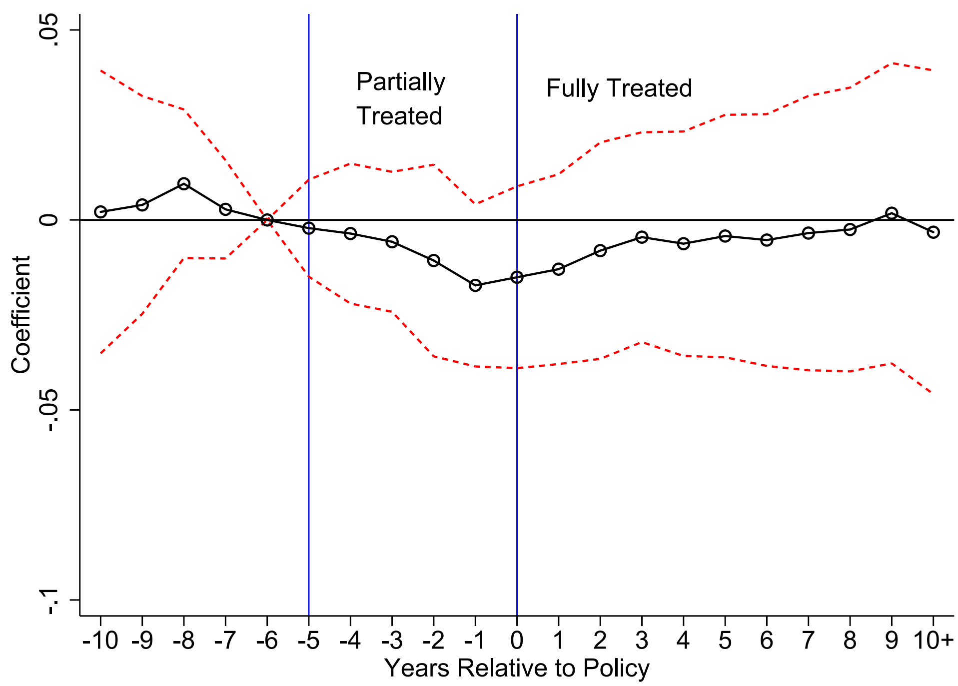
Data Source: IPEDS. Data on graduation rates are for the 1991-2010 cohorts.

Figure 2: Event Study: Institution-Level Analysis

(a) 4-year Graduation Rates

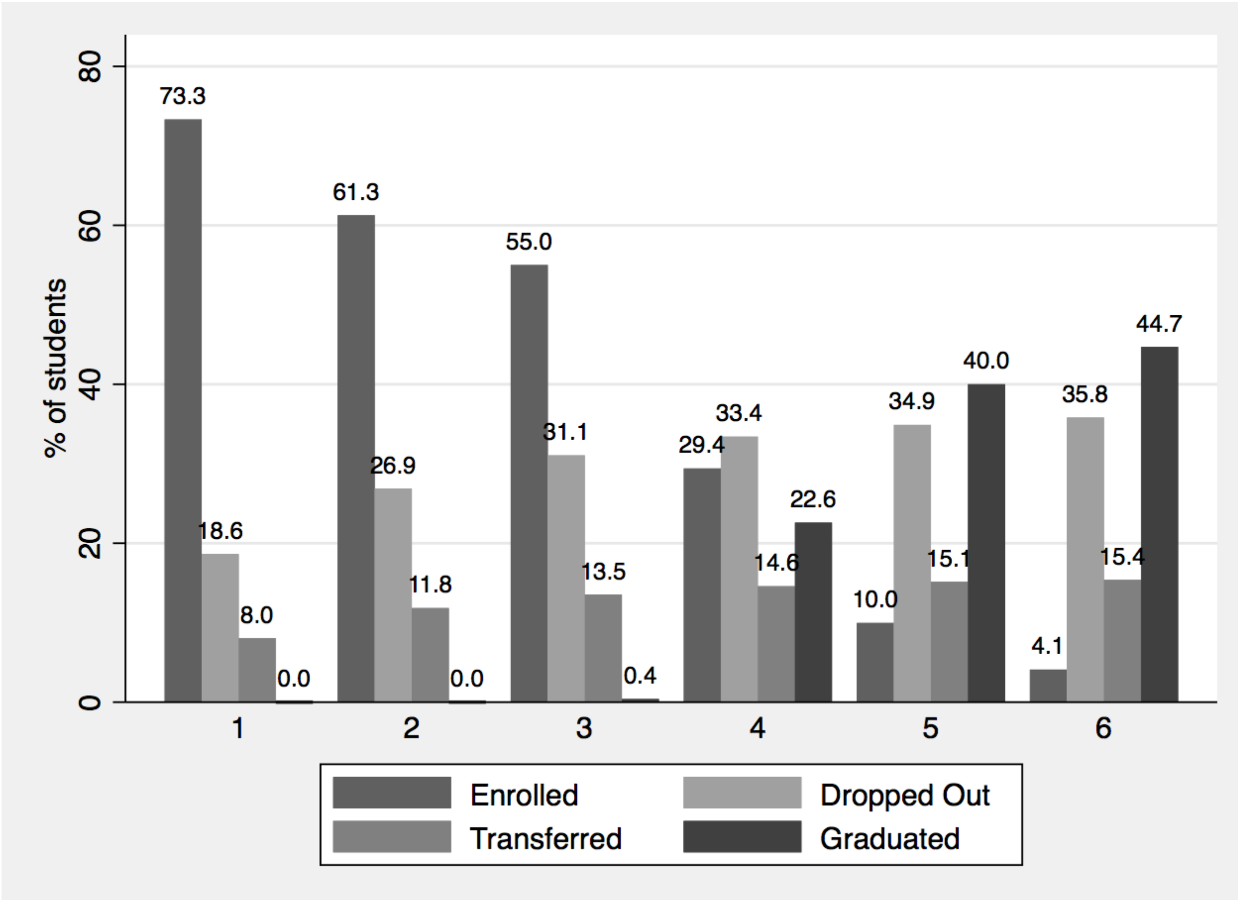


(b) 6-year Graduation Rates



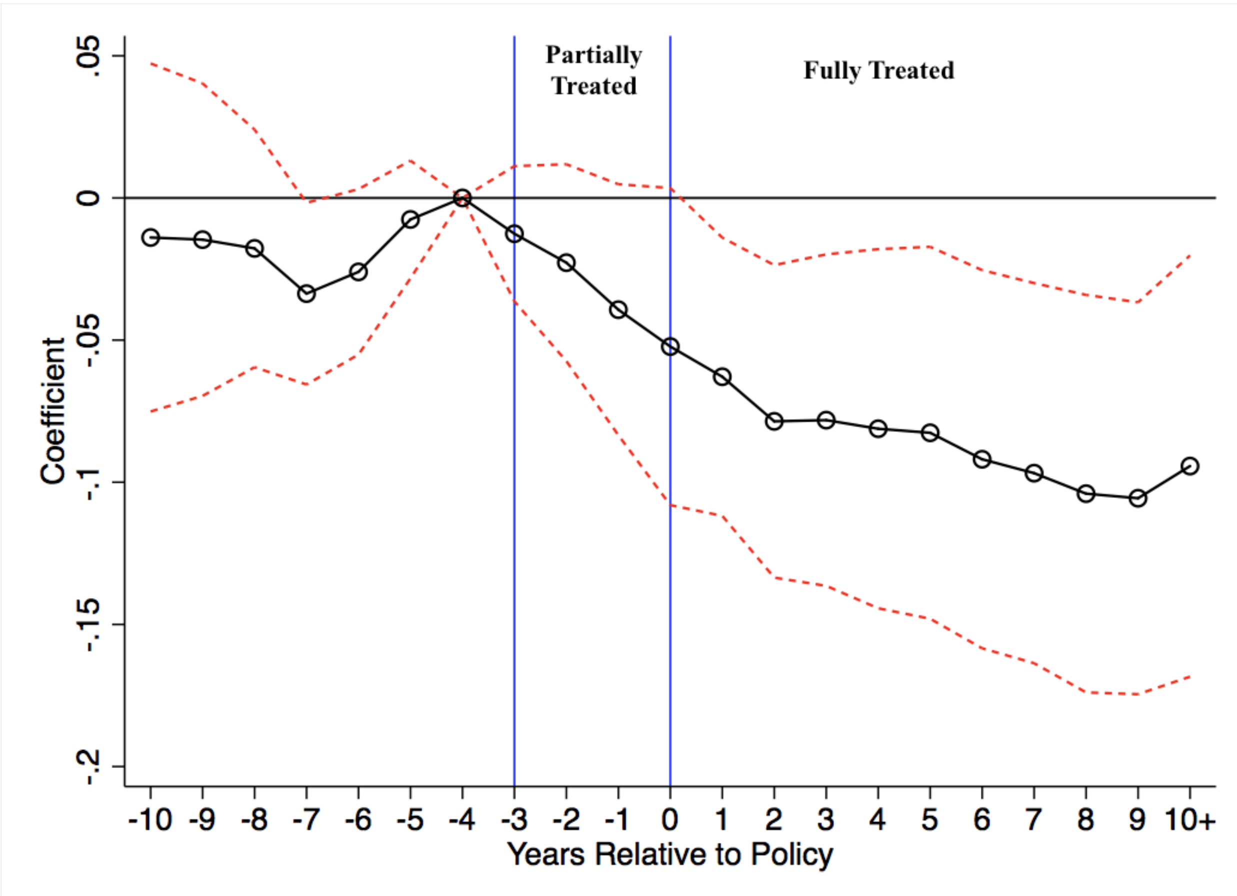
Data Source: IPEDS. Notes: The sample includes 731 institutions for 19 years (13,889 observations). This figure plots θ_k, and 95% confidence intervals in dashed lines, from estimating Eq. (1). Year and institution fixed effects, institution linear time trends, and time varying controls are included. Results are robust to excluding time varying controls.

Figure 3: Students' Enrollment Status By Year



Data Source: OLDA. Notes: X-axis measures years since initial matriculation. Sample includes all students who enroll as first-time freshmen at a bachelor's degree-granting public Ohio institution in the Fall terms of 1999-2011.

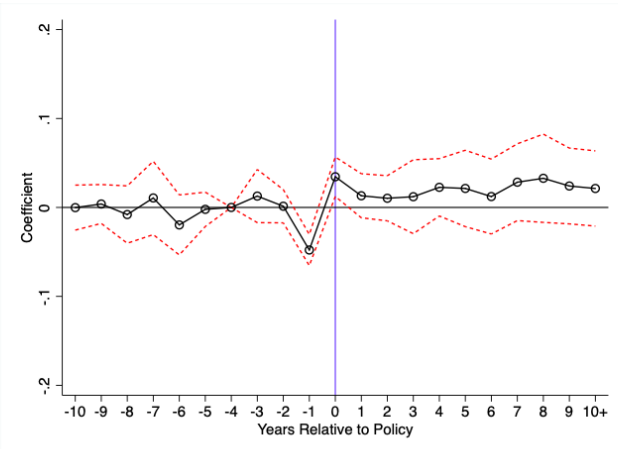
Figure 4: Event Study: Individual-Level Analysis - Four-Year Graduation Rates



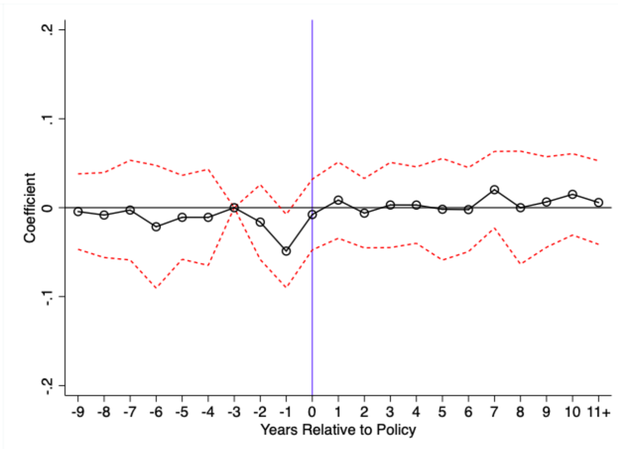
Data Source: OLDA. Notes: This figure plots θ_k, and 95% confidence intervals in dashed lines, from estimating Eq. (3). Year and institution fixed effects, institution linear time trends, and student-level controls are included. Standard errors are estimated using multiway clustering.

Figure 5: Event Study: Summer Employment (Q2 and Q3)

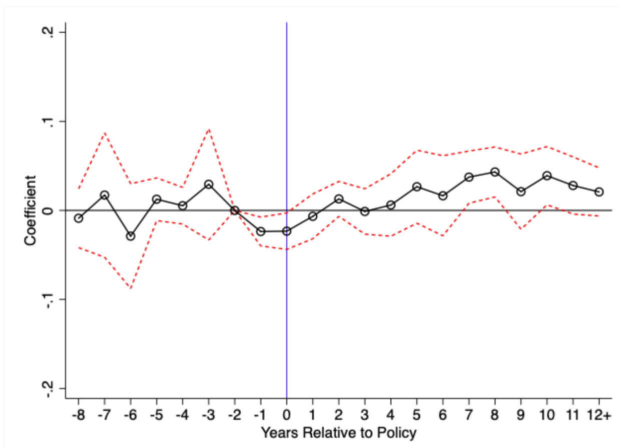
(a) Employment Following 1st Year



(b) Employment Following 2nd Year



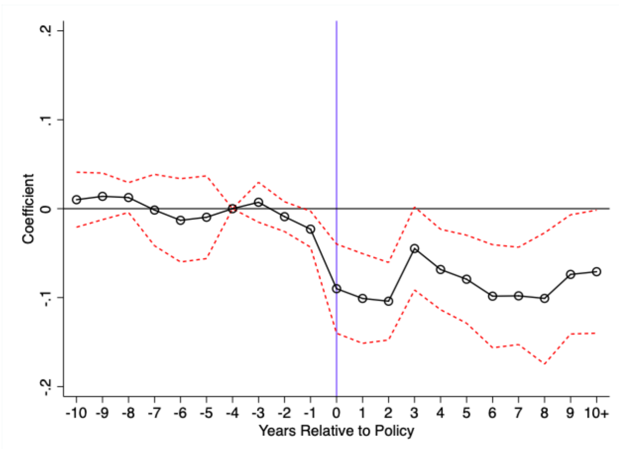
(c) Employment Following 3rd Year



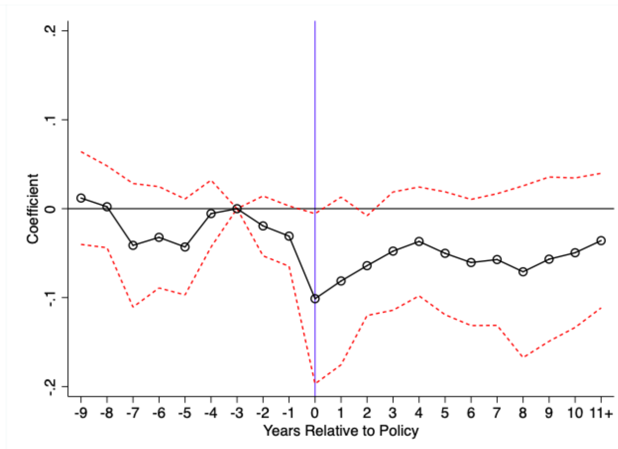
Data Source: OLDA merged with employment data from ODJFS. Notes: This figure plots θ_k, and 95% confidence intervals in dashed lines, from estimating Eq. (3). Year and institution fixed effects and student-level controls are included. Standard errors are estimated using multiway clustering.

Figure 6: Event Study: School Year Employment (Q1 and Q4)

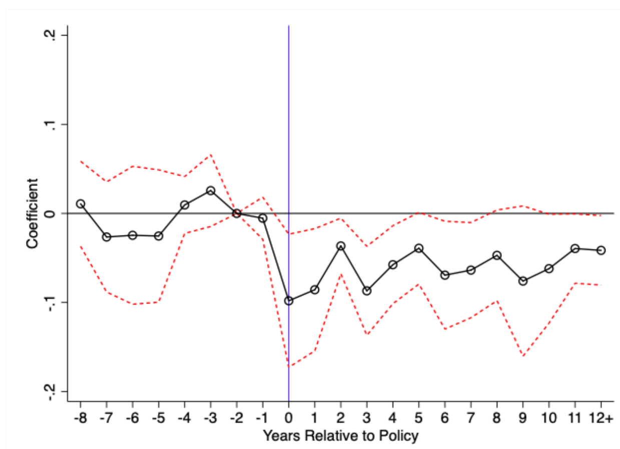
(a) Employment 1st Year



(b) Employment 2nd Year



(c) Employment 3rd Year



Data Source: OLDA merged with employment data from ODJFS. Notes: This figure plots θ_k, and 95% confidence intervals in dashed lines, from estimating Eq. (3). Year and institution fixed effects and student-level controls are included. Standard errors are estimated using multiway clustering.

Table 1: Institution-Level Summary Statistics

	All	Never Switchers	Switchers
	(1)	(2)	(3)
Semester calendar	0.93	0.96	0.72
	(0.25)	(0.20)	(0.46)
Four-yr grad rate	0.36	0.37	0.28
	(0.22)	(0.22)	(0.16)
Four-yr women grad rate	0.40	0.42	0.34
	(0.22)	(0.23)	(0.18)
Four-yr men grad rate	0.30	0.31	0.23
	(0.21)	(0.22)	(0.15)
Four-yr URM grad rate	0.29	0.30	0.21
	(0.20)	(0.21)	(0.14)
Four-yr non URM grad rate	0.37	0.39	0.30
	(0.22)	(0.23)	(0.17)
Six-yr grad rate	0.58	0.59	0.54
	(0.18)	(0.18)	(0.17)
Six-yr women grad rate	0.61	0.62	0.57
	(0.17)	(0.17)	(0.17)
Six-yr men grad rate	0.55	0.55	0.51
	(0.19)	(0.19)	(0.18)
Six-yr URM grad rate	0.51	0.51	0.46
	(0.19)	(0.19)	(0.17)
Six-yr non URM grad rate	0.60	0.61	0.56
	(0.18)	(0.18)	(0.17)
Public	0.46	0.42	0.71
	(0.50)	(0.49)	(0.45)
FTE faculty	340.00	330.24	420.58
	(382.59)	(372.32)	(450.77)
Cohort size	1,099.45	1,065.99	1,375.54
	(1,183.03)	(1,148.55)	(1,406.74)
In-state tuition	11,088.47	11,554.71	7,240.46
	(9,181.55)	(9,298.51)	(7,063.64)
Total expenditures ($/1M)	192.10	185.14	249.54
	(400.54)	(390.65)	(470.59)
Observations	13,889	12,388	1,501

Data Source: IPEDS. Note: The balanced panel dataset includes the 1991-2010 entering cohorts. There are 731 institutions and 19 years. An observation is an institution-year. Standard deviations are reported in parentheses.

Table 2: The Effect of Semesters on Institution and Student Characteristics

	Institution Characteristics				Student Characteristics		
	FTE Faculty	Costs	In-State Tuition	Cohort Size	% URM	% White	% Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Semester	-0.282	6.435	346.717***	-33.185	0.009	-0.011	-0.005
	(6.733)	(7.961)	(115.741)	(40.368)	(0.008)	(0.008)	(0.004)
Mean of outcome	340.00	192.10	11,088.47	1,099.45	0.25	0.71	0.56
Observations	13,889	13,889	13,889	13,889	13,889	13,889	13,889

Data Source: IPEDS. Note: Each column represents a separate regression, where different pre-treatment characteristics are the outcomes. All regressions include a dummy for being on a semester calendar, year fixed effects, and institution fixed effects. Standard errors are reported in parentheses and are clustered at the institution level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3: Effect of Switching to Semesters on Institution-Level Graduation Rates

	All	All	All	Women	Men	URM	Non-URM	Public	Private
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Effect on 4-year Grad. Rates									
G1 - partially treated	-0.003	-0.021**	-0.024***	-0.023**	-0.025***	-0.022**	-0.024**	-0.022**	-0.015
	(0.016)	(0.009)	(0.009)	(0.010)	(0.008)	(0.011)	(0.010)	(0.010)	(0.015)
G2 - fully treated	-0.009	-0.035***	-0.037***	-0.037***	-0.038***	-0.028*	-0.039***	-0.032**	-0.036*
	(0.020)	(0.012)	(0.012)	(0.014)	(0.012)	(0.017)	(0.013)	(0.014)	(0.020)
Panel B: Effect on 6-year Grad. Rates									
G1 - partially treated	0.010	-0.011	-0.013	-0.014	-0.011	-0.020	-0.010	-0.013	-0.011
	(0.019)	(0.009)	(0.008)	(0.009)	(0.008)	(0.013)	(0.009)	(0.009)	(0.007)
G2 - fully treated	0.024	-0.012	-0.014	-0.017	-0.009	-0.027	-0.013	-0.012	-0.019*
	(0.018)	(0.010)	(0.010)	(0.012)	(0.010)	(0.019)	(0.010)	(0.011)	(0.011)
Observations	13,889	13,889	13,889	13,865	13,824	13,883	13,799	6,365	7,524
School, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trends	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Data Source: IPEDS. Note: The sample includes 731 institutions for 19 years. All regressions are weighted by average cohort size. Within each panel and column, point estimates come from the same regression. The left out category is G0, the pre-treatment years, and is defined as ($k \leq -4$) for the four-year graduation outcome and ($k \leq -6$) for the six-year graduation outcome. Standard errors are reported in parentheses and are clustered at the institution level. Results are robust to holding constant the sample size across the columns. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Individual-Level Summary Statistics

	Mean	Std. Dev.	N
Panel A: Characteristics - First Year Students			
Female	0.53	0.50	709,404
White	0.78	0.41	709,404
Black	0.11	0.31	709,404
Hispanic	0.02	0.15	709,404
Asian	0.02	0.14	709,404
Other race	0.07	0.25	709,404
Foreign-born	0.02	0.13	709,404
Age	19.03	3.18	709,404
Panel B: Graduation Outcomes			
Four-yr grad rate	0.23	0.42	627,988
Five-yr grade rate	0.40	0.49	585,935
Panel C: First Year Academic Outcomes			
Drop out	0.20	0.40	709,404
Transfer out	0.08	0.27	709,404
Full course load	0.54	0.50	709,404
Cummulative GPA	2.53	1.06	709,404
Cumm. GPA <2.0	0.24	0.43	709,404
Switch major	0.11	0.32	709,404

Data Source: OLDA. Note: Observation counts in panels A and C include all students who enroll as first-time freshmen at a bachelor's degree-granting public institution in the fall term of the years 1999-2015. In panel B, 4-yr graduation rates are measured only for the F1999-F2013 cohorts and 5-yr graduation rates are measured only for the F1999-F2012 cohorts.

Table 5: Effect of Switching to Semesters on Graduation Rates - Individual-Level Analysis

	All (1)	Women (2)	Men (3)	URM (4)	Non-URM (5)	Low-Income (6)	Higher-Income (7)
Panel A: Effect on 4-year Grad. Rates							
G1 - partially treated	**-0.010**	**-0.004**	**-0.017**	**-0.007**	**-0.011**	**-0.015**	**-0.006**
SE multi-way clustered	(0.013)	(0.014)	(0.014)	(0.019)	(0.013)	(0.012)	(0.018)
SE clustered by campus	(0.016)	(0.017)	(0.016)	(0.025)	(0.016)	(0.015)	(0.021)
Wild cluster bootstrap p-value	[0.64]	[0.78]	[0.48]	[0.64]	[0.62]	[0.51]	[0.81]
G2 - fully treated	**-0.044**	**-0.044**	**-0.045**	**-0.026**	**-0.050**	**-0.039**	**-0.045**
SE multi-way clustered	(0.021)**	(0.021)**	(0.022)**	(0.030)	(0.021)**	(0.017)**	(0.023)*
SE clustered by campus	(0.030)	(0.031)	(0.029)	(0.044)	(0.030)*	(0.024)	(0.033)
Wild cluster bootstrap p-value	[0.35]	[0.41]	[0.32]	[0.53]	[0.21]	[0.24]	[0.44]
Mean of Outcome Variable	0.23	0.28	0.18	0.11	0.25	0.18	0.29
Observations	627,988	333,163	294,825	82,423	545,565	292,380	298,730
Panel B: Effect on 5-year Grad. Rates							
G1 - partially treated	**-0.007**	**-0.007**	**-0.008**	**-0.009**	**-0.007**	**-0.005**	**-0.004**
SE multi-way clustered	(0.012)	(0.012)	(0.013)	(0.022)	(0.012)	(0.011)	(0.018)
SE clustered by campus	(0.015)	(0.015)	(0.016)	(0.028)	(0.014)	(0.014)	(0.021)
Wild cluster bootstrap p-value	[0.70]	[0.70]	[0.73]	[0.75]	[0.71]	[0.70]	[0.85]
G2 - fully treated	**-0.034**	**-0.025**	**-0.043**	**-0.011**	**-0.042**	**-0.020**	**-0.035**
SE multi-way clustered	(0.018)*	(0.017)	(0.020)**	(0.033)	(0.017)**	(0.014)	(0.024)
SE clustered by campus	(0.028)	(0.024)	(0.033)	(0.051)	(0.027)	(0.021)	(0.035)
Wild cluster bootstrap p-value	[0.44]	[0.47]	[0.38]	[0.83]	[0.20]	[0.52]	[0.55]
Mean of Outcome Variable	0.40	0.43	0.37	0.23	0.43	0.33	0.49
Observations	585,935	310,836	275,099	76,528	509,407	274,577	277,320

Data Source: OLDA linked to Mean Household Income by Zip Code from the 2006-2010 American Community Survey (https://www.psc.isr.umich.edu/dis/census/Features/tract2zip/). Notes on panel A: Includes students who enter as first-time freshmen in the F1999-F2013 cohorts. The partially treated group, G1, includes students who first enroll at a university 1 to 3 years prior to the adoption of a semester calendar and the omitted category includes all students who enroll more than 3 years prior to the calendar switch. Notes on panel B: Includes student who enter in the F1999-F2012 cohorts. The partially treated group includes students who first enroll 1 to 4 years prior to the switch to semesters and the omitted category includes all students who enrolled more than 4 years prior to the switch. In both panels, the fully treated group, G2, includes all students who first enroll at a university that is on a semester calendar. All regressions include age, age-squared, sex, a foreign-born indicator, indicators for race/ethnicity, campus and year fixed-effects, and campus-specific linear time trends. ***p<0.01, **p<0.05, *p<0.10.

Table 6: Effect of Switching to Semesters on First Year Outcomes

	Drop Out (1)	Transfer Out (2)	On-Track Course Taking (3)	Cum. GPA <2.0 (4)	Switch Major (5)
Panel A: Mean Estimates					
G2 - fully treated	**0.020**	**0.001**	**-0.068**	**0.049**	**-0.071**
SE multi-way clustered	(0.007)***	(0.005)	(0.036)*	(0.010)***	(0.007)***
SE clustered by campus	(0.009)**	(0.007)	(0.044)	(0.013)***	(0.009)***
Wild cluster bootstrap p-value	[0.05]**	[0.95]	[0.25]	[0.01]***	[0.00]***
Panel B: Dynamic Estimates					
First Fully Treated Cohort (k=0)	**0.017**	**0.002**	**-0.078**	**0.062**	**-0.066**
SE multi-way clustered	(0.008)**	(0.006)	(0.036)**	(0.014)***	(0.008)***
SE clustered by campus	(0.009)*	(0.007)	(0.041)*	(0.016)***	(0.009)***
Wild cluster bootstrap p-value	[0.09]*	[0.82]	[0.08]*	[0.00]***	[0.00]***
Second Fully Treated Cohort (k=1)	**0.017**	**-0.001**	**-0.046**	**0.039**	**-0.059**
SE multi-way clustered	(0.009)*	(0.005)	(0.038)	(0.013)***	(0.009)***
SE clustered by campus	(0.009)*	(0.007)	(0.045)	(0.015)**	(0.010)***
Wild cluster bootstrap p-value	[0.13]	[0.90]	[0.59]	[0.06]*	[0.00]***
Third Fully Treated Cohort (k=2)	**0.024**	**0.001**	**-0.064**	**0.046**	**-0.089**
SE multi-way clustered	(0.009)***	(0.005)	(0.039)	(0.015)***	(0.008)***
SE clustered by campus	(0.010)**	(0.007)	(0.048)	(0.017)**	(0.011)***
Wild cluster bootstrap p-value	[0.04]**	[0.93]	[0.29]	[0.05]*	[0.00]***
Future Fully Treated Cohorts (k≥3)	**0.026**	**0.001**	**-0.083**	**0.048**	**-0.076**
SE multi-way clustered	(0.010)***	(0.006)	(0.037)**	(0.014)***	(0.008)***
SE clustered by campus	(0.012)**	(0.008)	(0.050)	(0.018)**	(0.010)***
Wild cluster bootstrap p-value	[0.05]**	[0.96]	[0.15]	[0.07]*	[0.00]***
Mean of Outcome	0.20	0.08	0.54	0.24	0.11
Observations	709,404	709,404	709,404	709,404	709,404

Data Source: OLDA. Note: The sample includes students who enter as first-time freshmen in the F1999-F2015 cohorts. The omitted category includes all students who enroll prior to the adoption of semesters. In panel B, the fully treated cohorts are divided into 4 groups, there are 3 indicator variables for each of the first 3 fully treated cohorts and a 4th indicator for students who enrolled 3 or more years after the adoption of a semester calendar. All regressions include age, age-squared, sex, a foreign-born indicator, indicators for race/ethnicity, campus and year fixed-effects, and campus-specific linear time trends. ***p<0.01, **p<0.05, *p<0.10.

Table 7: Effect of Switching to Semesters on Student-Level Cumulative Outcomes by Year in School

	Year 1 (1)	Year 2 (2)	Year 3 (3)	Year 4 (4)
Panel A: Outcome - Cumulative # of Credits is On-Track				
G1 - partially treated		**-0.069**	**-0.053**	**-0.051**
SE multi-way clustered		(0.014)***	(0.021)**	(0.018)***
SE clustered by campus		(0.014)***	(0.026)**	(0.023)**
Wild cluster bootstrap p-value		[0.00]***	[0.17]	[0.15]
G2 - fully treated	**-0.083**	**-0.112**	**-0.108**	**-0.115**
SE multi-way clustered	(0.042)*	(0.026)***	(0.031)***	(0.026)***
SE clustered by campus	(0.048)*	(0.032)***	(0.041)**	(0.040)***
Wild cluster bootstrap p-value	[0.26]	[0.01]***	[0.06]*	[0.03]**
Mean of Outcome	0.73	0.68	0.64	0.62
Panel B: Outcome - Cum. GPA < 2.0				
G1 - partially treated		**0.013**	**0.015**	**0.018**
SE multi-way clustered		(0.003)***	(0.004)***	(0.005)***
SE clustered by campus		(0.004)***	(0.005)***	(0.006)***
Wild cluster bootstrap p-value		[0.02]**	[0.03]**	[0.02]**
G2 - fully treated	**0.013**	**0.009**	**0.014**	**0.012**
SE multi-way clustered	(0.006)**	(0.005)*	(0.004)***	(0.005)**
SE clustered by campus	(0.008)	(0.006)	(0.006)**	(0.006)*
Wild cluster bootstrap p-value	[0.24]	[0.29]	[0.15]	[0.14]
Mean of Outcome	0.06	0.04	0.03	0.04
Panel C: Outcome - Has Ever Switched Major				
G1 - partially treated		**-0.030**	**0.005**	**0.040**
SE multi-way clustered		(0.028)	(0.033)	(0.028)
SE clustered by campus		(0.034)	(0.039)	(0.034)
Wild cluster bootstrap p-value		[0.43]	[0.91]	[0.35]
G2 - fully treated	**-0.064**	**-0.082**	**-0.052**	**-0.022**
SE clustered by campus	(0.011)***	(0.025)***	(0.036)	(0.033)
SE clustered by campus	(0.011)***	(0.032)**	(0.043)	(0.041)
Wild cluster bootstrap p-value	[0.00]***	[0.00]***	[0.33]	[0.62]
Mean of Outcome	0.13	0.40	0.55	0.61

Data Source: OLDA. Note: Sample includes first-time freshmen in the F1999-F2013 cohorts who remain enrolled for 4+ years (341,646 obs). The partially treated group is defined separately for each year/column. In column (2), G1 includes students who enrolled 1 year before the switch to semesters. In columns (3) and (4), G1 also includes student who enroll 2 and 3 years before the calendar switch, respectively. In all specifications, the fully treated group, G2, includes all students who first enroll at a university that is on a semester calendar. All regressions include age, age-squared, sex, a foreign-born indicator, indicators for race/ethnicity, campus and year fixed-effects, and campus-specific linear time trends. ***p<0.01, **p<0.05, *p<0.10.

Table 8: Effect of Switching to Semesters on Summer Employment (Q2 or Q3)

	Share of Summers Employed	Employed Summer After: 1st Year	Employed Summer After: 2nd Year	Employed Summer After: 3rd Year
	(1)	(2)	(3)	(4)
Panel A: Excluding Retail and Food Service Employment				
G2 - fully treated	**0.022**	**0.026**	**0.022**	**0.013**
SE multi-way clustered	(0.011)*	(0.009)***	(0.013)*	(0.014)
SE clustered by campus	(0.015)	(0.012)**	(0.016)	(0.019)
Wild cluster bootstrap p-value	[0.25]	[0.11]	[0.29]	[0.66]
Mean of Outcome	0.51	0.51	0.51	0.51
Panel B: Retail and Food Service Employment Only				
G2 - fully treated	**-0.017**	**-0.002**	**-0.021**	**-0.021**
SE multi-way clustered	(0.010)*	(0.006)	(0.010)**	(0.012)*
SE clustered by campus	(0.013)	(0.008)	(0.013)	(0.015)
Wild cluster bootstrap p-value	[0.38]	[0.85]	[0.27]	[0.38]
Mean of Outcome	0.23	0.23	0.23	0.22

Data Source: OLDA linked to ODJFS Unemployment Insurance Quarterly Wage data. Notes: Sample includes first-time freshmen in the F2000-F2013 cohorts who remain enrolled for 4+ years and can be linked to ODJFS employment data (296,416 obs). The partially treated group (not reported) is defined separately for each year/column and always includes the short summer preceding the calendar switch. In column 2, G1 includes students who enrolled 1 year before the switch to semesters. In column 2 it includes student enrolled 1 and 2 years before, and in column 3 (and column 1) it includes those enrolled 1-3 years before. In all specifications, the fully treated group, G2, includes all students who first enroll at a university that is on a semester calendar. All regressions include age, age-squared, sex, a foreign-born indicator, indicators for race/ethnicity, and campus and year fixed-effects. ***p<0.01, **p<0.05, *p<0.10.

Table 9: Effect of Switching to Semesters on School-Year Employment (Q4 or Q1)

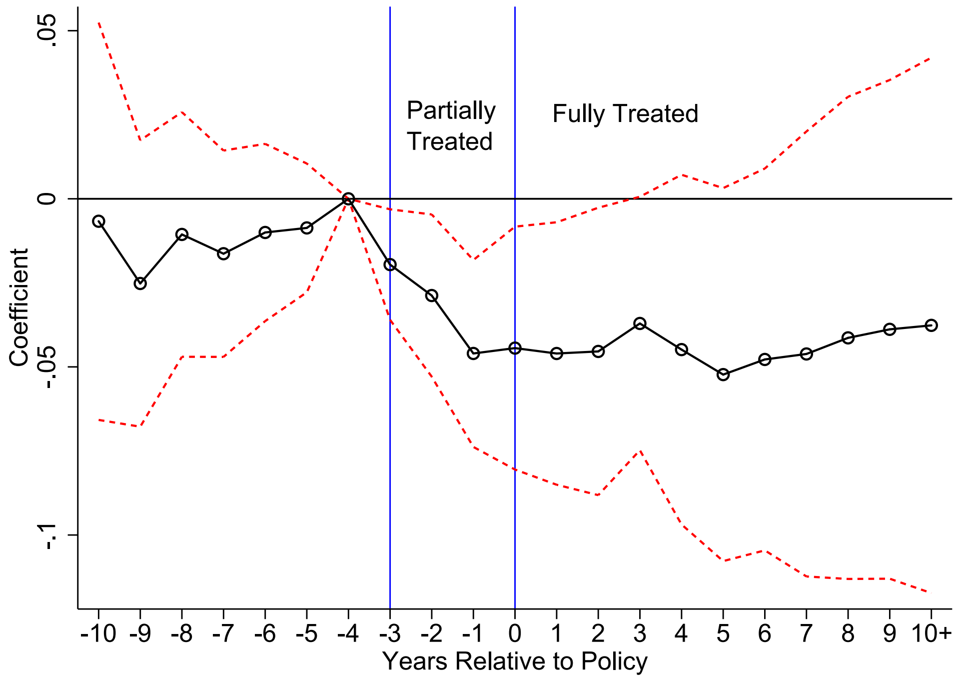
	Share of School-Years Employed	Employed During: 1st Year	2nd Year	3rd Year	4th Year
	(1)	(2)	(3)	(4)	(5)
Panel A: Excluding Retail and Food Service Employment					
G2 - fully treated	**-0.032**	**-0.056**	**-0.016**	**-0.021**	**-0.037**
SE multi-way clustered	(0.015)**	(0.022)**	(0.017)	(0.009)**	(0.023)
SE clustered by campus	(0.019)	(0.024)**	(0.019)	(0.014)	(0.028)
Wild cluster bootstrap p-value	[0.14]	[0.01]***	[0.43]	[0.21]	[0.22]
Mean of Outcome	0.38	0.31	0.37	0.41	0.43
Panel B: Retail and Food Service Employment Only					
G2 - fully treated	**-0.048**	**-0.041**	**-0.047**	**-0.049**	**-0.040**
SE multi-way clustered	(0.010)***	(0.007)***	(0.010)***	(0.011)***	(0.011)***
SE clustered by campus	(0.012)***	(0.009)***	(0.013)***	(0.015)***	(0.013)***
Wild cluster bootstrap p-value	[0.01]***	[0.00]***	[0.01]***	[0.02]**	[0.03]**
Mean of Outcome	0.21	0.20	0.22	0.22	0.21

Data Source: OLDA linked to ODJFS Unemployment Insurance Quarterly Wage data. Notes: Sample includes first-time freshmen in the F2000-F2013 cohorts who remain enrolled for 4+ years and can be linked to ODJFS employment data (296,416 obs). The partially treated group (not reported) is defined separately for each year/column. In column (3), G1 includes students who enrolled 1 year before the switch to semesters. In columns (4) and (5), G1 also includes student who enroll 2 and 3 years before the calendar switch, respectively. In column (1), G1 includes student who enrolled 1-3 years before the calendar switch. In all specifications, the fully treated group, G2, includes all students who first enroll at a university that is on a semester calendar. All regressions include age, age-squared, sex, a foreign-born indicator, indicators for race/ethnicity, and campus and year fixed-effects. ***$p<0.01$, **$p<0.05$, *$p<0.10$.

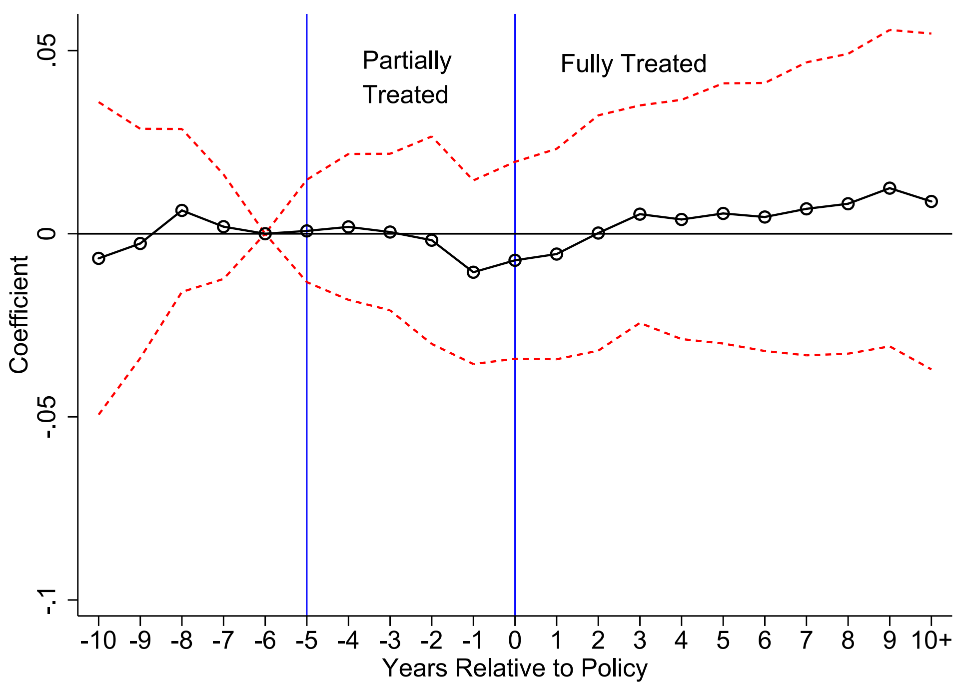
Appendix

Figure A1: Event Study: Institution-Level Analysis (omitting first 10 years of sample)

(a) 4-year Graduation Rates

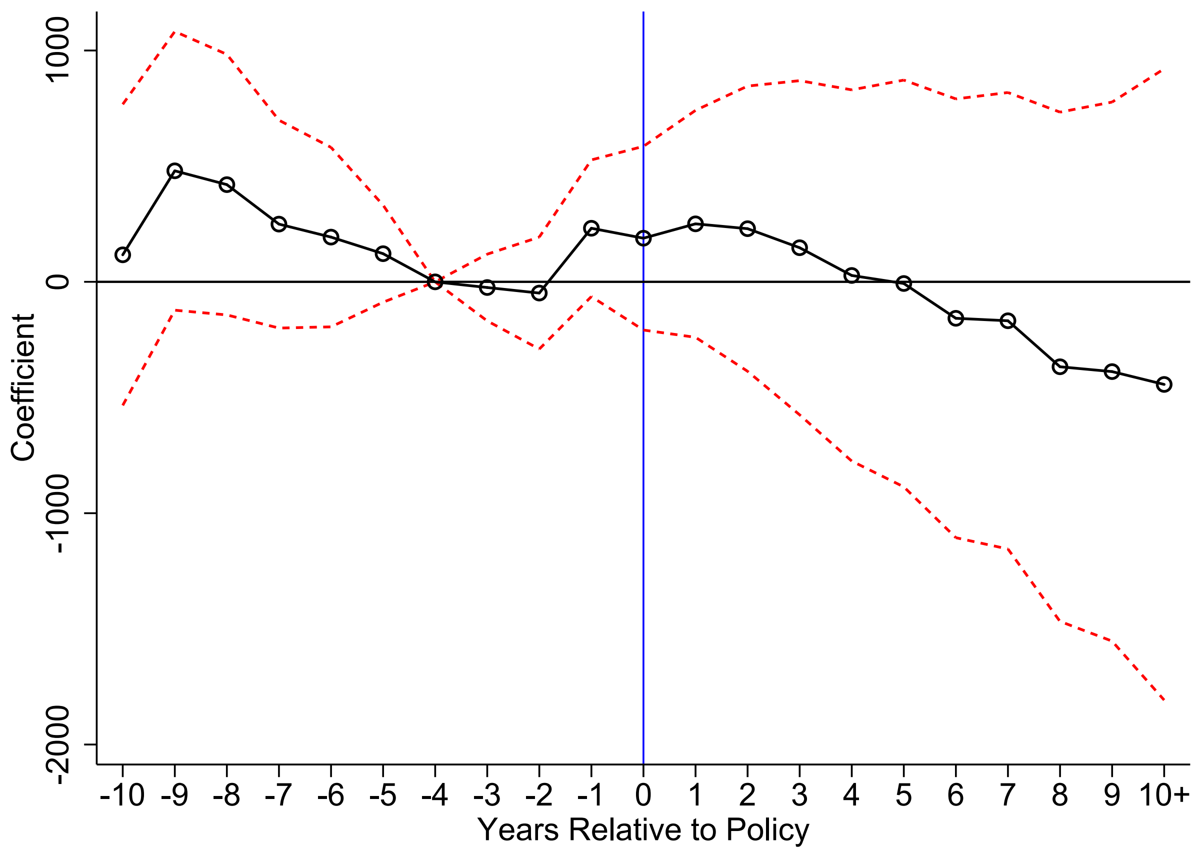


(b) 6-year Graduation Rates



Data Source: IPEDS. Notes: The sample includes 13,889 observations. This figure plots θ_k, and 95% confidence intervals in dashed lines, from estimating Eq. (1). Year and institution fixed effects, institution linear time trends, and time varying controls are included. Results are robust to excluding time varying controls.

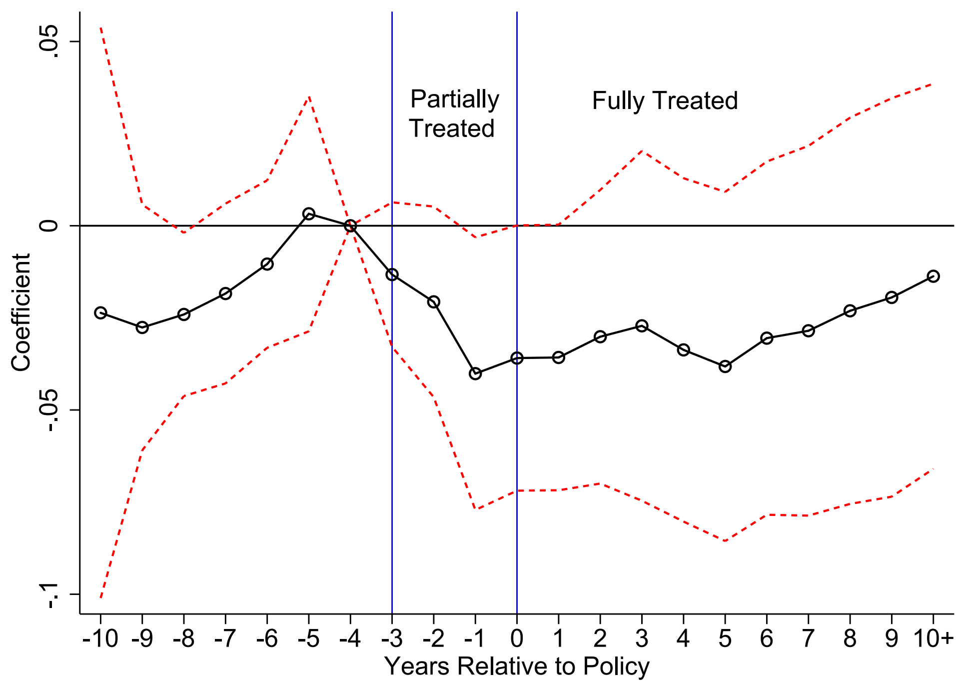
Figure A2: Event Study: Institution-Level Analysis, In-State Tuition



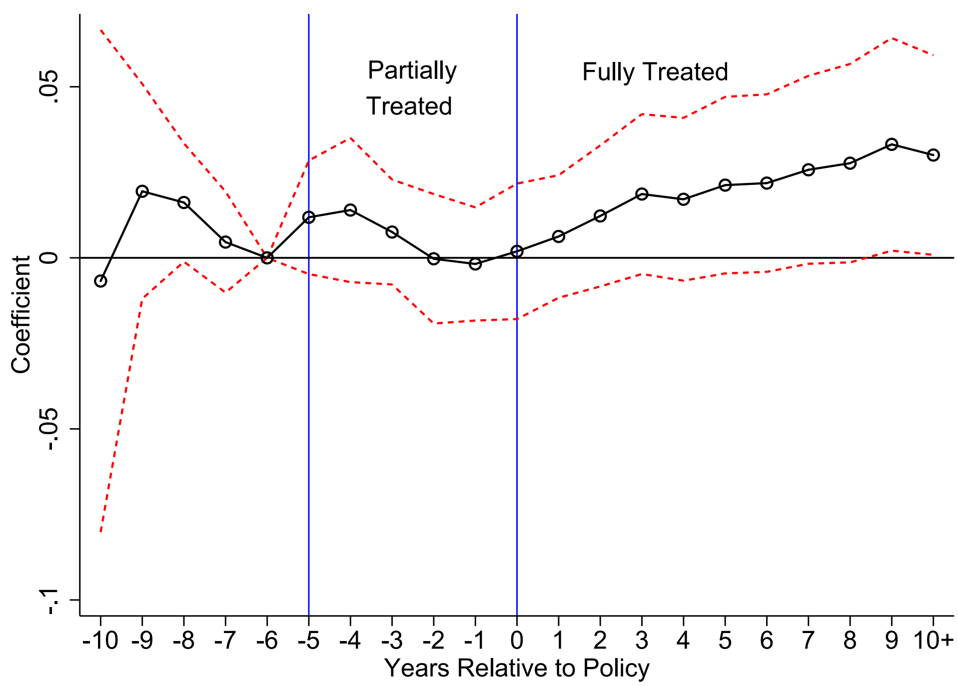
Data Source: OLDA. Notes: This figure plots θ_k, and 95% confidence intervals in dashed lines, from estimating Eq. (3). Year and institution fixed effects, institution linear time trends, and student-level controls are included. Standard errors are estimated using multi-way clustering.

Figure A3: Event Study: Institution-Level Analysis (excluding institution linear time trends)

(a) 4-year Graduation Rates



(b) 6-year Graduation Rates



Data Source: IPEDS. Notes: The sample includes 731 institutions for 19 years. This figure plots θ_k, and 95% confidence intervals in dashed lines, from estimating Eq. (1). Year and institution fixed effects, and time varying controls are included. The regression is weighted by average cohort size. Results are robust to excluding time varying controls.

Table A1: Effect of Switching to Semesters on Graduation Rates (Unbalanced Panel)

	All (1)	All (2)	All (3)	Women (4)	Men (5)	URM (6)	Non-URM (7)	Public (8)	Private (9)
Panel A: Effect on 4-year Grad. Rates									
G1 - partially treated	0.004	-0.006	-0.008	-0.006	-0.009	-0.006	-0.007	-0.004	-0.017*
	(0.014)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)	(0.013)	(0.010)
G2 - fully treated	-0.013	-0.020*	-0.021*	-0.019	-0.022**	-0.014	-0.024**	-0.016	-0.021
	(0.016)	(0.011)	(0.011)	(0.012)	(0.011)	(0.014)	(0.012)	(0.013)	(0.013)
Panel B: Effect on 6-year Grad. Rates									
G1 - partially treated	0.019	0.001	-0.001	-0.002	0.001	-0.006	-0.000	0.001	-0.003
	(0.016)	(0.010)	(0.009)	(0.010)	(0.009)	(0.015)	(0.009)	(0.011)	(0.008)
G2 - fully treated	0.028*	0.006	0.006	0.005	0.010	-0.004	0.005	0.010	-0.004
	(0.015)	(0.011)	(0.011)	(0.012)	(0.011)	(0.020)	(0.011)	(0.013)	(0.012)
Observations	22,089	22,089	22,089	22,010	21,489	22,074	21,528	8,845	13,244
School, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trends	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Data Source: IPEDS. Note: The sample includes 1,253 institutions over 19 years. Not all institutions are observed in each year. All regressions are weighted by average cohort size. Within each panel and column, point estimates come from the same regression. The left out category comprises the pre-treatment years, and is defined as ($k \leq -4$) for the four-year graduation outcome and ($k \leq -6$) for the six-year graduation outcome. Standard errors are clustered at the institution level. *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Effect of Switching to Semesters on Institution-Level Graduation Rates - Unweighted

	All (1)	Women (2)	Men (3)	URM (4)	Non-URM (5)	Public (6)	Private (7)
Panel A: Effect on 4-year Grad. Rates							
G1 - partially treated	-0.013	-0.012	-0.017*	-0.018	-0.010	-0.010	-0.011
	(0.008)	(0.009)	(0.009)	(0.011)	(0.010)	(0.010)	(0.016)
G2 - fully treated	-0.028***	-0.028**	-0.031***	-0.035***	-0.026*	-0.018	-0.036*
	(0.010)	(0.012)	(0.011)	(0.014)	(0.015)	(0.012)	(0.019)
Panel B: Effect on 6-year Grad. Rates							
G1 - partially treated	-0.010	-0.010	-0.011	-0.024*	0.002	-0.009	-0.004
	(0.008)	(0.009)	(0.010)	(0.013)	(0.011)	(0.011)	(0.011)
G2 - fully treated	-0.003	-0.004	-0.001	-0.027	0.006	0.002	-0.006
	(0.011)	(0.013)	(0.013)	(0.017)	(0.020)	(0.014)	(0.014)
Observations	13,889	13,865	13,824	13,883	13,799	6,365	7,524
School, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Data Source: IPEDS. Note: The sample includes 731 institutions for 19 years. Within each panel and column, point estimates come from the same regression. The left out category comprises the pre-treatment years, and is defined as ($k \leq -4$) for the four-year graduation outcome and ($k \leq -6$) for the six-year graduation outcome. Standard errors are clustered at the institution level. *** p<0.01, ** p<0.05, * p<0.1.

Table A3: Timing of Calendar Switch for Ohio Campuses

Institution	Year of Switch to Semesters	Total # Obs	% of Obs on Semesters
Kent State University - Ashtabula Campus	1979	3,584	100%
Kent State University - East Liverpool Campus	1979	1,468	100%
Kent State University - Geauga Campus	1979	2,545	100%
Kent State University - Main Campus	1979	56,134	100%
Kent State University - Salem Campus	1979	3,117	100%
Kent State University - Stark Campus	1979	9,727	100%
Kent State University - Trumbull Campus	1979	5,126	100%
Kent State University - Tuscarawas Campus	1979	4,957	100%
Bowling Green State University - Firelands Campus	1982	4,463	100%
Bowling Green State University - Main Campus	1982	51,081	100%
Miami University - Hamilton Campus	Before 1987	9,732	100%
Miami University - Main Campus	Before 1987	48,914	100%
Miami University - Middletown Campus	Before 1987	6,025	100%
University of Akron - Main Campus	Before 1987	53,784	100%
University of Akron - Wayne Campus	Before 1987	4,830	100%
University of Toledo	1997	50,344	100%
Cleveland State University	1999	18,180	100%
Youngstown State University	2000	32,925	95%
Central State University	2005	7,926	71%
Shawnee State University	2007	13,738	61%
Ohio State University - Agricultural Technical Institute	2012	5,126	24%
Ohio State University - Lima Campus	2012	6,067	25%
Ohio State University - Main Campus	2012	94,822	24%
Ohio State University - Mansfield Campus	2012	6,977	28%
Ohio State University - Marion Campus	2012	6,410	25%
Ohio State University - Newark Campus	2012	14,673	31%
Ohio University - Chillicothe Campus	2012	4,471	28%
Ohio University - Eastern Campus	2012	1,793	22%
Ohio University - Lancaster Campus	2012	5,338	27%
Ohio University - Main Campus	2012	54,890	23%
Ohio University - Southern Campus	2012	3,978	22%
Ohio University - Zanesville Campus	2012	4,111	20%
University of Cincinnati - Clermont Campus	2012	8,216	26%
University of Cincinnati - Main Campus	2012	54,972	23%
University of Cincinnati - Raymond Walters Campus	2012	10,729	36%
Wright State University - Lake Campus	2012	2,640	31%
Wright State University - Main Campus	2012	35,591	23%
Total		709,404	64%

Data Source: OLDA. Notes: Sample includes all students enrolling as first-time freshmen at a bachelor's degree-granting public Ohio institution in the Fall terms of 1999-2015.

Table A4: The Effect of Semesters on Student Characteristics

	Female	Age	Foreign-Born	URM
	(1)	(2)	(3)	(4)
Semester	-0.004	0.180	0.009	-0.006
	(0.009)	(0.133)	(0.008)	(0.015)
Mean of outcome	0.53	19.0	0.02	0.13
Observations	709,404	709,404	709,404	709,404

Data Source: OLDA. Note: Sample includes all students who enroll as first-time freshmen at a bachelor's degree-granting public institution in the fall term of the years 1999-2015. All regressions include year and campus fixed effects. Standard errors are reported in parentheses and are clustered at the campus level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.