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

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There exists a long-standing debate in higher education on which academic calendar is optimal. 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 the negative effects persist beyond the transition. Using transcript data, we replicate this analysis at the student level and investigate possible mechanisms. Shifting to a semester: (i) lowers first-year grades, (ii) decreases the probability of enrolling in a full course load, and (iii) delays the timing of major choice. (*JEL* I23, I28)

There has been a long-standing debate in higher education about which academic calendar is optimal: semesters or quarters. While semesters have always been the predominant calendar, recently a large number of institutions have converted from quarters to semesters, making quarters increasingly rare. These conversions have been widespread, directly affecting nearly 2 million students at 132 colleges and universities since 1987.[1] Many of these calendar adoptions are the result of state-level mandates, whereby all schools within a state system are required to convert to semesters within a specified time frame. Among schools that remain on a quarter calendar, the possibility of switching to semesters is a hotly debated issue.

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

One such school, the University of California, Los Angeles (UCLA) has actively considered a calendar switch for decades. Twice since 1985, the school has moved to make the change to semesters but both times was blocked by faculty opposition. In March 2019, Chancellor Gene Block spoke in favor of a switch to semesters in an attempt to gain support from students and faculty, stating, "The quarter system, in my view, is a failed system" (Warner and Hanczor 2018; Fitzmorris 2019; Morris 2019). State and university officials often assert that the reasons for adopting semesters are to improve students' academic outcomes and increase their odds of securing summer internships, yet surprisingly little evidence exists on the effects of the academic calendar 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 I provides a more complete discussion of the costs and benefits associated with each calendar.)

This paper provides the first comprehensive, at-scale analysis of this policy change using two complementary datasets: an institution-level panel of the near universe of four-year nonprofit institutions and student-level transcript data from one of the largest state public education systems. 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 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).[2] The public university system in Ohio is one of the largest comprehensive

[2]The OLDA 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 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, see http://chrr.osu.edu/projects/ohio-longitudinal-data-archive.

postsecondary systems in the nation—serving over 300,000 students annually at 13 4-year universities and 24 regional 4-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 grade point average (GPA) that is below the 2.0 threshold for academic probation, less likely to enroll in the recommended number of credits per year, and 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 does not provide compelling evidence that the switch to a semester calendar improves summer employment in the types of jobs that are most likely to represent internship employment.[3]

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, analyze the longer-term effects of these changes, and address this question at a large scale using the near universe of institutions in the United States. The few existing case studies on university calendar changes focus on a small subset of schools and compare outcomes at those schools in the one- to two-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, Bolte, and Franklin (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 that the conversion from quarters to semesters might be academically harmful 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.

This study also 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 the adjusting of school start times (Bostwick 2018; Cortes, Bricker, and Rohlfs 2012; Hinrichs 2011; Edwards

[3] Because state unemployment records do not indicate whether a job is an internship, we use a proxy and define internship-type jobs as employment in nonretail and non-food-service industries using the North American Industry Classification System code.

2012; Carrell, Maghakian, and West 2011).[4] For the most part, these reforms have been adopted at the elementary and secondary levels 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.

More generally, we add to a growing body of work focused on understanding postsecondary graduation outcomes. 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 percent 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 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, Marx, and Turner 2019; Deming and Walters 2017; Cohodes and Goodman 2014; Bound, Lovenheim, and Turner 2010, 2012; Bound and Turner 2007; Singell 2004).[5] We contribute to this line of research by considering how an institution's academic calendar affects graduation rates and investigating the underlying mechanisms.

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 I provides a background of the two academic calendars and includes a discussion of the potential costs and benefits associated with each. Section II presents the institution-level analysis: the data; the empirical framework, including a discussion of the identifying assumption; and the results. Section III presents the individual-level analysis: a replication of the main results, a mechanism analysis, and employment findings. Section IV concludes.

I. Background

While semesters have always been more common, quarters were first introduced to the United States in 1891 at the University of Chicago. When the school was

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

[5] Denning et al. (forthcoming) 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.

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 percent of US institutions reported being on a semester calendar, and 22 percent on quarters. During the 1960s, several large statewide educational systems switched from semesters to quarters to accommodate enrollment booms caused by the baby boomers, e.g., most notably the University of California system. However, starting in 1970, this trend reversed. In 1970, 70 percent of schools operated on semesters (Day 1987), but by 1990, that share had increased to 87 percent. Many of the recent calendar shifts occurred in the late 1990s, but some universities such as the University System of Ohio converted more recently (2012), and many schools in the California State University and University of California systems are considering switching in the near future (Gordon 2016). As of 2019, about 95 percent 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 2 15-week terms where a student takes 5 3-credit-hour courses per term.[6] Generally, courses meet either 3 times per week for 50 minutes, 2 times per week for 75 minutes, or 2 times per week for 60 minutes with an additional 1-hour-per-week lab or discussion section. A typical quarter schedule includes three ten-week terms where students take three or four four-to-five-credit-hour courses per term.[7] There are a number of ways that quarter courses are configured, and there is some heterogeneity across institution type, i.e., liberal arts versus research institutions. Four-credit courses usually include 2 2-hour lectures per week or 4 50-minute lectures per week. It is also common for courses to meet 2 times per week for 75 minutes and include an additional hour discussion section to attain the 4 hours of weekly contact. Quarter systems also allow for a full ten-week summer term.

Under either regime, in order to be on track to graduate in 4 years, students must take, on average, 15 credit hours per term. As such, a minimum of 120 credit hours are required to graduate in a semester system, and 180 hours in a quarter system. In summary, full-time on-track students will attend approximately 15 hours of class per week regardless of the calendar, but the number of simultaneous courses is higher on a semester calendar. Scheduling five courses is likely more challenging than scheduling three or four courses and could make scheduling extracurriculars, like part-time employment, an additional challenge.

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, universities, and community colleges (Smith 2012). School administrators believe that there are many benefits of a common schedule. Because a majority of schools operate on a semester calendar, institutions on quarters feel that their students are

[6] Credit hours refer to the number of instructor contact hours per week; i.e., three credit hours correspond to approximately three hours of lecture per week.

[7] There is some heterogeneity in credit hours per course across schools and disciplines. For instance, science, technology, engineering, and mathematics (STEM) courses in quarter institutions are usually five credit hours, and humanities courses are usually four. As a robustness check, we conduct heterogeneity analyses by STEM and non-STEM majors and find that the effects are nearly identical across the two subgroups.

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 middle 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.[8] If a student must repeat a course, it is more likely on a semester calendar that she will have to wait until the following school year to do so. 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 exploring and switching across majors more costly. Generally, there are fewer courses to choose from in a semester calendar, and students are exposed to fewer professors.[9] To highlight the added cost, consider a full-time semester student who wishes to switch majors midway through her freshman year. She spends one-eighth 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 one-twelfth 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 given time. On the other hand, students on semesters have more time with instructors and more time to master complex material. 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 GPA 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 multiyear 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

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

[9] Although descriptive in nature, a comparison of course offerings from UCLA (which is on a quarter schedule) and UC Berkeley (semester schedule) in psychology, English, and political science shows that UCLA offers substantially more courses in each department: 61 percent, 37 percent, and 43 percent more, respectively (Ramzanali 2010).

curriculum and courses to fit within the longer term. Guidance and scheduling counselors must also be retrained 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 costs 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.

II. Institution-Level Analysis

We begin our analysis at the institution level by employing data on the near universe of four-year nonprofit institutions. 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.

A. *Institution-Level Data*

All data for the institution-level analysis come from the IPEDS, a branch of the National Center for Education Statistics (NCES), and comprise a school-level panel that covers almost all four-year, nonprofit higher education institutions within the United States (US Department of Education and NCES, 1991–2016). 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 on-time 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 4-year college or university between 1991 and 2010.[10] 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). The final dataset includes 731 institutions over 19 years for a total of 13,889 observations.[11]

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 IPEDS, includes 7 different mutually exclusive categories: (i) 2

[10] 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.

[11] In online 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.

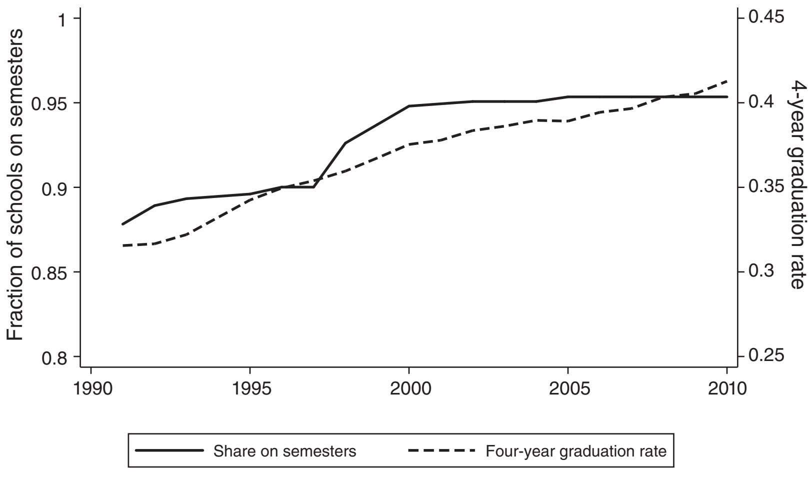

Figure 1. Fraction of Schools on Semesters and Four-Year Graduation Rates

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

15-to-16-week semesters; (ii) 3 10-to-12-week quarters plus a summer quarter; (iii) 3 12-to-13-week trimesters without a summer term; (iv) a 4-1-4 system consisting of 2 4-month (semester) blocks with a 1-month, 1-course block; (v) nontraditional calendar systems used often for online courses; (vi) calendar systems that differ by program, commonly used by vocational and occupational programs; and (vii) a continuous academic calendar system that allows students to enroll at any time during the year.

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 moves 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 percent of the institutions in our sample are on trimesters, and 8 percent of the institutions are on a 4-1-4 schedule. Our results 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 freshmen and thus exclude transfer students.

Figure 1 visually displays the policy variation that we exploit; in 1991, about 87 percent of schools operated on a semester calendar, and this increased to 95 percent 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

Table 1—Institution-Level Summary Statistics

	All (1)	Never switchers (2)	Switchers (3)
Semester calendar	0.93	0.96	0.72
	(0.25)	(0.20)	(0.45)
Four-year graduation rate	0.36	0.37	0.28
	(0.22)	(0.22)	(0.16)
Four-year women graduation rate	0.40	0.42	0.34
	(0.22)	(0.23)	(0.18)
Four-year men graduation rate	0.30	0.31	0.23
	(0.21)	(0.22)	(0.15)
Four-year URM graduation rate	0.29	0.30	0.21
	(0.20)	(0.21)	(0.14)
Four-year non-URM graduation rate	0.37	0.39	0.30
	(0.22)	(0.23)	(0.17)
Six-year graduation rate	0.58	0.59	0.54
	(0.18)	(0.18)	(0.17)
Six-year women graduation rate	0.61	0.62	0.57
	(0.17)	(0.17)	(0.17)
Six-year men graduation rate	0.55	0.55	0.51
	(0.19)	(0.19)	(0.18)
Six-year URM graduation rate	0.51	0.51	0.46
	(0.19)	(0.19)	(0.17)
Six-year non-URM graduation rate	0.60	0.61	0.56
	(0.18)	(0.18)	(0.17)
Public	0.46	0.43	0.71
	(0.50)	(0.49)	(0.45)
Full-time-equivalent 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 ($/million)	192.10	185.14	249.54
	(400.54)	(390.65)	(470.59)
Observations	13,889	12,388	1,501

Notes: 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.

Source: IPEDS

is 36 percent, with women having a significantly higher rate, 40 percent, than men, 30 percent. Underrepresented minority (URM) graduation rates are just below male rates at 29 percent. As expected, the average six-year graduation rate is much higher, 58 percent. 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 did not change their calendar system between 1991 and 2010 and those that changed to semesters during the time period. The most striking difference between the two groups is the share of public institutions; 71 percent of switchers are public, compared to 42 percent of never-switchers. This difference also drives differences in the average cohort size (1,376 versus 1,066) and the average in-state tuition (\$7,240 versus \$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 from large public universities. However, these differences in means between switchers and never-switchers do not threaten the internal validity of the estimates presented in Section IIC, as the causal interpretation of the results does not rely on covariate similarity between the two groups.

B. *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:

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

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. The function 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 $k = 0$. When considering four-year graduation rates as the outcome variable, 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, $k = -4$. For estimation with six-year graduation rates as the outcome variable, the partially treated cohorts are $k = \{-1, -2, -3, -4, -5\}$ and the omitted category is $k = -6$.

We restrict the effect of treatment on all cohorts who enrolled more than ten years before or after the calendar switch to semesters to be unchanging so that θ_{-10} and θ_{10} represent the average effect ten or more years prior to or after the calendar switch, respectively.[12] There are a total of 25 prepolicy years and 22 post years in the sample. The vector X_{st} includes time-varying university-level controls, including in-state tuition, number of full-time-equivalent faculty, annual operation costs, percent of students female, percent of students white, and percent

[12] 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 ten years before the start of our sample. In online Appendix Figures A1a and A1d, we show that our results are robust to dropping the first ten years of IPEDS data where these classification errors might occur.

of students who are URMs.[13] The variables γ_s and ϕ_t are university and year fixed effects, respectively. The model also includes institution-specific linear time trends, $\rho_s \times t$. We discuss how the inclusion of linear time trends impacts identification in the text below and address the results of excluding linear time trends in detail in Section IIC. All regressions are weighted by average cohort size, and standard errors are clustered by institution.[14]

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

$$(2) \qquad Y_{st} = \beta_1 G1_{st} + \beta_2 G2_{st} + X'_{st}\alpha + \gamma_s + \phi_t + \rho_s \times t + \varepsilon_{st}.$$

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 one to five 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 equation (1).

The identifying assumption for estimating a standard difference-in-difference model in this setting is that the adoption of the semester calendar is uncorrelated with other unobserved time-varying determinants of four-year and six-year graduation rates (i.e., the parallel trends assumption). The inclusion of institution and year fixed effects controls for time-invariant institution-level variables and overall time trends that might affect graduation rates. However, by also including institution-specific linear time trends, we control for differential trends in graduation rates across schools over time. As such, the identifying assumption in this model is that in the absence of a change in academic calendar, the switchers and the nonswitchers would continue along the same differential trends in graduation rates. This parallel growth assumption is less restrictive than the standard parallel trends assumption in that we have already controlled for differences in trends across the switchers and nonswitchers (with the institution-specific linear time trends), and it now requires only that the difference between the growth rates of these groups is constant in the absence of a calendar conversion (Mora and Reggio 2019).

While the identifying assumption is not directly testable, several indirect tests support its plausibility. First, the change in the four-year graduation rate precisely coincides with the timing of semester adoption. This is visually evident in the results of estimating equation (1) shown in Figure 2. Second, we examine whether adoption of the semester calendar is correlated with changes in other observed time-varying

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

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

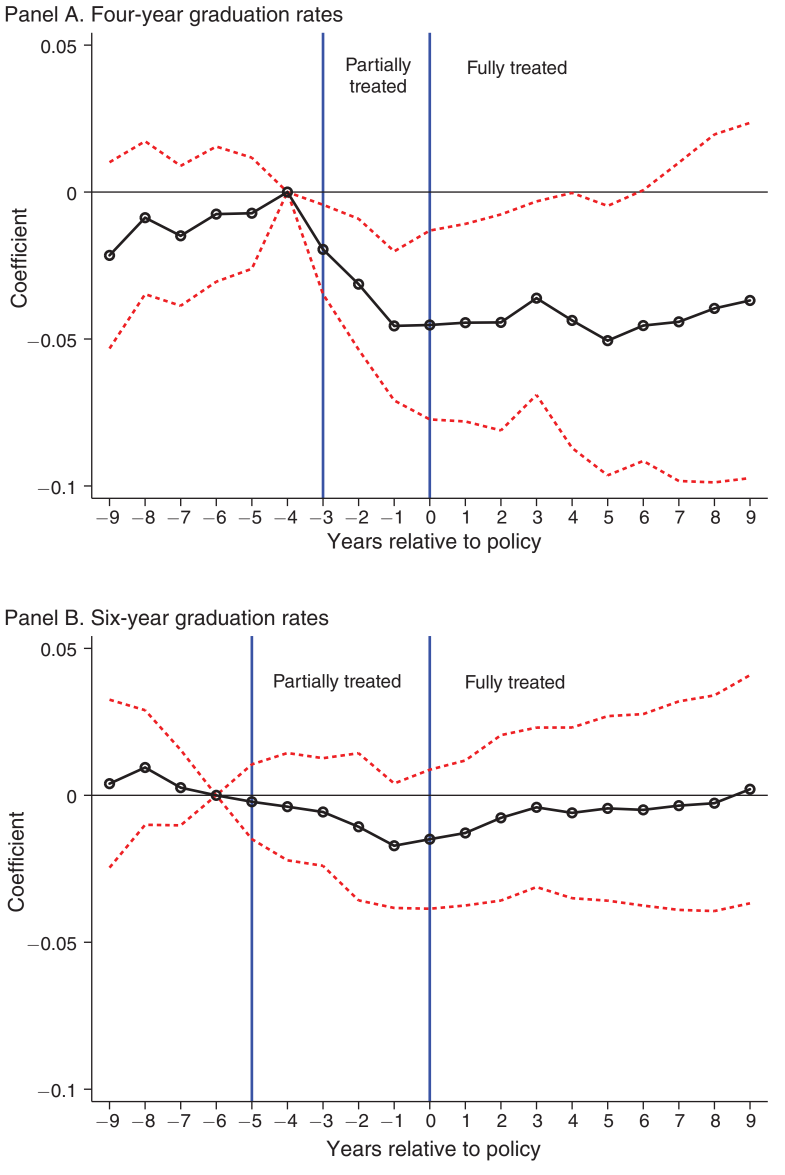

Figure 2. Event Study: Institution-Level Analysis

Notes: The sample includes 731 institutions for 19 years (13,889 observations). This figure plots θ_k, and 95 percent confidence intervals in dashed lines, from estimating equation (1). Year and institution fixed effects, institution-specific linear time trends, and time-varying controls are included. See online Appendix Figures A1 and A3 for various robustness checks. Standard errors are clustered at the institution level.

Source: IPEDS

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, and percent female) on a semester calendar indicator, year and institution fixed effects, and

Table 2—Effect of Switching to Semesters on Institution and Student Characteristics

	Institution characteristics				Student characteristics		
	FTE faculty (1)	Costs (2)	In-state tuition (3)	Cohort size (4)	Percent URM (5)	Percent white (6)	Percent female (7)
Semester	−0.282	6.435	346.717	−33.185	−0.011	0.009	−0.003
	(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

Notes: Each column represents a separate regression where different pretreatment characteristics are the outcomes. All regressions include a dummy for being on a semester calendar, year fixed effects, institution fixed effects, and institution-specific linear time trends. Standard errors are reported in parentheses and are clustered at the institution level.

Source: IPEDS

institution-specific linear time trends. For the most part, Table 2 shows no sign of a relationship between changes in 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 online Appendix Figure A2) and find that institutions are raising tuition by about 3 percent, 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 I. Regardless, this small increase in tuition likely cannot account for the sizable decline in on-time graduation that we find in Section IIC.[15] 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.

A third concern that could confound the interpretation of our results is the possibility that schools enact other policies or 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 final concern is that institutions that change to a semester system may be inherently different from those that 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 that 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

[15] We also find that student employment during the school year 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 IIIF.

570 at private schools, and nearly half of the institutions in the dataset are public—our results are relevant to a majority of students in the United States. Second, in a heterogeneity analysis, we find similar results among the subset of private schools, again suggesting that our results extend widely.

C. *Institution-Level Results*

The main results are represented in the event study in Figure 2 and come from estimating equation (1). Figure 2, panel A reports the effect of policy adoption on four-year graduation rates (on-time graduation), and Figure 2, panel B on six-year graduation rates. For four-year graduation rates, the pretreatment region, which represents all untreated cohorts, is $k < -3$. 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 $k \in [-3, -1]$. 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 posttreatment region, $k \geq 0$, includes cohorts who are fully treated. For six-year graduation rates, the omitted cohort is $k = -6$ and the partially treated region is $k \in [-5, -1]$.

Figure 2, panel A 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, grows larger as cohorts become more fully treated (i.e., as they are exposed to more years of a semester calendar), and levels out and remains negative among the fully treated cohorts. 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. Figure 2, panel A reveals that cohorts enrolling nine or more years after the adoption of semesters experience a similar reduction in on-time graduation of approximately 5 pp. As such, the negative impact on student outcomes is not merely a short-term consequence of the calendar switch but a longer-term effect likely driven by some characteristic of the semester calendar.

Figure 2, panel B repeats this exercise for six-year graduation rates. After adoption of the semester calendar, there is no statistically significant impact on six-year outcomes. This smaller and statistically insignificant effect of the calendar change on six-year graduation rates suggests that students on a semester calendar are increasing their time to degree but not necessarily dropping out. Ideally, we would like information on retention but instead use six-year graduation rates as a proxy for whether students ever graduate because retention is not observable in the IPEDS data. In Section III, we employ an alternative dataset to more directly address the question of whether there is an effect on student retention.

There are at least two important takeaways from the event study figures. First, the pretreatment regions in both panels of Figure 2 reveal that prior to semester adoption, there are no statistically significant deviations from trends in graduation rates between institutions that switch and those that switched at different times or not at all, conditional on the included controls. This helps assuage concerns that events or policies enacted in the years prior to the calendar switch might confound the estimates. Second, the change in the outcome coincides precisely with the timing of semester adoption. This minimizes the concern that the estimated effect is driven by a trend and strongly suggests that the change in four-year graduation rates is a causal effect of the switch to semesters. Finally, since many of the calendar adoptions are a result of state-level mandates, in a robustness check, we consider the subsample of public institutions in such states (Georgia, Minnesota, North Dakota, Ohio, Texas, and Utah). The point estimates for this subset of institutions that did not select into treatment are nearly identical to the main estimates; see online Appendix Figure A3.

Next, we probe the decision to include institution-specific linear time trends. To begin, consider online Appendix Figures A1b and A1e, which report event studies without controls for 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 four-year graduation rates are differentially trending upward for the switching schools 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. The trends in online Appendix Figure A1e are less clear, but it appears that six-year graduation rates are differentially trending upward over all periods for switching schools, and consistent with Figure 2, panel B, there is no noticeable effect of the calendar change on this outcome.

An alternative approach to dealing with these trends is to consider a more comparable control group. In online Appendix Figures A1c and A1f, we keep only those institutions in the sample that experienced a calendar switch and estimate the event study without linear time trends. By excluding never-switchers, our estimates represent only comparisons between switchers and those that switched at different points in time. The estimates do not represent comparisons between switchers and never-switchers. The results are nearly identical to those presented in Figure 2—even though the specification excludes linear trends—and show clearly that there are no differential pretrends in four-year graduation rates across schools that switched to semesters in different years.

Table 3A presents results from equation (2), which leverages the difference-in-difference 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 (2021), we separately estimate the effects of the partially treated cohorts from the fully treated cohorts rather than estimating a single postperiod indicator because, as evident in Figure 2, panel A, the treatment effects are heterogeneous over these cohorts. Each column within panel A represents a separate regression. Columns 1–3 include varying levels of controls. Unsurprisingly—given the event studies discussed above—the point estimates presented in columns 1 and 2, which come from specifications that do not include institution-specific linear time trends, are small in magnitude and indistinguishable from zero. The results from the main specification (column 4), which include the

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

	All (1)	All (2)	All (3)	All (4)
Panel A. Effect on four-year graduation rates				
G1 - partially treated	−0.003	−0.013	−0.021	−0.024
	(0.016)	(0.010)	(0.009)	(0.009)
G2 - fully treated	−0.009	−0.015	−0.035	−0.037
	(0.020)	(0.017)	(0.012)	(0.012)
Panel B. Effect on six-year graduation rates				
G1 - partially treated	0.010	0.002	−0.011	−0.013
	(0.019)	(0.013)	(0.009)	(0.008)
G2 - fully treated	0.024	0.016	−0.012	−0.014
	(0.018)	(0.014)	(0.010)	(0.010)
Observations	13,889	13,889	13,889	13,889
School, year fixed effects	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Institution-specific time trends	No	No	Yes	Yes

Notes: 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 pretreatment 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 clustered at the institution level. Results are robust to holding constant the sample size across the columns.

Source: IPEDS

full set of controls, 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 percent; thus, a 3.7 pp reduction is equivalent to a 10 percent reduction at the mean. The partially treated cohorts experience a smaller negative effect of 2.4 pp. These point estimates are robust to the inclusion of time-varying institution-level covariates, as seen by comparing columns 3 and 4.

Next, we divide the data into several subgroups: males, females, URMs, non-URMs, public institutions, and private institutions. We estimate the model separately for each subgroup and report these results in Table 3B. 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 3A 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 only marginally significant.

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 percent). Using regression discontinuity analyses, Denning, Marx, and Turner (2019) find that eligibility for the maximum

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

	Women (1)	Men (2)	URM (3)	Non-URM (4)	Public (5)	Private (6)
Panel A. Effect on four-year graduation rates						
G1 - partially treated	−0.023	−0.025	−0.022	−0.024	−0.022	−0.015
	(0.010)	(0.008)	(0.011)	(0.010)	(0.010)	(0.015)
G2 - fully treated	−0.037	−0.038	−0.028	−0.039	−0.032	−0.036
	(0.014)	(0.012)	(0.017)	(0.013)	(0.014)	(0.020)
Panel B. Effect on six-year graduation rates						
G1 - partially treated	−0.014	−0.011	−0.020	−0.010	−0.013	−0.011
	(0.009)	(0.008)	(0.013)	(0.009)	(0.009)	(0.007)
G2 - fully treated	−0.017	−0.009	−0.027	−0.013	−0.012	−0.019
	(0.012)	(0.010)	(0.019)	(0.010)	(0.011)	(0.011)
Observations	13,865	13,824	13,883	13,799	6,365	7,524
School, year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Institution-specific time trends	Yes	Yes	Yes	Yes	Yes	Yes

Notes: 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 pretreatment 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 clustered at the institution level. Results are robust to holding constant the sample size across the columns.

Source: IPEDS

Pell Grant award leads to a 10 percent 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 6 years by 22 percent.

III. Individual-Level Analysis

We next turn to a student-level analysis using detailed transcript data from all 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 IIC. 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 GPA, and major choice. We also link these data to employment files from Ohio's unemployment insurance (UI) system to study the effects of the calendar conversion on student employment.

A. *Individual-Level Data*

The student-level data are provided by the OLDA and include administrative transcript records for all students attending public colleges in Ohio between summer 1999 and spring 2017 (Ohio Department of Higher Educations, 1999-2017). 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.[16] 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 United States and more than half of the schools in Ohio switched from quarters to semesters in the sample time period.[17] 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.[18] In total, 64 percent of students in the full sample first enrolled under a semester calendar, while 36 percent 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 (i) graduate, (ii) drop out, and (iii) transfer to another school (within the dataset) in year $y \in [1,5]$ of enrollment.[19] 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.[20]

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 percent female, predominantly white (78 percent), and almost entirely US born (98 percent). Panel B of Table 4 shows summary statistics for the individual outcome variables in this sample. The 4-year graduation rate is 23 percent (this is lower than the national average of 36 percent shown in Table 1), and the 5-year graduation rate is 40 percent. 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 percent of students drop out in their first year, while 8 percent transfer to another public Ohio college, and only 54 percent of students enroll in a full-time course load.[21] Note that while graduation rates increase significantly from year 4 to year 5 of enrollment (shown in panel B), dropout 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 six 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 six years to graduate (only 4.7 percent).

[16] 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.

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

[18] 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.

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

[20] To classify majors, we rely on the Classification of Instructional Programs crosswalk from the NCES (US Department of Education and NCES, 1990-2010).

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

Table 4—Individual-Level Summary Statistics

	Mean	Standard deviation	Observations
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-year graduation rate	0.23	0.42	627,988
Five-year graduation 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
Cummulative GPA $<$ 2.0	0.24	0.43	709,404
Switch major	0.11	0.32	709,404

Notes: 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, four-year graduation rates are measured only for the F1999–F2013 cohorts, and five-year graduation rates are measured only for the F1999–F2012 cohorts.

Source: OLDA

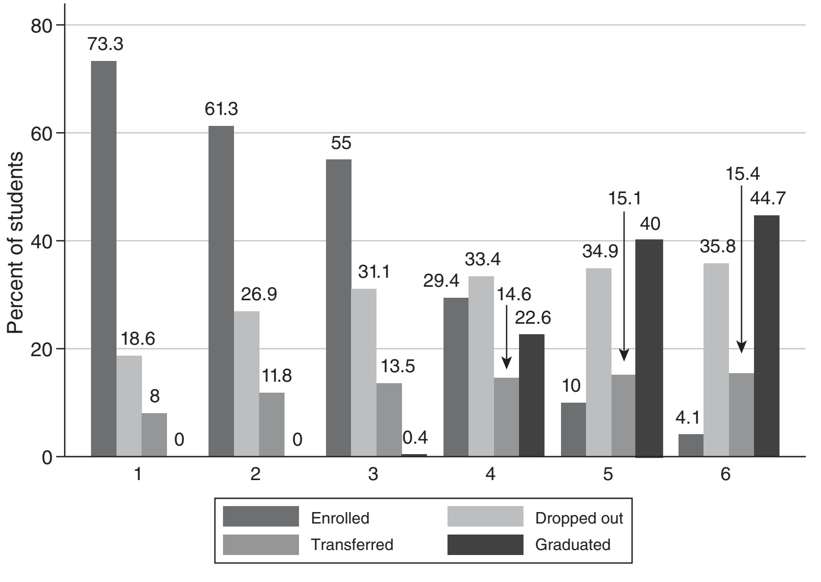

Figure 3. Students' Enrollment Status by Year

Notes: x-axis measures years since initial matriculation. Sample includes all students who enrolled as first-time freshmen at a bachelor's-degree-granting public Ohio institution in the fall terms of 1999 to 2011.

Source: OLDA

In Section IIIC, we report estimates for separate subgroups of students who we define to be from high- or low-income backgrounds. Unfortunately, we do not observe parental income or financial aid eligibility. As such, in order to identify students in the sample who are high or low income, we link the OLDA transcript data to the 2006–2010 American Community Survey (ACS) (Ruggles et al. 2020). In the transcript data, we observe the zip code where each student's high school was located and map this to the mean household income reported for that zip code in the ACS.[22] 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 Section IIIF, we link the transcript data to quarterly wage data from the Ohio Department of Job and Family Services (ODJFS 1999-2018).[23] The ODJFS collects quarterly earnings data through the UI system for all individuals working in Ohio who are not (i) self-employed or (ii) 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.[24] The data also include the industry in which each student was employed, as categorized by 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.

B. *Empirical Framework: Individual Level*

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

$$Y_{ist} = \sum_{k=-10}^{5} \theta_k G_{stk} + X_i'\alpha + \gamma_s + \phi_t + \rho_s \times t + \varepsilon_{ist}, \tag{3}$$

where Y_{ist} is an indicator that individual i enrolled at school s completes a bachelor's degree within four 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, respectively, and $\rho_s \times t$ are campus-specific linear time trends. As in equation (1), G_{stk} is an indicator for k years from the adoption of a semester system. We restrict the effect of the treatment on all cohorts who enrolled more than ten years before or more than five years after the switch to semesters to be unchanging so that θ_{-10} and

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

[23] To convert earnings data from nominal into real dollars, we also link these data to the Bureau of Labor Statistics' Consumer Price Index (US Bureau of Labor Statistics 2004-2020).

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

θ_5 represent the average effect ten or more years prior to or five or more years after the switch, respectively.[25]

We also employ a difference-in-difference approach that is analogous to equation (2) for the individual-level data:

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

where $G1_{ist}$ is an indicator for students who are in partially treated cohorts. That is, students who first enroll at a university one to three 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 one to four 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 equation (3).

We estimate both of the above models using ordinary least squares (OLS).[26] 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, Duflo, and Mullainathan (2004) show that this approach can lead to underestimated standard errors and overrejection 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 IIIC. 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, Gelbach, and Miller 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, Gelbach, and Miller 2011) along five dimensions. These dimensions correspond to the five 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 $i \neq j$, $E[\varepsilon_{ist}\varepsilon_{jsr} | x_{ist}, x_{jsr}] = 0$ unless (i) $t \in [r-4, r]$, (ii) $t \in [r-3, r+1]$, (iii) $t \in [r-2, r+2]$, (iv) $t \in [r-1, r+3]$, or (v) $t \in [r, r+4]$. This multiway clustering structure allows for arbitrary correlation between the errors of any two students who enroll

[25] Unlike in the institution-level analysis in Section II, here the majority of switching occurs late in the sample (2012), so we do not attempt to separately estimate treatment effects for cohorts more than five years post–calendar adoption.

[26] Estimates using a probit maximum likelihood estimator are very similar.

at the same campus within four years of each other and assumes a zero correlation between students who either attend the same university five or more years apart or 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 three 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 IIIC, we report (i) standard error estimates using multiway clustering, (ii) standard error estimates using campus-level clustering, and (iii) p-values for a Wald test estimated using wild cluster bootstrapping.

C. *Individual-Level Results*

We first focus on replicating the results shown in Figure 2 and Table 3A from the institution-level analysis. The individual-level event studies are estimated from equation (3) and shown in Figure 4. Each point on the figures represents an estimate of θ_k, while the dashed lines plot the 95 percent confidence intervals estimated using multiway clustered standard errors. In Table 5, we replicate the results from Table 3A by estimating equation (4) with the individual-level data.

Broadly, the results from both analyses confirm the findings from the institution-level data, 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 subpopulations (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 and find imprecisely estimated negative effects that are smaller in magnitude.[27] Unfortunately, we are unable to estimate the effect of switching for the six-year graduation outcome, as in the institution-level analysis, because we only observe five years of data for the large group of universities that switched to semesters in 2012.

D. *Mechanism Exploration*

Next, we seek to better understand *why* a semester schedule leads to reduced on-time graduation. One possible contributing factor is that students are more likely to leave a university on semesters, either as a dropout or to transfer to a different institution. It is also possible that the reduction in on-time graduation is driven by an increase in time to degree. In the analysis that follows, we probe both possibilities and the underlying channels.

[27] Note that panel A includes students 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 five years of data).

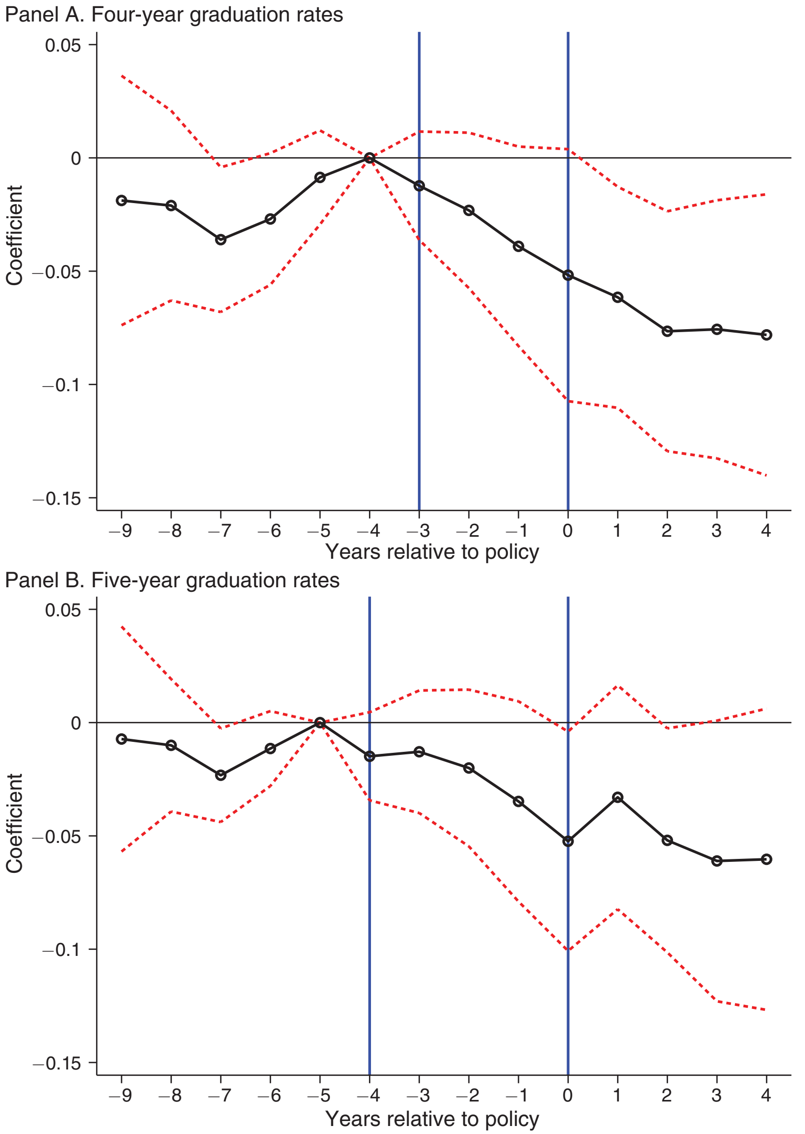

Figure 4. Event Study: Individual-Level Analysis

Notes: In panel A, the sample includes students who enter as first-time freshmen in the F1999–F2013 cohorts (627,988 observations). In panel B, it includes students who enter as first-time freshmen in the F1999–F2012 cohorts (585,935 observations). This figure plots θ_k, and 95 percent confidence intervals in dashed lines, from estimating equation (3). Year and institution fixed effects, institution linear time trends, and student-level controls are included. Standard errors are estimated using multiway clustering.

Source: OLDA

There are a number of potential channels that could drive dropping-out behavior and/or an increase in time to degree. First, students and advisors may have difficulty navigating the transition to a new calendar system. We rule out this proposed channel because the estimated effects shown in Figure 2, panel A and Figure 4, panel A

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 four-year graduation rates							
G1 - partially treated	−0.010	−0.004	−0.017	−0.007	−0.011	−0.015	−0.006
Standard error multiway clustered	(0.013)	(0.014)	(0.014)	(0.019)	(0.013)	(0.012)	(0.018)
Standard error 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
Standard error multi-way clustered	(0.021)	(0.021)	(0.022)	(0.030)	(0.021)	(0.017)	(0.023)
Standard error 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 five-year graduation rates							
G1 - partially treated	−0.007	−0.007	−0.008	−0.009	−0.007	−0.005	−0.004
Standard error multiway clustered	(0.012)	(0.012)	(0.013)	(0.022)	(0.012)	(0.011)	(0.018)
Standard error 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
Standard error multiway clustered	(0.018)	(0.017)	(0.020)	(0.033)	(0.017)	(0.014)	(0.024)
Standard error 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

Notes: 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 one to three years prior to the adoption of a semester calendar, and the omitted category includes all students who enroll more than three years prior to the calendar switch. Panel B: Includes students who enter in the F1999–F2012 cohorts. The partially treated group includes students who first enroll one to four years prior to the switch to semesters, and the omitted category includes all students who enrolled more than four 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.

Source: OLDA linked to mean household income by zip code from the 2006–2010 ACS (https://www.psc.isr.umich.edu/dis/census/Features/tract2zip/)

are clearly evident in the long term. If the reduction in on-time graduation is driven by temporary confusion following the conversion, then one would expect students and faculty to adjust to the new calendar in the medium to longer run and for effects to fade to zero in later cohorts.

A second potential channel is that students may find it challenging to juggle more simultaneous courses per term, as is required with a semester calendar. If this is a primary channel, students may earn lower grades or underenroll—that is, take fewer credits per term than what constitutes a full load. Underenrollment would necessarily increase time to degree, all else constant. Lower grades could lead to an increase in time to degree if students are retaking courses for a better grade. Furthermore, if a student's grades are low enough, they may face academic probation and potential dismissal from the university.

Table 6—Effect of Switching to Semesters on First-Year Outcomes

	Drop out (1)	Transfer out (2)	On-track course taking (3)	Cumulative GPA < 2.0 (4)	Switch major (5)
G2 - fully treated	0.020	0.001	−0.068	0.049	−0.071
Standard error multiway clustered	(0.007)	(0.005)	(0.036)	(0.010)	(0.007)
Standard error 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]
Mean of outcome	0.20	0.08	0.54	0.24	0.11
Observations	709,404	709,404	709,404	709,404	709,404

Notes: 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. 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.

Source: OLDA

Finally, reduced scheduling flexibility associated with semesters caused by the longer-term length and higher number of required courses per term may be an important channel. Students might opt to take fewer courses per term to avoid unappealing class times (e.g., early morning classes). If they must retake a course, it is possible that they will have to wait an entire year before the course is offered again. Both of these factors would likely increase time to degree. It is also possible that scheduling flexibility impacts the timing and/or likelihood that a student switches majors, as major exploration is more costly under a semester calendar. Students who take longer to settle on a major are likely to experience a longer time to degree.

Mechanism Analysis for First-Year Students.—We test each of these hypothesized mechanisms in turn by considering the following outcomes: dropout and transfer-out behavior, course taking, grades, and major switching. To begin, we analyze the impact of the policy on first-year students. Focusing on outcomes measured in students' first year of enrollment allows us to utilize the full sample and abstract from the selection issues present in second-to-fourth-year outcomes, as all follow-on years are only observed conditional on continued enrollment.[28] First-year students may also be the most vulnerable. More than half of all dropping out and transferring out occurs in a student's freshman year (see Figure 3).

Table 6 reports the 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 equation (4) in Table 6, we do not include the $G1_{ist}$ variable, and those students are absorbed into the omitted category of untreated students.

[28] 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.

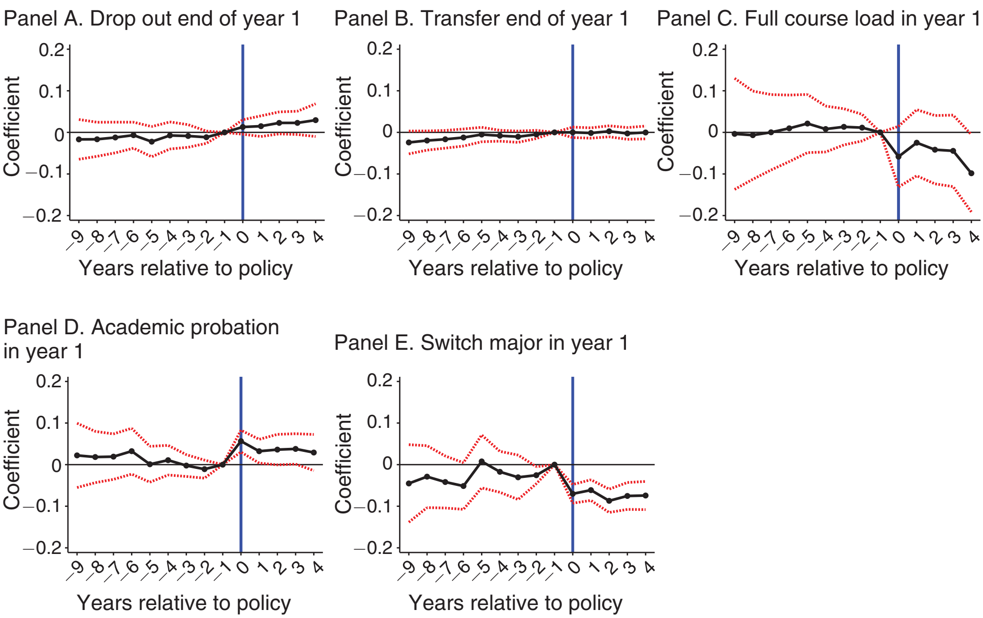

Figure 5. Event Study: Mechanism Analysis—First-Year Outcomes

Notes: The sample includes students who enter as first-time freshmen in the F1999–F2015 cohorts (709,404 observations). This figure plots θ_k, and 95 percent confidence intervals in dashed lines, from estimating equation (3). The vertical line at $t = 0$ indicates the year of semester adoption; i.e., $t = 0$ represents the cohort of students who are in their first year in the year of adoption. Year and institution fixed effects, institution linear time trends, and student-level controls are included. Standard errors are estimated using multiway clustering.

Source: OLDA

Taken at face value, the estimate in column 1 of Table 6 suggests that switching to a semester calendar increases freshman-year dropouts by 2.0 pp.[29] Column 2 of Table 6 reveals no evidence of an effect of the calendar change on transfer behavior, where a transfer is defined as a student who moves to another public four-year institution in Ohio. However, to probe the robustness of these findings, we also estimate event studies for all first-year mechanism outcomes, which are reported in Figure 5. Figure 5, panels A and B reveal suggestive evidence of preexisting differences in the dropout and transfer-out outcome variables. Note that these estimates include controls for institution-specific linear time trends. Thus, the negative point estimates in the pretreatment region suggest that the growth rates of those trends in dropping out and transferring out differ across treated and untreated cohorts. The point estimates reported in columns 1 and 2 of Table 6 are likely capturing, in part, these nonlinearities in the trends rather than the causal effect of the calendar conversion. Moreover, there is no noticeable change in the outcome among switchers at the time

[29] 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 as well as students who transfer to private institutions, public institutions in another state, or non-bachelor's-degree-granting institutions.

of calendar adoption. In summary, we cannot provide conclusive evidence that the policy is causing an increase in dropout or transfer-out behavior.

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 underenrolling 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 GPA of 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.[30] 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 percent evaluated at the mean and is very likely a driving mechanism behind the reduction in on-time graduation.[31]

In column 5 of Table 6, we assess the effect of switching to a semester calendar on the probability of switching one's declared major in the first year of enrollment. We do not code the 8.6 percent of students who enroll as an undeclared major as switchers when they first declare a major. However, the estimates are very similar if we code those initial declarations as switches. The estimates reveal that first-year students are 7.1 pp less likely to switch majors in their first year under a semester calendar. Two possibilities emerge: (i) students are overall less likely to switch majors, or (ii) they delay major switching to a later year. We investigate this at the end of the section in Table 7.

In order for any of the hypothesized mechanisms to be a good candidate, it is necessary that the effect of the calendar switch is long lasting since the main effect on graduation rates persists well beyond the initial transition period. Figure 5 displays the event study estimates for all first-year mechanism variables. These estimates indicate that for first-year students, underenrolling in credits, the increased probability of earning a cumulative GPA of less than 2.0, and the decreased probability of switching majors are lasting effects—affecting cohorts enrolling five or more years after the switch—and are not merely a temporary response to the calendar change.

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

[31] 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.

In order to further minimize any concern over potential bias from differential trends for these mechanism variables, we implement an alternate empirical strategy that focuses on a limited sample: cohorts entering into college in the year immediately preceding semester adoption and in the year of adoption. By restricting the sample in this way and excluding institution-specific linear time trends, we are able to home in on the effect of the calendar switch net of potential confounding trends in the outcome variable. The primary disadvantage of this strategy is that it can only be used to study outcomes measured in the first year of enrollment.

The results of this alternate approach (shown in online Appendix Table A4) are consistent with the findings from our main approach, showing a negative (but not statistically significant) effect on course taking and large negative effects on both first-year GPA and major switching. Importantly, these results are also consistent with conclusions drawn from the event studies regarding first-year dropouts (see Figure 5, panel A). They indicate that the estimated effect of the switch to a semester calendar on dropping out shown in Table 6 is capturing bias from differential trends. Thus, we conclude that there is no convincing evidence of an effect of the calendar change on dropout behavior.

Mechanism Analysis for Continuing Students.—To further home in on the mechanisms underlying the increase in time to degree, we next analyze the effect of the calendar conversion on the potential mechanism variables for those students who persist past the first year of enrollment. Specifically, we restrict the sample to students who remain enrolled for at least four years and, as such, are very unlikely to ever drop out. 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.[32] Recall that for first-year outcome variables, there is no partially treated group. For outcomes measured in students' second year, the partially treated group (represented by $G1_{ist}$ in equation (4)) is defined to be students who first enrolled at a given university one year prior to the adoption of a semester calendar. For outcomes measured in students' third year, the partially treated group also includes students who enrolled two years prior to the switch to semesters. And for outcomes measured in students' fourth year, the partially treated group includes students who enrolled one to three years prior to the adoption of a semester calendar.[33]

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.[34] These estimates show that among students who do not drop out and instead persist through at least four years of enrollment, those in their fourth year on a

[32] To avoid bias due to attrition, we estimate these regressions using only those students in the F1999–F2013 cohorts (those for whom we observe at least four years of data) who remain enrolled for a minimum of four years. These results, however, are robust to using the full sample; see online Appendix Table A5.

[33] Online Appendix Figure A4 presents event studies for each of the outcomes in Table 7 and overall indicates parallel pretrends in the outcomes and a divergence from the trend for the treated cohorts at the timing of calendar adoption.

[34] The outcome measures cumulative course taking at the end of each year such that a student on 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 outcome value equal to 1 in column 2.

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 number of credits is on track				
G1 - partially treated		−0.069	−0.053	−0.051
Standard error multiway clustered		(0.014)	(0.021)	(0.018)
Standard error 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
Standard error multiway clustered	(0.042)	(0.026)	(0.031)	(0.026)
Standard error 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: Cumulative GPA < 2.0				
G1 - partially treated		0.013	0.015	0.018
Standard error multiway clustered		(0.003)	(0.004)	(0.005)
Standard error 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
Standard error multiway clustered	(0.006)	(0.005)	(0.004)	(0.005)
Standard error 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
Standard error multiway clustered		(0.028)	(0.033)	(0.028)
Standard error 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
Standard error multiway clustered	(0.011)	(0.025)	(0.036)	(0.033)
Standard error 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

Notes: Sample includes first-time freshmen in the F1999–F2013 cohorts who remain enrolled for 4+ years (341,646 observations). The partially treated group is defined separately for each year/column. In column 2, G1 includes students who enrolled one year before the switch to semesters. In columns 3 and 4, G1 also includes student who enroll two and three 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.

Source: OLDA

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 persist through four years of enrollment, the change to a semester calendar increases the probability of being below this academic probation threshold in each of the first four 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 years 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 in their college careers.[35,36]

With these cumulative outcome variables that are measured repeatedly for each individual student, we are able to implement an alternative empirical strategy that utilizes individual-level fixed effects. The advantage of this approach is that it relies solely on variation within an individual over time, ruling out the possibility of certain types of selection bias. The disadvantage is that the effect of the switch to a semester calendar is identified exclusively from those students who experience both calendar systems, i.e., students who are caught in the transition period. Results from this alternate approach are shown in online Appendix Table A6 and indicate that the semester calendar reduces the probability of attempting an on-track course load, increases the probability of dropping below the academic probation threshold, and has no effect on the overall probability of switching majors. These results support our prior findings shown in Table 7 and indicate that our results are likely not driven by preexisting trends or selection bias.

E. *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 show in an experimental setting that individuals perform relatively worse when assigned to a multitasking 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 AM start time, especially since the cost of underenrolling is not realized until a future period.

[35] In a separate analysis, we find no evidence of an effect of the semester calendar on which major students choose. That is, students are no more or less likely to choose a STEM major versus a non-STEM major after the switch to semesters.

[36] Another possible channel is summer course taking. It is possible that the semester calendar offers fewer opportunities for summer course taking, as noted by the University of Chicago (see Section I). In a separate analysis, we investigate this possibility and do not find support for this mechanism.

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, Laliberté, and Oreopoulos (2018) provide 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.

F. *Employment Analysis*

Beyond academic outcomes, student employment may also be affected by the switch to semesters. In fact, one reason that 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 toward a semester calendar—the most prevalent academic calendar—such that students at institutions on quarters are disadvantaged. One reason that students and administrators care about internship opportunities is because of the immediate benefits. While some internships are unpaid, Jaeger et al. (2020) find that 71 percent of full-time internships are paid.[37] Perhaps more importantly, another reason is because of the expected postgraduation labor market returns. A recent study that exploits mandatory firm internships at German universities finds postgraduation labor market returns of 6 percent (Margaryan et al. forthcoming). In a related vein, a 2017 survey of US employers reports that the most influential characteristic of an applicant in terms of obtaining an offer at their firm postgraduation is an internship at their firm or in that specific industry (National Association of Colleges and Employers (NACE) 2017).

Internships are common across the distribution of institution selectivity; however, there is some variation in prevalence across majors. A 2018 survey conducted by the NACE reports that 60 percent of recent college graduates hold an internship at some point during college (NACE 2018). This number is higher for business majors.

[37] They also show that internships are more likely to be paid the closer the association with a specific occupation, when there is lower unemployment, and when the local and federal minimum wage are the same.

According to *Bloomberg Businessweek*'s 2014 undergraduate business school rankings, 75 percent of business students have an internship during college (Rodkin 2018). Student internships are not isolated to elite institutions. In a recent *US News and World Report*, Stanford University and the Massachusetts Institute of Technology are among the top ten institutions with respect to internship placement, as are less selective institutions, including Northeastern University, the University of Cincinnati, and Drexel University (*US News and World Report* 2020).

To investigate whether the calendar switch has a meaningful impact on summer employment, we link the transcript data to employment information from the 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 first to third year of enrollment. Summer employment is coded as one if the student garnered 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 (e.g., 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 rough proxy for summer internship employment.[38]

Summary statistics for the linked employment data are reported in Table 8. Similar to the survey data described above, in the ODJFS data, we find that in any given summer, approximately 51 percent of students attending a public university in Ohio are employed in nonretail and non-food-service sectors. Students on a semester calendar are employed in a higher share of summers in these types of positions (52.1 percent) compared to those on a quarter calendar (51.5 percent), and those two means are statistically different at the 1 percent level. This difference is even larger (53.0 percent versus 49.9 percent) for employment in the most critical summer: the summer following one's third year of college enrollment. On the other hand, students on semesters are much less likely to be employed during the school year in all types of jobs. The share of school years that semester students are employed in nonretail and non-food-service sectors is 36.7 percent, compared to 41.6 percent for quarter students. The pattern is similar for retail and food service employment. Taken together, these statistics suggest that a semester calendar may slightly improve the odds of obtaining summer employment and reduce the likelihood of being employed during the school year. Next, we turn to a causal framework to further investigate this possibility.

We begin with an event study analysis by separately estimating equation (3) for all types of employment in each of the three summers and report the findings in Figure 6.[39] The vertical line at $t = 0$ indicates the first treated cohort for a given summer; e.g., in Figure 6, panel B, $t = 0$ represents the summer following second year for the cohort of students who are in their second year when the policy is adopted. There are three key takeaways: (i) there is minimal evidence of pretreatment trends

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

[39] 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.

Table 8—Employment Summary Statistics

	All students (1)	Students on quarters (2)	Students on semesters (3)
Panel A. Excluding retail and food service employment			
Share of summers employed	0.512	0.515	0.521
	(0.388)	(0.381)	(0.393)
Employed summer after Y1	0.508	0.532	0.509
	(0.500)	(0.499)	(0.500)
Employed summer after Y2	0.514	0.514	0.524
	(0.500)	(0.500)	(0.499)
Employed summer after Y3	0.514	0.499	0.530
	(0.500)	(0.500)	(0.499)
Share of school years employed	0.379	0.416	0.367
	(0.355)	(0.356)	(0.356)
Employed during SY1	0.310	0.366	0.289
	(0.462)	(0.482)	(0.453)
Employed during SY2	0.371	0.415	0.358
	(0.483)	(0.493)	(0.479)
Employed during SY3	0.405	0.437	0.397
	(0.491)	(0.496)	(0.489)
Employed during SY4	0.429	0.448	0.423
	(0.495)	(0.497)	(0.494)
Panel B. Retail and food service employment only			
Share of summers employed	0.228	0.212	0.240
	(0.334)	(0.322)	(0.342)
Employed summer after Y1	0.235	0.222	0.246
	(0.424)	(0.415)	(0.430)
Employed summer after Y2	0.229	0.215	0.241
	(0.420)	(0.411)	(0.428)
Employed summer after Y3	0.219	0.200	0.234
	(0.413)	(0.400)	(0.423)
Share of school years employed	0.210	0.210	0.214
	(0.308)	(0.306)	(0.311)
Employed during SY1	0.197	0.209	0.193
	(0.398)	(0.407)	(0.395)
Employed during SY2	0.215	0.220	0.217
	(0.411)	(0.415)	(0.412)
Employed during SY3	0.216	0.213	0.224
	(0.412)	(0.409)	(0.417)
Employed during SY4	0.211	0.199	0.223
	(0.408)	(0.399)	(0.416)
Observations	296,416	92,679	171,746

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; 31,991 "partially treated" observations are not included in columns 2 and 3.

Source: OLDA linked to ODJFS UI quarterly wage data.

in summer employment, (ii) 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 (iii) there is suggestive evidence of a modest increase in summer employment after a student's first year as a result of semester calendar adoption.

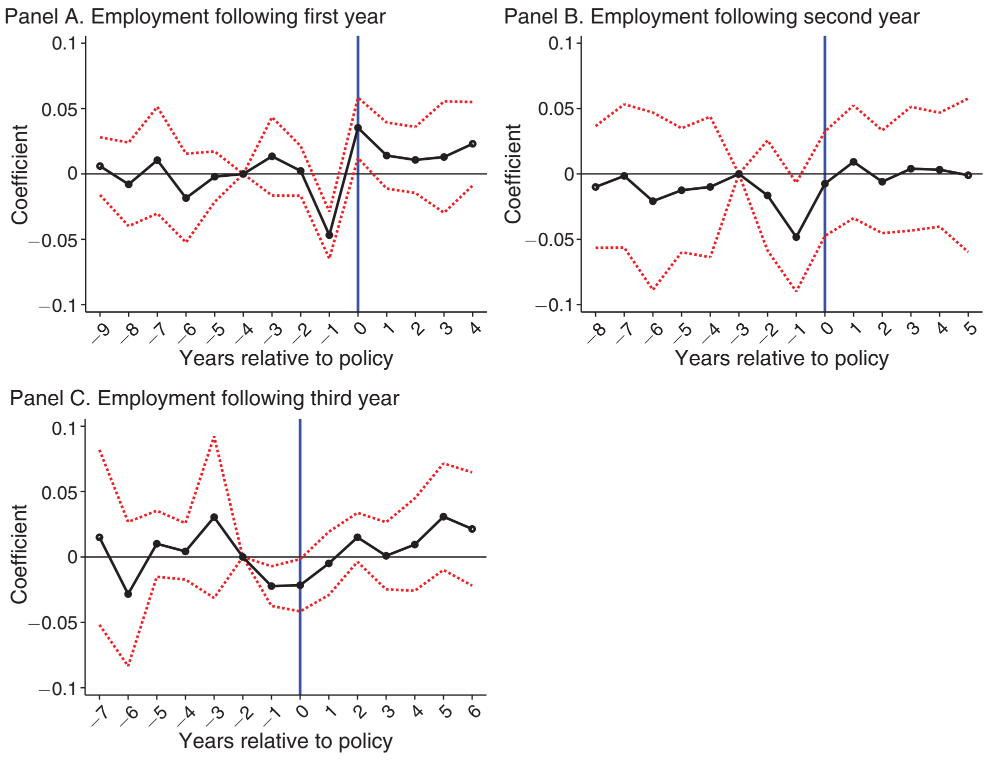

Figure 6. Event Study: Summer Employment (Q2 and Q3)

Notes: The sample includes students who enter as first-time freshmen in the F2000–F2013 cohorts who remain enrolled for 4+ years and can be linked to ODJFS employment data (296,416 observations). This figure plots θ_k, and 95 percent confidence intervals in dashed lines, from estimating equation (3). The vertical line at $t = 0$ indicates the first treated cohort for a given summer; e.g., in Figure 6, panel B, $t = 0$ represents the summer following second year for the cohort of students who are in their second year when the policy is adopted. Year and institution fixed effects and student-level controls are included. Standard errors are estimated using multiway clustering.

Source: OLDA merged with employment data from ODJFS

Next, we estimate a model similar to equation (4) for the summer employment outcome variables. These results are shown in Table 9. 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 a student's fourth year, because a substantial subset of those students will have graduated. Columns 2–4 report the effect of the switch to semesters on summer employment separately for each of those three summers.[40] As in Table 7, we restrict the sample to students who remain enrolled for at least four years in the F1999–F2013 cohorts.[41]

[40] 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 one year before the switch to semesters. Column 3 includes students enrolled one and two years before, and in column 4 (and column 1), it includes those enrolled one to three 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.

[41] Results for the unrestricted sample are very similar and are reported in online Appendix Table A7.

TABLE 9—EFFECT OF SWITCHING TO SEMESTERS ON SUMMER EMPLOYMENT (Q2 OR Q3)

	Share of summers employed	Employed summer after:		
		First year	Second year	Third year
	(1)	(2)	(3)	(4)
Panel A. Excluding retail and food service employment				
G2 - fully treated	0.022	0.026	0.022	0.013
Standard error multiway clustered	(0.011)	(0.009)	(0.013)	(0.014)
Standard error 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
Standard error multiway clustered	(0.010)	(0.006)	(0.010)	(0.012)
Standard error 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

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 observations). 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 one year before the switch to semesters. In column 3, it includes student enrolled one and two years before, and in column 4 (and column 1), it includes those enrolled one to three 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.

Source: OLDA linked to ODJFS UI quarterly wage data.

The dependent variables in panel A of Table 9 indicate summer employment *excluding* the retail and food service industries. Overall, the results confirm the findings from Figure 6—that is, there is suggestive evidence that the calendar adoption improves the probability of summer employment in non-food-service and nonretail industries following one's first year. However, the point estimate for the summer employment outcome following a student's third year (column 4) is economically and statistically insignificant. An internship following one's third year is likely the most correlated with one's field of study and, thus, likely the most critical for improving postgraduation labor market outcomes. Panel B reports results for retail and food service employment only. These coefficients are all negative, but none attain statistical significance at a conventional level when considering the two more conservative standard error estimates: campus clusters and wild cluster bootstrap *p*-values.

While the impetus of the employment analysis is to investigate whether semesters change summer employment, the calendar adoption may also affect school year employment, particularly if semesters affect scheduling flexibility. Table 10 reports results for student employment during the school year—quarters 1 and 4 (October–March)—and Figure 7 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 that a student is employed in a retail or food service job by 4.8 pp, which is equivalent to a 23 percent reduction at the mean

Table 10—Effect of Switching to Semesters on School Year Employment (Q4 or Q1)

	Share of school years employed (1)	Employed during: First year (2)	Second year (3)	Third year (4)	Fourth year (5)
Panel A. Excluding retail and food service employment					
G2 - fully treated	−0.032	−0.056	−0.016	−0.021	−0.037
Standard error multiway clustered	(0.015)	(0.022)	(0.017)	(0.009)	(0.023)
Standard error 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
Standard error multiway clustered	(0.010)	(0.007)	(0.010)	(0.011)	(0.011)
Standard error 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

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 observations). The partially treated group (not reported) is defined separately for each year/column. In column 3, G1 includes students who enrolled one year before the switch to semesters. In columns 4 and 5, G1 also includes student who enroll two and three years before the calendar switch, respectively. In column 1, G1 includes student who enrolled one to three 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.

Source: OLDA linked to ODJFS UI quarterly wage data.

(Table 10, column 1).[42] 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, when considering the body of evidence (Figure 6 and Table 9), we cannot say with much certainty that the calendar switch produces meaningful improvements for nonretail and non-food-service summer employment. We do not find any evidence of an effect on this type of employment in the most crucial summer (the summer following one's third year). We do find a positive effect on nonretail and non-food-service employment in the summer following students' first year. However, the results in Figure 6 reveal that this is largely driven by an isolated effect on students in the very first treated cohort. Students who enroll in subsequent cohorts one or more years after the switch to semesters do not experience a significant increase in employment during their first summer break. Furthermore, following the calendar switch, we show that students are substantially less likely to be employed during the school year in 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 do not find this to be the case. It is important to keep in mind that this analysis only includes students who are employed in the state of Ohio and

[42] Results are similar for the sample of students who continue to enroll through the year in which the outcome is measured; see online Appendix Table A8.

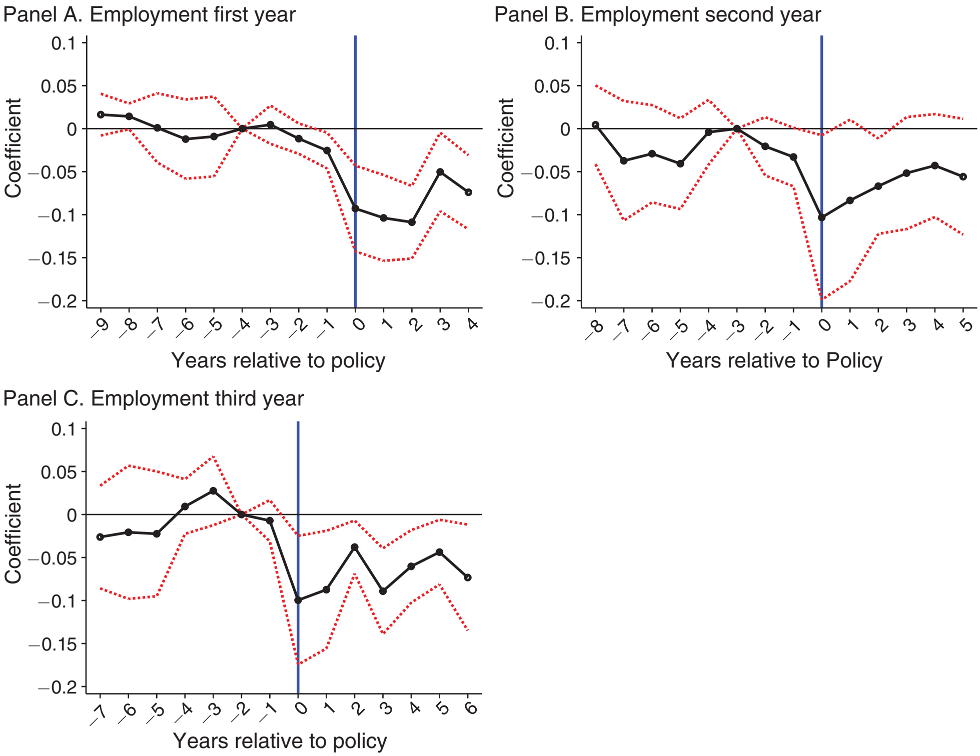

Figure 7. Event Study: School Year Employment (Q1 and Q4)

Notes: The sample includes students who enter as first-time freshmen in the F2000–F2013 cohorts who remain enrolled for 4+ years and can be linked to ODJFS employment data (296,416 observations). This figure plots θ_k, and 95 percent confidence intervals in dashed lines, from estimating equation (3). The vertical line at $t = 0$ indicates the first treated cohort for a given school year; e.g., in Figure 7, panel B, $t = 0$ represents the cohort of students who are in their second year when the policy is adopted. Year and institution fixed effects and student-level controls are included. Standard errors are estimated using multiway clustering.

Source: OLDA merged with employment data from ODJFS.

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.

IV. 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 an NCES report, the cost of 1 year of tuition at a 4-year public institution in 2014 was $18,110, and the average starting salary for 2014 graduates was $26,217.[43] Thus, the total cost of an additional year of schooling for a public university student is approximately $44,327.43.[44] 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 1-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 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 implementing 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 postperiod 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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[43] This salary was calculated using the 2014 March Current Population Survey. It includes all individuals who are age 22–24 with a 4-year degree who are not in school, and it includes those with a zero wage.

[44] 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.

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