INCREASING TIME TO BACCALAUREATE DEGREE IN THE UNITED STATES

John Bound
Department of Economics
University of Michigan
and National Bureau
of Economic Research
Ann Arbor, MI 48109-1220
jbound@umich.edu

Michael F. Lovenheim
(corresponding author)
Department of Policy Analysis
and Management
Cornell University
and National Bureau
of Economic Research
Ithaca, NY 14853
mfl55@cornell.edu

Sarah Turner
Department of Economics
University of Virginia
and National Bureau
of Economic Research
Charlottesville, VA 22904
sturner@virginia.edu

Abstract
Time to completion of the baccalaureate degree has increased markedly in the United States over the past three decades. Using data from the National Longitudinal Survey of the High School Class of 1972 and the National Educational Longitudinal Study of 1988, we show that the increase in time to degree is localized among those who begin their postsecondary education at public colleges outside the most selective universities. We consider several potential explanations for these trends. First, we show that changes in the college preparedness and the demographic composition of degree recipients cannot account for the observed increases. Instead, our results identify declines in collegiate resources in the less selective public sector and increases in student employment as potential explanations for the observed increases in time to degree.

1. INTRODUCTION

Over the past three decades, the share of baccalaureate (BA) degree recipients that graduate within four years has decreased and, more generally, the length of time it takes college graduates to attain degrees has increased. This shift, which involves substantial costs for graduates in terms of forgone earnings and additional tuition expenditures, has drawn increased public policy attention.[1] Although researchers have done some work to understand the determinants of time to degree in the cross section, there has been little documentation or examination of the determinants of the observed extension in time to degree.

In this article we examine how time to completion of the BA has changed in the past three decades by comparing outcomes for two cohorts from the high school classes of 1972 and 1992 using data from the National Longitudinal Study of 1972 (NLS72) and the National Educational Longitudinal Study of 1988 (NELS:88). We find evidence of large shifts in the time to degree distribution: in the 1972 cohort, 53 percent of eventual BA degree recipients graduated within four years of finishing high school, but for the 1992 high school cohort only 39 percent did so. This extension of time to degree, and the associated reduction in "on-time" degree completion, did not occur evenly over the different sectors of higher education. Increased time to degree is largest among graduates beginning college at less selective public universities as well as at community colleges.

The extension of time to degree cannot be explained by the lengthening of the time between high school and college or by longer times to high school graduation. We also document little change in "stopping out" among eventual graduates across cohorts, where students take time off from enrollment and later return. Our data point to a reduction in the pace at which relatively continuously enrolled students complete college credits. There also is no evidence that increased time to degree reflects more human capital accumulation among students; rather, students are accumulating the same number of college credits more slowly.

We seek to assess the underlying reasons behind the increases in time to degree that we document over the past thirty years. Previous work has demonstrated that time to degree is negatively related to student academic ability as well as to student background characteristics, such as income and parental education (e.g., Flores-Lagunes and Light 2010; DesJardins, Ahlberg, and McCall 2002; Ishitani 2006; Adelman 2006; Bowen, Chingos, and McPherson 2009; Garibaldi et al. 2012). As the returns to a college degree have increased over

1. To underscore this point, twelve states recently conducted studies of elongating time to degree in the public postsecondary system, and California and Colorado passed legislation to attempt to curb time to degree increases.

the past four decades (Autor, Katz, and Kearney 2008), more students with relatively low levels of precollegiate preparedness are attending college. These students are likely to require a longer period of enrollment to finish a degree.

Another important trend in higher education over the time period of our analysis is an overall reduction in resources combined with increased stratification of resources: the wealthier schools have become richer while the less resource-intensive schools have become poorer (Hoxby 2009; Bound, Lovenheim, and Turner 2010a; Ehrenberg and Webber 2010). To the extent that institutional resources affect students' ability to make it through degree programs in a timely manner, the pace of degree progression may decline among students at those institutions where available resources per student have declined.

Rising college costs also may play a role in explaining the time to degree trends we document. In particular, as students face increased challenges to financing full-time enrollment, they may increase labor supply while in college. To the extent that employment crowds out credit attainment, increases in student work hours may contribute to lengthening time to degree.

We take several empirical approaches to identifying the role of each of these types of factors in explaining the elongation of time to degree. First, using the detailed demographic data that we have linked to institutional-level information for each student, we follow a semi-parametric reweighting strategy based on DiNardo, Fortin, and Lemieux (1996) to determine the extent to which changes in time to degree can be attributed to changes in observed student background characteristics, changes in student academic preparation for college, changes in institutional resources, and an unexplained residual component.

Strikingly, despite the strong cross-sectional relationship between high school academic ability and time to degree (Flores-Lagunes and Light 2010; DesJardins, Ahlberg, and McCall 2002; Adelman 2006; Bowen, Chingos, and McPherson 2009), we find no evidence that *changes* in the academic preparation of eventual college graduates or in the demographic characteristics of these graduates can explain any of the time to degree increases. This is because the less academically prepared students who were drawn into the postsecondary sector over this interval graduate at very low rates (Bound, Lovenheim, and Turner 2010a). Indeed, the observable characteristics of college graduates, including high school test scores and high school grade point average (GPA), have become *more* favorable across cohorts.

In contrast, we find evidence that decreases in institutional resources at public colleges and universities are important for explaining changes in time to degree. With a significant link between institutional resources (e.g., student-faculty ratios) and time to degree, the declines in resources per student at public sector colleges and universities predict some of the observed extension of time to degree.

Throughout this analysis, we proxy for institutional resources using student-faculty ratios. However, no one measure of resources is a perfect proxy (Black and Smith 2006). Thus we supplement the decomposition analysis with a state-level analysis that uses changes in the population of eighteen-year-olds in the state as a proxy for institutional resources (Bound and Turner 2007). The results from this analysis support the finding from our decompositions that changes to institutional resources are an important part of why time to degree has increased.

Finally, we present suggestive evidence that students' growing difficulties in financing a college education contribute to increased time to degree. We show that the size of the increase in student labor supply is large enough to explain a large portion of the increases in time to degree we document under plausible assumptions about how work time crowds out school time. We also show that time to degree among students from lower-income families has increased the most, which suggests a relationship between increased difficulties students have in financing college and lengthening time to degree.

This article's contribution is to combine a careful description of the change in time to BA degree completion with consideration of the factors contributing to this change. In other work (Bound, Lovenheim, and Turner 2010a) we have considered the role of student background characteristics and supply-side determinants in higher education in explaining the change over time in *whether* students complete college; in this article we analyze the changes in the length of time taken by degree recipients in their studies. The length of time it takes students to complete degrees reflects the magnitude of individual investments in college education, given the greater opportunity cost of extended enrollment, and serves as an important indicator of how students move from enrollment to degree attainment.

The rest of this article is organized as follows: section 2 describes the increase in time to degree found in the data. Section 3 outlines the potential explanations for these trends that inform our empirical analysis. Section 4 describes our data, and section 5 presents our empirical approach and the results from our empirical analyses. Section 6 concludes.

2. INCREASED TIME TO DEGREE

Evidence of increased time to college degree conditional on graduating can be found in a range of data sources. The Current Population Survey (CPS) provides a broad overview of trends in the rate of collegiate attainment by age (or birth cohort). While the share of the population with some collegiate

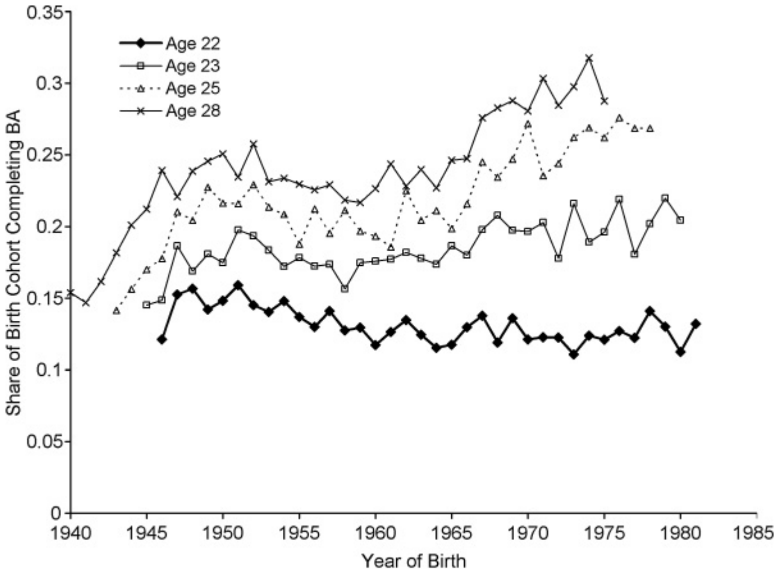

Figure 1. College Completion Rates by Age, 1940–80 Birth Cohorts. Source: Data are from authors' tabulations using the October CPS, 1968–2005. Individual weights are used. See Turner 2004 for additional details.

participation increased substantially between the 1950 and the 1975 birth cohorts, the share obtaining the equivalent of a college degree by age twenty-three increased only slightly over this interval, as shown in figure 1. Extending the period of observation through age twenty-eight, however, shows a more substantial rise in the proportion of college graduates among recent birth cohorts.[2] Taken together, the inference is that time to degree has increased.

Time to Degree in NLS72 and NELS:88

To measure changes in time to degree in connection with microdata on individual and collegiate characteristics, this analysis uses the National Longitudinal Study of the High School Class of 1972 (NLS72) and the National Educational Longitudinal Study (NELS:88). These surveys draw from nationally representative cohorts of high school and middle school students, respectively, and

2. Data from cross sections of recent college graduates assembled by the Department of Education from the Recent College Graduates and Baccalaureate and Beyond surveys corroborate this finding. For example, from 1970 to 1993 the share of graduates taking more than six years rose from less than 25 percent to about 30 percent, while the share finishing in four years or less fell from about 45 percent of degree recipients in 1977 to only 31 percent in the 1990s (see Horn, Knepper, and McCormick 1996 and Bradburn et al. 2003). Adelman (2004) uses data from the NLS72 and NELS:88 to trace time to degree and shows an increase from 4.34 years to 4.56 years. The differences between Adelman's estimates and ours are likely due to the fact that we examine only students attending college within two years of high school and he focuses only on students who have complete college transcripts.

track the progress of students through collegiate and employment experiences. These microlevel surveys afford two principal advantages over the CPS. First, the data include measures of precollegiate achievement that allow us to analyze the relationship between time to degree attainment and precollegiate academic characteristics. Second, these data identify the colleges attended, permitting us to analyze outcomes by collegiate characteristics.

To align these longitudinal surveys, we focus on outcomes within eight years of high school graduation among those who entered college within two years of their cohort's high school graduation. We measure time to degree in each survey as the number of months between cohort high school graduation and BA receipt.[3] Cohort high school graduation is June 1972 for NLS72 respondents and June 1992 for NELS:88 respondents. With the NELS:88 cohort followed for only eight years after high school graduation, our approach affords eight years of post–high school observation for both cohorts.

Our sample includes those who do not graduate high school on time. Because the NLS72 survey follows a twelfth-grade cohort and the NELS:88 survey follows an eighth-grade cohort, there are more late high school completers in the latter sample. However, when one conditions on college completion within eight years, over 99 percent of respondents finish high school on time in both samples. The focus of this analysis is on explaining the increase in time to degree among "traditional" college students beginning college soon after high school graduation; our data do not permit analysis of time to degree among nontraditional students entering the collegiate pipeline in their twenties.

In table 1, we show the cumulative share of BA recipients who attained their degree in years 4–8 beyond their cohort's high school graduation. The table demonstrates that there has been a sizable outward shift in time to degree among BA recipients across the two cohorts. Not only did the proportion finishing within four years decline by a statistically significant 13.7 percentage points, or 25.8 percent relative to the NLS72 baseline, but the entire distribution shifted outward. In addition, mean time to degree increased from 4.48 to 4.81 years (a 7.1 percent increase).

3. Because the last NELS:88 follow-up was conducted in 2000, we are forced to truncate the time to degree distributions at eight years, reflecting the time between cohort high school graduation and the last follow-up. Empirically, however, the proportion of eventual college degree recipients receiving their degrees within eight years has not changed appreciably. The 2003 National Survey of College Graduates allows us to examine year of degree by high school cohort. For the cohorts from the high school classes of 1960 to 1979 for which there are more than twenty years to degree receipt, we find that the share of eventual degree recipients finishing within eight years holds nearly constant at between 0.83 and 0.85. Focusing on more recent cohorts (and hence observations with more truncation), we find that in the 1972 high school graduating cohort, 92.3 percent of those finishing within twelve years had finished in eight years, with a figure of 92.4 percent for the 1988 cohort.

Table 1. Eight-Year Cumulative Time to Degree (TTD) Distributions for the Full Sample and by First Institution

	TTD Distribution				Mean	HS	Attendance	Any	Number of
	4	5	6	7	TTD	Lag	Lag	Stop-out	Stop-outs
Full sample:									
NLS72	53.1	81.8	90.6	96.3	4.48	−0.01	0.30	0.06	1.48
NELS:88	39.4	72.7	88.3	94.7	4.81	−0.03	0.25	0.04	1.56
Difference	−13.7	−9.1	−2.3	−1.7	0.32	−0.01	−0.04	−0.02	0.08
	(1.9)	(1.3)	(0.9)	(0.7)	(0.04)	(0.004)	(0.01)	(0.01)	(0.23)
Non–top 50 public:									
NLS72	49.7	82.3	91.1	96.3	4.49	−0.02	0.29	0.07	1.33
NELS:88	29.1	68.8	87.8	95.1	4.93	−0.03	0.23	0.03	1.18
Difference	−20.6	−13.6	−3.3	−1.2	0.44	−0.01	−0.06	−0.04	−0.15
	(2.7)	(2.1)	(1.4)	(1.0)	(0.05)	(0.01)	(0.01)	(0.12)	(0.12)
Top 50 public:									
NLS72	52.7	81.5	89.2	96.4	4.49	−0.01	0.28	0.05	1.56
NELS:88	39.7	82.0	93.7	96.6	4.66	−0.03	0.22	0.05	2.00
Difference	−13.0	0.5	4.5	0.2	0.16	−0.02	−0.05	0.01	0.44
	(3.8)	(2.5)	(1.8)	(1.3)	(0.07)	(0.01)	(0.02)	(0.02)	(0.72)
Less selective private:									
NLS72	66.7	87.3	94.0	98.7	4.28	−0.01	0.29	0.04	1.50
NELS:88	58.0	84.6	93.4	98.6	4.60	−0.02	0.24	0.04	1.21
Difference	−8.7	−2.7	−0.6	−0.01	0.15	−0.02	−0.04	0.00	−0.29
	(3.1)	(2.2)	(1.7)	(0.6)	(0.06)	(0.01)	(0.01)	(0.02)	(0.20)
Highly selective private:									
NLS72	65.2	88.2	93.8	96.8	4.31	0.00	0.29	0.05	2.04
NELS:88	73.1	91.9	98.1	99.8	4.20	−0.01	0.26	0.01	1.93
Difference	7.9	3.7	4.3	2.9	−0.12	−0.01	−0.03	−0.04)	−0.11
	(6.1)	(3.3)	(1.8)	(1.2)	(0.08)	(0.01)	(0.01)	(0.01)	(0.61)
Community colleges:									
NLS72	36.5	67.8	83.0	92.6	4.90	−0.00	0.37	0.06	1.68
NELS:88	15.5	44.2	70.8	83.6	5.58	−0.02	0.35	0.05	1.80
Difference	−21.0	−23.6	−12.2	−9.0	0.68	−0.02	−0.02	−0.01	0.12
	(4.4)	(4.2)	(3.5)	(2.8)	(0.11)	(0.01)	(0.04)	(0.02)	(0.29)

Notes: NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations. The variable “HS Lag” refers to the number of months between high school graduation and cohort high school graduation. “Attendance Lag” is the number of months between cohort high school graduation and first college enrollment. “Any Stop-out” is an indicator equal to 1 if a student has a semester of non-enrollment between first enrollment and graduation, and “Number of Stop-outs” is the number of stop-out spells, conditional on having any spell. The NLS72 and NELS:88 samples are restricted to those who attend college within two years of cohort high school graduation and who finish within eight years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample. The difference between NELS:88 and NLS72 is in each third row. The standard error of this difference is in parentheses and is clustered at the high school level, which is the primary sampling unit.

Higher education in the United States is characterized by substantial heterogeneity across institution types. Moreover, students beginning a community college must transfer in order to obtain a BA, and, to the extent that transfer-related factors and between-school non-enrollment spells slow down the pace of collegiate attainment, it is important to distinguish students by the type of first institution. To capture differences among institutions, we categorized the first colleges and universities attended by BA recipients into five broad sectors:[4] non–top fifty ranked public universities, top fifty ranked public universities, less selective private schools, highly selective private schools, and community colleges. Table 1 presents cumulative time to degree distributions by these sectors and shows that the elongation of time to degree is far from uniform across types of undergraduate institutions. Extensions are pronounced in the non–top fifty public sector, in which the likelihood of a BA recipient graduating within four years dropped from 49.7 percent to 29.1 percent, a statistically significant decline of 20.6 percentage points (or 41.4 percent); as with the full sample, the proportion graduating within each subsequent time frame also declined significantly. Mean time to degree consequently increased in the non–top fifty public sector from 4.49 to 4.93 years.

The time to degree increases were even more dramatic for BA recipients whose first institution was a community college, where there was a 21 percentage point decline in the likelihood of completing within four years, a 23.6 percentage point decline in completion within five years, and a 0.7 year increase in mean time to degree. In the NELS:88 cohort, less than 16 percent of BA recipients who started at a community college earned their degree within four years.

In the top-fifty public sector, while the share of degree recipients finishing within four years declined, the share finishing within five years did not. Mean time to degree increased only by a small amount as well. We also find little evidence of time to degree increases in the private sector. While the likelihood of graduating within four years dropped by 8.7 percentage points in the less selective private sector, this is only a 13 percent decline. Mean time to degree also increased by 0.15 years, or 3.5 percent. In the elite private sector, time to degree declined, although the standard errors are relatively large due to small sample sizes. Table 1 illustrates one of the central descriptive findings of this analysis: time to degree increased most dramatically across surveys

4. We use the 2005 *U.S. News and World Report* undergraduate college rankings to classify institutions into these five categories. The highly selective four-year private schools are the top sixty-five ranked private universities and the top fifty private liberal arts schools. Less selective four-year private schools are all other private universities. Highly selective private schools and top fifty public schools are listed in appendix table A.2. While admittedly crude, this breakdown correlates well with several measures of quality, such as average SAT scores and high school GPAs. Other metrics, such as resources per student or selectivity in undergraduate admissions, give similar results.

among graduates beginning their studies at less selective public schools and at community colleges.

Credit Attainment and Time Spent in College

Given observed increases in time to degree, it is natural to ask whether these changes reflect increased difficulty in passing through the course sequences, increased course taking, or less time spent in college. In the remaining columns of table 1, we examine whether elongating time to high school graduation (Deming and Dynarski 2008), the lag between high school graduation and college entry, and enrollment breaks in college (i.e., stopping out) can contribute to the lengthening time between high school cohort graduation and BA receipt. The HS Lag column shows time (in months) between actual graduation and cohort high school graduation. These differences are all very small and show no evidence of a change over time. The Attendance Lag column demonstrates that students are not taking longer between high school and college either.

The final two columns of the table show the incidence of non-enrollment spells among eventual degree recipients. We define a stop-out spell as a semester of non-enrollment between first enrollment and graduation, which we measure using the transcript data. Stopping out is not prevalent, which is due mostly to the fact that the likelihood of graduating conditional on stopping out is low (DesJardins, Ahlberg, and McCall 2002, 2006). Furthermore, if anything, non-enrollment spells have declined over time among eventual graduates. Conditional on stopping out, the number of stop-out spells also has not increased, which is shown in the final column of table 1.

Table 1 indicates that time to degree has lengthened due to longer periods of time spent enrolled relatively continuously in college. However, it could be that more time spent enrolled has led to more human capital accumulation. At the extreme, if increased time to degree primarily captures increased attainment in the form of course credits, policy concern over the effects of time to degree might be misplaced. Using transcript data, we chart time paths of credit accumulation, which are shown in figure 2. For students at non–top fifty four-year public institutions and community colleges, we find a slower pace of credit accumulation in the 1992 cohort relative to the 1972 cohort; although students in both cohorts accumulated a similar number of credits after eight years, students in the 1992 cohort took longer to do so. For example, students starting at non–top fifty public schools in the later cohort accumulated, on average, about 9.7 fewer credits within four years of high school graduation than did their counterparts in the 1972 cohort. After four years, the credit gaps track the time to degree gaps from table 1 closely within each school type.

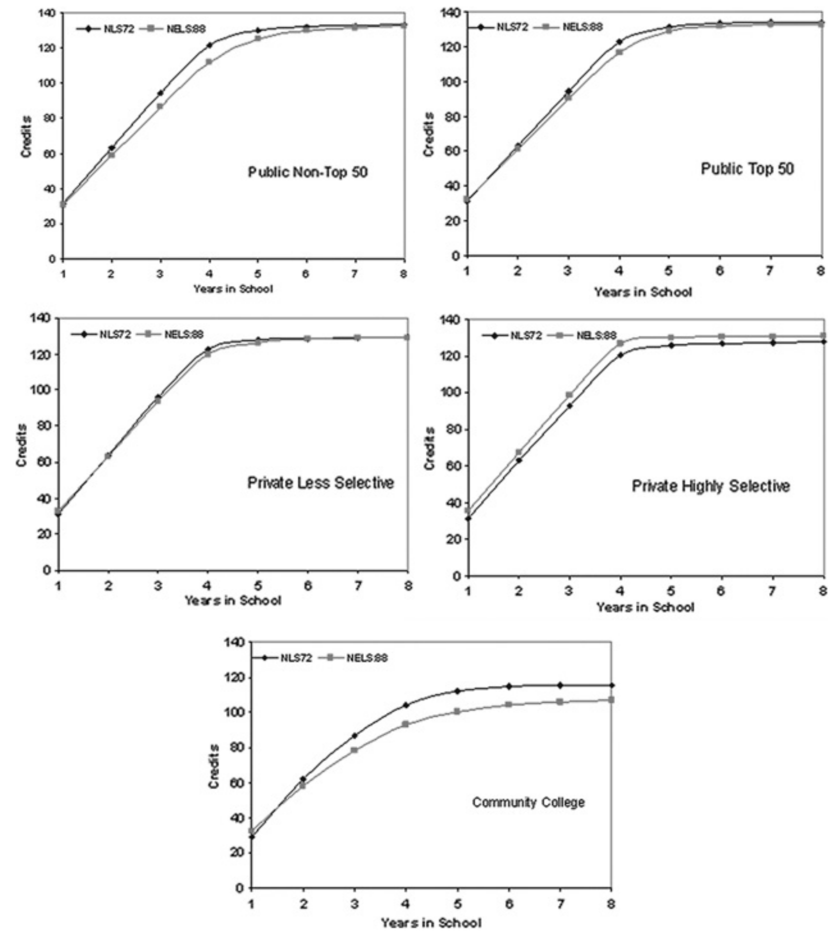

Figure 2. Credit Accumulation by Type of Initial Institution for Eight-Year BA Recipients

We also have explored cross-cohort differences in the ratio of attempted credits to accumulated credits. We found only a small increase in this ratio over the period of study. Furthermore, we considered double majoring, but its prevalence is too low in the sectors that experienced increased time to degree to explain a significant portion of the phenomenon. Finally, we examined whether graduates took more difficult courses in areas such as mathematics and science and reduced the number of courses they took per term to better their chances of success in such classes. Using the course-level transcript files, we find no large changes in course-taking behavior or majors across fields that could explain the time to degree increases we document.

With no supporting evidence of greater credit accumulation to suggest a link between time to degree and human capital accumulation, we interpret observed increases in time to degree as a reduction in the rate of human capital accumulation rather than an increase in the amount of human capital, with this change concentrated outside the top public schools and private institutions.[5] We now turn to explanations of why time to degree has shifted in the manner observed in the data.

3. POTENTIAL EXPLANATIONS FOR INCREASED TIME TO DEGREE

There are multiple theoretically plausible explanations for the observed changes in time to degree, and we consider these explanations as a framework for guiding our empirical approach and interpreting our results. Note that, ceteris paribus, an increase in the returns to education, which raises the opportunity cost of time spent in school, will *reduce* time to degree. One explanation for the rise in time to degree is a change in the composition of college graduates to include more students who are less academically prepared for college and who likely require a longer time to obtain a BA. Such a compositional change would shift outward average time to degree. Second, decreases in collegiate resources per student may extend time to degree through, for example, reductions in course offerings needed for degree progress. Third, increases in the direct cost of education may lead students to increase employment and reduce the rate of credit accumulation. We discuss the theoretical grounding for each of these explanations in turn.[6]

Demand-side explanations for increased time to degree are driven by the changing characteristics of the student body: increasing returns to education since the 1980s have resulted in higher enrollment among less prepared students (Bound, Lovenheim, and Turner 2010a). If this change in the composition of enrolled students has led to a change in the composition of students who complete college, aggregate time to degree may increase because more students are completing with weaker academic backgrounds. Several articles have established a strong cross-sectional relationship between academic ability and time to degree, which leads to the question of whether academic preparation

5. It is unclear from the data whether the reduced pace of credit accumulation is due to part-time enrollment. If one defines part-time enrollment as taking less than a full course load, then our data indicate a sizable rise in part-time enrollment among eventual BA recipients. Because the data do not indicate students' actual part-time/full-time status, we are unable to examine this issue in more detail.

6. Another potential explanation for increasing time to degree is the rise in student transferring behavior and the associated challenges of articulation, which may lead to inefficient degree progression (McCormick 2003; Adelman 2006; Goldrick-Rab 2006; Goldrick-Rab and Pfeffer 2009). While we view the role of transferring as interesting and important, we lack a credible way to isolate the causal relationship between transferring and time to degree. We also note that transferring itself may be a response to family or institutional resource constraints, which our empirical estimates would capture.

(or other student characteristics) among graduates has changed in such a way that could cause an increase in time to degree. Note as well that increasing returns to college will increase time to degree in the aggregate only if the number of marginal students induced to complete college is large relative to the effect of rising returns on infra-marginal students.

A second type of explanation for increases in time to degree is changes on the supply side of the market that reduce per student college resources. If reductions in resources produce queuing and course enrollment constraints, time to degree may increase. Resource reductions occur when increases in student demand are not accompanied by proportional increases in public funding; such dilution in resources is particularly likely in the public sector, where state subsidies are a significant component of revenues. Bound and Turner (2007) examine supply-side adjustments to variation in student demand generated by changes in cohort size and find evidence that neither state appropriations nor public subsidies fully offset changes in student demand. Moreover, public colleges and universities adjust to demand increases in somewhat different ways across the strata of higher education. Top-tier public and private schools use selectivity in admissions to regulate enrollment, and time to degree likely is unchanged or even decreases with increased demand. In contrast, enrollment is relatively elastic among less selective public four-year universities and for community colleges, leading to reductions in resources per student when increased enrollment is not met with increases in appropriations from public sources and other non-tuition revenues. Queuing and shortages of courses may result if students in relatively large cohorts cannot be accommodated fully, and it is straightforward to see how such institutional barriers lead to delays in degree progress.

Increases in collegiate demand therefore can affect the supply side of higher education and the rate of collegiate attainment by reducing resources per student in the public sector, particularly at open-access four-year institutions and community colleges. Because these institutions are unable to adjust fully to demand shocks, the sectors absorbing the bulk of the students will experience a reduction in resources. The result is increased stratification of resources across the sectors of higher education in the United States, which could produce the patterns of time to degree shifts we observe in the NLS72 and NELS:88 data.

Finally, as students face increased difficulties in paying for college in the face of limited access to student loans and parental resources, they may work more, which may directly affect the pace of degree attainment. As college costs have increased over the past several decades, and with relatively modest availability of federal aid and limited institutional financial aid funds outside the most affluent colleges and universities, students from low- and moderate-income families may face considerable borrowing constraints (see, e.g., Ellwood and Kane 2000; Belley and Lochner 2007; Lovenheim 2011).[7] In the context of the Becker-Tomes (1979) model of intergenerational transfers, rising tuition charges and falling family income lead to the expectation that students will shoulder a higher fraction of college costs. Students will respond to the increased burden of college financing with some combination of reducing consumption, borrowing more, and working more hours.[8] To the extent that students are induced to work more hours while in college to finance attendance, academic pursuits may be crowded out by work time, thereby increasing time to degree.[9]

Intertemporal choices about course taking and employment also may be affected by the structure of tuition, with charges by the term rather than by the course generally increasing the cost of extension of time to degree. When we split our sample according to the pricing structure of tuition rather than the quality rank of the institution, we find that only modest increases have occurred at institutions that charge by the term, with the bulk of the increases occurring at schools that charge by the unit. While the pricing structure has not changed notably over the past forty years, it is not implausible that the effect of extended hours of employment on time to degree is somewhat greater at those institutions, including many less selective publics, where tuition charges occur by the credit rather than by the semester. However, because tuition structures are highly correlated with institutional characteristics, it is difficult to distinguish this explanation in the data.

4. DATA

Student Attributes from the NLS72 and NELS:88 Surveys

The NLS72 and NELS:88 data sets we use contain a rich set of student background characteristics. The student attributes we analyze are high school math and reading test percentiles,[10] quartile of the student's high school GPA,

7. In addition, Brown, Scholz, and Seshadri (2012) show that many students are credit constrained because their parents do not provide the expected family contribution assumed in financial aid calculations. As tuition rises, such students are likely to be the most affected by financial constraints.
8. See Bound, Lovenheim, and Turner (2010b) for a further discussion of this issue and of the pricing information.
9. Keane and Wolpin (2001) show that in a forward-looking dynamic model with limited access to credit, increases in employment while enrolled in school are the expected response to tuition increases. An alternative reason for students working while in school is that there is a potential post-graduation return to this employment experience (Light 2001). For example, working for a professor may teach valuable skills or generate a strong and credible reference letter. However, the majority of jobs held by college students are in the trade and service sectors of the economy, such as working as a waiter or waitress (Scott-Clayton 2012). While such jobs may enhance soft skills, there are likely decreasing marginal returns to work experience in these sectors.
10. The math and reading tests refer to the exams administered by the National Center for Education Statistics (NCES) that were given to all students in the longitudinal surveys in their senior year of high school. Because the tests in NLS72 and NELS:88 covered different subject matter, were of different lengths, and were graded on different scales, we construct the percentile of the score

father's education level, mother's education level, real parental income levels, gender, and race.[11] Appendix A provides a detailed description of the construction of the analysis data set.

Table 2 presents means of selected observable characteristics for the sample of respondents who obtain a BA within eight years of cohort high school graduation in the two surveys overall and for each type of institution. Means of the full set of variables used in our analysis are shown in appendix table B.1. Table 2 shows clearly that there has been no aggregate reduction in academic preparedness among college graduates over time, as measured by math and reading test percentiles or high school GPA. Indeed, in all sectors except community colleges, math test percentiles actually increased among college graduates across cohorts, and in all sectors GPAs increased among graduates.[12] The increased demand for higher education that took place across surveys did not translate into reductions in academic preparation for college among graduates for two reasons. First, enrollment increases occurred across the distribution of academic preparation, muting the impact that the increased demand for higher education had on the average preparedness of college students. Second, the less prepared students induced to attend college had very low completion rates, both because they disproportionately attended sectors in which completion rates are low and because they were not well prepared for college (Bound, Lovenheim, and Turner 2010a). As a result, despite the importance of academic ability in explaining cross-sectional variation in time to degree, changes in academic preparedness among graduates go in the wrong direction to explain the lengthening of time to degree. Furthermore, tables 2 and B.1 show that both the educational attainment and real income of parents

distribution for each test type and for each survey. The comparison of students in the same test percentile across surveys is based on the assumption that the distribution of overall achievement did not change over this time period. This assumption is supported by trends in nationally representative National Assessment of Education Progess (NAEP) scores. Since the 1970s, mean NAEP scores in math and English have been essentially unchanged, and the distribution of these scores has changed little as well (Rampey, Dion, and Donahue 2009).

11. An important demographic variable that we cannot observe in our data is family structure. The overall likelihood of growing up in a two-parent family declined significantly over our period of observation. For example, Census Bureau tabulations show the proportion of all children living with two parents falling from 83 percent to 73 percent between 1972 and 1992. Yet because changes in family structure measured in the CPS among those graduating from college are quite small, we conclude that changes in this variable cannot be a primary determinant of changes in time to degree.

12. While we include a measure of high school GPA because this measure provides information that predicts collegiate attainment beyond test scores, including high school GPA is potentially problematic because it is often difficult to standardize grading across schools and over time. For these reasons we did not include high school GPA in our initial analysis; estimates that exclude high school GPA are shown in Bound, Lovenheim, and Turner (2010b) and are qualitatively similar to those shown below.

Table 2. Means of Selected NLS72 and NELS:88 Variables for College Graduates within Eight Years, by School Type

	Full Sample		Non–top 50 Public		Public Top 50		Private Less Selective		Private Highly Selective		Community College	
Variable	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**
Math test percentile	70.8	71.6	68.3	70.0	78.3	81.0	69.9	70.1	81.2	84.3	68.0	58.5
Reading test percentile	66.8	68.0	65.0	66.4	74.0	75.2	66.0	68.9	75.6	78.1	60.4	55.9
GPA												
Lowest quartile	0.121	0.045	0.123	0.034	0.049	0.016	0.128	0.049	0.060	0.008	0.230	0.120
Second quartile	0.120	0.132	0.138	0.144	0.065	0.070	0.129	0.135	0.080	0.047	0.134	0.224
Third quartile	0.434	0.284	0.452	0.316	0.406	0.208	0.444	0.295	0.402	0.225	0.416	0.324
Top quartile	0.325	0.539	0.287	0.506	0.480	0.707	0.299	0.521	0.459	0.719	0.219	0.331
Father's education												
No HS diploma	0.171	0.058	0.217	0.064	0.105	0.062	0.155	0.037	0.083	0.028	0.170	0.085
BA or more	0.361	0.515	0.290	0.453	0.494	0.612	0.382	0.528	0.533	0.792	0.298	0.342
Mother's education												
No HS diploma	0.133	0.052	0.165	0.058	0.093	0.053	0.118	0.032	0.042	0.016	0.154	0.086
BA or more	0.216	0.420	0.158	0.358	0.271	0.524	0.248	0.414	0.398	0.688	0.181	0.262
Parental income < $25,000	0.139	0.155	0.155	0.171	0.121	0.152	0.143	0.153	0.045	0.069	0.166	0.189
Parental income > $50,000	0.404	0.543	0.349	0.490	0.488	0.646	0.402	0.518	0.614	0.751	0.350	0.437
Asian	0.017	0.058	0.014	0.039	0.036	0.084	0.011	0.041	0.018	0.088	0.016	0.072
Hispanic	0.019	0.055	0.018	0.056	0.023	0.056	0.008	0.055	0.024	0.043	0.033	0.060
African American	0.072	0.074	0.094	0.073	0.035	0.069	0.080	0.105	0.041	0.043	0.048	0.064
Male	0.523	0.453	0.488	0.436	0.542	0.483	0.513	0.410	0.650	0.473	0.550	0.495
Number of observations	4,350	4,137	1,959	1,397	652	733	802	822	392	562	545	623
Proportion of sample	1	1	0.450	0.338	0.150	0.177	0.184	0.199	0.090	0.136	0.125	0.151

Notes: Standard deviations are in parentheses. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations. See appendix table B.1 for complete tabulations. The NLS72 and NELS:88 samples are restricted to those who attend college within two years of cohort high school graduation and who finish within eight years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 and June 1992 for the NELS:88.

increased for college graduates. These shifts are in the direction of shortening time to degree, all else equal.

Supply-Side Variables

Changes in the supply side of the higher education market may affect the rate of attainment by shifting the distribution of students among different types of institutions or by altering the level of resources per student within institutions. Across the two cohorts, there was a sizable change in where BA recipients first attend college. However, these shifts are on the whole in a direction that favors a reduction in time to degree. While there is some increase in eventual graduates entering community colleges, the distribution of initial school types within the four-year sector shifted toward the more selective public and private institutions and away from the non–top fifty public institutions. To illustrate, the share of degrees awarded by highly selective private schools grew from 9.4 percent to 11.3 percent, and the share of degrees awarded by top public institutions rose from 14.4 percent to 17.5 percent, while the share of degrees awarded by non–top fifty public universities declined from 44.9 percent to 34.5 percent between the two cohorts. Thus, shifts in the distribution of degree attainment by sector can explain little of the aggregate time to degree increases evident in the data.

We proxy for institutional resources using student-faculty ratios,[13] which are calculated from the 1972 Higher Education General Information System (HEGIS) survey and the 1992 Integrated Postsecondary Education Data System (IPEDS) survey. Table 3 contains the means and distributions for our sample of eight-year BA recipients. Overall, student-faculty ratios increased (i.e., per student resources decreased) across the two cohorts from 25.5 to 29.8. While these increases occurred throughout the student-faculty ratio distribution, they are largest among institutions in the highest deciles, pointing to increased stratification of resources over time.

The remaining panels of table 3 show student-faculty ratios by school type. The increases have been most dramatic in the sectors that experienced the largest time to degree increases: non–top fifty public schools and community colleges. In the elite public and private schools, student-faculty ratios actually decreased. These tabulations present further evidence that resources not only have declined overall but have become more stratified over time across higher education sectors. They also are suggestive of a role for institutional resources in explaining increasing time to degree.

13. In particular, we calculate the ratio of nine- and twelve-month faculty members to total student enrollment.

Table 3. Undergraduate Student-Faculty Ratios and Expenditures per Student by Initial School Type among College Graduates

Survey	Student-Faculty Ratios Percentile: Mean	25th	50th	75th	90th	Median Expenditures per Student	Median Subsidy per Student
Panel A: Full Sample							
NLS72	25.5	19.4	23.1	28.8	35.0	$14,318	$10,885
NELS:88	29.8	20.5	24.9	32.3	52.8	$15,445	$10,160
Panel B: Public Four-Year Non–top 50							
NLS72	25.0	20.7	23.9	28.6	32.7	$13,172	$10,956
NELS:88	27.6	22.9	26.5	31.3	36.1	$11,886	$9,378
Panel C: Public Four-Year Top 50							
NLS72	23.0	20.3	23.0	24.2	30.9	$19,755	$17,085
NELS:88	22.2	20.4	22.1	24.6	26.9	$18,515	$15,325
Panel D: Private Four-Year Less Selective							
NLS72	22.0	16.1	18.7	24.5	33.5	$16,576	$8,753
NELS:88	23.9	17.0	21.1	26.7	33.9	$18,689	$9,048
Panel E: Private Four-Year Highly Selective							
NLS72	18.7	14.3	18.4	23.1	25.0	$24,996	$14,086
NELS:88	18.4	13.4	17.4	23.1	28.3	$34,212	$17,450
Panel F: Community College							
NLS72	40.9	28.9	36.4	52.0	65.1	$6,316	$5,153
NELS:88	57.1	38.9	55.2	70.3	92.2	$6,140	$5,542

Notes: NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations. Data on faculty, enrollment, expenditures, and revenues are from the HEGIS/IPEDS surveys from the Department of Education. Median expenditures per student are for all education-oriented expenditures, which are all operating expenditures minus expenditures on research, extension services, and hospitals. Per student subsidies are student-oriented expenditures minus tuition revenue per student. All financial figures are in real 2007 dollars and are deflated by the Higher Education Price Index (HEPI).

The final columns of table 3 show median student-related expenditures per student and subsidies per student, measured as the difference between student-related expenditures and net tuition revenues. Median expenditures per student increased slightly overall, yet this modest gain combines declines in the public sector with increases in the private sector, most notably among the most selective private schools, where expenditures increased by about 37 percent. Overall and in the public sector, subsidies per student fell, reflecting declining state support. That tuition also increased at these institutions points to a shift from public sector funding to student funding of higher education. In the private sector, by contrast, the subsidy component rose somewhat. These calculations echo other findings of a divergence between the public and private sectors and the more general increased stratification in the higher education market. Kane, Orszag, and Gunter (2003) document how declines in state appropriations led to declines in spending per student at public schools relative to private schools, with the ratio of per student funding dropping from about 70 percent in the mid-1970s to about 58 percent in the mid-1990s. In addition, Hoxby (1997, 2009) shows that tuition, subsidies, and student quality have stratified over the past five decades, with a particularly substantial divergence among institutions in the top percentiles of selectivity and resources from the baseline of observation.

While we use student-faculty ratios as a proxy for institutional resources, we note that this is one among many potential proxies, each of which is an imperfect total resource measure (Black and Smith 2006). We favor using student-faculty ratios for several reasons. First, this ratio is likely correlated with the college's ability to meet student demand for courses and with quality of student advising. Second, changes in monetary measures, such as per student expenditures, confound changes in resources with changes in the price of those resources. As faculty salaries and research costs have risen in real terms, one would expect expenditures to have increased. Yet this increase reflects inflation in the inputs to higher education, not necessarily a real resource increase. Student-faculty ratios are measured in the same units in both periods, which makes them a more consistent measure of institutional resources across cohorts. However, table 3 shows that expenditures and student-faculty ratios have largely changed in a similar manner, which underscores that fact that our empirical findings are not very sensitive to which proxy we use for college resources.

5. EMPIRICAL METHODOLOGY AND RESULTS

Changing Student Attributes, Institution Type, and Student-Faculty Ratios

We first examine whether changes in observed student background characteristics, student preparedness for college, and institutional characteristics such as institution type and student-faculty ratios can explain the observed shifts in the time to degree distribution. Appendix table B.2 demonstrates the cross-sectional relationship in NELS:88 between time to degree and the observable characteristics in our data. We focus on NELS:88 because our decompositions implicitly use the cross-sectional relationships from this survey. Consistent with previous work, we find a strong cross-sectional relationship between our student academic preparation variables and time to degree, suggesting that more prepared students graduate faster. The estimates also point to an important role for institutional resources, as proxied by student-faculty ratios, as well as for initial school type.[14]

Given these cross-sectional relationships, we examine how changes in observable precollegiate characteristics of graduates and characteristics of the universities they attend relate to changes in time to degree. We conduct a semi-parametric reweighting, following DiNardo, Fortin, and Lemieux (1996), to decompose the observed change in the distribution of time to degree in order to distinguish the roles of changes in the distribution of observable student and collegiate attributes. We reweight the NELS:88 time to degree distribution using NLS72 data on the characteristics of graduates and the first postsecondary institution they attend. This calculation leads to a counterfactual time to degree distribution in which the proportion of graduates with a given characteristic or a given set of characteristics has not changed between the two surveys. By comparing the observed NELS:88 outcomes and the reweighted NELS:88 outcomes, we can determine the proportion of the observed change in the time to degree distribution that is due to changes in the mix of graduates with a given set of attributes. The remainder reflects changes in other determinants of time to degree as well as changes over time in how characteristics affect time to degree. What we are estimating is the change in time to degree conditional on various observable characteristics, integrated over the distribution of characteristics (see Barsky et al. 2002 for a further discussion).

We generate weights by estimating logistic regressions of a dummy variable equal to 1 if an observation is in the NLS72 cohort on the observable student characteristics. The weights used in the reweighting analysis are $\frac{W_i}{1-W_i}$, where W_i are the predicted values from the logistic regressions. These weights are used to generate our counterfactual NELS:88 time to degree distributions.

The validity of our counterfactual calculations (e.g., the time to degree for those completing college in the 1990s had they been as academically prepared for college as graduates in the 1970s) depends crucially on the cross-sectional

14. One of the reasons why time to degree may vary across sectors is because of differences in peer quality. Unfortunately, we are unable to examine peer effects directly with our data, but how peers affect the timing of degree receipt is an important area for future research.

association between background characteristics and college outcomes, reflecting a causal relationship not seriously influenced by confounding factors. For example, we simulate the time to degree distribution under a counterfactual distribution of test scores and high school GPAs. For this simulation to accurately represent the counterfactual, it must be the case that the cross-sectional relationship between these academic measures and time to degree reflects the impact of precollegiate academic preparedness on this outcome. Regardless of whether the reweighting calculation produces the true counterfactual, the results present a clear accounting framework for assessing the descriptive impact of the change in the composition of students and the institutions they attend on the timing of degree receipt.

Table 4 presents the results from this decomposition. As shown in panel A, changes in academic background variables as well as changes in all precollegiate student attributes predict a *downward* shift in time to degree across cohorts, despite the large upward shift observed in the data. For example, the shift in math, reading, and GPAs among graduates (row 3) predicts a 3.7 percentage point increase in the likelihood of graduating in four years (conditional on graduating in eight years) and a 0.12-year decline in mean time to degree. This finding is due to the fact that academic background is a strong predictor of time to degree, but as shown in table 2, academic preparation for college among graduates has *risen* across cohorts. Other changes in this population, such as parental education, also go in the wrong direction to explain the increase in time to degree. We found that regardless of which variables we standardized on or how we performed the standardization, changes in the characteristics of students graduating from college could not explain the observed increased time to degree.

The other rows of table 4, panel A, show the extent to which measured changes in the supply side of higher education can provide empirical traction in explaining time to degree increases. These estimates include student-faculty ratios as well as institution-type fixed effects in the logistic weighting function, which means the counterfactual time to degree distribution is the distribution expected if student-faculty ratios and the initial school type distribution remained at their 1972 levels while all other variables changed to their 1992 levels. We calculate this counterfactual by taking the difference between the full counterfactual estimates (row 4) and the estimates that account for only individual-level attributes (row 2).

These estimates point to a role for supply-side shifts in explaining time to degree increases. Changes in student-faculty ratios and where graduates attend college can account for more than 12.4 percent of the overall mean time to degree increase. In addition, such changes are associated with about 8 percent of the decline in the proportion of the sample graduating within four years

Table 4. Decompositions of Time to Degree Distribution Changes by Type of Institution

Panel A: Full Sample

Row		4	5	6	7	Mean
1	Observed difference (NELS:88 − NLS72)	−13.7	−9.1	−2.3	−1.7	0.32
2	Difference from observable individual characteristics	4.8	5.0	3.5	3.1	−0.16
3	Difference from student academic preparedness	3.4	3.3	2.1	1.5	−0.10
4	Difference from all observables	3.7	3.3	2.8	2.7	−0.12
5	Net effect of institutional resources (row 4 − row 2)	−1.1	−1.7	−0.7	−0.4	0.04

Panel B: Non–top 50 Public

Row		4	5	6	7	Mean
1	Observed difference (NELS:88 − NLS72)	−20.6	−13.6	−3.3	−1.2	0.44
2	Difference from observable individual characteristics	1.1	3.6	4.6	3.5	−0.12
3	Difference from student academic preparedness	2.4	2.6	1.6	0.6	−0.07
4	Difference from all observables	−2.4	0.7	3.3	3.0	−0.05
5	Net effect of institutional resources (row 4 − row 2)	−3.5	−2.9	−1.3	−0.5	0.07

Panel C: Top 50 Public

Row		4	5	6	7	Mean
1	Observed difference (NELS:88 − NLS72)	−13.0	0.5	4.5	0.2	0.07
2	Difference from observable individual characteristics	3.1	4.9	1.8	1.7	−0.06
3	Difference from student academic preparedness	3.4	3.7	2.4	1.7	−0.10
4	Difference from all observables	3.8	4.4	1.1	1.2	−0.09
5	Net effect of institutional resources (row 4 − row 2)	0.7	−0.5	−0.7	−0.5	−0.03

Panel D: Private Less Selective

Row		4	5	6	7	Mean
1	Observed difference (NELS:88 − NLS72)	−8.7	−2.7	−0.6	−0.01	0.15
2	Difference from observable individual characteristics	5.8	4.9	2.3	1.2	0.02
3	Difference from student academic preparedness	4.3	3.8	1.8	0.0	0.07
4	Difference from all observables	6.0	4.9	1.2	1.0	0.03
5	Net effect of institutional resources (row 4 − row 2)	0.2	0	−1.1	−0.2	0.01

Panel E: Private Highly Selective

Row		4	5	6	7	Mean
1	Observed difference (NELS:88 − NLS72)	7.9	3.7	4.3	2.9	−0.12
2	Difference from observable individual characteristics	12.7	1.4	−0.1	0.0	−0.01
3	Difference from student academic preparedness	8.4	2.8	1.2	0.0	−0.11
4	Difference from all observables	13.0	1.2	−0.1	0.0	−0.01
5	Net effect of institutional resources (row 4 − row 2)	0.3	−0.2	0	0	0.00

Table 4. Continued.

	Panel F: Community College					
Row		**4**	**5**	**6**	**7**	**Mean**
1	Observed difference (NELS:88 − NLS72)	−21.0	−23.6	−12.2	−9.0	0.68
2	Difference from observable individual characteristics	2.1	4.2	4.5	4.8	−0.15
3	Difference from student academic preparedness	1.3	3.0	4.1	3.4	−0.11
4	Difference from all observables	1.3	3.6	5.4	6.1	−0.14
5	Net effect of institutional resources (row 4 − row 2)	−0.8	−0.6	0.9	1.3	0.01

Notes: NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the regression. School type samples refer to first institution attended. The NLS72 and NELS:88 samples are restricted to those who attend college within two years of cohort high school graduation and who finish within eight years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample. Data on faculty and enrollment are from the HEGIS/IPEDS surveys from the Department of Education. Row 1: observed difference between NELS:88 − NLS72; Row 2: observed NELS:88 − predicted outcome assuming the distribution of individual characteristics is the same in 1992 as in 1972; Row 3: observed NELS:88 − predicted outcome assuming the math percentile, reading percentile, and GPA quartile distributions are the same in 1992 as in 1972; Row 4: observed NELS:88 − predicted outcome assuming the distribution of individual characteristics, math percentiles, reading percentiles, GPA quartiles, student-faculty ratios, and initial institution types is the same in 1992 as in 1972; Row 5: row 4 − row 2.

and almost 19 percent of the reduced likelihood of graduating within five years. Thus while supply-side changes cannot fully account for the increases in time to degree that we document, reduced institutional resources and shifts in where students enter the postsecondary system are important factors in explaining why degree time has elongated.

In the remaining panels of table 4, we perform the reweighting analysis separately by type of first institution. Similar to the results for the full sample, we find that changes in precollegiate academic preparation of college graduates and in the background characteristics of these graduates more generally explain none of the increase in time to degree across cohorts within each type of institution. Table 4 provides a strong rejection of the hypothesis that changing individual characteristics among college graduates can explain the extension of time to degree.

Panel B of table 4 points to a significant role for student-faculty ratio increases in explaining time to degree increases in the non–top fifty public sector. Rising student-faculty ratios alone account for 3.5 out of the 20.8 percentage point drop (16.8 percent) in the share of degree recipients completing within four years and for 2.8 of the 11.2 percentage point drop (25 percent) in the share of degree recipients completing within five years. They also can explain 15.9 percent of the mean time to degree increase in this sector. In no other sector do we find a role for changes in institutional resources, as proxied by student-faculty ratios, in explaining changes in time to degree.[15]

As discussed above, student-faculty ratios are an imperfect proxy for school resources. To the extent that the resultant measurement error is classical, it will cause us to understate the effect of institutional resource shifts on time to degree. To generate estimates of the effect of changing collegiate resources on time to degree that are less susceptible to such biases, we next turn to a state-level estimation strategy that uses demand shocks for college as an instrument for institutional resources.

Institutional "Crowding" Estimates

Given that over 85 percent of students attend college in their state of residence, we generate exogenous variation in higher education resources using changes in the number of eighteen-year-olds in each state between 1972 and 1992. As long as educational subsidies do not completely adjust to these demand shocks, the demand shifts generate exogenous variation in institutional resources per student (Bound and Turner 2007). Thus, our analysis is at the state level, as this is the governmental level of control for public universities and, in turn, the division used in determining access for in-state tuition and fees. We use as our key dependent variables the probability of graduating in four years conditional on graduating in eight years, log time to degree, and time to degree in years.

A potential confounding factor in analyzing the relationship between the change in time to degree and the eighteen-year-old population is the role of changing demographic characteristics within each state. For example, if states that witness an increase in their eighteen-year-old population also experience an increase in the number of students with low achievement or from groups with traditionally lower collegiate attainment, and if more college students are pulled from this group, we would observe a time to degree increase regardless of the effect of resources per student on this outcome. To address this problem, we use a two-stage estimator. First, we regress the dependent variable of interest on the student characteristics described in appendix table B.1, a

15. For community colleges, the correlation between observed resource measures (both student-faculty ratios and expenditure metrics) is notably weak; as a result, observed resource declines have no predictive power for explaining elongating time to degree for this sector. Traditional resource measures at community colleges may be appreciably noisier than those in other sectors of higher education, given the reliance on part-time faculty and the substantial provision of nondegree credit services such as federally funded job training. Moreover, students starting at a community college who receive a BA degree receive at least one-half of their eventual credits from a four-year institution, and we do not account for these additional potential institutional resource effects. This finding is consistent with other work on community colleges, which shows that measured community college resources do not affect student outcomes (Stange 2012).

state-specific indicator variable, a cohort-specific dummy (NELS:88 = 1), and a state-cohort interaction term at the individual level:

$$\ln TTD_{ijt} = \alpha + \phi X_{ijt} + \gamma_j S_j + \delta_t D92 + \lambda_j S_j D92 + \varepsilon_{ijt}.$$

We then construct a counterfactual time to degree measure equal to the expected time to degree in state j if the NELS:88 cohort had the same distribution of observables as the NLS72 cohort:

$$\ln(TTD)^{92}_{j72} = \alpha + \phi X_{ij72} + \gamma_j S_j + \delta_t + \lambda_j S_j.$$

In cases where the dependent variable is binary, we use a logit specification to estimate the parameters of this regression; otherwise we use ordinary least squares (OLS). Our goal is to compare the observed NLS72 outcome and the counterfactual outcome for NELS:88 if observable characteristics of students had remained unchanged over time.

Second, we take state-level means of the observed outcomes and the counterfactual outcomes and estimate the second stage:

$$\ln(TTD_{j72}) - \ln\left(TTD^{92}_{j72}\right) = \alpha + \beta d\ln(P_{jt}) + \eta_{jt},$$

where $d\ln(P_{jt})$ is the change in log population of eighteen-year-olds in each state.

Results are reported in table 5. In the first column, the dependent variables are the changes in actual state-level outcomes that are not regression adjusted, NELS:88 − NLS72. In the second column, the dependent variables are the differences between the NELS:88 counterfactual and the actual NLS72 value of the outcome variable. This difference represents the average change within each state in the outcome variable that is not attributable to changes in observable background characteristics.

The results are consistent with the hypothesis that time to degree has expanded the most in states where cohort size has increased, in turn reducing resources per student. In panel A of table 5, which shows results for the full sample, a 10 percent increase in a state's eighteen-year-old population decreases the likelihood of graduating in four years by 3.17 percent and increases time to degree by 1.24 percent, or 0.06 years. The estimates are attenuated somewhat but are qualitatively similar when we control for covariates, as shown in the second column.

In panel B we present results for the sample of respondents whose first institution is a public non–top fifty school. Because these institutions are more open access and because their funding is much more tied to state appropriations than private or top public schools, the effect of demand shocks should be

Table 5. State-Level Estimates of the Effect of Crowding on Multiple Time to Degree Measures: Second-Stage Estimates

Independent Variable: Change in Log 18-Year-Old Population (1992–72)		
Panel A: Full Sample		
Dependent Variable	Actual 92 − Actual 72 Coefficients	Counterfactual 92 − Actual 72 Coefficients
P(graduate in 4 | graduate in 8)	−0.317**	−0.201
	(0.138)	(0.127)
Log time to degree	0.124**	0.078*
	(0.046)	(0.041)
Time to degree	0.631**	0.436**
	(0.185)	(0.174)
Panel B: Public Non–top 50		
Dependent Variable	Actual 92 − Actual 72 Coefficients	Counterfactual 92 − Actual 72 Coefficients
P(graduate in 4 | graduate in 8)	−0.372*	−0.269
	(0.200)	(0.209)
Log time to degree	0.184**	0.146**
	(0.047)	(0.060)
Time to degree	0.943**	0.768**
	(0.276)	(0.301)

Notes: NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the regression. All samples include only those who begin college within two years of cohort high school graduation and obtain a BA within eight years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample. The public non–top 50 samples refer to initial institution of the respondent. Robust standard errors are in parentheses.
*significant at 0.10%; **significant at 0.05%

larger in this sector. Results are consistent with that hypothesis: a 10 percent increase in a state's population of eighteen-year-olds reduces the probability of four-year graduation by 3.7 percent and increases time to degree by 1.8 percent, or 0.09 years. All three estimates are statistically significant at either the 10 percent or 5 percent level and are robust to adjusting for changes in observable characteristics of respondents.[16]

The results presented in table 5 can be thought of as the reduced form of the structural model in which cohort size is used as an instrument for resources. The implied first-stage regression of student faculty ratios (in logs)

16. When we repeat this analysis for the elite public and private schools and for both private sectors, we find no statistically significant evidence that time to degree is influenced by the size of the eighteen-year-old population, which is an expected result, as these sectors should be less responsive to demand shocks because enrollment is less responsive to demand. Similar to our reweighting results, we find no evidence that reductions in resources brought about by crowding extend time to degree among students beginning at community colleges.

on cohort size, adjusting for covariates, produces a coefficient of 0.086 (0.051) for the full sample and 0.143 (0.060) for the non–top fifty public institutions. When multiplied through by the student-faculty ratio changes shown in table 3, these estimates imply a large role for declining resources in explaining increased mean time to degree. However, since both the reduced form results in table 5 and the first-stage coefficients are rather imprecisely estimated, they encompass both large and small effects of changing institutional resources, and we view these results as suggestive evidence of the importance of resource declines that supports our decomposition results.

Credit Constraints and Student Labor Supply

Over the past several decades, the number of hours worked by students increased dramatically. Between 1972 and 1992, average weekly hours worked (unconditional) among those enrolled in college increased by about 2.9 hours, from 9.5 to 12.4, as measured for eighteen- to twenty-one-year-old college students in the October CPS, with a further increase to 13.2 hours per week evident in 2005. Figure 3 shows trends in work hours for those enrolled in college by broad type of institution from the CPS. Employment rates among eighteen- to nineteen-year-old college students rose steadily over the last quarter century, particularly in the two-year and four-year public sectors. Moreover, the share of enrolled students working more than twenty hours per week also increased. These estimates are consistent with several previous studies showing student work hours increasing, particularly among students enrolled at public colleges and universities (see, e.g., Riggert et al. 2006; Stern and Nakata 1991; Fitzpatrick and Turner 2007; Scott-Clayton 2012). Scott-Clayton (2012) does a number of tabulations suggesting that rising college costs leading to increased credit constraints are the most plausible explanation for the relative increase in hours of work for college students in the 1990s.

Consistent with observations from the CPS and with previous work in this area, the comparison of the NLS72 and NELS:88 cohorts shows that hours worked rose sharply for students in their first year of college.[17] For the full sample, average unconditional weekly hours worked increased from 6.6 to 13.0 hours and increased from 14.9 to 20.5 hours on the intensive margin. This increase in working behavior occurred differently across initial school types. For the public non–top fifty sample, average hours increased from 7.3 to 13.5 and from 10.2 to 18.2 hours for students in the sample entering two-year colleges. In the public top fifty sector, average hours increased from 4.8 to

17. The NELS:88 survey does not allow one to track work histories fully between the 1994 and 2000 follow-ups. Thus we restrict the analysis of working hours in both surveys to those enrolled in college in the first year following high school cohort graduation.

Figure 3. Employment among Those Enrolled in College by Type of Institution. Source: Data are from authors' tabulations using the October CPS. Individual weights are used.

10.6, while average hours rose from 5.6 to 11.8 and from 4.1 to 10.1 in the less and highly selective private sectors, respectively. We expect the effect of work hours on time to BA attainment to be increasing in hours of work as the potential for crowd-out within the time constraint increases, although the effect of work hours on time to degree likely is magnified outside the most selective sectors, where the tuition structure imposes a large penalty for less than full-time enrollment.

Estimating the effect of working while in school on the rate of collegiate attainment is difficult because the decision to work and the choice of hours of employment are endogenous.[18] That said, it is hard to imagine that increases of the magnitude seen would not have an impact on time spent on academic pursuits and therefore on time to degree. To approximate the potential effects of hours worked on time to degree, consider a student with a time budget of fifty hours per week available for coursework and employment. With this fixed budget, increased hours worked necessarily reduce the time available for study.

18. Stinebrickner and Stinebrickner (2003) present evidence from a natural experiment at Berea College that students who work more do worse academically. See Riggert et al. (2006) for a critical review of the student employment literature. This review highlights the inherent difficulties in identifying the causal role of student employment on collegiate outcomes.

One of the key parameters in estimating the effect of labor supply on time to degree is the extent of crowd-out of school time for work time. Obtaining credible estimates of this parameter is difficult. However, as Stinebrickner and Stinebrickner (2003) show using experimental data from Berea College, reduced form relationships understate (in absolute value) the negative effect of working time on school time. Thus we infer that the simple correlation between time spent in school and hours of employment will provide a lower bound on the extent of crowd-out.

We use the American Time Use Survey (ATUS) from 2003 to 2006, which is linked to the CPS. The ATUS asks respondents about minutes worked and minutes spent in school (both study time and class time) on the day of the interview. We use interviews from Monday through Friday only, as students may allocate their time differently on weekends, and we scale the time measures to hours per five-day week. For the population of enrolled students, we regress total amount of time spent on school on the total amount of work time and find a crowd-out on the order of −0.3.[19] Using this estimate, we measure the extent to which "effective time to degree" has changed, which is measured as the amount of nonworking time (in months) that it takes each individual to obtain a baccalaureate degree out of high school. For each respondent, effective time to degree is calculated as

$$ttd_i^e = ttd_i * (1 - (h_i/50) * crowdout),$$

where *crowdout* is our ATUS estimate of 0.3. The variable h is hours per week worked in the first year after cohort graduation for NLS72 and NELS:88 respondents. In the above calculation, we assume the average student would spend fifty hours per week on schooling if she did not work. For example, out of a fifty-hour week, if a student works twenty hours, she then will have $(1 - (2/5)^* 0.3) = 88$ percent of her time for study. If we observe that it takes this student five years to graduate, we calculate the effective time to degree as $(1 - 0.12) \times 5 = 4.4$ years.

We estimate an increase in "effective" time to degree from 4.31 to 4.46 years for the full sample assuming a 0.3 crowd-out, suggesting that increases in time working can explain 47.4 percent of the observed mean increase in time to degree across samples of 0.32 years. For students beginning at public non–top fifty schools, effective time to degree increases by 0.28 years, which is 62.6 percent of the observed mean increase in time to degree in this sector. In the community college sector, work increases explain 55.7 percent of the increase

19. See Bound, Lovenheim, and Turner (2010b), appendix table B.2, for regression estimates. These estimates are qualitatively similar when we include weekend hours.

in mean time to degree. Thus, under plausible and conservative assumptions, higher student labor supply can explain a large proportion of the observed mean time to degree increase in our data.

If increased working hours increase time to degree in the less selective public universities and two-year schools, why have they not also done so at private colleges and the top fifty public universities? While we do not have a definitive answer to this question, we believe there are two plausible explanations for the difference. First, the increases in work were off a smaller base in these sectors.[20] In addition, as discussed above, pricing structures differ dramatically across sectors, with it being substantially more difficult and expensive to increase the time to degree in the selective public and private sectors than it is in the less selective public sector.

Consistent with the interpretation that increased student employment and the associated extension in time to degree reflects constraints in the capacity to finance college, we find some evidence of a widening difference in time to degree for students from above and below the median of the income distribution. Rising tuition charges coupled with declines in the availability of federal financial aid lead to unambiguous increases in net cost for low-income students, as little additional financial aid is offered by public universities, particularly outside the most selective few. Over time, as college costs have risen, families as well as individual students have been expected to shoulder an increasingly larger portion of the cost of college attendance.

Although our descriptive statistics and decomposition analysis lead to rejection of the hypothesis that family economic circumstances among college graduates worsened, students in the 1992 cohort from below the median family income level may have faced greater challenges in paying for college. Figure 4 plots the distribution of time to degree, holding the distribution of student achievement constant, for graduates above and below the median family income in both cohorts. Overall and for students at the public institutions outside the top fifty, the gap in time to degree grows appreciably between 1972 and 1992 between below-median family income students and their peers above the median. Notably, among students at public colleges outside the top fifty, the time path of BA completion across these family income levels was similar for the 1972 cohort, with about 84 percent of eventual completers from both income groups finishing in five years. For the 1992 cohort, however, a substantial gap emerged in outcomes by socioeconomic circumstances, with

20. In the top fifty sector, effective time to degree increased by 0.04 years, and it increased by 0.02 years in the less selective private sector. In the highly selective privates, effective time to degree actually declined. Thus increased labor supply affected degree length the most in the non–top fifty public and community college sectors.

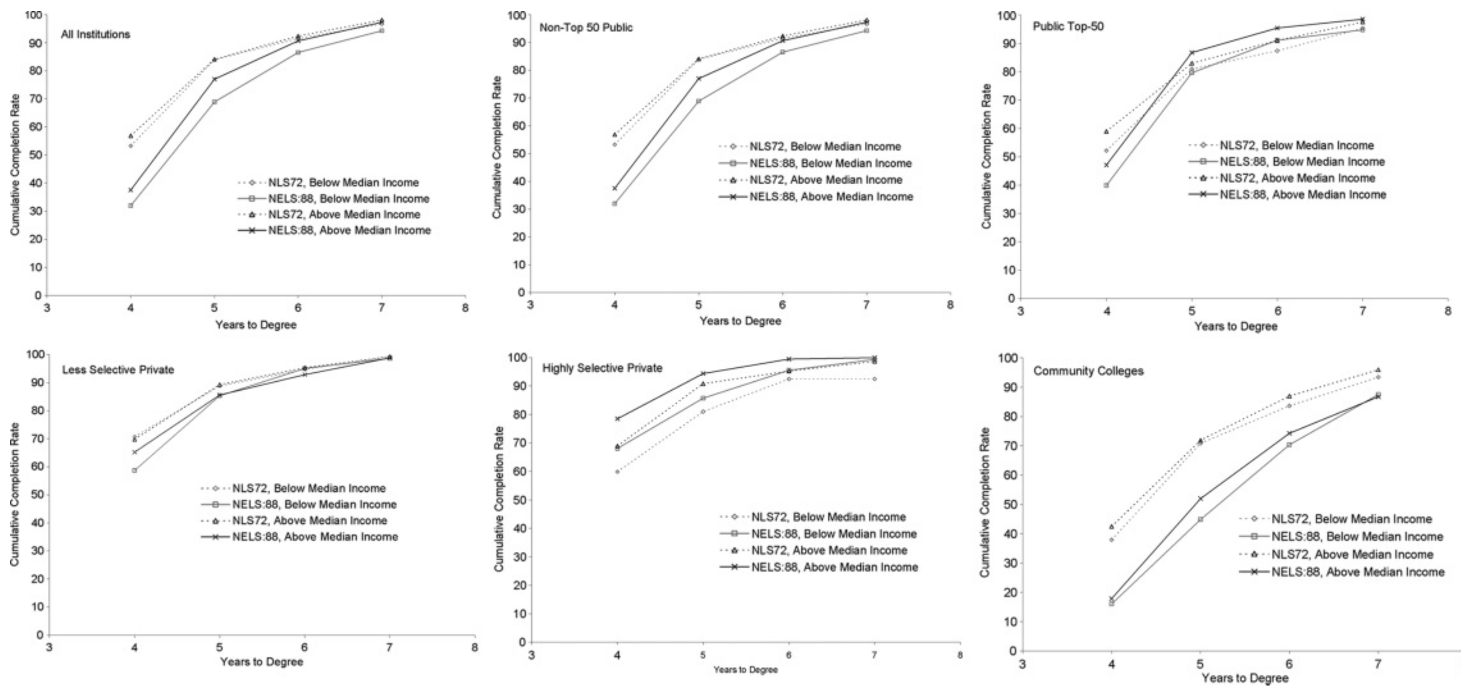

Figure 4. Time to Degree by Family Income and Type of Institution. NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations. The NLS72 and NELS:88 samples are restricted to those who attend college within two years of cohort high school graduation and who finish within eight years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample. Median income is defined as greater than $15,000 in the NLS72 sample and as greater than $50,000 in the NELS:88 sample in nominal dollars.

75 percent of high-income degree recipients finishing in five years relative to 69 percent of low-income degree recipients.

6. DISCUSSION

The data are clear with respect to the growth in time to degree for BA recipients over the past three decades. While we focus our analysis on the inter-cohort comparison afforded by NLS72 and NELS:88, this finding is reinforced in other data sets, including the CPS and the National Survey of College Graduates. Furthermore, it is clear that the rise in time to degree is largely concentrated among students beginning at non–top fifty ranked public universities and two-year colleges. Although we are constrained by limited exogenous variation that would provide sharp identification of causal mechanisms, we marshal substantial proximate evidence for and against three main explanations for the time to degree increases we observe.

First, we find no evidence that changes in student background characteristics or incoming academic preparation can explain these shifts. In fact, changes in these observables go in the wrong direction to explain time to degree increases.

Second, our analysis points to the importance of changes in resources on the supply side of public higher education in explaining time to degree changes. We present evidence that increases in student-faculty ratios can explain some of the expansion in time to degree we document, particularly in the non–top fifty public sector. Furthermore, we find that increases in cohort size within some states led to declines in resources per student at non–top tier public institutions—schools that could not ration access through selective admissions. The resulting increased stratification in per student resources within the public sector led to substantial extension of time to degree for students beginning college at non–top fifty public four-year colleges compared with only very modest increases for students at top-tier public universities.

Third, we argue that increased student labor supply in response to increases in direct college costs, plausibly reflecting credit constraints that limit the capacity of students to finance full-time attendance, is empirically relevant to explaining increased time to degree. For many students, family economic circumstances have eroded relative to the cost of college, contributing to the need to increase employment to cover a greater share of college costs. Consequently, students in the more recent cohorts are working a significantly higher number of hours while they are in school. Although the magnitude of the effect of increased employment on degree progress is hard to ascertain with precision, the direction of the effect is unambiguous, and our calculations suggest that increased working behavior alone can explain about half of the mean increase in time to degree and almost two-thirds of the increase in the non–top fifty sector.

The sum total of our evidence points strongly toward the central role of declines in both personal and institutional resources available to students in explaining the increases in time to baccalaureate degree in the United States. That these increases are concentrated among students attending public colleges and universities outside the most selective few suggests a need for more attention to how these institutions adjust to budget constraints and student demand and how students at these colleges finance higher education. Moreover, the fact that students from below-median-income families have experienced the largest increases in time to degree not only supports the hypothesis that credit constraints limit the rate of collegiate attainment but also points to substantial distributional consequences, as extended time to degree has unambiguously large private costs.

While clear evidence in the United States and abroad indicates that the rate of degree attainment responds to incentives in financial aid and tuition pricing,[21] our analysis points to the need for careful consideration of whether reducing students' financial burdens while enrolled in college would help reduce time to degree. Our finding of increased stratification in resources among colleges and universities—both between public and private and within the public sector—suggests that the attenuation of resources at less selective public universities in particular limits the rate of degree attainment. To this end, further work to understand how students, public funders, and colleges assume the costs of increased time to degree is important to better understand the social welfare implications of policies designed to reduce time to degree.

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21. Scott-Clayton (2011) presents evidence of increases in four-year degree completion among students receiving a West Virginia scholarship contingent on completion of a full course load. Garibaldi et al. (2012) also show that students facing higher tuition are more likely to finish in four years using tuition discontinuities at Bocconi University in Italy.

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APPENDIX A: TECHNICAL APPENDIX

1. NLS72 AND NELS:88 DATA

Time to Degree and Degree Completion

Time to degree and degree completion are calculated using NLS72 and NELS:88 survey responses from the first through fifth follow-ups in NLS72 and the fourth follow-up in NELS:88. The NLS72 study participants were seniors in high school in the spring of 1972. Following the base year interview, participant follow-up surveys were administered in 1973, 1974, 1976, 1979, and 1986 (for a subsample), with questions covering collegiate participation and degree attainment. In addition, the Department of Education collected detailed high school records and postsecondary transcripts.

The NELS:88 survey started with students who were in the eighth grade in 1988 (high school class of 1992) and conducted follow-up surveys with participants in 1990, 1992, 1994, and 2000. Similar to the NLS72 survey, NELS:88 contains high school records and collegiate transcripts as well as a host of background information that may be relevant to time to degree.

Although degrees can be awarded throughout a year, we calculate time to degree as the number of months between high school cohort graduation and the month of BA receipt. Cohort high school graduation is defined as June 1972 for the NLS72 sample and June 1992 for the NELS:88 sample.

Because the NELS:88 survey is composed of eighth graders from 1988 and the NLS72 survey follows twelfth graders from the class of 1972, the NELS:88 survey contains more students who graduate high school after their cohort's high school graduation. In our base sample, 1.3 percent of respondents in NLS72 and 4.4 percent of respondents in NELS:88 finish high school after June of their respective cohort graduation year. However, looking only at eight-year BA recipients, 0.3 percent and 0.6 percent, respectively, in NLS72 and NELS:88 did not finish high school on time. It therefore is unlikely that the larger preponderance of late high school graduates in the NELS:88 survey biases our time to degree calculations.

Table A.1 contains variable names and definitions used to define the sample and to calculate time to degree and degree completion in both the NLS72 and NELS:88 surveys.

School Type and Collegiate Start Dates

We define enrollment as those who start at an academic institution within two years of cohort high school graduation. Academic institutions are all four-year schools and public two-year schools. We exclude private two-year

Table A.1. Variable Names and Definitions for Calculation of Time to Degree and Degree Completion in NLS72 and NELS:88

Panel A: NLS72

Variable Name	Variable Definition	Follow-Up
Fq2	High school completion dummy	2
Edatt86	Educational attainment as of 1986	1–5
Fq3b	High school graduation year	2
Fq3a	High school graduation month	2
Tq48ea	BA completion dummy as of 10/1/1976	3
Tq48eb	Month BA received as of third follow-up	3
Tq48ec	Year BA received as of third follow-up	4
Ft76ea	BA completion as of fourth follow-up	4
Ft76eb	Month BA received as of fourth follow-up	4
Ft76ec	Year BA received as of fourth follow-up	5
Fi19b1ey–Fi19b4ey	Year ended most recent school attended, first through fourth time	5
Fi19b1em–Fi19b4em	Month ended most recent school attended, first through fourth time	5
Fi19h	Course of study in most recent school attended	5
Fi19i	Completed requirements in most recent school attended	5
Fi20b1ey–Fi20b4ey	Year ended second most recent school attended, first through fourth time	5
Fi20b1em–Fi20b4em	Month ended second most recent school attended, first through fourth time	5
Fi19h	Course of study in second most recent school attended	5
Fi19i	Completed requirements in second most recent school attended	5

Panel B: NELS:88

Variable Name	Variable Definition	Follow-Up
F4hsgradt	High school graduation date	4
F4ed1	Degree receipt date–first degree received	4
F4edgr1	Degree type received–first degree	4
F4ed2	Degree receipt date–second degree received	4
F4edgr2	Degree type received–second degree	4
F4ed3	Degree receipt date–third degree received	4
F4edgr3	Degree type received–third degree	4
F4ed4	Degree receipt date–fourth degree received	4
F4edgr4	Degree type received–fourth degree	4
F4ed5	Degree receipt date–fifth degree received	4
F4edgr5	Degree type received–fifth degree	4
F4ed6	Degree receipt date–sixth degree received	4
F4edgr6	Degree type received–sixth degree	4

schools because they typically are not oriented toward allowing students to obtain a BA after graduation.

College transcript data and self-reported enrollment records from the first through fourth follow-up surveys for the NLS72 survey and from NCES-aggregated responses in the NELS:88 survey are used to define the type of institution of initial collegiate enrollment. We use the transcript for the first post–high school institution attended by respondents in the transcript files to assign first institution attended for most respondents. In the cases in which there are multiple first transcripts from different institutions on the same date, we assign each student to the school at which she took the most credits during the first semester. Some students report attending college within two years of their cohort's high school graduation but do not have any transcripts. In NLS72, 6.8 percent of the sample reporting attendance do not have transcripts, and in NELS:88, 8.2 percent of the sample falls into this category. For these respondents, we use the first institution reported by them in the survey files.

In the NLS72 survey, we begin by determining the year in which a student first enrolls in an academic postsecondary institution, where *academic* is defined as granting at least an associate's degree or BA. In each follow-up, students were asked about colleges they attended (up to three) in each year since the previous survey. The first college attended is identified from the entry the first time a student reports attending an academic institution, and we record the institutional identifier (FICE code) either directly from transcript files or from the student survey responses about which institution he or she attended. We then merge institutional-level information that contains public/private status, two-year/four-year identifiers, and collegiate rankings and classify the respondent's initial institution accordingly.

In the NELS:88 survey, we use a similar methodology to identify each respondent's initial institution. The NCES has constructed variables that identify first institution attended in the transcript files (the ref variables). We use the transcript-based NCES-constructed institutional identifier (unitid) code when it is available. For those who report college attendance and the sector of first attendance but are not assigned a transcript-based first institution identifier by NCES, we use the NCES-constructed variables that report individual enrollment histories from the survey data that identify first institution of enrollment (unitid) and first institution type (f4efsect).

For students with postsecondary experience preceding high school graduation, we use the first start date and institution after high school graduation taken from the postsecondary transcript files. For all other students in the NELS:88 survey, first start date is identified by f4efmy, which is the NCES-constructed date of first postsecondary attendance.

A list of the top fifty public schools from the 2005 *U.S. News and World Report* rankings as well as the top sixty-five private schools and the top fifty liberal arts colleges plus the U.S. Armed Services Academies, which constitute the highly selective private schools, is shown in table A.2.

Background Characteristics

Math and Reading Tests

In both surveys, tests of academic achievement were administered to students in the senior year. The NLS72 exam was administered as a sixty-nine-minute test book with sections on vocabulary, picture numbers (associative memory), reading, letter groups, mathematics, and mosaic comparisons. Each section was fifteen minutes (except for the mosaic comparison, which was nine minutes). We use the reported scaled math score (scmatsc) test score measure in NLS72.

The NELS:88 cognitive test batteries were administered in each of the first three waves, with sections on reading, math, science, and social studies. The tests were eighty-five minutes and consisted of 116 questions, 40 of which were on math and 21 of which were on reading comprehension. Unlike the NLS72 exams, the NELS:88 tests covered more material and tested more skills. Further, because the NELS:88 tests were given in subsequent waves, students were given harder or easier tests in the first and second follow-ups, depending on their scores in the previous wave, to guard against floor and ceiling effects. We use the math and reading item response theory (IRT) theta scores (f22xmth and f22xrd) from the second follow-up as the base measure of test scores. These scores are psychometric evaluation scores of each student's ability that account for the difficulty of the exam.

Because the tests in NLS72 and NELS:88 covered different subject matter, were of different lengths, and were graded on different scales, the scores are not directly comparable across surveys. Instead, we construct the percentile of the score distribution for each survey among all high school graduates on each exam. The comparison of students in the same test percentile across surveys is based on the assumption that overall achievement did not change over this time period. This assumption is supported by the observation that there is little change in the overall level of test scores on the nationally representative NAEP over our period of observation. Similarly, examination of time trends in standard college entrance exams such as the SAT provides little support for the proposition that achievement declined appreciably over the interval. For the SAT, the ratio of test takers to high school graduates increased from 33 percent to 42 percent, while mean math scores declined from 509 to 501 over the 1972–92 interval (NCES 2006).

Table A.2. Top 50 Public Schools, Top 65 Private Schools, and Top 50 Liberal Arts Colleges from the 2005 *U.S. News and World Report* Rankings

	Highly Selective Private Schools		
Top 50 Public Schools	**Top 65 Private Schools**		**Top 50 Liberal Arts**
University of California–Berkeley	Harvard University	University of Tulsa	Amherst College
University of Virginia	Princeton University	Texas Christian University	Williams College
University of Michigan–Ann Arbor	Yale University	University of Dayton	Swarthmore College
University of California–Los Angeles	University of Pennsylvania	Drexel University	Wellesley College
University of North Carolina–Chapel Hill	Duke University	Illinois Institute of Technology	Carleton College
College of William and Mary	MIT	University of San Diego	Middlebury College
University of Wisconsin–Madison	Stanford University	Catholic University	Pomona College
University of California–San Diego	California Institute of Tech.	Loyola University	Bowdoin College
University of Illinois	Columbia University	Univ. of San Francisco	Davidson College
Georgia Institute of Technology	Dartmouth College	University of the Pacific	Haverford College
University of California–Davis	Northwestern University	New School	Claremont-McKenna
University of California–Irvine	Washington Univ. of St. Louis	Northeastern University	Wesleyan University
University of California–Santa Barbara	Brown University	Seton Hall University	Grinnell College
University of Texas–Austin	Cornell University	University of St. Thomas	Vassar College
University of Washington	Johns Hopkins University		Harvey Mudd College
Pennsylvania State University	University of Chicago		Washington and Lee
University of Florida	Rice University		Smith College
University of Maryland–College Park	Notre Dame University		Hamilton College
Rutgers University–New Brunswick	Vanderbilt University		Colgate University
University of Georgia	Emory University		Oberlin College
University of Iowa	Carnegie Mellon University		Colby College
Miami University (Ohio)	Georgetown University		Bates College
Ohio State University	Wake Forest University		Bryn Mawr College
Purdue University	Tufts University		Colorado College
Texas A&M–College Station	Univ. of Southern California		Macalester College
University of Connecticut	Brandeis University		Scripps College

University of Delaware	New York University	Mt. Holyoke College
University of Minnesota–Twin Cities	Case Western Reserve	Barnard College
University of Pittsburgh	Boston College	Bucknell University
Indiana University	Lehigh University	Kenyon College
Michigan State University	Univ. of Rochester	College of the Holy Cross
Clemson University	Tulane University	Trinity College
SUNY at Binghamton	Rensselaer Polytechnic	Lafayette College
University of California–Santa Cruz	Yeshiva University	Occidental College
University of Colorado–Boulder	George Washington Univ.	Bard College
Virginia Tech.	Pepperdine University	Furman University
University of California–Riverside	Syracuse University	Whitman College
Iowa State University	Worcester Polytechnic	Union College
North Carolina State University	Boston University	Franklin and Marshall
University of Alabama	University of Miami	Sewanee College
University of Missouri–Columbia	Fordham University	University of Richmond
Auburn University	Southern Methodist Univ.	Connecticut College
University of Kansas	Brigham Young University	Centre College
University of Tennessee–Knoxville	Clark University	Dickinson College
University of Vermont	Stevens Inst. of Technology	Skidmore College
Ohio University	St. Louis University	Gettysburg College
University of Arizona	Baylor University	Pitzer College
University of Massachusetts–Amherst	American University	DePauw University
University of Nebraska–Lincoln	Howard University	Rhodes College
University of New Hampshire	Marquette University	Reed College
	University of Denver	

Notes: Schools are listed in the order they appear in the *U.S. News and World Report* ranking. The rankings include many ties, in which case schools are listed alphabetically within rank. This table lists schools within rank in the same manner. The highly selective private school category also includes the four U.S. Armed Services Academies: U.S. Naval Academy, U.S. Air Force Academy, U.S. Military Academy at West Point, and U.S. Coast Guard Academy.
Source: 2005 *U.S. News and World Report* rankings of colleges and universities in the United States.

In the multiple imputation of missing variables in the NELS:88 survey, we use IRT theta test scores from the first follow-up for math and reading (f12xmth and f12xrd) and from the base year for math and reading (by2xmth and by2xrd). The NCES scales the IRT theta scores to a common metric across years. The imputed math and reading test scores from the senior year in each survey are used to construct the test percentiles used in the main analysis.

High School GPA

In the NLS72 survey, the NCES provides a measure of the high school GPA: imptaver. This variable is constructed by the NCES using high school transcripts and is on a 1–13 scale.

In NELS:88, we use the F2rgpa variable, which is the NCES-constructed measure of high school GPA as of the last year of high school attended.

Because these GPA variables are on different scales and are potentially calculated in different ways, we calculate the quartile of the high school GPA for each student in the data, separately for each sample. These quartiles are constructed using the full sample, not just the sample of college completers.

Parental Education

We obtain student-reported measures of father's and mother's education separately. In the NLS72 survey, we have three different measures of this variable. For mother's education we use the variables cmoed, bq90b, and fq78b. For father's education we use the variables cfaed, bq90a, and fq78a. If there are disagreements across measures, fq78b and fq78a take precedence.

In the NELS:88 survey, we also use student reports of father's education (bys34a) and mother's education (bys34b). For the multiple imputation model, we include parent self-reports of their own education from the base year and second follow-up parental surveys. In the base year parent survey, we combine information on whether the respondent and his or her spouse is the father or mother (byp1a1 and byp1a2) with reported self (byp30) and spouse (byp31) educational attainment. A similar methodology is used for the second follow-up parent survey, using f2p1a and f2p1b to identify the gender of the respondent and the spouse, respectively, and f2p101a and f2p101b to identify educational attainment of the respondent and the spouse, respectively. The base year and second follow-up parental education information are aggregated into two variables, father's education and mother's education, used in the multiple imputation model.

Parental Income Levels

The parental income variables are bq93 for NLS72 and f2p74 for NELS:88. The former is reported by the student, while the latter is reported by the parents. Unfortunately, NLS72 does not contain a parent-reported measure and the NELS:88 survey does not contain a student-reported measure, so these variables are the most closely aligned parental income measures across the two surveys.

Rather than asking directly for parental income levels, the NELS:88 and NLS72 surveys ask for income ranges from respondents. Because we are interested in measuring parents' ability to finance college, the variable of interest is the real income level, not one's place in the income distribution. We thus align the income blocks across the two surveys using the consumer price index. In NLS72, we construct the following measured income groups: less than \$3,000, \$3,000–\$6,000, \$6,000–\$7,500, \$7,500–\$10,500, \$10,500–\$15,000, and greater than \$15,000. In NELS:88, we create the following corresponding real income blocks: less than \$10,000, \$10,000–\$20,000, \$20,000–\$25,000, \$25,000–\$35,000, \$35,000–\$50,000, and greater than \$50,000. Across surveys, the six income groups are comparable in real terms.

Race

Race is measured in the NLS72 survey using crace and race86. The latter is used if the former is blank due to nonresponse. In the NELS:88 survey, race is measured using the race variable available in the data files.

2. PROCEDURES TO HANDLE MISSING DATA

Multiple Imputation

There is a considerable amount of missing data in the NLS72 and NELS:88 surveys. Table A.3 presents the number of unweighted missing observations by variable and survey. These observations are not missing completely at random; respondents who have no math test scores have lower time to degree conditional on finishing.

Casewise deletion of missing observations will therefore cause a bias in the calculation of the base trends we are seeking to explain in this analysis. To deal with this problem, we use the multiple imputation by chained equation (MICE) algorithm developed by Van Buuren, Boshuizen, and Knook (1999) that is implemented through the Stata module ICE (see Royston 2004 for a detailed discussion of ICE).

MICE is implemented by first defining the set of predictor variables ($x_1 \dots x_k$) and the set of variables with missing values to be imputed: math test scores, reading test scores, high school GPA, father's education, mother's education, and parental income levels ($y_1 \dots y_6$). The MICE algorithm implemented by ICE first randomly fills in all missing values from the posterior

Table A.3. Number of Imputed Observations by Survey and Variable (Unweighted)

	Number of Imputed Observations	
Variable	**NLS72**	**NELS:88**
Math test score	1,197	690
Reading test score	1,197	690
High school GPA	688	1,220
Mother's education	27	520
Father's education	26	540
Parent income	979	520
Total	4,350	4,140

Notes: Observation counts include only those respondents who enroll in college within two years of cohort high school graduation at a four-year institution or a non-private two-year college and who graduate within eight years. Per the restricted data license agreement with the National Center for Education Statistics, all unweighted NELS:88 sample sizes are rounded to the nearest 10.

distribution of each variable. Then, for each variable with missing data, y_i, STATA runs a regression (or ordered logit) of y_i on $y_{\sim i}$ and $x_1 \ldots x_k$ and calculates expected values from these regressions for all missing data points. The expected values then replace the randomly assigned values for the missing data points. A sequence of regressions for each y_i is a cycle, and this process is repeated for ten cycles, replacing the missing values with the new expected values from each regression in each cycle. The imputed values after ten cycles constitute one imputed data set, and this process is repeated five different times to generate five imputed data sets.

There are two important specifications in implementing MICE: determination of the predictor variables and determination of the imputation models. Because of the different structure of the two surveys, different variables are used in the imputation procedure across surveys. In both surveys we include dummy variables for cumulative time to degree from four to eight years, dummy variables for initial school type, interactions between these variables, an indicator for college attendance within two years of cohort high school graduation, and race and gender indicators.

Due to the structure of the NELS:88 survey, there is more background information with which to impute missing data. We use eighth- and tenth-grade math test scores, parental reports of their education from the base year and second follow-up parent surveys, and parental reports of their income level from the base year parent survey. The definitions of the variables used in the imputation models are discussed in the preceding section.

Because the math and reading test scores are continuous variables, we use OLS regressions to impute these variables. Mother's and father's education, GPA, and income, however, are categorical variables. Because of the ordered nature of these variables, we use ordered logits to impute the missing values of these variables. While these model choices are reasonably arbitrary, they are used only to draw ranges of plausible estimates of missing data.

The multiple imputation procedure creates five different data sets, each with different imputed values for the missing observations. All reported statistics and results in our analysis are averages across data sets. In other words, we conduct each analysis separately for each data set and average the final result. The average of final results is what is reported in the article's tables and figures.

Dropped Observations and Missing Transcript Data

The base sample in this analysis consists of all respondents who graduate high school, attend college within two years of their cohort's high school graduation, and obtain a BA within eight years of their cohort's high school graduation. We further restrict the sample to exclude those whose only enrollment over this time period is at a private two-year institution, as these schools are predominantly professional without a BA track. Table A.4 presents information on the number of observations that are dropped by survey and the reason for dropping the observation. For example, in NLS72 168 respondents are dropped because they are not high school graduates, whereas in NELS:88 720 are dropped for this reason. The apparently higher dropout rate in NELS:88 is because the universe of students is all those enrolled in the eighth grade in 1988, whereas the universe in NLS72 consists of all those enrolled in twelfth grade in 1972.

In the NLS72 survey, 63 observations are dropped because they report attending college but provide no information on either the type of institution or the date they first began attending this institution; in NELS:88, 50 respondents do not provide this information. In addition, 200 observations were dropped because they were not in all four waves of the NELS:88 survey. In other words, they have a sample weight of zero. Furthermore, in NELS:88, 4,140 observations are dropped because they do not earn a BA within eight years, and 2,868 observations are dropped in NLS72 for this reason.

Of potential concern in constructing our sample is the exclusion of those beginning college more than two years after high school cohort graduation. We exclude these observations because we are interested in the truncated, eight-year time to degree distribution. These statistics have a different interpretation for a student who began college directly after high school than for a student who began college, for instance, five years after high school. In NLS72, 889

Table A.4. Number of Dropped Observations by Category (Unweighted)

NLS72		
Sample Change	**Dropped Observations**	**Remaining Observations**
Original Base: Fifth follow-up sample		12,841
High school dropouts	168	12,673
Missing initial school information	63	12,610
Never attended college	4,503	8,107
Time between HS and college > 2 years	889	7,218
College dropout	2,868	4,350
NELS:88		
Sample Change	**Dropped Observations**	**Remaining Observations**
Original base: Fourth follow-up sample		12,140
High school dropouts	720	11,420
Observations not in all 4 waves	200	11,220
Missing initial school information	50	11,170
Never attended college	1,920	9,250
Time between HS and college > 2 years	970	8,280
College dropout	4,140	4,140

Notes: Per the restricted data license agreement with the National Center for Education Statistics, all unweighted NELS:88 sample sizes are rounded to the nearest 10.

respondents attend college more than two years after their cohort's high school graduation, and in NELS:88, 970 do so. Given the similarity of these numbers, shifts in when students began attending college cannot account for the trends in time to degree reported in the main text.

Table B.1. Means of NLS72 and NELS:88 Variables for College Graduates within Eight Years, by School Type

	Full Sample		Non–Top 50 Public		Public Top 50		Private Less Selective		Private Highly Selective		Community College	
Variable	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**
Math test percentile	70.7	71.6	68.3	70.0	78.3	81.0	69.9	70.1	81.2	84.3	64.0	58.5
Reading test percentile	66.8	68.0	64.9	66.4	73.9	75.2	66.0	68.9	75.6	78.1	60.4	55.9
Lowest GPA quartile	0.121	0.045	0.123	0.034	0.049	0.016	0.128	0.049	0.060	0.008	0.230	0.120
Second GPA quartile	0.120	0.132	0.138	0.144	0.065	0.069	0.129	0.135	0.080	0.047	0.134	0.224
Third GPA quartile	0.434	0.284	0.452	0.316	0.406	0.208	0.444	0.295	0.402	0.225	0.416	0.324
Top GPA quartile	0.325	0.539	0.287	0.507	0.480	0.707	0.299	0.521	0.459	0.719	0.219	0.331
Student-faculty ratio	25.538	29.807	24.993	27.562	22.974	22.237	22.088	23.859	18.773	18.435	40.899	57.117
Ln(student-faculty ratio)	3.172	3.276	3.191	3.290	3.113	3.088	3.022	3.087	2.878	2.853	3.624	3.953
Missing S/F ratio	0.099	0.052	0.083	0.036	0.144	0.060	0.074	0.055	0.117	0.070	0.124	0.058
Initial school type												
Non–top 50 public	0.449	0.345										
Top 50 public	0.144	0.175										
Less selective private	0.186	0.199										
Highly selective private	0.094	0.113										
Community college	0.126	0.168										
Father's education												
No HS diploma	0.171	0.058	0.217	0.064	0.105	0.062	0.155	0.037	0.083	0.028	0.170	0.085
HS diploma	0.221	0.208	0.231	0.217	0.200	0.169	0.226	0.214	0.143	0.070	0.261	0.315
Some college	0.248	0.219	0.262	0.266	0.200	0.159	0.237	0.221	0.241	0.109	0.272	0.258
BA	0.199	0.249	0.172	0.258	0.282	0.237	0.223	0.275	0.188	0.311	0.175	0.171
Graduate school	0.162	0.266	0.118	0.195	0.212	0.375	0.159	0.253	0.345	0.482	0.123	0.171

Table B.1. Continued.

	Full Sample		Non–Top 50 Public		Public Top 50		Private Less Selective		Private Highly Selective		Community College	
Variable	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**	**NLS72**	**NELS:88**
Mother's education												
No HS diploma	0.133	0.052	0.165	0.058	0.093	0.053	0.118	0.032	0.042	0.016	0.154	0.086
HS diploma	0.367	0.266	0.406	0.294	0.329	0.198	0.341	0.310	0.237	0.069	0.405	0.359
Some college	0.284	0.263	0.272	0.290	0.306	0.226	0.293	0.244	0.324	0.227	0.260	0.293
BA	0.153	0.245	0.118	0.214	0.195	0.277	0.196	0.264	0.231	0.365	0.105	0.169
Graduate school	0.063	0.175	0.040	0.144	0.076	0.247	0.052	0.150	0.166	0.322	0.075	0.093
Parental income												
<$3,000/<10,000	0.025	0.030	0.028	0.031	0.022	0.028	0.022	0.034	0.009	0.023	0.031	0.033
$6,000/20,000	0.050	0.063	0.055	0.082	0.039	0.049	0.051	0.070	0.020	0.015	0.066	0.062
$7,500/25,000	0.064	0.062	0.073	0.058	0.059	0.075	0.070	0.048	0.016	0.030	0.069	0.094
$10,500/35,000	0.186	0.102	0.231	0.121	0.124	0.054	0.161	0.111	0.095	0.047	0.197	0.140
$15,000/50,000	0.272	0.200	0.265	0.218	0.267	0.148	0.294	0.222	0.247	0.134	0.287	0.235
$15,000+/50,000+	0.404	0.543	0.349	0.490	0.488	0.646	0.402	0.515	0.614	0.751	0.350	0.437
Race/ethnicity												
Asian	0.017	0.058	0.014	0.039	0.036	0.084	0.011	0.041	0.018	0.088	0.016	0.072
Hispanic	0.019	0.055	0.018	0.056	0.023	0.056	0.008	0.055	0.024	0.043	0.033	0.060
African American	0.072	0.074	0.094	0.073	0.035	0.069	0.080	0.105	0.041	0.043	0.048	0.064
White	0.892	0.813	0.874	0.832	0.906	0.790	0.901	0.799	0.917	0.827	0.903	0.805
Male	0.523	0.453	0.488	0.436	0.542	0.483	0.513	0.410	0.650	0.473	0.550	0.495
Number of observations	4,350	4,137	1,959	1,397	652	733	802	822	392	562	545	623

Notes: NLS72 calculations were made using the fifth follow-up weights included in the survey. Fourth follow-up weights were used for the NELS:88 survey calculations. Only those participating in these follow-ups are included in the tabulations. The NLS72 and NELS:88 samples are restricted to those who attend college within two years of cohort high school graduation. Cohort high school graduation is defined as June 1972 for the NLS72 and June 1992 for the NELS:88.
Source: Authors' tabulations from the NELS:88 and NLS72 surveys.

Table B.2. OLS Estimates of the Relationship between Student and Institutional Characteristics and Time to Degree among NELS:88 Graduates

Dependent Variable	Full Sample	Non–Top 50 Public	Top 50 Public	Private Less Selective	Private Highly Selective	Two-Year College
Ln(student-faculty ratio)	0.163**	0.487**	0.347	0.016	−0.050	−0.007
	(0.078)	(0.153)	(0.265)	(0.107)	(0.145)	(0.154)
Missing student-faculty ratio	0.160	1.387**	0.941	−0.046	−0.371	0.161
	(0.272)	(0.513)	(0.829)	(0.365)	(0.526)	(0.674)
Math test percentile	−0.003**	−0.003	−0.001	−0.002	−0.010**	−0.006
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
Reading test percentile	0.001	0.002	−0.003	−0.001	0.0004	0.006**
	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)
Second GPA percentile	−0.411	−0.325	−1.726**	−0.256	−0.680	−0.240
	(0.280)	(0.323)	(0.680)	(0.593)	(0.715)	(0.306)
Third GPA percentile	−0.663**	−0.656**	−1.757**	−0.341	−0.537	−0.685**
	(0.242)	(0.286)	(0.639)	(0.559)	(0.645)	(0.304)
Top GPA percentile	−0.850**	−0.709**	−1.876**	−0.578	−0.710	−1.063**
	(0.238)	(0.266)	(0.627)	(0.571)	(0.642)	(0.333)
Parent income $6,000/20,000	0.035	0.312	−0.110	−0.102	0.784	−0.092
	(0.180)	(0.318)	(0.285)	(0.340)	(0.659)	(0.390)
Parent income $7,500/25,000	0.107	0.480	0.057	−0.216	−0.297	0.151
	(0.170)	(0.312)	(0.312)	(0.249)	(0.392)	(0.376)
Parent income $10,500/35,000	0.098	0.358	0.167	0.001	0.116	−0.112
	(0.161)	(0.341)	(0.315)	(0.234)	(0.414)	(0.337)
Parent income $15,000/50,000	0.158	0.588*	0.114	−0.054	−0.112	−0.200
	(0.158)	(0.315)	(0.302)	(0.243)	(0.380)	(0.365)
Parent income $15,000+/50,000+	0.027	0.375	−0.045	−0.020	−0.301	−0.300
	(0.153)	(0.294)	(0.258)	(0.235)	(0.369)	(0.365)
Father HS diploma	−0.236	0.113	−0.002	−0.442**	−0.308	−0.589*
	(0.163)	(0.207)	(0.212)	(0.219)	(0.499)	(0.328)
Father some college	−0.227	−0.067	−0.043	−0.214	−0.618	−0.251
	(0.147)	(0.219)	(0.189)	(0.239)	(0.424)	(0.324)
Father BA	−0.371**	−0.123	−0.265	−0.481*	−0.380	−0.489
	(0.160)	(0.247)	(0.183)	(0.259)	(0.451)	(0.342)
Father graduate school	−0.233	0.031	0.056	−0.311	−0.393	−0.547*
	(0.164)	(0.247)	(0.226)	(0.264)	(0.449)	(0.314)
Mother HS diploma	−0.110	−0.141	−0.266	0.022	0.004	−0.126
	(0.196)	(0.354)	(0.213)	(0.208)	(0.555)	(0.294)
Mother some college	−0.103	−0.259	0.079	0.033	0.117	−0.071
	(0.169)	(0.318)	(0.219)	(0.240)	(0.539)	(0.323)
Mother BA	−0.020	−0.124	−0.255	0.137	0.168	0.155
	(0.189)	(0.359)	(0.251)	(0.234)	(0.528)	(0.351)
Mother graduate school	−0.206	−0.433*	−0.355	−0.155	0.177	0.051
	(0.196)	(0.316)	(0.264)	(0.260)	(0.519)	(0.362)
Asian	0.060	0.332**	−0.113	0.249	−0.035	0.033
	(0.107)	(0.129)	(0.124)	(0.218)	(0.074)	(0.308)

Table B.2. Continued.

Dependent Variable	Full Sample	Non–Top 50 Public	Top 50 Public	Private Less Selective	Private Highly Selective	Two-Year College
Hispanic	0.267*	0.526*	0.067	0.435**	0.150	−0.232
	(0.138)	(0.300)	(0.156)	(0.173)	(0.170)	(0.185)
Black	0.141	0.253*	0.318	0.262	−0.167	0.024
	(0.123)	(0.151)	(0.233)	(0.227)	(0.149)	(0.400)
Male	0.169**	0.211**	0.262**	0.254**	0.075	−0.029
	(0.050)	(0.084)	(0.094)	(0.095)	(0.083)	(0.132)
Top 50 public	−0.149**					
	(0.071)					
Less selective private	−0.475**					
	(0.064)					
Highly selective private	−0.533**					
	(0.082)					
Two-year college	0.429**					
	(0.096)					
Constant	5.695**	3.693**	5.889**	5.221**	6.318**	6.859**
	(0.461)	(0.759)	(1.200)	(0.734)	(0.870)	(0.744)

Notes: Fourth follow-up weights were used for the calculations. Only those participating in this follow-up are included in the regression. All samples include only those who begin college within two years of cohort high school graduation and obtain a BA within eight years of cohort high school graduation. Cohort high school graduation is defined as June 1992 for the NELS:88 sample. All school types refer to the initial institution of the respondent. Standard errors clustered at the primary sampling unit (HS) level are in parentheses.
*significant at 10%; **significant at 5%
Source: Authors' calculations as described in the text from the NELS:88 survey.