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How Many Credits Should an Undergraduate Take?

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Abstract Low completion rates and increased time to degree at U.S. colleges are a widespread concern for policymakers and academic leaders. Many 'full time' undergraduates currently enroll at 12 credits per semester despite the fact that a bachelor's degree cannot be completed within 4 years at that credit-load. The *academic momentum* perspective holds that if, at the beginning of their first year in college, undergraduates attempted more course credits per semester, then overall graduation rates could rise. Using nationally-representative data and propensity-score matching methods to reduce selection bias, we find that academically and socially similar students who initially attempt 15 rather than 12 credits do graduate at significantly higher rates within 6 years of initial enrollment. We also find that students who *increase* their credit load from below fifteen to fifteen or more credits in their second semester are more likely to complete a degree within 6 years than similar students who stay below this threshold. Our evidence suggests that stressing a norm that full time enrollment should be 15 credits per semester would improve graduation rates for most kinds of students. However, an important caveat is that those undergraduates whose paid work exceeds 30 h per week do not appear to benefit from taking a higher course load.

Keywords Academic momentum · Credit load · College completion · Propensity score matching

Introduction

In the United States, enrollments in higher education have expanded for decades. The share of high school graduates continuing immediately into higher education increased from 51 % in 1975 to nearly 70 % in 2010 (Aud et al. 2012). Undergraduate enrollments in

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higher education grew by 45 % from 1997 to 2011 and are projected to grow by another 13 % by 2022 (Hussar and Bailey 2013). However, rates of timely college completion have stagnated since the mid-1990s, and currently only 43 % of bachelor's degree recipients complete their baccalaureate degrees within 4 years (Cataldi et al. 2011; Kelly and Schneider 2012; McCormick and Horn 1996). This has led to calls for reform to boost these rates from scholars as well as from major institutional players such as the College Board (2008) and the Department of Education (2006).

In their search for ways to promote completion, scholars and policy makers have examined the impact of need-based aid (Dynarski 2003), student loans (Dowd and Coury 2006), remedial and developmental courses (Bettinger and Long 2009), and student learning communities (Bloom and Sommo 2005). The academic momentum perspective, pioneered by Clifford Adelman, emphasizes instead the importance of timely accumulation of credits, especially during the first year of college. Adelman, in a series of analyses (1999, 2004, 2006), found that students who earned 20 or fewer credits in their first year of college were much less likely to graduate than those who did not, after controlling for student test scores, other measures of academic preparation, and socio-demographic covariates. Testing the momentum perspective in a study of community college students, Doyle (2011) reported that the number of credits earned in the first year is linearly related to the probability of transfer to a 4-year school.

What policy levers exist to incentivize undergraduates to accumulate credits more quickly, and what evidence do we have that such a shift might improve matters? One panel of experts advising the College Board suggested restricting the receipt of a full Pell grant to undergraduates who enroll at 15 credits per semester, instead of the current 12 credits (Baum et al. 2011). The policy group *Complete College America* advocates financial incentives for students to take fifteen credits per semester, and reports that states such as Massachusetts and Indiana have begun to do so (Complete College America 2013). In a consistent development, West Virginia has placed provisions in its merit aid program which cause a student to lose their aid if they earn fewer than 30 credits in a year. An evaluation found that this program improved 4-year completion rates by between 5.8 and 10 percentage points, and five-year completion rates by between 3.1 and 4.8 percentage points (Scott-Clayton 2011).

The notion that encouraging students to take more credits will by itself improve overall completion rates rests implicitly on the largely untested assumption that credit load, particularly in early semesters, has an *independent causal effect* on ultimate academic success. But since, as we demonstrate, more academically prepared and more advantaged students are overrepresented among students taking heavier course loads, naïve estimates of this effect are heavily biased and misleading. It is thus essential to minimize selection effects to the greatest degree possible in order to generate useful evidence as to whether such an independent relationship in fact exists.

In this study we make use of propensity-score matching methods to reduce selection bias, and consider whether 'full-time students' would benefit by increasing their credit-loads from a current 'full time' of 12 to a new norm of 15 credits. Additionally, we pose this question for different groups of students, especially those who are less likely to complete: students with weaker academic preparation, Black and Latino students, first-generation college-goers, and those with substantial work obligations. Finally, we examine whether students who increase course-taking from less than fifteen credits in their first semester to fifteen or more in their second thereby improve their graduation chances.

We find that, after adjusting for differences in observable background characteristics, attempting more credits in the first semester does appear to improve the odds of graduating within 6 years of initial enrollment. Taking a 15-course credit load rather than a 12-credit load seems to have positive impacts on students at both two-year and 4-year colleges. Our subgroup analyses lead us to conclude that taking 15 credits rather than 12 in the first semester would be particularly beneficial for Black and Hispanic students, first generation college goers, and students with lower levels of academic preparation. However, one important caveat is that we find no evidence that undergraduates who are employed for thirty or more hours per week would benefit from taking a higher credit load.

Theory and Prior Research

Academic Momentum

The term *academic momentum* was first coined by Clifford Adelman in his influential report *Answers in the Toolbox* (Adelman 1999). In Adelman's work, academic momentum was an empirical concept and was theoretically underdeveloped; it has remained so in most subsequent research (Doyle 2011; Goldrick-Rab 2007). Additionally, in Adelman's research the term had diffuse application; at different points, college performance (GPA) was said to be a *result* of academic momentum as well as its manifestation. An effort to further develop and refine academic momentum conceptually was made by Attewell et al. (2012). Investigations of momentum, they argue, ought to be restricted to the direct behavioral causes of rapid credit accumulation – such as immediate enrollment after high school or first semester courseload—and should not confuse momentum with its potential consequences, such as student performance. In this paper, we follow this more specific formulation. That is, for us, *academic momentum* refers to the speed of progress towards a degree resulting from the rate of credit accumulation. Thus academic momentum can increase by taking more credits per semester, but also through enrolling in summer sessions and in bridge programs prior to beginning school.

Attewell et al. (2012) posit three theoretical reasons for why academic momentum could have a causal impact on college completion. First, they argue, intense enrollment brings a student into more regular contact with professors and fellow students, augmenting their integration into the social and academic life of their college (Tinto 1975). This is likely to be particularly consequential for non-residential students, whose contact with college as an institution takes place nearly exclusively through coursework and class time. Second, rapidly accumulating credits could improve one's sense of efficacy and of academic self-concept (Bong and Skaalvik 2003), reinforcing commitment to degree completion. Moreover, rapid credit accumulation could make the degree seem to be increasingly within reach, buttressing a student's optimism in their ability to complete their goal. Third, a heavier course load could effectively "crowd out" other attachments, such as those at work and in peer groups, which would otherwise garner more attention and discourage the devotion of effort towards schooling. Here the momemtum perspective coincides with Astin's (1984) theory of student involvement, which emphasizes channeling students' time toward educational pursuits as a means of augmenting the depth of their academic focus and thus their commitment to completion. Martin, et al. (2013) have built on this work, noting the affinity of the academic momentum perspective with generative theories of learning which emphasize scaffolding and cumulative nature of the learning process.

We add that many students' ability to continue attending college is contingent upon the persistence of a fairly fragile set of social and economic arrangements; this is particularly true of students from disadvantaged backgrounds. Unforeseen adverse events, such as a layoff, a pregnancy, or a family illness are capable of derailing them from their collegiate trajectory, at least temporarily. And the cumulative probability of such an adverse event occurring during their schooling increases the longer this schooling takes to complete. Faster progress through school minimizes one's exposure to the risk of such a disruptive event, improving the odds of completion.

But perhaps the most important reason for why specifically taking fifteen rather than twelve credits per semester could increase completion rates is that this is what is required, mathematically, for on-time completion. Most bachelor's degree programs require a student to complete 120 credits; a student completing twelve credits per semester will have compiled only 96 credits at the end of 4 years and will require an extra year of full time attendance to graduate. Similarly, most associate degrees can be obtained with no fewer than 60 credits. The twelve-credit full time norm thus puts students on a 5 year path to the bachelor's degree or a five-semester route to an associate degree. And if a student fails or withdraws from a course at any point, this further prolongs their time in school beyond an already extended baseline.

Given this, why would students register for any *fewer* than fifteen credits? Some students, of course, sign up for fewer courses because of real time constraints, such as those owing to full-time employment or childcare responsibilities. Others, particularly those who experienced academic failure in the past, may fear the intensity of a full course load and opt for a smaller, more "manageable" number of courses. But for many students, it may be that a twelve credit load is chosen by default. It has, after all, become the norm on many if not most college campuses; a fifteen credit load is seen as "heavy". Many students, and especially entering freshman, may register for twelve credits because this is what they are told is done, and because this is what they see most other students doing; importantly, it may be that no credible authority steps in and suggests that this course of action will leave the student unable to reach their goal on time.

While most studies have investigated the impacts of credit *accumulation* (Adelman 1999, 2006; Doyle 2011), a few have directly probed the effects of *enrollment intensity* early in one's college career. Examining an entering cohort of students at a regional university in Texas, Szafran (2001) finds that students taking heavier course-loads tended to earn higher GPAs and were more likely to remain enrolled after 1 year, net of demographic factors and academic preparation. Attewell et al. (2012), using data from the National Educational Longitudinal Survey (NELS:88/2000), find that attending part time in the first semester is related to a reduced probability of degree completion, but that taking a very high load (eighteen or more credits) does not improve the odds of completion relative to taking twelve credits.[1]

Confounding Factors

There is a vast literature, both theoretical and empirical, on the determinants of college completion (e.g. Attewell et al. 2011; Bailey and Dynarski 2011; Tinto 1975; Turner, 2004). But in order to assess the independent relationship between early course-taking and later outcomes, it is essential to account, to the greatest degree possible, for the influence of

[1] The authors note, however, that they find no evidence that taking a high credit load *lowers* the odds of completion either—the finding is null. We suspect that the commonsense suspicion that past a certain point taking additional credits is likely to no longer be beneficial and possibly even injurious has merit. The question is not whether this point exists but where it is. Our hypothesis is that for most students, fifteen credits per semester does not push them beyond this saturation point.

the factors which are specifically likely to impact *both* one's choice of initial credit-load and one's capacity for college success. We consider these factors under three broad headings: the resources available to students; their pre-college academic achievement and curricular intensity; and attributes of the institutions in which they enroll.

The most obvious resource relevant for student success is money. Students from wealthier families are well-known to complete college at higher rates (Bailey and Dynarski 2011; Bowen et al. 2009; Conley 2001), as they able to attend expensive colleges with more generous student supports, are less likely to be distracted by financial stressors and crises, and are less likely to have to support themselves through work during college. But of potentially greater importance for college success is the availability to the student of individuals who possess what Bourdieu (1986; 1977) refers to as cultural capital, which encompasses, among other things, knowledge and understanding of how the academic institutions operate and thus of how best to negotiate them (Dumais and Ward 2010; Lareu and Weinenger 2003). The children of the college educated can, for instance, more profitably consult with their parents for advice regarding course-taking, writing papers, and procuring assistance on campus. A further resource of great importance is student time—in that other factors, such as being responsible for the care of a child, needing to work substantially, or living off-campus and having to commute, can lead students to be less able to devote time to academic effort. Such factors are particularly important to take into account in the case of non-traditional students (Bean and Metzner 1985; Taniguchi and Kaufman 2005).

One of the most useful predictors of how well one does in college is one's academic performance prior to college (Jackson and Kurlaender 2014). But of perhaps equal importance is the intensity of the curriculum to which one was exposed while in high school (Gamoran and Hannigan 2000; Long et al. 2012). Adelman (1999, 2006), for instance, identified the highest math course to which one is exposed and the number of courses taken in subjects such as science, foreign language, and English as indicators of the likelihood that a student is adequately prepared for college. However, the exposure to a rigorous curriculum is graded by class, race, and gender (Gamoran 1987; Gamoran and Mare 1989; Lucas 2001; Reigel-Crumb 2006); poorer students both attend schools offering less intense curricula and are less likely to take an intense curriculum within a given high school (Attewell and Domina 2008). Prior performance and curricular intensity are likely to build confidence in one's academic abilities (Zimmerman et al. 1992), leading students both to take a more intensive and challenging courseload in their initial years in college and to be more likely to complete college.

Finally, there are attributes of institutions which may impact students' initial enrollment intensity as well as their capacity to complete. At larger institutions, competition for space in classes may lead to some students being unable to enroll in courses that they need and taking a slightly lower credit load as a result. Some research suggests that institutional crowding in the public sector due to scarce resources has reduced completion and increased time-to-degree in recent decades (Bound et al. 2010; 2012). There is also evidence that institutional selectivity has an independent impact on completion rates (Bowen and Bok 1998; Brand and Halaby 2006; Cohedes and Goodman 2012), though whether this is owing to differences in institutional resources, peer cultures and "quality", faculty quality, or a combination of these factors is unclear. We suspect that institutions have differing practices regarding initial enrollment behavior, and that less selective institutions on the whole may do less to encourage their entering freshmen to take a challenging fifteen course load. This may be because they have less confidence in the ability of their students to thrive at this level of enrollment, because they take more of a hands-off approach to advisement, or because they have settled into an institutonal pattern of regarding twelve credits as full-time and fifteen as heavy.

Data and Methods

Our data come from the most recent wave of the Beginning Postsecondary Students Longitudinal Study (BPS 04/09), which followed a nationally-representative sample of freshmen entering post-secondary education in the 2003–2004 academic year (National Center for Education Statistics 2011). Subjects were initially interviewed in the spring of 2004, and follow-up interviews occurred in 2006 and 2009. Given this time horizon, the data permit us to investigate degree completion within 6 years of initial enrollment. Transcripts were collected from all institutions attended; we matched transcripts to interview data, permitting the examination of course-taking on a semester-by-semester basis (National Center for Education Statistics 2012). Measures of student outcomes, such as degree attainment, are taken from transcripts rather than student self-report.

We restrict our analysis to degree-seeking undergraduates in community colleges or in public or private nonprofit 4-year institutions, producing a sample of 8230 undergraduates for descriptive statistics and 6730 for main analyses.[2] As data are derived from a complex probability sample, we employed NCES-provided bootstrap replicate weights.

Addressing Selection Bias Through Propensity-Score Matching

It is easy to show that, on average, students who attempt more credits at college entry are more likely to graduate. However, dramatically different types of students tend to take different credit-loads; as documented in Table 1, undergraduates who take more credits in their first semester are younger, whiter, more affluent and more likely to have college educated parents. At both community colleges and 4-year institutions, those who take more credits are also more academically accomplished, having higher mean SAT scores and higher high school GPAs. They are also less likely to have dependents or to work full-time.

Thus, our principal methodological problem is self-selection into different levels of credit-taking. Many of the background factors that lead a person to enroll at a higher course load are associated with higher odds of degree completion in their own right, so we need to remove these confounding influences if we are to estimate the independent effect of varying credit-loads. Researchers have devised a number of methods for addressing selection bias, such as instrumental variables, regression discontinuity designs, and differences-in-differences. We employ propensity-score matching (PSM), developed in 1983 by Rubin and Rosenbaum (1983a, 1973).

PSM operates in two stages. First, one leverages a large number of potentially confounding variables in order to estimate, through probit or logistic regression, the probability of exposure to treatment. In our context, 'treatment' refers to taking twelve credits rather than fifteen (considered to be the baseline or 'control' condition; we discuss our reasons for this methodological decision below). This procedure collapses the information contained in a large set of covariates or background variables into a single-number summary known as a "propensity score" reflecting the individual's likelihood of being "treated", given observable characteristics. Next, this propensity score is used to match

[2] Sample sizes are rounded to the nearest 10 in accordance with NCES data restrictions.

Table 1 Descriptive statistics (means) for community college students, by 1st semester credits attempted (N = 2570)

	Credits attempted in 1st semester			
	6	9	12	15
Age	24.10	21.71	20.12	19.66
Black	12.24 %	12.75 %	10.81 %	8.51 %
Latino	18.83 %	16.40 %	13.94 %	12.98 %
Asian	4.81 %	7.58 %	2.03 %	4.06 %
White	59.46 %	58.17 %	68.85 %	70.57 %
Female	61.61 %	58.89 %	58.12 %	53.13 %
Household income	$54,326	$52,666	$56,429	$60,566
Assets > $10 k	18.83 %	19.03 %	18.36 %	25.38 %
Own home	65.82 %	69.88 %	78.97 %	80.12 %
Parental ed: < HS	14.65 %	10.51 %	7.43 %	3.25 %
Parental ed: HS	38.70 %	34.96 %	34.21 %	30.40 %
Parental ed: some college	19.34 %	28.46 %	28.88 %	28.56 %
Parental ed: BA+	27.31 %	26.06 %	29.47 %	37.78 %
Took SAT/ACT	39.47 %	58.36 %	69.01 %	77.87 %
SAT math	422.60	450.23 %	454.36	473.40
SAT verbal	433.29	453.02 %	458.81	468.32
HS math: <Algebra 2	58.27 %	36.07 %	29.43 %	20.03 %
HS math: Pre-calculus or calculus	9.17 %	19.13 %	20.02 %	30.29 %
HS GPA $\geq$ 3.0	24.13 %	39.96	49.51 %	53.08 %
Years foreign language in HS	1.33	1.56	1.91	2.01
Years math in HS	1.51	2.34	2.64	3.01
Independent	42.31 %	26.57 %	14.37 %	12.42 %
Has dependents	28.86 %	15.98 %	9.83 %	6.40 %
Worked freshman year	83.02 %	80.18 %	79.84 %	81.67 %
Worked 20+ hours	52.43 %	42.28 %	32.98 %	27.81 %
Degree expectation: Higher than BA	46.41 %	44.87 %	49.05 %	50.77 %
Degree goal < BA	29.32 %	28.17 %	23.72 %	19.59 %

Source NCES (2011, 2012)

cases which are empirically observed to have taken the treatment with a control population which is as similar as possible to the treated population in terms of the propensity to be treated, but consisting of individuals who did not in fact take the treatment. In practice, this amounts to assigning weights to untreated cases which lead them as a whole to resemble the treatment group. This is referred to as “matching on the propensity score”, and it has the convenient statistical quality of tending to generate groups which are very similar not only in terms of the propensity score itself, but also in terms of the covariates which were used to derive the propensity score.

PSM enables researchers to estimate the effect of a “treatment” by comparing treated and control groups which are balanced in terms of measured characteristics, in essence approximating the conditions of a randomized trial (Rosenbaum 2002). If and when treated and control groups are sufficiently balanced on all relevant characteristics, treatment assignment can be said to be ignorable and PSM is said to produce unbiased estimates of the causal effect of treatment (Austin 2011; Guo and Fraser 2010; Morgan and Winship 2007; Stuart 2010). In this case, the Conditional Independence Assumption (CIA), which states that the matched groups would not differ on the outcome in the absence of treatment, would be valid. But the CIA is a strong assumption, for it presumes that balance is attained not only on observed but also on relevant *un*observed characteristics, and this is in no way guaranteed through the application of PSM. Indeed, bias due to selection on unobserved traits is the principal threat to the validity of PSM estimates.[3] We employ sensitivity tests, discussed below, to consider the degree to which our findings are likely to be impacted by such unobserved factors.

We implement PSM using the user-generated program ‘psmatch2’ in Stata (Leuven and Sianesi 2003). We impose “common support” on the treated group by dropping treated cases with a propensity score higher than the highest propensity score among the control group. Additionally, we “trim” from the sample the 5 % of treated cases with the least dense common support among the control group, a strategy suggested by Guo and Fraser (2010) and Heckman et al. (1997) to improve the efficiency of estimates by removing cases for whom potential matches likely to be few in number.

Several PSM algorithms have been developed. We rely here on kernel matching (Heckman et al. 1997, 1998), which assigns a weight to each control case that reflects a summation of the average distances of that case to all treated cases, with ‘distance’ expressed in terms of a kernel function; here we employ the Epanechnikov kernel. When using kernel matching, one must specify a bandwidth parameter. Higher values of the bandwidth result in a smoother estimated density function, and therefore more precise estimates of treatment effects, but also potentially in more biased estimates. We set the bandwidth parameter to 0.06, which is the default setting in ‘psmatch2’ and represents a reasonable trade-off between decreased variance and heightened risk of bias. This bandwidth is within the range in which kernel matching has been shown to outperform one-nearest neighbor matching (Frölich 2005).[4] And because ‘standard’ standard errors for the treatment effect are inappropriate in the PSM context given that the propensity score is itself estimated (Caliendo and Kopeinig 2008), we produce standard errors through bootstrapping (500 replications each).

Estimands and Subgroup Analyses

The causal effects literature distinguishes between the overall treatment effect (average treatment effect or ‘ATE’), the effect of treatment on the type of individuals who tend to be exposed to it (average effect of the treatment on the treated, or ‘ATT’), and the effect of treatment on those who are typically *not* exposed to it (the average effect of the treatment on the *un*treated, or ‘ATU’). Though the behavior we are interested in investigating is taking fifteen credits rather than twelve, we are more interested in the effects of signing up for a higher credit load on those who do *not* currently do so. There are two ways of investigating this using PSM. We could designate taking fifteen credits as the treatment and

[3] Selection on unobserved characteristics does not threaten the validity of estimates from an IV estimator. But IV requires that a valid and effective instrument (one with substantial effect on assignment to treatment) be identified, and we did not to identify such an instrument.

[4] We tested the robustness of our findings to changes in bandwidth; estimates were quite stable when bandwidths were between 0.01 and 0.15. Results at different bandwidths are presented in the Appendix in Tables 11, 12, and 13.

investigate the effect of the treatment on the untreated (the ATU), or conversely we could choose taking *twelve* credits as our treatment and estimate an ATT. These approaches are mathematically identical; both involve reweighting those who initially take fifteen credits in such a manner that they collectively resemble, on observable characteristics, those who take twelve.

We opt for the latter strategy. That is, we designate taking twelve credits as the treatment status and calculate average treatment effects on the treated, because doing so is compatible with a straightforward description and more intuitive understanding of propensity score matching. If taking fifteen credits rather than twelve boosts outcomes, our choice of treatment group and estimand will result in *negative* estimated treatment effects. These can be interpreted as what students who take twelve credits *give up* by *not* taking fifteen credits, relative to demographically and academically similar students who *did* take this higher credit load.

In our subgroup analyses we estimate propensity-score matched differences between treated and control groups for sample subsets defined by some identifiable characteristic. This approach, which consists of 'blocking' on given covariates, effectively combines propensity score matching with exact matching on the covariate being used to block. As a result, results may be more accurate than in the full model. This approach has been employed by Lechner (2002) and Heckman et al. (1997, 1998).

Assessing the Quality of Matching

As stated above, in PSM we estimate, using a set of covariates, the propensity of each case to take up the treatment—which in this case is taking twelve rather than fifteen credits. We depict, in Fig. 1, the distribution of this propensity among students at both community colleges and 4-year schools prior to matching. The solid-outline bars represent the distribution of the propensity to take twelve credits among those who actually took this credit load, and the bars with dashed outlines represent this distribution among students who in fact took fifteen credits. For both groups the distributions are left-skewed, reflecting the dominance of the twelve-credit norm, but the mean for the treatment group is clearly higher than that for the untreated. This graph also demonstrates—and this is crucial for PSM to produce valid estimates—that there is substantial overlap in these two populations'

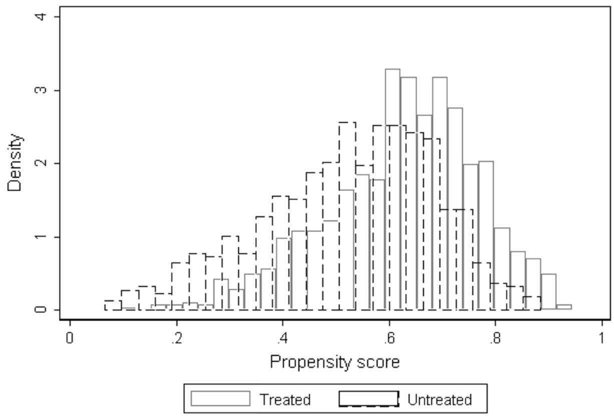


Fig. 1 Distribution of propensity score: all students

Table 2 Descriptive statistics (means) for 4-year college students, by 1st semester credits attempted (N = 5660)

	Credits attempted in 1st semester			
	6	9	12	15
Age	22.73	19.47	18.92	18.64
Black	15.37 %	10.09 %	11.39 %	8.04 %
Latino	16.17 %	17.78 %	11.22 %	7.52 %
Asian	12.62 %	8.53 %	5.92 %	4.14 %
White	52.68 %	54.99 %	64.89 %	77.15 %
Female	65.19 %	57.98 %	56.96 %	56.27 %
Household income	$46,763	$66,787	$72,503	$76,887
Assets > $10 k	18.63 %	24.47 %	29.05 %	31.86 %
Own home	65.42 %	79.69 %	84.12 %	86.33 %
Parental ed: <HS	15.12 %	7.59 %	4.58 %	2.32 %
Parental ed: HS	27.09 %	24.18 %	20.26 %	19.69 %
Parental ed: some college	25.98 %	16.65 %	17.06 %	19.11 %
Parental Ed: BA+	31.81 %	51.56 %	58.09 %	58.88 %
Took SAT/ACT	67.80 %	91.23 %	93.23 %	97.34 %
Mean SAT math	471.27	522.00	524.97	535.00
Mean SAT verbal	475.42	510.15	526.20	533.88
HS Math: <Algebra 2	34.38 %	12.87 %	8.63 %	5.40 %
HS Math: took pre-calculus	31.07 %	51.16 %	52.62 %	55.95 %
HS Math: calculus	14.06 %	25.06 %	26.06 %	26.68 %
HS GPA > 3.0	53.97 %	74.45 %	76.24 %	82.12 %
Years foreign language in HS	1.72	2.46	2.56	2.65
Years math in HS	2.44	3.34	3.50	3.66
Independent	30.47 %	9.86 %	6.95 %	2.46 %
Has dependents	20.01 %	5.37 %	3.71 %	1.29 %
Worked freshman year	75.98 %	60.80 %	60.78 %	58.54 %
Worked 20+ hours	37.53 %	16.52 %	14.44 %	10.36 %
Degree expectation: higher than BA	60.86 %	73.37 %	75.12 %	75.25 %

Source NCES (2011, 2012)

propensity score distributions. Propensity score matching effectively reweights the control group so that its distribution of the propensity score resembles that of the treatment group. Comparable graphs for community college-goers and 4-year students are included in the Appendix (Figs. 4, 5 respectively).

In assessing the success of matching, we inspected not only the propensity score but also covariates used to generate this score. We relied on two measures of covariate balance: the standardized bias and a two-sample *t* test of covariate means, both suggested by Rosenbaum and Rubin (1985). A partial list of the covariates appears in Tables 1 and 2; the full set appears in Appendix Tables 8, 9, 10. In the interest of brevity, we include full propensity score matching tables only for main analyses (Table 3) of the full sample, community college students and 4-year students in the Appendix (in Tables 8, 9, 10). For subgroup analyses we present, instead, the largest standardized bias for any variable in the

Table 3 Propensity-score matching estimates of the effect of taking twelve rather than fifteen credits

	All students (N = 6730)			Community college students (N = 1670)			Four-year college students (N = 5070)		
	Mean(T)	Mean(C)	Treatment effect	Mean(T)	Mean(C)	Treatment effect	Mean(T)	Mean(C)	Treatment effect
Retained after 1 year	0.8751	0.9116	−0.0032 (0.0101)	0.7923	0.8095	0.0181 (0.0249)	0.9166	0.9343	−0.0032 (0.0079)
Earned bachelor's degree	0.4821	0.6289	−0.0555 (0.0118)***	0.1593	0.2770	−0.0499 (0.0210)*	0.6438	0.7070	−0.0321 (0.0142)*
Earned degree or still enrolled	0.6642	0.7389	−0.0283 (0.0113)*	0.5272	0.6060	−0.0302 (0.0318)	0.7756	0.8075	−0.0089 (0.0123)
Earned associate or bachelor's	0.5692	0.6918	−0.0599 (0.0124)***	0.3566	0.5108	−0.0910 (0.0293)**			−0.0254 (0.0123)*
Transferred to a 4-year college				0.3967	0.5050	−0.0302 (0.0237)			
Largest bias			2.16			4.43			2.50
Mean bias			0.91			1.75			0.85
p of propensity score			0.452			0.666			0.436
Lowest covariate p			0.438			0.343			0.454

Treatment effects on the treated (ATT) are reported. T = 12 credits in first semester; C = 15 credits in first semester. *Source* NCES (2011, 2012)

* $p < 0.05$; * $p < 0.01$; * $p < 0.001$

analysis, the mean standardized bias across variables, the p value of the t-test for difference between the mean propensity scores of the matched treated and control groups, and the *lowest* p value among all of the t-tests comparing post-matching covariate means. The largest bias and lowest p value indicate the upper limit of 'bad' matching, whereas the mean bias and propensity score p value indicate the overall success of matching. In none of our PSM analyses was the difference in means between the matched treatment and control groups statistically significant at $p < 0.05$ for any observed confounder.

Sensitivity to Unobserved Confounders

As mentioned above, PSM remains vulnerable to bias resulting from unmeasured differences between groups. To meet this methodological challenge, researchers have developed methods to determine the sensitivity of estimated treatment effects to the presence of an unmeasured confounders. In essence, these sensitivity tests permit the researcher to estimate how large the independent impact of an unmeasured confounder would have to be on selection into treatment in order to invalidate the previously estimated treatment effect. If analysis suggests that a confounder with a modest effect on selection into treatment could nullify observed effects, findings are cast into doubt. Conversely, if it appears that only a substantial confounder could account for estimated effects, this increases confidence in one's findings. This is particularly true if the initial estimated propensity score model contained a rich set of covariates, because in order to introduce bias the unobserved confounder must be independent of the entire set of covariates on which balance has been attained. The framework for such analyses was developed by Rosenbaum and Rubin (1987, 1983b); additional methods have been developed by Becker and Caliendo (2007) and Ichino et al. (2008).

In this paper we rely on the sensitivity test developed by lchino, Mealli and Nannicini,[5] implemented with the user-generated 'sensatt' program in Stata (Nannicini 2007). This procedure simulates the size of a treatment effect given the existence of a hypothetical confounder of a given influence. The hypothetical confounder is conceptualized as influencing two quantities simultaneously: assignment to treatment (the *selection effect*) and the probability of a positive outcome (the *outcome effect*). For obvious reasons, if a variable does not impact *both* the probability of taking up the treatment *as well as* the outcome then it would not act as a confounder and the initial estimated treatment effect would be accurate. In this method, as in most sensitivity analyses, it is presumed that the unmeasured confounder is binary.

The size of selection effects is indicated by the value of parameter s, and outcome effects by d. S is defined as the difference in the probability of observing the hypothetical confounder in the treatment group and that of observing it in the control group. If, for instance, the hypothetical confounder was participation in a particular academic course prior to college, and this course had been taken by 48 % of the treatment group but only 30 % of the control group, s would be equal to 0.18. D, meanwhile, is the effect of the confounder on the outcome in the absence of treatment. We will illustrate this again through the example of our hypothetical academic course. If, among control cases, a degree was eventually earned by 75 % of those who took the course but by only 45 % of

[5] We prefer this simulation-based method because it permits the researcher to adjust two parameters simulateously: the effect of the confounder on selection as well as the effect of the confounder on the outcome. Both Rosenbaum bounds and Mantel–Haenszel bounds only permit adjustment of the hypothetical effect of the confounder on selection, leaving its effect on the outcome obscure.

those who did not, then $d = 0.30$. In our application of this method, we similate what treatment effects *would* be in the presence of a hypothetical unobserved confounder with effects of different strength on both selection into treatment and on the outcome.

Results

Descriptive Statistics and Outcomes

We begin by presenting descriptive statistics for incoming freshmen by first semester credit-load, using a larger range of credit loads to better demonstrate patterns. Because part-time enrollment is far more prevalent at community colleges, we present these statistics separately by college type; Table 1 depicts characteristics of community college students. Socio-economically advantaged students (in terms of household income and parental education) are more likely to attempt a higher credit-load in their initial semester. A similar gradient is observed for academic preparation: students with higher test scores, higher GPAs, more years of math and more advanced math in high school – attempt more credits. Finally, older students, students who work more hours, and students who have dependents tend to enroll with fewer credits when they enter college. Thus, even among community college-goers, students with different initial credit-loads differ by socio-economic status, academic preparation, and work/family obligations.

Table 2 reveals the same pattern among 4-year entrants. Higher initial course load is associated with multiple dimensions of socio-economic advantage and with stronger academic preparation (especially in math). Additionally, economically independent students and students working for more than 20 h per week are especially likely to attempt fewer credits.

How consequential for graduation prospects is first semester course load? Figure 2 shows the bivariate relationship separately for community college entrants, entrants to all 4-year colleges, and entrants to non-selective 4-year colleges. Among all groups, students who attempt more credits in their first semester at college are considerably more likely to attain a bachelor's degree.

The proportion of entering community college entrants who earn a BA is low. In Fig. 3 we report data on additional measures of attainment for this group: earning at least 60 credits, transferring to a 4-year college, and obtaining either an associate or a bachelor's degree. For each indicator, initial credit load is monotonically associated with attainment.

Fig. 2 Bachelor's attainment by 1st semester credits attempted

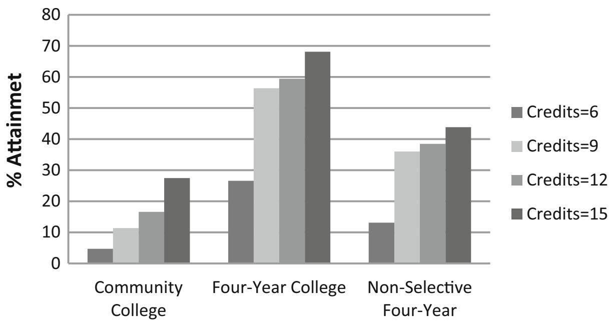
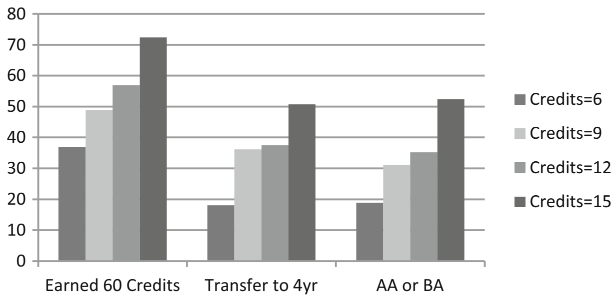


Fig. 3 Attainment among community college beginners



Multivariate Models

The bar charts above suggest strongly that taking a larger credit load improves one's likelihood of graduation, but in order to better isolate the effect of taking different credit loads we must engage in a more rigorous multivariate analysis. We employ propensity-score matching to address selection into different levels of initial enrollment intensity. As discussed above, for in this section taking *twelve* credits, rather than fifteen, will be considered the "treatment", and we estimate the impact of taking this treatment on the sorts of students likely to take it.

Table 3 presents findings from PSM analyses of taking twelve rather than fifteen credit-loads among all entering freshmen, and then repeats the analyses separately for community college and 4-year entrants. We display raw differences as well as PSM estimates to indicate the degree to which PSM adjusts for selection on the observables. Among the full sample, students taking twelve rather than fifteen credits are 5.5 percentage points less likely to earn a bachelor's degree in 6 years; the estimated effect is similar for community college beginners and slightly smaller but still statistically significant among 4-year beginners. Among community college students, taking twelve rather than fifteen credits leads to a 9.1 percentage point lower probability of completing either a bachelor's or an associate degree. However, the larger credit load seems to have little impact on one-year retention or, among community college beginners, on the chances of transfer to a 4-year school.

It is possible that though a higher credit load aids timely completion, students who take fewer credits simply persist beyond 6 years and finish slower. To investigate this, we constructed an additional outcome which is equal to 1 if a student earned a degree *or was still enrolled* at the end of the study, and 0 otherwise. Treatment effects are much smaller for this outcome, and attain statistical significance only for the pooled sample of students. It is therefore possible that some of the advantage of higher credit intensity will ultimately be eroded by lower-intensity students taking longer to complete a degree.

Sensitivity Analysis

Though we included a rich set of covariates in our PSM model, it remains possible that observed effects are due to unmeasured factors and not to first semester credit load in and of itself. "Motivation", cultural capital, cognitive and non-cognitive skills which did not impact high school performance, as well as physical and psychological health are all potentially correlated with both higher enrollment intensity and collegiate success. These factors might exert enough of an influence on student behavior, independent of their relationship with the set of variables leveraged in the PSM analysis, to account for estimated treatment effects. If so, our results would be spurious. The question then becomes, how large would these unobserved independent effects have to be?

Table 4 simulates the treatment effect of taking twelve rather than fifteen credits on completing a bachelor's (for the full sample) in the presence of hypothetical confounders which exert differing degrees of influence. We have italicized effects which are either non-significant (at $p < 0.05$) or negative. In the top left corner, we show a baseline effect—that is, the estimated effect without taking unmeasured confounders into consideration—of −6.9 percentage points. This, the reader might note, is 1.4 percentage points larger than the effect size we estimated in Table 3. This is because the program we use to perform the sensitivity analysis employs a slightly different kernel-matching algorithm than the one used to generate the prior results (the variables included in the PSM model are the same in both analyses). Though the results are different, they are well within each others' 95 % confidence intervals.

Results suggest that a confounder would have to be highly influential in terms of *both* selection into treatment and on the outcome in order to nullify our estimated effect. The unobserved confounder would have to exert both a modest effect on selection of 0.2 and a tremendous 0.5 effect on the outcome, for instance. Or, perhaps more plausibly, it would have to have an effect on both selection and the outcome of 0.3.

To determine the likelihood of unmeasured confounders with effects of this size, we calculated s and d for a number of *measured* variables, and include the results in Table 5. The variables were chosen either because of their demonstrated empirical value in predicting both credit load and college completion or because of their conceptual proximity to the ever-suspected 'unmeasured motivation' confounder. Results clearly demonstrate that none of these variables even approach the influence which an unobserved confounder would have to possess in order to invalidate the estimated effects. Having a high school GPA of 3.5 or higher (rather than lower than 3.5), for instance, has a selection effect (s) equal to −0.12 and an effect on the outcome (d) equal to 0.29. But as Table 4 shows, at $d = 0.3$, the selection effect would have to be substantially larger than −0.12; it would have to be between −0.2 and −0.3. Meanwhile, having a parent with a bachelor's degree has an effect on selection of −0.05, and an effect on the outcome of 0.09. With such a small effect on selection into treatment, the effect on the outcome would have to be higher than 0.60. This is equivalent to, for instance, the children of the college-educated graduating at a rate of 80 % and those whose parents have less education graduating at a rate of only 20 %. Indeed, in general selection effects here are quite muted, which confirms patterns already seen in the descriptive statistics in Tables 1 and 2. That is, twelve- and fifteen-credit takers do differ in terms of important characteristics, but not hugely.

This does not mean, of course, that our estimated effect size is the true causal effect of a higher credit load. Undoubtedly there are unmeasured factors which influence both selection into taking a higher credit-load and the probability of degree completion. Given this consideration, we interpret our estimated effect as something like an upper bound of plausible effect sizes, and believe that the true effect is likely somewhat smaller. At the same time, the sensitivity analysis leads us to conclude that there is most likely *some* independent positive effect of taking fifteen credits rather than twelve in the first semester on eventual completion. We cannot entirely rule out the existence of a lurking, unmeasured, massively influential confounder. But in order to account for the *entirety* of the effect we estimate, this confounder would have to be, first, *independent* of all of the

Table 4 Sensitivity test of the effect of taking 15 (rather than 12) credits on earning a BA, all students

		S = −0.1 (Λ = 1.4–2.1)	s = −0.2 (Λ = 2.3–3.6)	s = −0.3 (Λ = 3.5–5.7)	s = −0.4 (Λ = 5.3–8.7)	s = −0.5 (Λ = 8.8–16.3)
Baseline	−6.9 %					
d = 0.1 (Γ = 1.5–1.8)		−6.0	−5.2	−4.5	−3.9	*−0.9 (NS)*
d = 0.2 (Γ = 2.3–5.2)		−4.8	−3.5	−2.3	−1.2	*−0.2 (NS)*
d = 0.3 (Γ = 3.6–6.4)		−4.6	−2.4	*−0.3 (NS)*	*2.1*	*5.7*
d = 0.4 (Γ = 5.7–9.6)		−4.2	−1.3	*−1.9*	*6.0*	*13.1*
d = 0.5 (Γ = 9.5–26.7)		−3.7	*0.0 (NS)*	*−4.1*	*9.8*	*19.6*

Ichino et al. (2007) methodology employed. Simulated effects are statistically significant at $p < 0.05$ unless indicated. Positive effects are left non-italicized, and non-significant effects (at $p < 0.05$) are labeled (NS)

Table 5 The effect on selection and outcome of a set of influential observed covariates

Measured confounder	Selection effect (*s*)	Outcome effect (*d*)
Independent	0.05	−0.06
Household assets > $10 K	−0.07	0.07
Graduate degree aspiration	−0.02	0.17
Parent has a bachelor's degree	−0.05	0.09
Household income: highest 10 % versus lowest 50 %	−0.02	0.14
Attended private high school	−0.00	0.05
High school GPA > 3.5	−0.12	0.29
Took calculus in high school	−0.03	0.15
College credits in high school	−0.04	0.16
Four years of foreign language in high school	−0.03	0.08

measures of SES, demographic characteristics, high school preparation, college selectivity, and other characteristics for which balance was obtained, and second, *substantially* more influential in terms of both one's likehood of taking fifteen credits *and* one's probability of graduating than having college-educated parents, a very high high school GPA, aspirations to attend graduate school, a household income that puts one in the top decile of all college-goers (relative to those in the bottom half of the income distribution), or taking either calculus or four years of a foreign language in high school. Though there is no way to prove that such a confounder does not exist, we find its existence implausible.

Analyses of Subgroups

We have thus far found positive *average effects* for all students, as well as for students in community colleges and 4-year colleges. It is worth asking, however, whether positive effects are the combination of large positive effects for some subgroups of students (defined, e.g., by racial/ethnic group or measured academic ability) and null or even negative effects for others. In order to maintain a reasonable sample size for subpopulation analyses, we pool our community-college and 4-year college samples. However, we included an indicator variable for collegiate sector in these propensity-score models.

Table 6 considers the effects of initially enrolling in 12 rather than 15 credits on various subgroups. For most groups, taking the smaller credit load appears to be detrimental. Black and Latino students appear to suffer a slightly greater penalty (10 percentage points) than White and Asian students (4.6 percentage points), and students with a low academic preparation in high school appear to be impacted more (8.4 percentage points) than their counterparts with stronger high school preparation (2.5 percentage points). An examination of the standard errors indicates that the differences in treatment *effects* between groups are not statistically significant; nonetheless, that disadvantaged groups seem in general to be more impacted by different credit-loads is suggestive. However, while students who work for less than 20 h per week benefit modestly from a higher credit load, we find no benefit from taking fifteen credits among students with demanding work schedules. This implies that among working students, the probability of graduation is less responsive to changes in first-semester credit-taking.

Table 6 Propensity-score matching estimates for taking 12 rather than 15 credits in the first semester (ATT), subgroup analysis

	White/Asian (N = 5140)	Black/Latino (N = 1230)	Continuing gen college-goers (N = 4460)	1st gen college-goers (N = 1470)	Working < 20 h (N = 4490)	Working 30+ h (N = 1030)	High academic prep (N = 4010)	Low academic prep (N = 2650)
Earned associate or bachelor's	−0.0463 (0.0147)**	−0.1046 (0.0285)***	−0.0410 (0.0144)**	−0.0770 (0.0336)*	−0.0435 (0.0160)**	−0.0122 (0.0367)	−0.0252 (0.0156)	−0.0841 (0.0219)***
Largest bias	2.401	6.731	3.026	6.251	2.775	8.300	2.400	3.853
Mean bias	0.901	2.060	0.769	2.256	0.777	2.034	0.853	1.341
p of propensity score	0.636	0.826	0.639	0.859	0.636	0.833	0.665	0.757
Lowest p	0.449	0.272	0.399	0.285	0.430	0.179	0.517	0.310

Source NCES (2011, 2012), * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Table 7 Propensity-score matched effects of not increasing to fifteen credits in the second semester, among students who took between at least nine but fewer than fifteen in the first

	All students (N = 3810)			Community college students (N = 1900)			Four-year college Students (N = 2710)		
	Mean(T)	Mean(C)	Treatment effect	Mean(T)	Mean(C)	Treatment effect	Mean(T)	Mean(C)	Treatment effect
Final GPA	2.799	2.972	−0.0242 (0.0209)	2.570	2.765	−0.0693 (0.0513)	2.915	3.028	0.0129 (0.0229)
Stopped out	0.2646	0.1816	0.0469 (0.0181)**	0.4055	0.2971	0.0596 (0.0422)	0.1930	0.1503	0.0185 (0.0159)
Earned BA	0.4763	0.6318	−0.0496 (0.0161)**	0.1305	0.2939	−0.0889 (0.0352)*	0.6518	0.7237	−0.0275 (0.0192)
Earned AA or BA	0.5740	0.6953	−0.0346 (0.0175)*	0.3561	0.4984	−0.0845 (0.0465)*	0.6846	0.7489	−0.0208 (0.0187)
Degree or still enrolled	0.6922	0.7868	−0.0367 (0.0150)*	0.5259	0.6325	−0.0699 (0.0469)	0.7767	0.8288	−0.0265 (0.0169)
Transferred to a 4-year college				0.3523	0.5015	−0.0920 (0.0421)*			
Largest bias			4.008			9.483			4.889
Mean bias			1.215			2.727			1.446
p of propensity score			0.560			0.640			0.671
Lowest covariate p			0.187			0.077			0.184

Treated group took less than 15 credits in the second semester. *Source* NCES (2011, 2012)

* $p < 0.05$; * $p < 0.01$; * $p < 0.001$

Increasing Course Load After the First Semester

The previous analyses considered the effects of enrolling at certain credit loads during one's first semester of college. From a policy standpoint however, one would also like to know what happens when students increase their credit load in the *second* semester, compared to their first semester. Studying this matter can shed light on whether students currently taking fewer than 15 credits ought to be encouraged to take more, by investigating what happens to those who *actually do* increase their enrollment intensity. In Table 7 we present the results of this analysis. Here, we include all students who enrolled in at least 9 but fewer than 15 credits in their first semester. The 'treated group' includes those who remained at below fifteen credits in the second semester, and they are compared with those who increased their credit-taking beyond this threshold. We note that in addition to covariates used in previous analyses, we are also able to include in these PSM models a number of measures of early collegiate academic performance: students' first-semester GPA, their ratio of credits completed to attempted, and the number of credits they attempted. Thus we estimate the effects of 'bumping up' to a fifteen-credit course load, *net of first semester performance*.

Overall, students who remain below 15 credits have a BA attainment rate that is roughly 5 percentage points lower than otherwise similar students who increase to 15 credits. The effects are stronger among two-year college students, where remaining below 15 credits is associated with an 8 percentage point lower degree attainment (BA or AA) and a 9 percentage point lower probability of transferring to a 4-year college. Finally, we inquire into the likelihood that the difference in degree attainment associated with increasing credit-load is likely to be temporary by investigating differences in the proportion of students who either have a degree or are still enrolled in college at the end of 6 years. As in the prior analysis, effects are smaller and statistically significant only in the pooled sample, where there is 3.6 percentage point difference between those who increase course taking above the fifteen credit threshold and those who remain below it.

Conclusion

Descriptive statistics indicate that even at college entry undergraduates vary considerably in the number of course credits they attempt, and that economically-disadvantaged and academically less-prepared students are over-represented among those who take fewer courses early on. Together with a strong bivariate relationship between early enrollment intensity and collegiate success, this suggests that initial course load is an important mechanism through which social inequality affects results in educational inequality.

Our multivariate models reveal that, conditional on a large set of observable characteristics, the credit-load attempted in one's first semester is related to one's odds of degree completion. The effects are substantial and are larger at community colleges than at 4-year colleges. 'Full-time' undergraduates at community colleges who enroll for twelve rather than fifteen credits are about 9 percentage points less likely to graduate with a degree and are at a 5 percentage point disadvantage in earning a bachelor's degree within 6 years, net of student background characteristics. In addition, our subgroup analyses indicate that the higher credit load benefits minority students and first generation students at least as well as, and possibly more than, white students and students from more educated families. Finally, our sensitivity analysis demonstrated that the observed treatment effects among all students are unlikely to be entirely attributable to unobserved factors.

And it is important to reiterate here that in throughout this paper we estimated the effects of taking differing credit loads *on the sort of students who presently take twelve*. Thus, this study provides evidence that students currently taking a lower credit load would benefit on average by moving to the higher load.

We thus advocate taking steps to challenge the twelve-credit full time norm where it prevails and to move towards a situation in which most students presume that going to college full time means taking fifteen credits. Much of this work can be done at the level of individual colleges. Advisement ought to proceed on the assumption that the student will take fifteen credits, and ought to challenge students who wish to take fewer classes to articulate why. Where course registration occurs solely online, without students having contact with a counselor, registration websites should clearly state that fifteen credits is what is required to graduate on-time and is what truly constitutes “full-time” attendance. Efforts ought to target first-time freshmen, many of whom have not been given adequate guidance prior to entering college and may be unaware that fifteen credits is what is needed to complete on-time. In addition, schools should specifically identify students who have performed well at a twelve-credit level and encourage them to ‘bump up’ to fifteen credits. Finally, we would encourage schools to charge the same tuition for fifteen as for twelve credits, so that students at the margin of being able to afford college will have no financial reason to forgo the larger load.

Additionally, there are steps that could be taken at the state and federal level to aid in a movement to a fifteen-credit norm. We advocate finding ways to financially *incentivize* taking a higher credit-load. Need-based grants could be made slightly higher for students taking fifteen credits, allowing us to gradually transition to a situation in which a new, higher maximum grant is rewarded only to students taking fifteen credits. The federal government could also consider forgiving a percentage of student loan debt if students complete degrees within 6 years.

However, we do *not* endorse the recommendation of Baum et al. (2011) that the Federal Pell grant program be immediately altered so that maximum grants are restricted to students who attempt fifteen credits. Though our empirical findings do not specifically contradict this proposal, it strikes us as a punitive and unsubtle policy shift, one which entirely foregoes carrots in favor of the stick. Though we did not specifically identify them, it is entirely possible that there are students who would not benefit and indeed might be actively harmed were they effectively penalized for taking only twelve credits. Additionally, some students take a lighter course-load during fall and spring semesters but compensate for this through summer course taking, and there is beginning to be evidence that continuing one’s education through the summer facilitates degree completion (Adelman 2006; Attewell et al. 2012, 2013). Policy ought to encourage such alternative strategies for student success, and at the very least ought not to block them. We believe that the best option involves pairing positive incentives in financial aid with an aggressive attempt at individual campuses to move to a fifteen-credit norm. Such a strategy can accomplish the task of shepherding more students towards timely completion without unduly penalizing disadvantaged students, some of whom may at times have good reason to take a slightly lower credit-load.

Acknowledgments This research was funded through a grant from the Bill and Melinda Gates foundation.

Appendix

See Tables 8, 9, 10, 11, 12, and 13. See Figs. 4 and 5.

Table 8 Balance statistics for propensity score matching analysis in Table 3, all students (cols 2–4)

Variable	12 credits (unmatched)	15 credits (unmatched)	p	12 credits (matched)	15 credits (matched)	p
Propensity score	0.4818	0.3959	<0.001	0.4645	0.4618	0.452
Black	0.1111	0.0780	<0.001	0.1003	0.1046	0.600
Latino	0.1090	0.0807	<0.001	0.1046	0.1001	0.578
Asian	0.0490	0.0387	0.040	0.0505	0.0512	0.900
Other (Ref = white)	0.0586	0.0335	<0.001	0.0537	0.0535	0.974
Female	0.5716	0.5746	0.804	0.5683	0.5649	0.793
Age	19.21	18.71	<0.001	18.95	18.98	0.772
US-born	0.9015	0.9282	<0.001	0.9076	0.9081	
Non-citizen	0.0415	0.0249	<0.001	0.0378	0.0340	0.443
Second-generation	0.1361	0.0943	<0.001	0.1313	0.1321	0.932
Primary language English	0.9026	0.9321	<0.001	0.9065	0.9100	0.647
Single parent	0.0363	0.0107	<0.001	0.0198	0.0211	0.741
Married	0.0264	0.0117	<0.001	0.0198	0.0198	0.991
Dependent child	0.0500	0.0159	<0.001	0.0303	0.0304	0.976
Any dependents	0.0569	0.0180	<0.001	0.0342	0.0343	0.998
Household size	3.9743	4.1171	<0.001	4.0105	4.0135	0.934
Independent	0.0919	0.0364	<0.001	0.0642	0.0617	0.704
Non-married parents	0.2524	0.2241	0.007	0.2547	0.2612	0.581
Number of dependents	0.1046	0.0301	<0.001	0.05449	0.0571	0.766
Father's education = less than high school	0.1306	0.0786	<0.001	0.1151	0.1120	0.719
Father's education = some college	0.1982	0.2164	0.068	0.2028	0.1992	0.743
Father's education = college grad (Ref = HS grad)	0.3947	0.4378	<0.001	0.4059	0.4140	0.542
Mother's education = less than high school	0.0874	0.0476	<0.001	0.0725	0.0718	0.916
Mother's education = some college	0.2489	0.2667	0.099	0.2544	0.2502	0.720
Mother's education = college grad (Ref = HS grad)	0.3751	0.4135	0.001	0.3886	0.3953	0.610
Home ownership	0.8216	0.8626	<0.001	0.8372	0.8403	0.756
Assets > \$10 K	0.2692	0.3021	0.003	0.2768	0.2818	0.675
Household income (log)	10.61	10.85	<0.001	10.71	10.73	0.517
No HS GPA data	0.0898	0.0458	<0.001	0.0707	0.0715	0.901
HS GPA = 0.5–1.9	0.0243	0.0125	<0.001	0.0216	0.0221	0.899
HS GPA = 2.0–2.9	0.2074	0.1601	<0.001	0.2071	0.2025	0.670
HS GPA 3.0 or higher	0.3655	0.4538	<0.001	0.3836	0.3854	0.886
No HS diploma	0.0270	0.0149	<0.001	0.0231	0.0226	0.914
Earned college credits in HS	0.3443	0.3776	0.005	0.3565	0.3577	0.928
Private HS	0.1255	0.1297	0.609	0.1292	0.1320	0.756
Years foreign Language in HS	2.391	2.575	<0.001	2.457	2.451	0.857
Years math in HS	3.259	3.559	<0.001	3.362	3.381	0.520
Years social studies in HS	3.235	3.349	<0.001	3.298	3.289	0.715

Table 8 continued

Variable	12 credits (unmatched)	15 credits (unmatched)	*p*	12 credits (matched)	15 credits (matched)	*p*
Years science in HS	3.092	3.280	<0.001	3.166	3.153	0.616
HS math = algebra 2	0.4159	0.4006	0.206	0.4207	0.4185	0.868
HS math = pre-cal/calc. (Ref = less than algebra 2)	0.4379	0.5217	<0.001	0.4590	0.4662	0.592
Did not take SAT	0.1371	0.0566	<0.001	0.1071	0.1008	0.438
SAT math = middle tercile	0.2582	0.3320	<0.001	0.2692	0.2740	0.688
SAT math = highest tercile	0.2849	0.3367	<0.001	0.2984	0.3012	0.817
SAT verbal = middle tercile	0.2729	0.3317	<0.001	0.2854	0.2928	0.544
SAT verbal = highest tercile	0.2770	0.3349	<0.001	0.2908	0.2904	0.973
Attended multiple colleges	0.0751	0.0754	0.955	0.0786	0.0814	0.704
Lived off-campus	0.1622	0.1022	<0.001	0.1421	0.1426	0.962
Lived with parents (Reference = lived on-campus)	0.3168	0.2230	<0.001	0.3100	0.3057	0.729
Suburban college	0.2582	0.2379	0.056	0.2551	0.2532	0.871
Town/rural college	0.1707	0.2442		0.1761	0.1764	0.971
Urban status missing (Reference = urban college)	0.0236	0.0272	0.356	0.0245	0.0263	0.676
Very selective college	0.2438	0.2361	0.462	0.2565	0.2637	0.542
Moderately selective college (Ref = non-selective)	0.3131	0.4848	<0.001	0.3284	0.3327	0.731
Two-year college	0.3336	0.1816	<0.001	0.3045	0.2963	0.502
Out-of-state college	0.1718	0.1905	0.049	0.1790	0.1840	0.627
International student	0.0133	0.0076	0.019	0.0119	0.0119	0.995
Private college	0.3021	0.3603	<0.001	0.3164	0.3221	0.653
Enrollment (log)	8.936	8.820	<0.001	8.927	8.909	0.527
% Federal grants in college	29.283	28.735	0.162	29.142	29.013	0.770
% Black/Latino in college	18.907	15.525	<0.001	18.219	18.127	0.868
Home-school distance (log)	3.649	3.955	<0.001	3.716	3.728	0.806
Took distance education course	0.0864	0.0694	0.010	0.0840	0.0848	0.924
Working 1–15 h/wk	0.2551	0.3058	<0.001	0.2609	0.2665	0.633
Working 16–30 h/wk	0.1316	0.0906	<0.001	0.1190	0.1205	0.867
Working more than 30 h/wk (Reference = not working)	0.2794	0.2350	<0.001	0.2786	0.2702	0.484
Identity: student who works	0.5888	0.5951	0.602	0.5976	0.5996	0.881
Identity: worker who studies	0.0775	0.0364		0.0609	0.0577	0.608
Degree expectation = AA	0.0452	0.0251		0.0411	0.0402	
Degree expectation = master's	0.4417	0.4520	0.398	0.4435	0.4464	0.827
Degree expectation = doctoral/1st professional	0.2376	0.2549	0.103	0.2417	0.2392	0.823
Took remedial math	0.1797	0.1381	<0.001	0.1707	0.1666	0.684
Took remedial English	0.1498	0.1160	<0.001	0.1421	0.1383	0.682

Table 9 Balance statistics for propensity score matching analysis in Table 3, community college students (cols 5–7)

Variable	12 credits (unmatched)	15 credits (unmatched)	*p*	12 credits (matched)	15 credits (matched)	*p*
Propensity score	0.6273	0.5232		0.6151	0.6125	0.666
Black	0.13361	0.0938	0.013	0.1221	0.1291	0.650
Latino	0.1284	0.0952	0.036	0.1264	0.1140	0.411
Asian	0.0246	0.0288	0.599	0.0259	0.0279	0.790
Other (Ref = white)	0.0513	0.0447	0.534	0.0486	0.0537	0.622
Female	0.5817	0.5425	0.112	0.5686	0.5614	0.754
Age	20.15	19.59	0.028	19.83	19.92	0.715
US-born	0.9116	0.9292	0.193	0.9145	0.9181	0.781
Non-citizen	0.0472	0.0331	0.155	0.0432	0.0381	0.577
Second-generation	0.1274	0.1125	0.359	0.1264	0.1275	0.947
Primary language English	0.9105	0.9292	0.169	0.9124	0.9122	0.991
Single parent	0.0750	0.0259	<0.001	0.0400	0.0480	0.398
Married	0.0513	0.0447	0.534	0.0518	0.0555	0.728
Dependent child	0.1017	0.0490	<0.001	0.0702	0.0790	0.475
Any dependents	0.1151	0.0548	<0.001	0.0800	0.0903	0.424
Household size	3.812	3.950	0.058	3.838	3.794	0.508
Independent	0.1726	0.1082	<0.001	0.1383	0.1524	0.391
Non-married parents	0.2651	0.2496	0.476	0.2735	0.2775	0.848
Number of dependents	0.2189	0.1010	<0.001	0.1383	0.1625	0.343
Father's education = less than high school	0.1870	0.1269	0.001	0.1773	0.1685	0.619
Father's education = some college	0.2261	0.2294	0.873	0.2270	0.2236	0.861
Father's education = college grad (Ref = HS grad)	0.1952	0.2539	0.004	0.2032	0.2050	0.922
Mother's education = less than high school	0.1243	0.0764	0.002	0.1124	0.1033	0.528
Mother's education = some college	0.2898	0.3088	0.404	0.2973	0.2895	0.715
Mother's education = college grad (Ref = HS grad)	0.1973	0.2395	0.039	0.2021	0.2154	0.482
Home ownership	0.7410	0.8008	0.004	0.7686	0.7511	0.378
Assets > \$10 K	0.1757	0.2395	0.001	0.1827	0.1827	0.999
Household income (log)	10.18	10.43	0.010	10.29	10.25	0.608
No HS GPA data	0.1531	0.1111	0.014	0.1318	0.1366	0.766
HS GPA = 0.5–1.9	0.0524	0.0375	0.154	0.0518	0.0493	0.804
HS GPA = 2.0–2.9	0.3484	0.3362	0.606	0.3578	0.3515	0.778
HS GPA 3.0 or higher	0.1459	0.2092	0.001	0.1513	0.1594	0.632
No HS diploma	0.0606	0.0461	0.201	0.0551	0.0603	0.629
Earned college credits in HS	0.1880	0.2611	<0.001	0.1945	0.1876	0.705
Private HS	0.0596	0.0793	0.114	0.0594	0.0653	0.603
Years foreign language in HS	1.845	1.968	0.036	1.901	1.888	0.809
Years math in HS	2.604	2.985	<0.001	2.698	2.682	0.808

Table 9 continued

Variable	12 credits (unmatched)	15 credits (unmatched)	*p*	12 credits (matched)	15 credits (matched)	*p*
Years social studies in HS	3.016	3.116	0.095	3.094	3.046	0.391
Years science in HS	2.732	2.854	0.038	2.795	2.764	0.560
HS math = algebra 2	0.5118	0.5267	0.549	0.5308	0.5333	0.914
HS math = pre-cal/calc. (Ref = less than algebra 2)	0.1880	2669	<0.001	0.1956	0.1941	0.936
Did not take SAT	0.3165	0.2034	<0.001	0.2929	0.2903	0.900
SAT math = middle tercile	0.2178	0.2135	0.833	0.2237	0.2283	0.814
SAT math = highest tercile	0.1963	0.3059	<0.001	0.2043	0.2029	0.940
SAT verbal = middle tercile	0.2476	0.2655	0.411	0.2551	0.2500	0.803
SAT verbal = highest tercile	0.2086	0.3001	<0.001	0.2194	0.2286	
Attended multiple colleges	0.0894	0.1168	0.067	0.0929	0.0995	0.632
Lived off-campus	0.2589	0.2337	0.240	0.2367	0.2429	0.756
Lived with parents (Reference = lived on-campus)	0.6279	0.5584	0.004	0.6464	0.6336	0.566
Suburban college	0.3155	0.2712	0.052	0.3167	0.3043	0.565
Town/rural college	0.1623	0.3030		0.1664	0.1672	0.964
Urban status missing (Reference = urban college)	0.0256	0.0202	0.465	0.0259	0.0282	0.764
Out of state college	0.0318	0.0620	0.003	0.0335	0.0319	0.853
International student	0.0082	0.0101	0.689	0.0086	0.0068	0.667
Enrollment (log)	8.790	8.621		8.783	8.782	0.980
% Federal grants in college	34.926	36.081	0.161	34.761	34.838	0.921
% Black/Latino in college	22.920	18.519	<0.001	22.429	22.058	0.668
Home-school distance (log)	2.495	2.769	<0.001	2.516	2.539	0.704
Took distance education course	0.1325	0.1342	0.924	0.1329	0.1347	0.913
Working 1–15 h/wk	0.1572	0.1976	0.032	0.1589	0.1609	0.907
Working 16–30 h/wk	0.2240	0.1962	0.172	0.2205	0.2231	0.894
Working more than 30 h/wk (Reference = not working)	0.4141	0.3997	0.554	0.4205	0.4084	0.597
Identity: Student who works	0.6392	0.6825	0.067	0.6475	0.6551	0.734
Identity: Worker who studies	0.1562	0.1111	0.008	0.1524	0.1373	0.357
Degree expectation = AA	0.1274	0.1197	0.640	0.1286	0.1202	0.582
Degree expectation = master's	0.3566	0.3520	0.849	0.3578	0.3554	0.916
degree Expectation = doctoral/1st Professional	0.1397	0.1414	0.924	0.1362	0.1402	0.804
Took remedial math	0.3073	0.2640	0.055	0.3016	0.3031	0.941
Took remedial English	0.2199	0.1976	0.273	0.2151	0.2137	0.942

Table 10 Balance statistics for propensity score matching analysis in Table 3, 4-year college students (cols 8–10)

Variable	12 credits (unmatched)	15 credits (unmatched)	*p*	12 credits (matched)	15 credits (matched)	*p*
Propensity score	0.4217	0.3597		0.4057	0.4029	0.436
Black	0.0998	0.0746	0.002	0.0948	0.0932	0.870
Latino	0.0993	0.0774	0.007	0.0937	0.0958	0.824
Asian	0.0612	0.0409	0.001	0.0606	0.0605	0.989
Other (Ref = white)	0.0622	0.0310	<0.001	0.0530	0.0478	0.464
Female	0.5666	0.5818	0.288	0.5698	0.5694	0.979
Age	18.75	18.51	<0.001	18.55	18.58	0.713
US-born	0.8965	0.9279	<0.001	0.9052	0.9062	0.915
Non-citizen	0.0386	0.0230	0.001	0.0325	0.0286	0.499
Second-generation	0.1405	0.0903	<0.001	0.1332	0.1290	0.703
Primary language English	0.8986	0.9327	<0.001	0.9057	0.9085	0.770
Single parent	0.0169	0.0073	0.001	0.0108	0.0112	0.916
Married	0.0139	0.0044	<0.001	0.0059	0.0053	0.820
Dependent child	0.0241	0.0086	<0.001	0.0119	0.0108	0.766
Any dependents	0.0277	0.0099	<0.001	0.0140	0.0129	0.772
Household size	4.055	4.154	0.008	4.097	4.072	0.550
Independent	0.0514	0.0204	<0.001	0.0281	0.0315	0.552
Non-married parents	0.2460	0.2183	0.023	0.2491	0.2478	0.925
Number of dependents	0.0473	0.0144	<0.001	0.0195	0.0170	0.666
Father's education = less than high school	0.1024	0.0678	<0.001	0.0883	0.0859	0.797
Father's education = some college	0.1842	0.2135	0.012	0.1896	0.1905	0.939
Father's education = college grad (Ref = HS grad)	0.4946	0.4787	0.271	0.5037	0.5039	
Mother's education = less than high school	0.0689	0.0413	<0.001	0.0574	0.0563	0.883
Mother's education = some college	0.2285	0.2574	0.020	0.2323	0.2331	0.957
Mother's education = college grad (Ref = HS grad)	0.4642	0.4521	0.401	0.4734	0.4758	0.883
Home ownership	0.8620	0.8764	0.139	0.8727	0.8690	0.740
Assets > $10 K	0.3160	0.3160	0.998	0.3217	0.3194	0.881
Household income (log)	10.832	10.946	0.002	10.900	10.896	0.918
No HS GPA data	0.0581	0.0313	<0.001	0.0395	0.0391	0.949
HS GPA = 0.5–1.9	0.0102	0.0070	0.215	0.0097	0.0095	0.956
HS GPA = 2.0–2.9	0.1369	0.1210	0.099	0.1343	0.1330	0.911
HS GPA 3.0 or higher	0.4755	0.5081	0.024	0.4907	0.4879	0.863
No HS diploma	0.0102	0.0080	0.399	0.0097	0.0100	0.919
Earned college credits in HS	0.4225	0.4034	0.179	0.4322	0.4268	0.737
Private HS	0.1585	0.1408	0.086	0.1587	0.1594	0.949
Years foreign language in HS	2.664	2.709	0.152	2.710	2.700	0.798
Years math in HS	3.588	3.686	<0.001	3.660	3.643	0.485

Table 10 continued

Variable	12 credits (unmatched)	15 credits (unmatched)	*p*	12 credits (matched)	15 credits (matched)	*p*
Years social studies in HS	3.345	3.400	0.023	3.398	3.393	0.866
Years science in HS	3.272	3.375	<0.001	3.331	3.322	0.751
HS math = algebra 2	0.3679	0.3727	0.735	0.3727	0.3672	0.730
HS math = pre-cal/calc.	0.5630	0.5782	0.286	0.5763	0.5765	0.990
Did not take SAT	0.0473	0.0240	<0.001	0.0276	0.0318	0.454
SAT math = middle tercile	0.3324	0.3266	0.666	0.3347	0.3333	0.927
SAT math = highest tercile	0.3093	0.3061	0.810	0.3169	0.3113	0.716
SAT verbal = middle tercile	0.3005	0.2849	0.235	0.3082	0.3040	0.782
SAT verbal = highest tercile	0.0473	0.0240	<0.001	0.0276	0.0318	0.454
Attended multiple colleges	0.0679	0.0662	0.819	0.0671	0.0692	0.807
Lived off-campus	0.1137	0.0730	<0.001	0.0948	0.0958	0.910
Lived with parents (Reference = lived on-campus)	0.1610	0.1485	0.229	0.1592	0.1633	0.735
Suburban college	0.2295	0.2305	0.934	0.2302	0.2328	0.849
Town/rural college	0.1749	0.2311	<0.001	0.1798	0.1789	0.941
Urban status missing (Reference = urban college)	0.0226	0.0288	0.183	0.0216	0.0244	0.579
Highly selective college	0.3638	0.2878	<0.001	0.3689	0.3639	0.755
Moderately selective college (Reference = minimally selective)	0.4642	0.5891	<0.001	0.4794	0.4870	0.642
Out-of-state college	0.2418	0.2190	0.059	0.2459	0.2442	0.902
International student	0.0159	0.0070	0.002	0.0124	0.0106	0.615
Private college	0.4534	0.4402	0.360	0.4555	0.4530	0.878
Enrollment (log)	9.009	8.864	<0.001	9.010	8.994	0.684
% Federal grants in college	26.457	27.104	0.139	26.158	26.193	0.944
% Black/Latino in college	16.898	14.861	0.001	16.268	16.491	0.742
Home-School distance (log)	4.226	4.218	0.866	4.274	4.247	0.639
Took distance education course	0.0633	0.0550	0.224	0.0617	0.0622	0.948
Working 1–15 h/wk	0.3041	0.3298	0.057	0.3104	0.3123	0.901
Working 16–30 h/wk	0.0854	0.0672	0.016	0.0731	0.0758	0.751
Working more than 30 h/wk (Reference = not working)	0.2120	0.1985	0.246	0.2118	0.2083	0.796
Identity: student who works	0.5635	0.5757	0.395	0.5677	0.5710	0.838
Identity: worker who studies	0.0380	0.0198	<0.001	0.0276	0.0254	0.684
Degree expectation = master's	0.4843	0.4742	0.485	0.4870	0.4867	0.986
Degree Expectation = doctoral/1st professional	0.2866	0.2801	0.618	0.2865	0.2869	0.977
Took remedial math	0.1158	0.1101	0.536	0.1137	0.1095	0.686
Took remedial English	0.1147	0.0979	0.057	0.1094	0.1062	0.756

Table 11 Robustness of treatment effects to changes in bandwidth parameter, all students (N = 6730)

Bandwidth setting	Outcome		
	One-year retention	Earned bachelor's	Degree or still enrolled
0.0001	0.0172	0.0430*	0.0217
0.001	0.0093	0.0550***	0.0312*
0.005	0.0016	0.0494***	0.0254
0.01	0.0021	0.0501***	0.0256*
0.02	0.0022	0.0497***	0.0253*
0.03	0.0023	0.0502***	0.0253*
0.04	0.0023	0.0514***	0.0258*
0.05	0.0027	0.0530***	0.0269*
0.06	0.0029	0.0545***	0.0277*
0.07	0.0033	0.0561***	0.0284*
0.08	0.0037	0.0578***	0.0292*
0.09	0.0042	0.0597***	0.0301*
0.10	0.0047	0.0616***	0.0310*
0.15	0.0084	0.0720***	0.0360***
0.2	0.0127	0.0837***	0.0423***
0.5	0.0264***	0.1217***	0.0630***

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Table 12 Robustness of treatment effects to changes in bandwidth parameter, community college students (N = 1670)

Bandwidth setting	Outcome		
	Transferred to 4-year	Earned bachelor's	Earned bachelor's or associate
0.0001	−0.0124	−0.0096	0.0418***
0.001	0.0392	0.0639	0.1069**
0.005	0.0173	0.0416*	0.0956***
0.01	0.0249	0.0473*	0.0965**
0.02	0.0270	0.0503**	0.0935**
0.03	0.0285	0.0500*	0.0914**
0.04	0.0294	0.0499**	0.0909***
0.05	0.0302	0.0499*	0.0918**
0.06	0.0311	0.0514**	0.0925**
0.07	0.0319	0.0519*	0.0931***
0.08	0.0324	0.0523**	0.0933**
0.09	0.0335	0.0532**	0.0941***
0.10	0.0347	0.0543**	0.0951***
0.15	0.0411	0.0612***	0.1009***
0.2	0.0488	0.0698	0.1071
0.5	0.0845*	0.0983***	0.1343***

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Table 13 Robustness of treatment effects to changes in bandwidth parameter, 4-year college students (N = 5070)

Bandwidth setting	Outcome		
	One-year retention	Earned bachelor's	Degree or still enrolled
0.0001	0.0217	0.0597*	0.0400
0.001	0.0096	0.0251	0.0014
0.005	0.0047	0.0296*	0.0025
0.01	0.0048	0.0309*	0.0036
0.02	0.0041	0.0310*	0.0047
0.03	0.0034	0.0295**	0.0046
0.04	0.0032	0.0303*	0.0054
0.05	0.0034	0.0309*	0.0063
0.06	0.0036	0.0316*	0.0070
0.07	0.0039	0.0323*	0.0075
0.08	0.0044	0.0330*	0.0081
0.09	0.0049	0.0337*	0.0087
0.10	0.0053	0.0344**	0.0093
0.15	0.0074	0.0378**	0.0126
0.2	0.0092	0.0413***	0.0157
0.5	0.0131	0.0483***	0.0204

* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

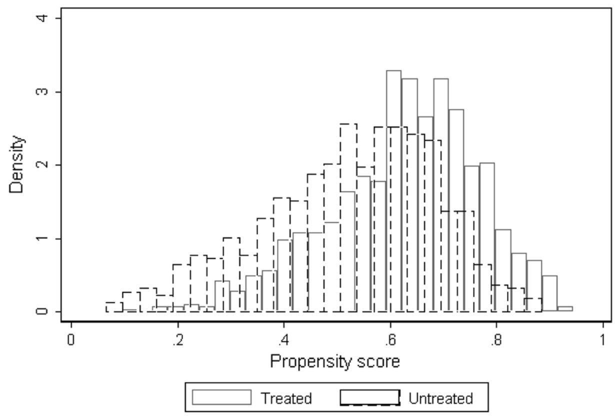


Fig. 4 Distrubution of propentisity score: community college students

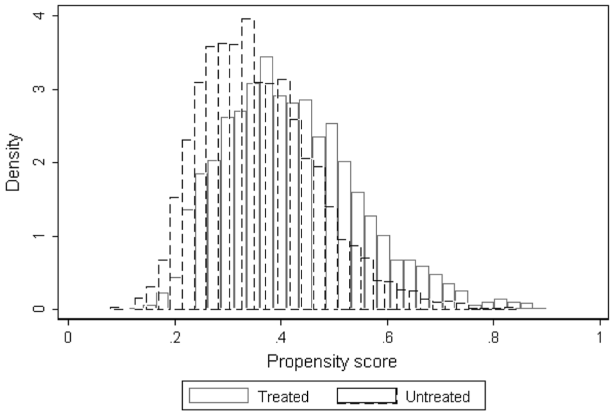


Fig. 5 Distribution of propentisity score: four year students

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