Educational Evaluation and Policy Analysis

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EDUCATIONAL EVALUATION AND POLICY ANALYSIS published online 18 March 2014
DOI: 10.3102/0162373714521865

The online version of this article can be found at:
http://epa.sagepub.com/content/early/2014/02/28/0162373714521865

Published on behalf of

American Educational Research Association

and

http://www.sagepublications.com

Educational Evaluation and Policy Analysis
Month 201X, Vol. XX, No. X, pp. 1–22
DOI: 10.3102/0162373714521865

The Community College Route to the Bachelor's Degree

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It is well established that students who begin post-secondary education at a community college are less likely to earn a bachelor's degree than otherwise similar undergraduates who begin at a 4-year school, but there is less consensus over the mechanisms generating this disparity. We explore these using national longitudinal transcript data and propensity-score methods. Inferior academic preparation does not seem to be the main culprit: We find few differences between students' academic progress at each type of institution during the first 2 years of college and (contrary to some earlier scholarship) students who do transfer have BA graduation rates equal to similar students who begin at 4-year colleges. However, after 2 years, credit accumulation diverges in the two kinds of institutions, due in part to community college students' greater involvement in employment, and a higher likelihood of stopping out of college, after controlling for their academic performance. Contrary to some earlier claims, we find that a vocational emphasis in community college is not a major factor behind the disparity. One important mechanism is the widespread loss of credits that occurs after undergraduates transfer from a community college to a 4-year institution; the greater the loss, the lower the chances of completing a BA. However, earlier claims that community college students receive lower aid levels after transfer and that transfers disproportionately fail to survive through the senior year are not supported by our analyses.

Keywords: *community college, college completion, propensity-score matching, college transfer, remediation*

Public 2-year colleges, or community colleges, enroll about 40% of America's undergraduates and typically offer programs that culminate in vocationally oriented certificates or in associate's degrees (Aud et al., 2012). Nowadays, however, many students entering community colleges say that their ultimate goal is a bachelor's degree or higher: 81% in one authoritative survey (Horn & Skomsvold, 2012). For such students, community college is a stepping stone to a 4-year institution that grants a bachelor's degree.

This trend is partly driven by economic factors. In-state tuition and fees at a community college are considerably cheaper than at a public 4-year college, averaging US$2,963 versus US$8,244 in nationwide data, and by commuting from home to a community college a student also can avoid the average US$8,549 cost of room and board at a residential college (College Board, 2011).

Unfortunately, prior research has found that an undergraduate's chances of completing a BA are much lower if that student begins at a community college. The goal of this article is to understand *why* and *when* this disparity in outcomes occurs, by comparing the academic progress of community college entrants with otherwise comparable 4-year college students, drawing upon a nationally representative sample of undergraduates for whom complete undergraduate transcripts have been collected. This rich source of data enables us to follow individuals from semester to semester, noting the numbers of course credits attempted and earned, seeing whether or not students transfer, and determining how those who transfer to a 4-year college fare in terms of credit accumulation and degree completion. Understanding when and why the disparity in graduation emerges has practical importance when many high school graduates are attracted to the stepping stone route to a BA, and when some state legislatures are seeking to reduce expenditures by emphasizing the cheaper community college route over the more expensive 4-year college path (Kelderman, 2010).

Previous Research

Many researchers have examined whether starting at a 2-year rather than at a 4-year college affects the likelihood of an undergraduate completing a bachelor's degree. The central methodological challenge in answering this question is the problem of selection: Students who begin at a community college differ on average from those who start at a 4-year college, in terms of sociodemographic background, academic preparation, and other dimensions.

Researchers make statistical adjustments to address selection and obtain a more accurate estimate of the effects of the type of college on graduation outcomes. Past studies have done this by limiting the sample to those kinds of students who are frequently found in both 2- and 4-year colleges (i.e., selecting an overlapping sample), by matching the 2- and 4-year student groups on their propensity for attending a 2-year college, by using instrumental variables (IV) as a proxy for college type, estimating Heckman two-stage selection models, or by using Oaxaca decomposition methods to separate the effects of "student quality" from college type.

Rouse (1995), using an IV approach, concluded that "overall, however, community colleges . . . do not appear to change the likelihood of attaining a bachelor's degree" (p. 217). As far as we are aware, this is the only study that has reported no significant effect of initial enrollment at a community college on BA attainment. By contrast, Alfonso (2006) analyzing data from the National Educational Longitudinal Study (NELS88) with an IV approach reported a 30 percentage point lower BA completion rate for community college students. Using the NELS88 data, Reynolds and DesJardins (2009) reported that entrants to community colleges had a 21.6 percentage point lower BA completion rate after 9 years, compared with counterparts who started at a 4-year college, and Reynolds (2012) estimated a 24.5 percentage point BA disadvantage for men and 31.5 points for women associated with starting at a community college. Stephan, Rosenbaum, and Person (2009) found a 23 percentage point community college disadvantage in BA completion, again analyzing NELS88 data.

Long and Kurlaender (2009) used administrative data from Ohio and found that community college entrants had a 14.5 percentage point lower probability of completing a baccalaureate degree than comparable 4-year college entrants. Doyle (2009) used a proportional hazards model to analyze the 1996 cohort of the Beginning Postsecondary Students (BPS) Longitudinal Study and concluded that attending a community college lowers the hazard of completing a BA to 68% of the baseline rate. Brand, Pfeffer, and Goldrick-Rab (2012) using data on school-to-college transitions in Chicago reported a 3 to 4 percentage point lower BA attainment rate for Chicago public school graduates entering a community college rather than a non-selective 4-year college. Sandy, Gonzalez, and Hilmer (2006) also found a significantly lower BA graduation rate associated with starting at a community college for the National Longitudinal Survey of Youth (NLSY72), High School and Beyond (HS&B), and BPS surveys.

Having established a disparity in baccalaureate attainment, researchers seek the mechanisms that give rise to this phenomenon. Recent scholarship on this question, including the analyses we add below, calls into question several of the purported mechanisms advanced by scholars in earlier decades.

Burton Clark (1960) advanced an early explanation for the disparity, which he termed "the cooling out function of community colleges." He argued that many students lowered their educational expectations during their years at community college. Those who began with hopes of completing a BA or higher credential experienced academic setbacks and were influenced by faculty and peers, such that over time many decided that it was more realistic to aspire to a credential short of a BA. This belief that the BA aspirations of community college entrants become undermined over time has been widely accepted by scholars (e.g., Brint & Karabel, 1989; Karabel, 1972; Zwerling, 1976). A variant of the cooling out hypothesis was argued by Dougherty (1994) who wrote that "The community college's concern with vocational education has led it to stint on transfer education, as it has shifted funds and attention to developing vocational programs" (p. 8). He elaborates, "Once in vocational programs, students are not strongly encouraged to consider transfer" (p. 95).

Although Clark's cooling out explanation is widely cited, its empirical validity in the current era has been challenged by Alexander, Bozick, and Entwisle (2008), who analyzed longitudinal data on degree aspirations and found that the opposite phenomenon to "cooling out" predominated. On average, community college students increased their degree expectations as they spend more time there, in a process Alexander termed "heating up."

Similarly, Leigh and Gill (2003) analyzed NLSY data that compared students' degree aspirations toward the end of high school and again in college, contrasting the changes in students' aspirations at community and 4-year colleges relative to their baseline aspiration measured during high school. In both kinds of college, "heating up" predominated over "cooling out." In community colleges, 37.7% of students heated up while 23.1% cooled. In 4-year colleges, 34.6% heated up while 19.9% cooled. (The remainder did not change their degree aspirations.)

If data contradict Clark's theory of cooling out and lowered expectations and if students at community colleges have high hopes for transfer, then what other factors account for the gap in BA attainment? This article examines students' college trajectories in three phases. First, we observe the initial 2- to 4-year period in college to determine what gaps, if any, emerge in students' academic performance and credit accumulation during that initial phase. We are guided here by a theory pioneered by Clifford Adelman (1999, 2006; Attewell, Heil, & Reisel, 2012) who discovered that students' early academic momentum is very consequential for ultimate degree attainment.

Second, we examine the process of transfer from a 2-year to a 4-year college, as well as the cost of transfer, in terms of credits "lost" in the transfer process. Earlier scholars noted this loss of credits (Bowles, 1988; Cohen & Brawer, 1989; Dougherty, 1994; Pincus & DeCamp, 1989; Richardson & Bender, 1987); our analyses of national data, reported below, estimate the association between credit loss and graduation and ascertain "what if" scenarios that illustrate the relative importance of this loss on BA completion.

Third, we study the prognosis for transfer students. Once they have moved to a 4-year college, how well do these students do compared with their counterparts who started at 4-year institutions? Research undertaken in the 1970s and 1980s uniformly found that " . . . community college transfers have a high rate of attrition, one that is considerably higher than for four-year college natives entering the junior year" (Dougherty, 1994, p. 97; see also a summary of other studies in Dougherty, 1994, Appendix Table A4-2). In addition, earlier scholars attributed the higher drop-out rate after transfer to receiving less financial aid (Dougherty, 1994, p. 98), and to a lack of academic preparedness and "transfer shock" (Hills, 1965).

It seems possible that this earlier understanding may be outdated. The National Student Clearinghouse Research Center (2012) recently reported that 45% of all bachelor's degrees are now awarded to students who have transferred from a community college. They also found that among all transfers from community colleges to 4-year colleges, 60% obtain a BA within 4 years of transfer, while among the subset who had completed an associate's degree prior to transfer the graduation rate was 71%. A similarly upbeat finding was reported by Melguizo, Kienzl, and Alfonso (2011) who found that community college students who transferred to 4-year institutions are just as likely to complete a baccalaureate as similar students who initially enrolled at 4-year schools.

In our analyses, we examine transfer and completion below, examining a more recent national cohort to determine whether there is a higher attrition rate among community college transfers to 4-year colleges compared with counterparts who began at 4-year colleges, and also whether transfers receive less financial aid.

Data and Method

Data

The analyses presented here draw upon a longitudinal survey that tracked a nationally representative cohort of American first-time freshmen for 6 years after their initial entry into college in 2004. Respondents in this wave of the "BPS Longitudinal Study"[1] were interviewed at three points in time: toward the end of their first year of college, and then 3 and 6 years after first entry. Each student reported every college attended during this period, both dual enrollment and transfers from one college to another (Wine, Janson, & Wheeless, 2011).

BPS researchers later obtained and coded transcripts from each college attended, creating a data set known as the "2004/2009 Beginning Postsecondary Students Longitudinal Study Restricted-Use Transcript Data Files and Documentation." The data and documentation are restricted to license holders because of confidentiality issues. Our analyses used these transcript data to track student progress semester by semester, including transfers and final degree attainment. Our analyses used weights developed by the BPS staff to adjust for panel attrition and non-response. In addition, National Center for Education Statistics (NCES) provides these data with missing values already filled in using multiple hot-deck imputation. We have rounded the sample sizes reported below to the nearest 10, as required by BPS confidentiality agreements.

What Is a Fair Comparison Group?

The student body of a community college is much different from that of a public 4-year college. Community colleges enroll many students seeking 1-year vocational certificates, part-time and older financially independent students, and larger proportions of low-income and minority students. In contrast, some 4-year colleges are academically extremely selective, and few if any students with a sufficiently strong academic background to be admitted into selective 4-year institutions attend community colleges (Attewell, Heil, & Reisel, 2011; Dougherty, 1994; Horn & Skomsvold, 2012).

Given these stark differences in student goals, skills, and backgrounds, it would be misleading to compare *all* community college entrants with *all* 4-year college entrants. An alternative strategy is to compare the kinds of 2-year college students who view community college as a stepping stone to a BA with demographically and academically similar students who start at public 4-year colleges (e.g., Long & Kurlaender, 2009). We therefore limit our statistical comparisons at the outset to "an overlap sample": Financially dependent students attending either community colleges or non- or minimally-selective 4-year institutions, who enrolled full-time in their first semester, were enrolled either in a bachelor's or associate degree program, and who said they aspired to a bachelor's degree or higher. Minimally selective 4-year schools are defined by NCES as those institutions which either do not require the submission of test scores for admission, or those whose acceptance rate and SAT scores are at the bottom of the institutional distribution.[2] In the full BPS sample, 12.7% of students in public and non-profit 4-year schools were in "minimally selective" institutions, and another 6.8% were in open-admission colleges.

This overlap sample omits students pursuing certificates and those who show no initial aspiration for a BA, and it also excludes students who begin as part-timers, and older students who are financially independent, and all students attending academically selective 4-year colleges. As the descriptive statistics for this sample in Table 1 indicate, community college entrants and minimally or nonselective 4-year entrants in this overlap sample are quite similar in terms of age, gender, racial composition, income, and wealth. There nevertheless remain a few statistically significant differences between those in community colleges compared with those at 4-year institutions. A larger proportion of the parents of the overlap community college entrants attained at most a high school diploma, while more parents of the 4-year college students had a bachelor's degree. There were also significant differences in mathematics course-taking in high school and in SAT scores.

Given these remaining differences within the overlap sample between community- and 4-year college entrants, we adopt an additional methodological step to separate the effects of starting at

TABLE 1

Descriptive Statistics for Community College and 4-Year Entrants

	All students (*n* = 13,000)			Overlap sample (*n* = 2,040)			PS-matched groups (*n* = 2,010)		
	Community college	4-year	*p*	Community college (*M*)	4-year (*M*)	*p*	Community college	4-year	*p*
Age	23.41	19.31	<.001	18.53	18.50	.559	18.53	18.52	.621
White	61.01%	69.51%	<.001	70.46%	66.38%	.423	69.67%	67.47%	.246
Black	13.74%	10.01%	.013	9.25%	12.96%	.379	11.42%	12.25%	.526
Latino	15.55%	9.99%	<.001	13.08%	12.96%	.968	11.67%	13.22%	.249
Asian	4.42%	5.74%	.035	3.49%	2.34%	.282	2.92%	3.02%	.887
Female	56.71%	55.69%	.520	53.11%	52.73%	.922	55.17%	55.44%	.893
English not first language	11.82%	10.06%	.083	8.91%	7.22%	.434	8.60%	9.50%	.452
Household income	US$50,687	US$72,739	<.001	US$63,319	US$65,273	.671	US$59,580	US$59,864	.882
Home ownership	69.55%	83.64%	<.001	87.89%	85.09%	.248	85.75%	84.84%	.531
Assets > US$10K	18.15%	29.82%	<.001	25.23%	26.60%	.668	24.50%	23.14%	.437
Parental education < HS	11.69%	4.17%	<.001	4.89%	3.59%	.360	5.08%	5.20%	.897
Parental education: HS grad	36.15%	21.21%	<.001	27.29%	19.24%	.003	25.75%	26.49%	.679
Parental education: Some college	23.51%	18.47%	<.001	33.31%	29.63%	.325	31.58%	31.42%	.931
Parental education: Bachelor's+	28.64%	56.15%	<.001	34.49%	47.52%	.002	37.58%	36.89%	.725
HS math: < Algebra 2	24.73%	5.53%	<.001	18.85%	7.84%	<.001	15.58%	14.95%	.666
HS math: Algebra 2	55.06%	38.74%	<.001	53.64%	48.81%	.270	56.75%	56.98%	.910
HS math: Pre-cal/calculus	20.21%	55.71%	<.001	27.50%	43.34%	<.001	27.67%	28.07%	.825
Did not take SAT	31.17%	2.89%	<.001	19.11%	6.52%	<.001	14.50%	12.98%	.279
Combined SAT score	902.5	1,060.7	<.001	922.0	976.0	.001	919.4	922.4	.690
Earned college credits in high school	21.34%	41.26%	<.001	25.58%	30.94%	.180	25.83%	25.29%	.758
Attended U.S. private HS	4.87%	12.95%	<.001	6.94%	12.26%	.031	7.33%	7.10%	.824
Part time in first semester	51.13%	15.95%	<.001	—	—	—	—	—	—
Enrolled in certificate program	6.07%	1.47%	<.001	—	—	—	—	—	—
Degree aspirations < BA	18.43%	1.41%	<.001	—	—	—	—	—	—

Source. National Center for Education Statistics (2011, 2012).
Note. p values are results from two-tailed *t* tests. PS = propensity score; HS = high school.

a 2-year college per se, from these observed background differences between the two types of students, as we now explain.

The Counterfactual Model of Causal Inference

The Counterfactual Model of Causal Inference was developed to evaluate the effects of "treatments" or policy interventions in contexts where data from random-assignment experiments were not available, but where there were non-experimental observational data on interventions and outcomes (Heckman, Ichimura, & Todd, 1998; Rosenbaum & Rubin, 1983b; Rubin, 1973).

In experiments, the process of randomly assigning subjects into a treatment or a control group ensures that these two groups are almost identical in terms of measured and unmeasured background characteristics (covariates). Because those characteristics are balanced via random assignment, any differences in outcomes observed between the experimental and control groups may be causally attributed to the treatment or intervention itself (Shadish, Cook, & Campbell, 2002).

In theory, a counterfactual approach might seek a balance on covariates by matching each subject in the treated group with one or more "untreated" subjects (controls) so that all the observed characteristics of each untreated subject were identical to those for the matched treated subject. In that hypothetical context, each control subject may be thought of as providing an estimate of what outcome a matched treated subject *would have had*, if the treated subject had *not* received the treatment (hence "counterfactual"; Morgan & Winship, 2007). The difference between the observed outcome and the counterfactual is a measure of the effect of treatment.

As a practical matter, however, it is impossible to match individual subjects exactly on large numbers of background characteristics, an issue that mathematicians term a *multivariate problem of high dimension*. Fortunately, the pioneers of the counterfactual method demonstrated that it is unnecessary to exactly match each treated subject with each untreated subject on all covariates (Heckman et al., 1998; Rosenbaum, 1989; Rosenbaum & Rubin, 1983b; Rubin, 1973). An equivalent balancing is possible if subjects are instead matched on their *propensity for treatment*. A single-number summary of this propensity is generated by estimating a logistic or probit regression in which the dichotomous dependent variable takes a value of one if the subject received the treatment and zero otherwise (untreated), and the predictors consist of all relevant measured characteristics for the individuals in the sample. Matching algorithms are discussed by Guo and Fraser (2010) and Morgan and Winship (2007).

Propensity-score matching reduces the bias in estimates due to differences in observables and provides a more accurate inference about any "treatment effect" that may exist (Shadish et al., 2002, pp. 161–165). As a result, when certain conditions are met, including achieving statistical balance on a range of substantively important covariates, it is argued that propensity models permit researchers to make causal inferences about the effect of a treatment (e.g., Guo & Fraser, 2010; Morgan & Winship, 2007; Reynolds & DesJardins, 2009). This is in part because, after balancing on the observables, it can be claimed propensity scores enable us to satisfy the Conditional Independence Assumption: Matched treatment and control groups would not differ systematically on the outcome in absence of the treatment.

We used kernel matching within a statistical procedure called "psmatch2" in the STATA statistics program (Leuven & Sianesi, 2003). Kernel matching differs from other matching methods like pair matching or *k*-nearest neighbors matching in that it leverages most if not all of the control cases available in the data, and thus is a more efficient matching algorithm (Heckman et al., 1998). In addition, we find that kernel matching tends to produce a better match in terms of the covariates included in the propensity-score model. In kernel matching, counterfactuals are constructed for each treated case using a weighted average outcome for all control cases, with weights determined through a kernel transformation of the difference in propensity scores. We used an Epanechnikov kernel with a bandwidth parameter of .06. These are the default settings in psmatch2, but our choice of bandwidth is consistent with the findings of earlier simulation studies (e.g., Frolich, 2004). In addition, we recalculated results using different bandwidth settings and found that estimates were quite stable across bandwidths which fell between .01 and .20.

To take further care that we achieved good internal validity in our propensity-score matching methods, we only produced estimates using cases in the region of common support (treated cases for which there were control cases with similar propensity scores), and we trimmed the 5% of treated cases with the highest propensity scores. When generating matched samples, we relied primarily on how well the groups were matched in terms of the covariates included in the propensity-score model. In general, we strove for measures of absolute bias lower than .10 (or, a difference in means less than 10% of an average standard deviation for the variable across treated and control groups), and for differences in means which had *p* values (derived from *t* tests) of .15 or higher. If these conditions were not met, we altered the specification of the propensity-score model by adding higher order terms and interactions.

Because we match control cases to treated cases, we are estimating a quantity that researchers call the Average Treatment Effect on the Treated (ATT). This means that our matched control group provides an estimate of what would have happened to treated cases had they not been exposed to the treatment. Finally, because propensity scores are themselves estimates, scholars have suggested that "standard" errors are not appropriate (Caliendo & Kopeinig, 2008). Accordingly, we bootstrapped standard errors for all outcomes, using 100 replications each. Significance levels reported are based on *T* statistics generated using bootstrapping.

The variables we used for matching included: age, race (Black, Latino, Asian, Other vs. White as the reference category), gender, primary language (English/other), immigrant status and generation, household income (logged), home ownership, wealth (family non-home assets in excess of US$10,000), parental marital status (divorced, single parent, or widowed vs. married), parental education (less than high school/unknown, some college, bachelor's or higher vs. high school graduate), household size, high school math (less than algebra 2, pre-calculus or calculus vs. algebra 2), whether or not a student took the SAT/ACT, SAT/ACT combined score (terciles), college credit taking while in high school, high school diploma attainment, high school type (public/private), high school grade point average (GPA; categories), and working in the summer prior to college. The third set of columns in Table 1 presents means of groups matched on the propensity score, along with *t* tests of the difference in these means. Our propensity-score matching resulted in groups that are quite well matched on these covariates, including on measures of parental education and academic preparation. These variables represent only a subset of those which were used in matching; the full set is included in the appendix.

Unobserved Differences and Sensitivity Analyses

Propensity-score matching is an effective strategy for removing the effects of differences between two groups on *observed characteristics*. However, there might still be *unobserved differences* remaining between the treatment and control groups. Consequently, the possibility remains that the estimated effects of a "treatment" might be conflated with unmeasured differences between students at the two types of college.

Researchers who use counterfactual models have developed methods for assessing how vulnerable their estimates are to some hypothetical unmeasured confounder. A sensitivity analysis calculates how large the effect on the probability of treatment of an unmeasured or hypothetical confound would have to be to cancel out or negate the treatment effect that has just been measured via matching (Guo & Fraser, 2010). If a sensitivity analysis shows that a hypothetical confounding variable with a relatively small effect size could cancel out the measured treatment effect, then a researcher has less confidence in the treatment effect. Conversely, if a sensitivity analysis indicates that the effect of a hypothetical confound needed to negate the observed treatment effect would have to be large, and if the researcher has already controlled for a wide range of plausible influences using measured covariates in the propensity model, then the researcher can be more confident that the measured treatment effect is unlikely to be spurious and most likely reflects the treatment itself. A number of sensitivity tests have been devised, all of which build off of the framework of

TABLE 2
Means and Propensity-Score Matched Treatment Effects Comparing Students Beginning Post-Secondary Education at Community Colleges to Those Beginning at 4-Year Institutions

	Naïve mean estimates		
	Community college (n = 1,260)	Non-selective 4-year college (n = 750)	PS-matched effect size
Earned bachelor's degree	24.48%	45.82%	−16.86%***
Bachelor's or still enrolled	40.28%	61.46%	−18.46%***
Final GPA	2.73	2.64	0.01
Credits earned, career	88.45	102.44	−8.33**
Non-remedial credits earned, career	85.34	100.70	−9.31***
% courses withdrawn	9.87%	5.68%	4.23%***
Took remedial math freshman year	50.72%	23.46%	19.24%***
Took remedial reading/writing freshman year	23.61%	14.94%	5.55%*
Ever stopped out	38.73%	25.82%	10.47%***
Had job freshman year	81.20%	69.24%	11.32%***
Average weekly hours worked freshman year, employed students	20.85	24.76	4.07***

Source. National Center for Education Statistics (2011, 2012).
Note. Restricted to BPS overlap sample. PS = propensity score; GPA = grade point average; BPS = Beginning Postsecondary Students.
*$p < .05$. **$p < .01$. ***$p < .001$.

Rosenbaum and Rubin (Rosenbaum, 1987; Rosenbaum & Rubin, 1983a). For our sensitivity tests, we rely on a method devised by Ichino, Mealli, and Nannicini (2008). In addition, we calculate Mantel–Haenszel bounds for unobserved bias using user-generated "mhbounds" in Stata (Becker & Caliendo, 2007) and include results in the appendix.

We have focused, in developing our empirical strategy, in maximizing the internal validity of our comparison between community college and 4-year students. However, in this case as in many others, there is some trade-off between internal and external validity. We cannot, and do not, make the claim that our results necessarily generalize beyond our overlap sample.

Findings

Differences Between 2- and 4-Year Entrants

The first two columns of Table 2 report descriptive statistics for 2- and 4-year entrants for the restricted overlap sample of BPS undergraduates discussed above. The table shows, for example, that almost 25% of 2-year entrants earned a BA within 6 years, compared with nearly 46% of students who started at a 4-year college. However, those figures do not adjust for any differences in demographic background, high school preparation, or other factors between the two groups of undergraduates. The right-hand third column of Table 2 reports the results of propensity-score matched analyses, giving estimates of the difference between entrants into each type of college, after adjusting statistically for the background factors listed in the methods section above.

The column titled "PS-matched effect size" indicates that undergraduates who start their higher education at a community college have a BA graduation rate that is nearly 17 percentage points lower than otherwise similar students who begin at a 4-year college, a large and statistically significant effect. A potentially problematic measurement issue is addressed by the second row of Table 2. The BPS tracks students only for 6 years. If community college entrants were simply progressing toward a degree more slowly than their 4-year counterparts, then an observed graduation gap might be an artifact of the 6-year survey cut-off. To study that possibility, we constructed an additional dependent variable that took a value of one if a student had either graduated with a BA or was still enrolled in college at the time of the last survey, and zero otherwise. A large, statistically significant gap in BA attainment (18.46 percentage points) persists between entrants to 2- and 4-year colleges on this second outcome measure.

Table 2 also reports that there are no statistically significant differences in final cumulative GPAs between the two groups. However, additional contrasts show that 2-year college students trail their 4-year counterparts by about 8 credits (or 9.3 non-remedial credits) by the study's 6-year cut-off date. Community college students have also withdrawn from a larger proportion of their courses (a 4.3 percentage point difference) within 6 years.

Community college students are substantially more likely to be exposed to remediation than their 4-year counterparts. And while they are only 5.5 percentage points more likely to take a reading or writing remedial class, they are fully 19.2 percentage points more likely to take a math remedial class. This is a particularly stark difference considering that we are comparing groups which are balanced in terms of high school math classes taken, high school grades, and SAT scores.

Ceteris paribus, more undergraduates who enter community college stop out—fail to enroll for a semester or more—during the first 6 years after entering college (a 10.47 percentage point difference). Two-year entrants are also significantly more likely to have a job during their first year of college, and (among those who do have a job) to work longer hours per week during term time.

Sensitivity Analysis

We have demonstrated that community college students are substantially less likely to earn a bachelor's degree than similar 4-year students after matching on a broad set of observable characteristics. But how sensitive are these results to the presence of an unmeasured confounder?

When conducting a sensitivity analysis, we ask two questions simultaneously. The first has to do with the size of the hypothetical *selection effect*: How much more common would an unmeasured characteristic have to be in the treatment rather than the control group (or vice versa) to eliminate the observed treatment effect? We are asking here about the impact of the unobserved variable on *assignment to treatment*. The second has to do with the *effect on the outcome*: How much more common must an unmeasured confounder be among "successes" (BA-earners) than "failures" (non-earners) to account for observed differences? The unobserved characteristic has to affect *both* selection into treatment *and* the outcome (probability of "success") to be a confounder. A variable which has a strong relationship with the outcome but which is present in equal quantities in the treatment and control groups would not have any impact on treatment effects. Neither would a variable which is more commonly encountered in the treatment than in the control group but which has no impact at all on the outcome. In our case, an unobserved confounder would be a characteristic more common among 4-year students than community college students, and more common among those who completed a baccalaureate rather than among those who did not.

The selection effect is denoted in Table 3 as s and indicates the difference between the probability of encountering the hypothetical confounder among community college students and of encountering it among direct 4-year entrants. That is, it tells us the strength of the relationship between treatment and the unobserved characteristic. This quantity is in our analysis always negative, because the unmeasured characteristic must be more commonly observed in the control (i.e., 4-year) group if it is to counteract a negative estimated treatment effect. The quantity d is the difference in the probability of observing the hypothetical confounder between successes and failures in the *control* group alone. That is, it tells us the effect of the confounder in the absence of treatment. In Table 3, these two parameters are moved simultaneously, and we estimate treatment effect sizes in these differing sets of conditions. Estimated treatment effects and their standard errors were calculated using bootstrapping (100 replications each). We indicate where

TABLE 3

Sensitivity Test for Unmeasured Bias in the Effect of Community College Attendance on Bachelors' Completion Using the Ichnino, Mealli, and Nannicini (2008) Methodology

	s = −.1 (Λ = .47–.64)	*s* = −.2 (Λ = .26–.40)	*s* = −.3 (Λ = .12–.26)	*s* = −.4 (Λ = .11–.17)	*s* = −.5 (Λ = .05–.11)
Baseline (−17.1%)					
d = .1 (Γ = 1.52–2.01)	−16.1	−15.5	−14.4	−13.7	−9.5
d = .2 (Γ = 2.39–4.30)	−14.6	−12.8	−11.5	−10.3	−8.2
d = .3 (Γ = 3.93–7.13)	−14.0	−11.6	−9.2	−6.8	*−3.1* (ns)
d = .4 (Γ = 6.02–11.64)	−13.7	−10.3	−7.3	−3.3	*2.9* (ns)
d = .5 (Γ = 10.51–30.94)	−13.2	−8.9	−4.9	*−0.1* (ns)	*7.4* (ns)

Note. All estimated treatment effects are statistically significant at $p < .05$ or better unless indicated. Estimates and standard errors generated through bootstrapping (100 replications each).

the treatment effect of initial enrollment at a community college would *not* be statistically different from zero.

Table 3 shows us an unobserved confounder would have to be very influential, in terms of both selection and relationship with the outcome, to account for the attainment gap between community college and 4-year entrants. The selection effect would have to be at least −.4 (e.g., the probability of the unobserved confounder would have to be .35 among community college students and .75 among direct 4-year enrollees), simultaneous with an effect on the outcome of .5 (i.e., the probability of the confounder among BA earning 4-year students would have to be something like .8, and only .3 among non-BA earning 4-year students). And, importantly, such selection and outcome effects would have to be uncorrelated with the whole set of observed characteristics included in the propensity-score model.

How likely is there to be an unmeasured confounding variable of this size? To get an idea of this, we calculated *s* and *d* for some of the most impactful observed characteristics in our data. Taking calculus in high school is very predictive of both attendance at a 4-year school and of completing a BA. But for calculus, $s = -.09$ and $d = .12$. Being in the highest tercile of combined SAT is slightly more powerful, the selection effect is $s = -.13$ and the impact on outcome is only $d = .15$. Finally, having college-educated parents has an impact on selection of −.14 and on outcome of .09. Given that these three highly influential characteristics have nowhere near the selection and outcome impacts needed for a confounder to obliterate our results, we conclude that such an unknown, unmeasured, and uncorrelated confounder is quite unlikely to exist.

Divergences Between 2- and 4-Year Entrants During the Initial Years

Table 4 examines gaps on measures of academic progress that appear during the first eight semesters of college. First, we consider attendance or enrollment. In this overlap sample, there is attrition from college for both 2- and 4-year students, but in the propensity-matched models the differences between the two types of college do not become statistically significant until Spring of the third year of college. By that time, although the majority of the cohort is still enrolled, the retention rate among community college students is roughly 9 percentage points lower than for their counterparts who entered a 4-year college.

A different panel of Table 4 analyzes the cumulative number of credits earned by semester, conditioned on enrolling that semester. Again, it takes about 3 years before a significant divergence appears: a 3.55 credit difference, roughly one course's worth, by the Fall semester of the third year, which increases over time to nearly 11 credits.

In terms of credits attempted, the effects are trivial in size, though statistically significant. By the third year, community college students were attempting on average about one fewer credit of coursework than their counterparts who started

TABLE 4

Academic Outcomes Over Time for Beginning at a 2-Year College, Among BPS Overlap Sample

	Naïve mean estimates		
	Community colleges (n = 1,260)	Non-selective 4-year colleges (n = 750)	PS-matched effect size
Cumulative credits earned			
Fall 1	12.08	12.98	−0.61**
Spring 1	23.13	24.84	−1.02*
Fall 2	33.24	35.84	−1.17
Spring 2	42.03	45.84	−1.89
Fall 3	49.74	56.01	−3.55**
Spring 3	56.32	65.44	−5.83***
Fall 4	63.04	75.17	−7.69***
Spring 4	68.67	83.44	−9.83***
Attendance			
Spring 1	95.61%	95.31%	−0.13%
Fall 2	83.39%	82.73%	−0.89%
Spring 2	78.79%	77.37%	−2.40%
Fall 3	66.58%	74.44%	−4.27%
Spring 3	62.08%	73.99%	−8.56%***
Fall 4	56.14%	68.52%	−9.26%***
Spring 4	52.44%	66.32%	−10.94%***
Credits attempted[a]			
Fall 1	14.02	14.72	−0.38**
Spring 1	13.97	14.62	−0.19
Fall 2	13.09	13.95	−0.36
Spring 2	13.72	14.17	−0.50*
Fall 3	12.09	13.78	−1.13***
Spring 3	12.69	14.03	−1.04***
Fall 4	11.61	13.50	−1.19***
Spring 4	12.21	13.75	−1.17***
Credits earned as % of attempted[a]			
Fall 1	83.61%	86.75%	−2.90%*
Spring 1	80.36%	82.67%	−1.45%
Fall 2	77.62%	86.17%	−4.39%**
Spring 2	80.57%	86.71%	−6.30%***
Fall 3	79.07%	86.73%	−5.91%***
Spring 3	80.61%	87.39%	−5.33%***
Fall 4	80.39%	88.68%	−5.93%***
Spring 4	84.07%	88.03%	−4.21%*

Source. National Center for Education Statistics (2011, 2012).
Note. BPS = Beginning Postsecondary Students; PS = propensity score.
[a]Conditional on attendance.
*$p < .05$. **$p < .01$. ***$p < .001$.

in 4-year colleges. Community college students were also completing fewer of the credits they attempted, this time by the end of the second year, suggesting that these students were more likely to fail or withdraw from a class. It is important to note that these semester-by-semester

TABLE 5

The Timing of Transfer Among Community College Students Who Transferred Immediately to a 4-Year School (Restricted to Students Who in Their First Year Had Plans to Transfer to a 4-Year Institution)

Credits earned at community college	% all of transfers	Probability of transfer to 4-year (%)	BA attainment rate (%)
<20	14.10	41.41	44.93
20–39	16.30	49.09	44.61
40–59.5	22.71	55.62	54.27
60+	46.89	58.80	52.96

Source. National Center for Education Statistics (2011, 2012).

differences are conditional on enrollment, so the effects of differential retention discussed previously have been removed from the estimates.

We were surprised to discover that substantial differences in the academic progress of the two groups of undergraduates generally do not appear until about the third year after entry. If community colleges were especially unwelcoming or unsupportive places, or if their peer and faculty cultures were antiacademic or dispiriting, then one might expect to observe short-term differences in attrition or attendance, in credits attempted, or in credits earned, when comparable students from community and 4-year colleges were tracked over time. Certainly, the implication of Adelman's (1999, 2006) and others' studies of academic momentum is that the seeds of long-term academic success or failure are found in students' performance in their initial 2 years of college. However, we do not observe those marked short-term differences on measures of academic accomplishment in the first 2 years after entry for our overlap sample.

Instead, the predominant pattern seems to be a slower divergence in student outcomes that become apparent only during the third year after entry. By then, in terms of credits earned, the community college students have fallen behind their 4-year counterparts. Those average differences in credits earned are largely accounted for by the fact that up to 10 percentage points more community college students have "stopped out" of college (interrupted their enrollment), compared with their counterparts in 4-year colleges, rather than due to community college students making less academic progress while enrolled.

The Transfer Process

Our BPS overlap sample is restricted to degree-seeking students who aspired to a BA degree. With rare exceptions, community colleges do not offer a BA degree, so BA-aspiring community college students need to transfer to a 4-year college to attain their goal. However, only 42% of BA-intending students who enter community college and say they hope to transfer actually do transfer to a 4-year institution; and 13% of community college entrants who do *not* say they intend to transfer when surveyed during their first year of college, nevertheless transfer.

Table 5 indicates that the probability of transfer rises markedly as community college students accumulate more credits at their 2-year institution. Inspection of confidence intervals (not shown) indicates that there is a significant jump in the likelihood of transferring once a community college student has completed 40 credits and a further significant increase in the probability of transferring for those completing 60 or more credits, which is the typical credit amount needed for an associate's degree.

However, the most striking feature of Table 5 is that even among students who do successfully accumulate 60 or more credits at their community college and who reported during their first year that they planned to transfer to a 4-year school, only roughly 60% actually transfer to a 4-year college. Evidently, many relatively academically successful community college students fail to transfer to a 4-year college.

At first impression, Table 5 also indicates that the probability of attaining a BA after transferring increases with greater number of credits

TABLE 6

Comparing BA Completion Among Community College Transfers and Students Who Started at a 4-Year College (Limited to Students in Non-Selective Institutions Who Earned at Least 48 Credits)

	Community college transferees (n = 1,440)	Non-selective 4-year beginners (n = 930)	PS-matched difference	p	Mean bias	p value of propensity score	Lowest covariate p value
All students	44.62%	47.46%	−2.33%	.295	1.08	.532	.499
Dependent students younger than 24	46.09%	51.37%	−2.19%	.362	1.33	.548	.356

Source. National Center for Education Statistics (2011, 2012).
Note. PS = propensity score.
$^{\dagger}p < .10$. $^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

earned at the first institution (the community college), from 44.9% to 52.6%. However, an inspection of the confidence intervals around these point estimates shows that these are not statistically significant differences: The BA attainment rate for those who transfer after earning a few credits is not statistically significantly different from that of students who completed 60 or more credits at their initial college.

Performance After Transferring to a 4-Year College

Do community college students perform adequately after making it to a 4-year school, or do they suffer "transfer shock," flounder, and dropout (Hills, 1965)? To answer this question, we compare, in Table 6, bachelor's degree attainment between undergraduates who started at a 4-year non-selective college and students who started at a 2-year college and later transferred to a 4-year institution. We are, the reader should note, no longer restricting our analysis to dependent students who initially enrolled full-time, as doing so is less relevant for this analysis and would result in substantially reduced statistical power. This analysis instead imposes a different restriction: Only those who completed at least 48 credits (roughly four semesters at full-time) were included. This sort of restriction is crucial to the analysis of transfer (Melguizo et al., 2011), for had we simply compared the BA attainment among all 4-year entrants with that among 2-year transfers, that would have skewed or biased the comparison in favor of community college transfers since substantial numbers of entrants to unselective 4-year colleges do drop out early on. That is, we do not want to compare the "survivors" or successes from 2-year colleges (the transfers) with all 4-year entrants including early dropouts, clearly an unfair comparison. However, an opposite bias would have been introduced had we imposed the credit restriction on 4-year beginners only. This is because some community college beginners transfer very early on in their career, after having earned only a few credits. These students would be exposed to the risk of dropout for longer than anyone in a credit-restricted pool of 4-year beginners. Accordingly, in Table 6, we compare the "survivors" (to 48 credits) among 4-year entrants with the "survivors" (to 48 credits) who transferred from 2-year colleges, a more equitable comparison. These alterations in the groups under analysis account for the differences in sample size between those reported in Table 2 and those in Table 6.

Table 6 presents unadjusted figures for the percent completing a BA, with separate figures for all students and for younger dependent students. After propensity-score matching,[3] there were no significant differences in BA completion between transfer and non-transfer students. This is consistent with findings by Melguizo et al. (2011): On average, those community college students who do successfully transfer are just as likely to complete a degree as similar students who start at a 4-year institution. However, it contradicts an earlier generation of scholarship that asserted that many more community college transfers dropped out of 4-year colleges, as

TABLE 7

Logistic Regression Predicting BA Attainment Among Community College Students Who Successfully Transfer to a 4-Year College (Controls Included, but Not Displayed, for Race, Income, Age, Gender, Dependent Status, High School Math, and SAT Combined Scores)

	Model 1	$\Delta y/\Delta x$	Model 2	$\Delta y/\Delta x$
Credits transferred: 50%–89%	1.912† (0.669)	13.83% (.0736)	1.882† (0.689)	13.33% (.0753)
Credits transferred: 90%–100% (*ref.: < 50%*)	2.996*** (0.912)	23.93% (.0609)	2.728*** (0.803)	21.49% (.0601)
Pre-transfer credits earned	1.002 (0.004)	0.01% (.0012)	1.001 (0.005)	0.01% (.0012)
Pre-transfer GPA	2.323*** (0.515)	18.22% (.0513)	2.387*** (0.629)	18.42% (.0508)
Destination: Moderately selective			1.952* (0.643)	14.18% (.0667)
Destination: Highly selective *(ref.: Non-selective destination)*			2.500** (0.934)	19.53% (.0761)
Constant	0.019*** (0.024)		0.008*** (0.0136)	
Observations	820		820	

Source. National Center for Education Statistics (2011, 2012).
Note. GPA = grade point average; ref. = reference category.
†$p < .10$. *$p < .05$. ***$p < .001$. **$p < .01$.

indicated in our earlier literature review. If those who successfully transfer have the same BA completion rate as 4-year entrants who also completed four semesters, then one can conclude, at least for successful transfers, that the education received at a community college was adequate preparation for completion of a BA degree.

Credit Loss Upon Transfer

Digging deeper into the transcript data, we find that many transfers from community college "lose credits" when they transfer, some course credits that they accumulated at their community college are not accepted by their post-transfer 4-year college. (That information was coded by BPS staff using the transcripts from the two institutions. These transcript credits did *not* count remedial courses taken in community college which are usually non-credit bearing.) In fact, about 14% of transfer students in this study essentially began anew after transferring: Their new institution accepted fewer than 10% of their community college credits. At the other extreme, only 58% of community college transfers were able to bring over 90% or more of their college credits to the 4-year institution (authors' calculations from NCES, 2011, 2012). The remaining 28% of transfers lost between 10% and 89% of their credits.

This widespread loss of credits associated with transfer from a community college to a 4-year institution is consequential: Students who lose credits have significantly lowered chances of graduation. Table 7 reports that students who have all or almost all their credits transferred have an odds of graduation more than 2.5 times greater than students with less than half their credits transferred (the reference category), while students who get between half and 89% of their credits accepted by their 4-year institution have a 74% higher odds. These large effects of credit transfer on degree completion occur after controls have been added for pre-transfer GPA and the number of credits earned at a community college, as well as controls for the selectivity of 4-year institution that the transfer student moved to. They suggest that transferability of credits earned at community colleges is an important factor for subsequent BA attainment.

We can use the model from Table 7 to estimate a "what if" scenario. What would BA graduation rates look like if community college transfer did

TABLE 8

Differences in Attainment by Initial Major Category Estimated Through Propensity-Score Matching

	Academic vs. vocational (n = 946)	Academic vs. undeclared (n = 663)	Vocational vs. undeclared (n = 1,015)
Earned 60 credits	−1.30%	8.82%*	2.35%
Earned associate degree	−1.13%	2.90%	3.16%
Transferred to 4-year	0.23%	3.83%	−5.89%
Earned BA	4.22%	3.71%	−1.17%
BA or still enrolled	5.63%	1.85%	−4.22%

Source. National Center for Education Statistics (2011, 2012).
$^*p < .05$.

not suffer losses of credits when they are admitted into 4-year colleges? This is not an entirely fanciful idea, since some states (New Jersey being one) have mandated that all for-credit courses earned in a state community college must count toward BA graduation after transferring to a state 4-year college (Western Interstate Commission for Higher Education, 2013). The model suggests that if credit loss did not occur, BA attainment rates among community college transfer students would be 54% rather than 45%.

Finally, we return to two mechanisms emphasized by an earlier generation of scholars as causes of the BA graduation gap. Dougherty (1994) among others suggested that the low BA graduation rate reflected the emphasis that community colleges placed on vocational programs. Table 8 reports propensity-matched analyses[4] contrasting academic outcomes for students who begin community college in vocational programs, compared with other community college students who began in academic programs, and a third group whose specialization was undeclared at entry to a community college. There are some differences in academic outcomes in BA attainment comparing all vocational against all academic students in community colleges, but there is not a statistically significant difference in BA attainment between the two types of student. We conclude that the explanatory weight previously placed upon vocational specializations as a major factor behind community college students' lack of BA completion was erroneous or is no longer the case.

Table 9 provides an analysis of financial aid comparing community college transfers with their counterparts who started at 4-year colleges. This was also advanced as an important explanation of the community college disadvantage by earlier scholars. The left-hand columns of Table 9 report aid differences with no statistical controls. Here we observe some significant differences in aid levels: Community college transfers, for example, received on average US$473 less than 4-year direct entry students in institutional need-based aid and US$604 less in institutional merit aid. However, the right-hand columns of Table 9 report differences in amounts of aid received after controlling for age, family income, and other pertinent factors.[5] Once those adjustments are made, there are no statistically significant differences in the amount of institutional aid that community college transfers receive once in 4-year institutions compared with ongoing 4-year students, and the few significant differences in Federal aid are very small in size, at most about US$100. We conclude that unequal aid provision is not an important factor in explaining the BA attainment gap.

Summary and Discussion

Our analyses of longitudinal transcript data for an overlap sample of students who entered 2-year and otherwise similar BA-aspiring undergraduates who began higher education at relatively unselective 4-year colleges found that the unadjusted BA attainment rate for community college entrants was 21 percentage points lower than for 4-year entrants. Adjusting for differences in observed background characteristics via

TABLE 9

*Estimated Differences in Aid Receipt Associated With Community College Transfer Status Among 4-Year College Students, in Dollars (*n *= 50,880)*

	Unadjusted differences		OLS estimates	
	All students	Conditional on receipt	All students	Conditional on receipt
Institutional need-based aid	−473.87***	−1,336.6***	0.12	5.55
Institutional merit aid	−604.28***	−879.64***	−0.02	−54.86
Federal aid	−14.62	−286.54***	−3.50**	−116.91**
State need-based aid	−72.50***	−195.47***	−0.25	−58.80
State merit aid	−80.55***	−8.20	−0.06	−40.02
Loans	242.00***	165.18†	25.12†	98.24

Source. National Center for Education Statistics (2009).
Note. OLS = ordinary least squares.
†$p < .10$. *$p < .05$. ***$p < .001$. **$p < .01$.

propensity-score matching, along with a sensitivity analysis, indicated a statistically significant BA attainment gap of about 17 percentage points, which is unlikely to be due to unobserved confounds. What lies behind this gap in BA attainment between otherwise similar undergraduates who enter community colleges and their 4-year college counterparts?

An examination of credits earned and related measures of academic momentum showed that community- and 4-year college students in an overlap sample do not significantly differ in educational progress during the first 2 years of college. Differences begin to emerge in the third and later years, when community college students start to fall behind 4-year-college counterparts in credit accumulation.

Many community college students do accumulate roughly 60 credits, equivalent to an associate's degree. Even among these relatively academically successful BA-seeking students at community colleges, however, only about 60% transfer to a 4-year college. What the mechanisms are which prevent students from transferring—whether they fail to apply, or apply to too few schools, or whether they are discriminated against by 4-year institutions—is a fruitful ground for future research.

Following those community college students who do transfer to a 4-year institution, we find that on average transfers are just as likely to graduate with a BA as equivalent students who started at a 4-year college. In addition, we find that many transfer students pay a penalty, in the sense that the receiving 4-year institution does not accept all their earlier credits as counting toward the BA. Only 58% of transfers in our national sample are able to bring all or almost all of their credits with them. We find that, even after controlling for college GPA and credits earned, those students who can transfer most of their credits are more likely to complete a BA. One implication is that the BA attainment rate among community college transfers would be even higher than 4-year entrants if this credit loss did not occur.

In sum, we have identified three main "choke points" that contribute to the gap in BA attainment of community college entrants compared with otherwise similar entrants to 4-year colleges, the most consequential of which are the lack of transfer among students who have completed 60 credits and the loss of credits among those who do transfer. Policymakers have long made efforts to facilitate transfer to 4-year institutions, and such efforts seem to have intensified more recently. Research on the effectiveness of articulation agreements has only just begun, but so far findings are not particularly promising. Anderson, Sun, and Alfonso (2006), employing a multilevel model to investigate an earlier iteration of the BPS, find no evidence that transfer agreements lead to a higher probability of transfer for community college students. Roksa and Keith (2008) conclude that "articulation policies do not appear to enhance bachelor's degree attainment in the public sector" (p. 247).

Another important avenue for research concerns remedial or developmental classes. As we found above, students entering community college are far more likely to take remedial classes than similar students entering 4-year schools. But the reasons for this disparity and whether or not it contributes to lowering retention and completion rates for community college students are not at this point known. In fact, at this point, we cannot be sure of whether remediation is on the whole beneficial or harmful, or if it has an impact at all. A number of studies with high internal reliability have investigated the impact of remediation, but results have been inconclusive (Bettinger & Long, 2009; Boatman & Long, 2010; Calcagno & Long, 2008; Martorell & McFarlin, 2011). However, it is clear that remediation is widespread and increasingly common in the higher educational landscape (particularly so at community colleges) and it is imperative that we understand its impacts.

More research is also needed into what prevents credit transfer for students moving from community colleges into 4-year institutions. Such research could suggest more far-reaching and effective interventions to help community college students attain their goals, turning the promise of the community college—the second-chance, low-cost route to the bachelors' degree—into a reality for more students.

Appendix

TABLE A1
Detailed Matching Statistics for Outcomes in Tables 2 and 4

	Pre-matching means		Post-matching means			
Variable	CC	4-year	CC	4-year	Standard Bias	$p > \|t\|$
Propensity score	.67131	.56176	.65969	.65682	1.9	.614
Age	18.54	18.46	18.53	18.53	−0.3	.948
White	.701	.684	.696	.673	5.1	.211
Black	.111	.109	.116	.124	−2.4	.575
Latino	.111	.135	.116	.131	−4.8	.240
Asian	.028	.030	.027	.029	−1.0	.793
Other race	.047	.039	.042	.041	0.7	.869
Female	.545	.558	.551	.555	−0.9	.827
English is first language	.916	.924	.915	.906	3.4	.424
Household income (log)	10.57	10.72	10.58	10.56	1.4	.743
Home ownership	.858	.860	.857	.848	2.7	.517
Assets > US$10K	.239	.280	.245	.230	3.5	.375
Non-married parents	.299	.260	.296	.303	−1.5	.710
Parental education < HS	.052	.030	.050	.052	−0.8	.852
Parental education = HS	.270	.193	.258	.264	−1.6	.711
Parental education = Some college	.317	.278	.316	.315	0.4	.922
Parental education = BA+	.359	.498	.374	.367	1.3	.744
Household size	4.10	4.16	4.10	4.08	2.1	.600
HS math < Algebra 2	.172	.087	.156	.149	1.9	.667
HS math = Algebra 2	.563	.487	.567	.569	−0.3	.935
High school math = Pre-calculus or calculus	.263	.425	.276	.280	−1.0	.798

(continued)

TABLE A1 (CONTINUED)

Variable	Pre-matching means		Post-matching means		Standard Bias	$p > \|t\|$
	CC	4-year	CC	4-year		
No SAT	.183	.072	.146	.132	4.3	.313
Low SAT tercile	.288	.201	.299	.301	−0.6	.886
High SAT tercile	.253	.417	.265	.252	2.8	.470
Earned college credits in HS	.260	.315	.258	.252	1.3	.744
AP credits dummy	.145	.182	.142	.144	−0.4	.916
Foreign student	.012	.010	.013	.014	−1.6	.711
Resident alien	.029	.026	.029	.033	−2.3	.594
Foreign-born citizen	.033	.042	.035	.040	−2.7	.497
Second generation	.133	.117	.128	.145	−5.1	.225
Did not earn HS diploma	.030	.022	.031	.034	−1.3	.767
Worked summer before college	.757	.748	.755	.747	1.9	.639
HS GPA 0.5–0.9	.004	.002	.004	.004	0.6	.894
HS GPA 1.0–1.4	.006	.005	.006	.007	−1.1	.799
HS GPA 1.5–1.9	.030	.021	.030	.032	−1.6	.716
HS GPA 2.0–2.4	.173	.082	.151	.157	−1.7	.700
HS GPA 2.5–2.9	.190	.147	.191	.187	1.3	.763
HS GPA 3.0–3.4	.338	.315	.346	.340	1.3	.759
HS GPA 3.5–4.0	.203	.382	.212	.208	0.8	.821
Public HS	.876	.823	.871	.867	1.1	.774
Private HS	.070	.132	.072	.070	0.6	.863
Foreign HS	.017	.017	.018	.023	−3.5	.428

Note. CC = community college; HS = high school; AP = advanced placement; GPA = grade point average.

TABLE A2

Matching Statistics for Propensity-Score Matching Routines in BPS (Tables 2 and 4)

	Outcomes	Restrictions	Mean bias	Largest bias	p of p-score	Lowest covariate p
1	All not specified below	Full overlap sample	1.67	4.88	.621	.247
2	Hours worked 2004	Employed students 2004	1.87	5.41	.648	.233
3	Spring 1 credits attempted, credits earned ratio	Attended Spring 1	1.37	4.74	.629	.260
4	Fall 2 credits attempted, credits earned ratio	Attended Fall 2	1.53	5.76	.662	.313
5	Spring 2 credits attempted, credits earned ratio	Attended Spring 2	1.79	7.22	.703	.145
6	Fall 3 credits attempted, credits earned ratio	Attended Fall 3	1.63	4.44	.730	.404
7	Spring 3 credits attempted, credits earned ratio	Attended Spring 3	1.70	6.19	.728	.242
8	Fall 4 credits attempted, credits earned ratio	Attended Fall 4	1.62	7.37	.767	.179
9	Spring 4 credits attempted, credits earned ratio	Attended Spring 4	1.74	7.64	.788	.192

Note. Matching technique: Kernel matching (Epanechnikov kernel, band width = .06), common support imposed, trimming 5% of treatment cases with least support. BPS = Beginning Postsecondary Students.

TABLE A3

Sensitivity Analyses for Key Outcomes in Table 2 Using Mantel–Haenszel Bounds[6]

	Γ = 1.0		Γ = 1.5		Γ = 2.0		Γ = 2.5	
Outcome	Treatment effect	*p*	Treatment effect	*p*	Treatment effect	*p*	Treatment effect	*p*
Earned BA	−9.64	<.001	−5.55	<.001	−2.71	.003	−0.51	.304
BA/still enrolled	−8.79	<.001	−4.57	<.001	−1.65	.049	−.50	.307
Took remedial math	9.40	<.001	5.23	<.001	2.31	.010	0.07	.473
Stop-out	4.62	<.001	0.76	.223	−1.85	.031	−3.97	<.001

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) disclosed receipt of the following financial support for the research, authorship, and/or publication of this article: This research was funded by a grant from the Bill & Melinda Gates Foundation.

Notes

1. Further methodological details about the Beginning Postsecondary Students (BPS) 04/09 are available online at http://nces.ed.gov/pubsearch/pubsinfo.asp?pubid=2012243

2. To identify the selectivity of schools, National Center for Education Statistics (NCES) makes an index that includes two measures: admission rate and the midpoint between the 25th and 75th percentiles of the SAT/ACT equivalized score distribution. "Breaks" in the distribution of this index across institutions are identified, and schools assigned to categories on the basis of these breaks.

3. Variables used for matching: race, gender, age, first language, immigrant generation, parental marital status, household size, household income, household home ownership, household wealth, parental education, high school math, SAT scores, high school grade point average (GPA), high school type, college credits in high school, dependency status, having dependents, and marital status.

4. Variables used in matching: race, gender, age, first language, immigrant generation, parental marital status, household size, household income, household home ownership, household wealth, parental education, high school math, SAT scores, institutional enrollment, institutional racial/ethnic breakdown, tuition, percent of tuition covered by grants, and percent of tuition covered by loans.

5. Our regression model contains controls for race, gender, age, U.S. citizenship, number of dependents, age, age greater than 23, marital status, parental education, years since beginning post-secondary education (PSE), enrollment intensity, months enrolled, years between high school completion and entering PSE, household income, household wealth, household home ownership, institutional selectivity, academic preparation (high school [HS] GPA, SAT score quartile, and highest high school math class), institutional enrollment (logged), out-of-state attendance, institution tuition and fees (logged), high school type, student level (freshman, sophomore, etc.), multi-institutional enrollment, and institution state.

6. Sensitivity analyses calculate treatment effect sizes and associated *p* values under different hypothetical conditions of unmeasured bias. The size of this bias is expressed through the parameter gamma. Specifically, gamma refers to unmeasured bias in the odds of assignment to the treatment group because of one or more unmeasured confounders. The baseline condition is Γ = 1.0, in which we assume no unmeasured bias. Under Γ = 2.0, we assume that there is some set of unmeasured confounders which double the odds of being in the treatment group; we then estimate the true unbiased treatment effect and its significance under this assumption. This permits us to say, for instance, if a significant treatment effect exists Γ = 2.0, that our finding would be robust to an unmeasured bias that doubles exposure to the treatment.

To perform sensitivity tests in Appendix Table A3, we calculate Mantel–Haenszel bounds using user-generated program "mhbounds" in Stata 12 (Becker & Caliendo, 2007). At present, this routine cannot be run after kernel matching; therefore, we recalculated treatment effects using three-nearest-neighbors matching and performed sensitivity analysis. Baseline effect size differences depart substantially from those reported in the article; this could therefore be due to the use of different matching algorithm to generate matched groups and/or to application of non-parametric

Mantel–Haenszel test rather than the difference in matched means produced by "psmatch2."

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Manuscript received May 13, 2013
First revision received October 8, 2013
Second revision received December 30, 2013
Accepted January 2, 2014