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What Is Academic Momentum? And Does It Matter?

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The academic momentum perspective suggests that the speed with which undergraduates initially progress in college significantly affects their likelihood of completing a degree, an effect separate from those of high school academic preparation and family socioeconomic status. Growth curve modeling of undergraduate transcript data reveals that the number of credits attempted in the first semester of college sets a trajectory that influences later chances of degree completion. Several techniques addressing selection bias indicate that delay between high school and starting college, and also attempting a low course load in the first semester (part-time attendance), are associated with lower degree completion, while attending summer school after freshman year is associated with significantly better graduation chances. In sum, the central claims of momentum theory are supported.

Keywords: *college, degree completion, momentum*

Many undergraduates fail to complete a degree, a matter of considerable concern to both policymakers and scholars (Berkner, Cuccaro-Alamin, & McCormick, 1996; Berkner, He, & Cataldi, 2002; Brainard & Fuller, 2010; Carroll, 1989; Horn, 1996; Horn & Berger, 2004). One influential perspective explaining degree noncompletion emphasizes the notion of academic momentum (Adelman, 1999, 2006). This perspective advances three core ideas. The first is that an undergraduate's initial academic course load and progress set a trajectory that strongly influences subsequent degree completion. In particular, an early loss of momentum greatly reduces a student's chances of graduation. Second, this early momentum is associated with degree attainment over and above the effects of a student's sociodemographic background and high school academic preparation; it has an influence of its own. Third, the theory suggests that certain components of academic momentum, such as enrolling in summer courses, may provide practical interventions for improving completion rates.

The momentum perspective offers important insights with implications for policy; however, there are conceptual as well as methodological problems with some momentum ideas and analyses. In this article, we modify and narrow the approach to avoid potential problems of selection bias and causal circularity, and we test the empirical validity of the modified theory, using college transcript data from a nationally representative sample of eighth graders who were tracked into college and beyond. We use alternative techniques to estimate effect sizes for four components of momentum, and we also examine whether these elements have a larger effect for some subgroups of undergraduates than for others (effect heterogeneity). We find empirical support for several but not all of the core ideas, and we discuss implications from our findings for policy.

Defining and Measuring Academic Momentum

Clifford Adelman (1999, 2006) pioneered the idea of academic momentum, noting that undergraduates who proceed through college at a certain rate of speed are more likely to complete their degrees than otherwise similar students who progress more slowly or who interrupt their studies. Using student transcript data from the National Education Longitudinal Study (NELS) of 1988, he identified several forms or phases of academic momentum, such as precollegiate course taking, transitioning immediately into college after completing high school, earning high numbers of credits during the 1st calendar year at college, and enrolling during the summer months in college courses or in other credit-bearing activities. These, he observed, were strongly associated with the probability of degree completion.

Unfortunately, Adelman's conceptualization of academic momentum was sometimes so broad that it was susceptible to problems of causal circularity or endogeneity. For example, college grades were discussed as an element within higher momentum: "Earning grades . . . in the top 40 percent of first-year GPA [grade point average] . . . is a strong—and positive—contributor to academic momentum" (Adelman, 2006, p. xxii). Analytically, we think it desirable to separate potential causes (momentum itself) from its possible effects (student performance) and to avoid conflating the two. This means a narrowing or shift in emphasis in our conceptualization of academic momentum, compared to his. Adelman (1999, 2006) treated academic momentum as a global measure of how well a student was doing in college, encompassing the amount of coursework taken, how well the student was performing in those courses (grades), and the trajectory over time (improvement in grades, increasing numbers of credits earned). He also studied momentum over the 1st calendar year of college. As detailed below, we narrow our analyses to only part of this process, looking at (a) delays between high school and college, (b) the course load attempted during the first semester of college, and (c) the effects of a student taking courses in the summer at the end of the 1st year of college.

Another methodological issue concerns selection bias: Students who delay entry to college, or who attend summer school, or begin college part-time, tend to differ on various background characteristics from those who do not. Hence, analyses of academic momentum should address selection issues. The potential for selection bias is a key challenge when estimating the causal effect of any of the aspects of academic momentum. Conventional multiple regression analysis with controls is nowadays viewed as an inadequate antidote to selection bias, but considerable caution must also be exercised in using formal techniques of causal inference (Morgan & Winship, 2007). In this article, we use several alternative techniques to formally address selection bias and generate multiple estimates of the magnitude of differences associated with different sorts of momentum.

The momentum framework that Adelman (1999, 2006) provided was mainly descriptive and correlational: His primary goal was to demonstrate that academic momentum was empirically associated with later degree attainment. He emphasized that his findings should not be read as implying causation. He did not theorize extensively about why initial momentum might be so important for the longer term. However, on a theoretical level, momentum may be related to several mechanisms. One, echoing Tinto's (1993) neo-Durkheimian approach, would posit that taking more courses is more likely to integrate the individual into the common life of students or to allow the individual to share college culture in deeper ways than a more occasional or part-time student might. In this view, integration is a mediating variable between momentum and degree outcomes and generates a level of commitment. A second plausible mechanism is primarily psychological, arguing that the experience of competence and accomplishment at the beginning of a college career enhances self-efficacy and/or academic self-concept, both of which are important for persistence in specific academic tasks as well as toward the longer goal of earning a degree (Bandura, 1997, 2001; Marsh & O'Mara, 2008; Zimmerman, 2000).

A third set of mechanisms might be thought of as life issues that prevent an undergraduate from studying full-time or taking a full course load. These include matters such as the adequacy of financial aid, having family responsibilities while a student, and undertaking regular paid employment while a student. These factors have all been linked to retention and degree completion, although they have not been linked specifically to the early momentum construct (Braxton, 2000; Kuh, Kinzie, & Schuh, 2010; Perna, 2010; Seidman, 2005; Tinto, 1993).

These various mechanisms are by no means mutually exclusive or exhaustive. In this article, we do not attempt to test mechanisms or their relationship to momentum. Instead, we focus on the logically prior question: To what extent does academic momentum early in an undergraduate's college career predict the student's later degree completion?

Data and Variables

We analyze data from NELS:88/2000, which followed a representative U.S. national cohort of eighth graders beginning in 1988, with follow-up and supporting data as late as spring 2001. For those who entered college on a typical schedule, the NELS data set contains college outcomes for over 8 academic years past the traditional fall freshman term. NELS staff members attempted to collect course-level college transcripts for all college students in the sample reflecting up to 8.5 years after entry. This is available as a restricted data set from the National Center for Education Statistics.

For this study, we limit our sample to those undergraduates for whom full postsecondary transcripts were obtained (Chen & Carroll, 2005). We omit any cases in which the degree level of the first institution is unknown or is less than a 2-year associate degree level. With the exception of the analysis of delayed entry to college, we include in the sample only students who were attending an associate- or baccalaureate-granting institution by fall 1992, the traditional semester of entry for this age cohort. To isolate the impact of delayed entry, however, we widened the inclusion criteria to any person who began college-level work by fall 1995 (provided that he or she had also graduated from high school by fall 1995). This expanded sample is also used in the initial growth-curve specification of momentum effects. Missing data values on the covariates were imputed through a multivariate chained equation method.

Table 1 provides descriptive statistics on the covariates and outcome variable used in the analyses that follow.

One important methodological issue is how to conceptualize and measure momentum. Our measure differs in important respects from Adelman's (1999, 2006). The core idea is similar: The number of courses an undergraduate takes in a given time period is an indicator of that student's academic momentum. Beyond this, we faced several measurement decisions. First, should a momentum measure count only courses passed, or should it count courses attempted? In our thinking, momentum is an aspect of the attempted course load a student undertakes. Whether the student succeeds in passing all courses attempted is a separate and of course important issue, and it is potentially an outcome of momentum, but it is not itself a measure of momentum. Our momentum measure is therefore a count of coursework attempted.

This raises the issue of how to deal with course withdrawals and failed courses. We included withdrawals and failures in our measure of the course load attempted in the first semester. From one perspective, this might seem to overstate the credits attempted for those students who withdraw from courses. If that is the case, that would create a conservative bias, making it harder to find a difference between students who had a low-credit versus higher credit course load. Because, as we will document, we do find a significant difference, despite potentially overstating "attempted" in the case of withdrawals, we feel confident in our observed credit attempted findings.

A related issue is whether a momentum measure should include remedial and developmental courses. We decided to count all courses attempted in the first semester of college—both regular for-credit courses and noncredit courses (including remedial courses)—in our measure of first semester momentum. Remedial or developmental coursework is widespread: About one third of all undergraduates at 4-year colleges and 58% of students at 2-year colleges take some remedial coursework (Adelman, 2006, p. 34; Attewell, Lavin, Domina, & Levey, 2006). About one in four undergraduates at public 2-year and public

TABLE 1
Descriptive Statistics

Variable	*M* or Proportion	*SD*	Minimum	Maximum
Two-year college entrants (weighted n = 2,570)				
Asian	0.04	0.19	0	1
Hispanic	0.13	0.33	0	1
Black	0.10	0.30	0	1
Female	0.51	0.50	0	1
Parents less than high school	0.08	0.27	0	1
Parents high school	0.19	0.40	0	1
Parents bachelor's degree	0.14	0.34	0	1
Parents master's degree	0.06	0.24	0	1
Parents professional degree	0.03	0.18	0	1
Parent SES at 8th grade	−0.07	0.68	−2.23	1.78
Free lunch in 8th grade	23.72%	23.10%	0%	100%
High school GPA	−0.53	0.88	−4.03	2.19
12th grade math test	−0.57	0.90	−2.75	2.00
12th grade history test	−0.44	0.96	−3.11	2.33
Delayed entry to college	0.30	0.46	0	1
First term part-time	0.46	0.50	0	1
First term high credits	0.10	0.30	0	1
Attended first summer	0.17	0.38	0	1
Associate's or higher degree by 2001	0.31	0.46	0	1
Associate's or higher degree within 5 years	0.23	0.42	0	1
Four-year college entrants (weighted n = 4,300)				
Asian	0.05	0.21	0	1
Hispanic	0.07	0.25	0	1
Black	0.10	0.30	0	1
Native American	0.01	0.07	0	1
Female	0.52	0.50	0	1
Parents less than high school	0.04	0.18	0	1
Parents high school	0.11	0.31	0	1
Parents bachelor's degree	0.24	0.43	0	1
Parents master's degree	0.17	0.38	0	1
Parents professional degree	0.09	0.28	0	1
Parent SES at 8th grade	0.33	0.69	−2.08	2.30
Free lunch in 8th grade	18.86%	21.67%	0%	100%
High school GPA (z score)	0.30	0.87	−4.32	2.93
12th grade math test (z score)	0.31	0.89	−2.46	2.54
12th grade history test (z score)	0.24	0.95	−2.72	3.08
Delayed entry to college	0.08	0.27	0	1
First term part-time	0.18	0.39	0	1
First term high credits	0.15	0.36	0	1
Attended first summer	0.23	0.42	0	1
Bachelor's or higher degree by 2001	0.68	0.47	0	1
Bachelor's or higher within 5 years	0.56	0.50	0	1

Note. GPA = grade point average; SES = socioeconomic status. Weighted data. For matching analyses other than delayed entry, the sample is restricted to those who entered without delay. Sample sizes have been rounded to the nearest 10 to comply with National Center for Education Statistics data security regulations. For dummy variables, the mean value can be read as a proportion. For example, a mean of 0.04 for Asian means that 4% of this sample was Asian.

nondoctoral 4-year colleges took remedial or developmental courses during their 1st year of college in 2007 and 2008 (Aud et al., 2011). Given how common developmental or remedial coursework is during the 1st year of college, we included remedial courses in our measure of first-semester course load attempted and tried to harmonize the various ways these credits are treated. If we had omitted remedial courses from our measure of course load attempted, this could have confounded or combined the effect of low course load per se with the effect of having to take remedial coursework. If we had then found that there was an effect of "low credits," this could be due either to a student's taking few courses or to taking many courses, some of which did not "count" as momentum because they were remedial. We avoided this problem by counting remedial courses along with regular courses as "courses attempted" in our measure of first-semester course load and making the following adjustment.

On the NELS transcripts, some failed courses and many remedial courses are simply reported as zero credits. Given the logic we have just discussed, such types of activities should count equally as credits attempted. Thus, we assigned to any failed or noncredit courses with zero credits the mean of the nonzero credits or hours values for that type of course; these were 2.66 credits for remedial courses and 2.88 credits for failed courses. As a result, the total number of credits attempted, as used in momentum analyses described below, is the sum of all nonzero credits listed on a transcript plus these substitutions for any attempted courses listed at zero credits. We separate 2-year and 4-year entrants for modeling purposes, because past research suggests sizable differences in their college trajectories (e.g., Long & Kurlaender, 2009); however, we count credits from every institution attended by each student, and thus none of our results are from a single-institution perspective.

The credit-hour measures we constructed do not include Advanced Placement or International Baccalaureate courses, which are typically taken in high school but are credited toward degree requirements by some colleges.

Types of Momentum

In the following analyses, we test four separate categorical indicators of momentum (in quasi-experimental jargon, the "treatments"). To avoid confounding the measurements, all except the first are constructed only for students who entered college without a delay, as discussed below.

1. For students in the NELS cohort who graduated high school early or on time (e.g., by summer 1992), we counted as not delayed for college entrance anyone who began college at any point in the 1992 calendar year. Conversely, anyone in that group who entered college in 1993 or later was counted as delayed entry to college. However, a minority of students in the cohort graduated late from high school. For those students, the dummy variable for delayed entry is defined as a lag of more than 4 months between finishing high school and beginning college. Approximately 17% of the combined associate and baccalaureate sample had a delay in entering college. The median starting date among those with a delay was fall 1993. Having a delay is coded 1 and all others 0.
2. Attending part-time in the first semester means attempting fewer than 12 credits or hours (including hour estimates for remediation and other not-for-credit courses) in fall 1992. A standardized semester-equivalent measure of credit hours provided by the NELS data set is used, so our measure takes into account differences for undergraduates attending schools on quarter or trimester calendars.
3. Taking a high course load in the first semester of college is calculated following the same approach as the part-time indicator. The indicator is coded 1 if the student attempted 18 or more credits in fall 1992 and 0 otherwise.
4. Students who enrolled in college coursework during the first summer after freshman year were coded 1, and all others were coded 0. The measure is whether the student attempted any college course, regardless of whether credits were earned, during summer 1993. This summer construct requires persistence to a certain point in time, so we construct a narrower control sample to make appropriate comparisons for summer attendance. The definition of the "treated" sample for the summer analyses includes anyone who began college without delay in fall 1992 and also took at least one course in the summer of 1993 and persisted into the next year. The comparison group contains students who also persisted into their 2nd year of college but did not take summer courses at the end of their 1st year of college.

Dependent Variables

Degree attainment is the primary outcome we focus on. For most analyses, the dependent variable is ever attaining (within the 8.5 years observed) a bachelor's degree or higher among 4-year program entrants or an associate's degree or higher for 2-year entrants. In one case, for the analysis of delayed entry, we constructed a 5-year degree indicator relative to their first date of attending college (between fall 1992 and fall 1995), with separate outcomes for baccalaureate and associate entrants. This particular measurement strategy was necessary to provide an even basis of comparison so that earlier college entrants are not in the "risk pool" of graduating for longer than later entrants (Scott & Kennedy, 2005). Finally, for our growth-curve analyses of academic momentum, the dependent variable is cumulative credits earned from all sources, except transfers, until a bachelor's degree is earned (if ever); degree attainment is not part of this measure otherwise. Transfer credits are excluded to prevent double counting, because there is normally an additional transcript available from the original institution at which the credits were earned.

Preliminary Analysis: A Growth-Curve Model of Momentum

Perhaps the simplest and most natural way to think about academic momentum in college is to consider students in different bands of momentum, measured at the very start of their undergraduate careers, and then observe any differences in their trajectories toward completing a degree in subsequent years. This conceptualization is amenable to a multilevel model formulation, with academic terms nested within students, where the outcome is cumulative credits earned at each date observed. If students in the different initial momentum bands have different typical curves as opposed to intercepts—in particular after a few years have passed—this would be evidence for one sort of momentum "effect" (although clearly not causal evidence at this stage).

We fit the following random-intercept model separately by degree level:

$$Y_{ij}^{\text{CREDITS}} = \zeta_j + \alpha_{k_0} + \alpha_{k_1} t_{ij} + \alpha_{k_2} t_{ij}^2 + \alpha_{k_3} t_{ij}^3 + \beta_q x_{qj} + \varepsilon_{ij}$$

for each student j having random intercept ζ, with t as time elapsed since starting college as of term i, where k represents separate coefficients for each quintile of first-term momentum. The α coefficients collectively describe the shape of the curve over time. In addition, q time-invariant controls for background differences include indicators for gender and race, an index of socioeconomic status (SES), standardized high school GPA, and standardized 12th grade math and history test scores.

Figure 1 presents the results of this model separately for 2-year and 4-year degree entrants. The five momentum bands are approximate quintiles of credits or hours attempted during the first semester in college. Person-term records in which cumulative credits equal 0 or exceed 150 have been removed from the analysis. Up to 6 years following the postsecondary entry date (year 0) are considered. To account for potential nonlinearities, each of the time parameters (linear, squared, and cubed) is allowed to differ by momentum band. The full regression results are shown in Table 2.

Lending support to the momentum hypothesis, it appears that students in the higher levels of credits attempted in their first semester continue to take and earn higher amounts of credits in later semesters, and thus the gap between the highest and lowest momentum bands widens as time passes, at least to a point. For students entering 4-year colleges, the parameter estimates suggest that the lowest momentum group has a significantly lower intercept and slope; in addition, the upper two groups appear to have higher linear slopes than the lower three. There is no clear pattern of difference in the nonlinear terms in the 4-year entrants' model, implying that the shape of the curve does not differ across groups.

Among 2-year college entrants, the bottom initial momentum quintile appears to fall substantially behind all the other levels, while there appears to be much smaller differences among the higher bands. The downward arching curve in later years, especially among 4-year entrants, likely reflects a subset of students who take time off from studies or have highly fluctuating credit loads or other special circumstances; the data indicate that those who remain in college more than 5 years or so have slower credit accumulation trajectories than their peers in earlier years, who finish and depart the scene (or move into graduate study, which we

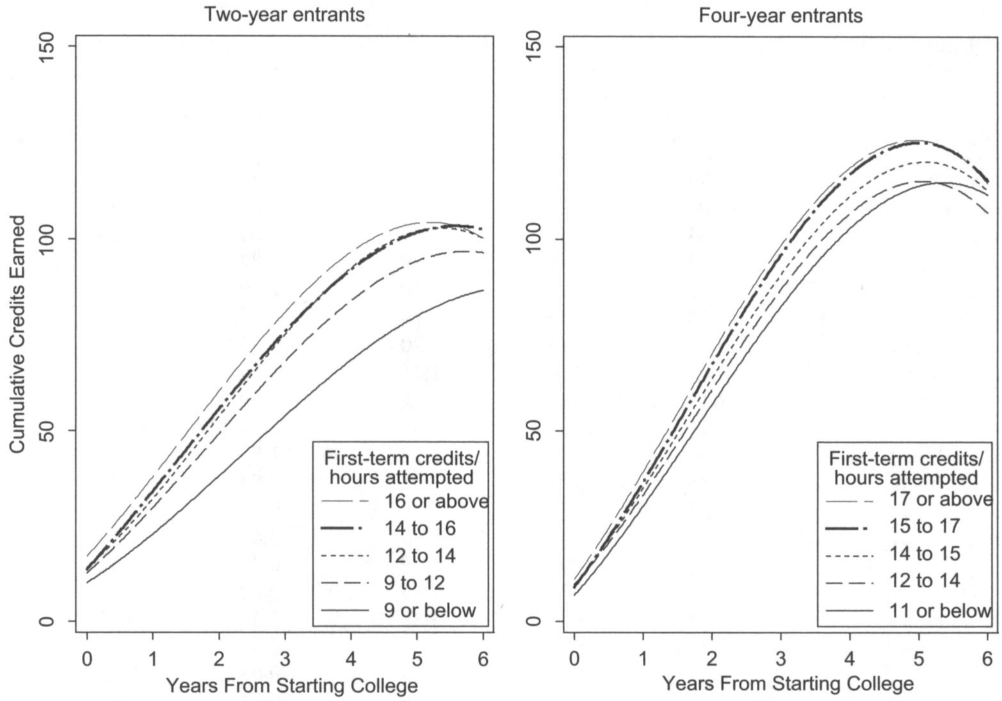

FIGURE 1. *Credit accumulation growth curves.*

exclude here). The gaps between the groups also appear to narrow again in the later years, conditioned on still attending college. (The curves say nothing directly about who stops or drops out.)

The growth models suggest that a systematic difference between momentum groups remains after simple statistical controls. It may be that refinements would produce a better model. Our purpose at this stage is to show that one version of the momentum thesis is plausible using a simple modeling strategy. In particular, it appears that the low end of the momentum scale is associated with a distinct pattern, especially at the associate's degree level. However, so far this does not take into account the possibility of selection bias, which could explain all of the patterns observed.

Methods for Addressing Selection

A wide range of methods have been proposed to adjust for selection effects (i.e., when not all members of a sample have equal chances of being in the treated vs. the control group). To the extent that the two subsamples have considerable overlap on observed characteristics, statistical techniques such as propensity-score adjustments and other forms of multivariate matching can reduce the bias of estimates due to observables and provide a more accurate inference about any "treatment effect" that may exist. When certain conditions are met, including achieving statistical balance on a range of substantively important covariates, it is argued that they permit researchers to make causal inferences (e.g., Morgan & Winship, 2007).

A drawback to these methods is that in most realistic applications the observables present a multivariate problem of high dimension, and exact one-to-one matching of cases with controls is seldom feasible without ignoring the vast majority of the data collected. In addition, the model for receiving the treatment is often not known and may not be readily theorized, so reaching an acceptable level of balance on the covariates is not guaranteed. As a result, analysts using propensity-score methods often consider several specifications of propensity models and test both the covariate balance and the model dependency of the matched result. As Morgan and Winship (2007) showed, a poorly specified propensity-based or distance-metric model may

TABLE 2

Credit Accumulation Growth-Curve Model by Quintiles of First-Term Credits or Hours Attempted

	Two-Year Entrants			Four-Year Entrants		
Variable	Coefficient	*SE*	*t* Ratio	Coefficient	*SE*	*t* Ratio
Q3 intercept	**14.07**	1.47	9.60	**9.50**	0.72	13.24
Q3 linear time	**15.03**	1.37	10.96	**22.20**	0.63	35.09
Q3 time squared	**3.64**	0.60	6.07	**4.09**	0.29	14.33
Q3 time cubed	**–0.63**	0.07	–8.80	**–0.82**	0.04	–23.22
Q1 vs. Q3 intercept	**–3.89**	1.77	–2.20	**–2.59**	0.84	–3.07
Q2 vs. Q3 intercept	–1.33	1.69	–0.79	0.25	0.87	0.29
Q4 vs. Q3 intercept	–0.35	1.76	–0.20	–0.43	0.89	–0.48
Q5 vs. Q3 intercept	3.03	1.77	1.71	1.64	0.88	1.86
Q1 vs. Q3 linear	**–4.14**	1.85	–2.23	**–1.80**	0.84	–2.16
Q2 vs. Q3 linear	0.11	1.74	0.06	**–2.39**	0.86	–2.76
Q4 vs. Q3 linear	**3.99**	1.83	2.18	**1.86**	0.89	2.08
Q5 vs. Q3 linear	**3.88**	1.86	2.08	**2.83**	0.89	3.19
Q1 vs. Q3 squared	–1.51	0.81	–1.87	–0.42	0.37	–1.12
Q2 vs. Q3 squared	–1.26	0.76	–1.65	0.45	0.39	1.17
Q4 vs. Q3 squared	–1.83	0.80	–2.29	0.24	0.41	0.58
Q5 vs. Q3 squared	–1.23	0.82	–1.49	–0.09	0.41	–0.21
Q1 vs. Q3 cubed	**0.32**	0.10	3.38	**0.13**	0.05	2.76
Q2 vs. Q3 cubed	**0.20**	0.09	2.17	–0.04	0.05	–0.79
Q4 vs. Q3 cubed	**0.21**	0.09	2.19	–0.08	0.05	–1.50
Q5 vs. Q3 cubed	0.08	0.10	0.85	–0.06	0.05	–1.23
Asian	0.66	1.62	0.41	**2.31**	0.76	3.05
Hispanic	**–5.28**	1.33	–3.97	**–3.83**	0.92	–4.18
Black	–3.26	2.07	–1.57	0.07	0.89	0.08
Female	**2.18**	0.92	2.36	**3.34**	0.48	7.02
Parents SES	–0.06	0.72	–0.08	**3.34**	0.35	9.69
History test score	0.43	0.58	0.73	**2.06**	0.32	6.48
Math test score	**3.18**	0.63	5.02	**2.85**	0.37	7.71
High school GPA	**4.89**	0.60	8.21	**3.16**	0.33	9.53
n (person-terms)	13,060			38,220		
Level 2 units	1,740			4,160		

Note. GPA = grade point average; Q = quintile; SES = socioeconomic status. Dependent variable is cumulative credits earned from all sources. *t* ratios with absolute values of 1.96 or greater are statistically significant at the .05 level. Statistically significant coefficients are indicated by boldface type. Sample sizes have been rounded to the nearest 10 to comply with National Center for Education Statistics data security regulations.

produce more biased estimates than simple regression.

In this article, we use three different matching techniques. Recent research has aimed to find more systematic solutions to matching problems by incorporating a criterion of maximal balance directly into the solution. We follow one such method offered by Hansen (2004), who uses a form of "optimal matching" (see also Rosenbaum, 1989) that locates globally best-fitting treatment-control strata by considering the relative amount of imbalance given alternate combinations; his result suggests this method is an improvement, for instance, over simple "nearest-neighbor" techniques. (Other elaborations in this area include "genetic matching" by Diamond & Sekhon, 2008; "coarsened exact matching" by Iacus, King, & Porro, 2009; and "synthetic matching" by Hainmueller, 2010.) Optimal matching aims to reduce the model dependency of a particular result and in general retains the entire sample in the analysis.

To evaluate the results of the first method, two additional matching methods were used. The first involves weighting on the predicted odds of treatment (Morgan & Todd, 2008). In this procedure, treated cases are all weighted equally (or, more accurately in this case, according to their original sampling weights), but control cases receive a weight in proportion to their predicted likelihood of receiving the treatment as determined by a binary regression. With this approach, it is customary to check for balance on covariates and, if needed, respecify the binary regression to improve balance; we went through several rounds of such model refinement for the treatments we present.

The second confirmation procedure we perform is weighting on the basis of the estimated propensity score using local linear regression smoothing (Heckman, Ichimura, & Todd, 1997, p. 627). This technique is similar to the previous one in that all of the treated cases are given a fixed weight; however, case weights for the controls are constructed using a smoothed nonparametric function of the estimated propensity score. In both of these methods, we imposed a common support rule that excludes cases from either the treatment or control group that were outside the range of shared propensity scores to avoid extrapolating from very dissimilar cases.

The literature on adjustments for selection bias rarely discusses the use of sampling weights with such techniques; generally, this appears justified in that the matched samples are deliberately not representative of the broader population. On the other hand, it is possible in stratified samples that weights contain useful information about the composition of the relevant subgroups. For these analyses we have incorporated sampling weights into the predicted odds of treatment weights, but we have omitted weighting from the optimal and local linear regression matches.

Using each method, we matched separately for each of our four dichotomous momentum treatments at both degree levels. A rich set of 27 student academic and personal background covariates was used in all the optimal matches; because balancing was more difficult for propensity score matching than for the other techniques, the propensity models varied more, and some contained upward of 70 covariates and interaction terms. A list of the covariates used in the matching process (though not all interactions) is provided in Appendix A (available online at http:/epa.sagepub.com/supplemental).

These alternative matching techniques yielded similar results (reported below), in some cases with somewhat different estimates of effect size, and those probably reflected the slightly different quality of the matches in particular contexts, as discussed below. We did not find that any one technique clearly outperformed the others, so we have kept our discussion of this methodological aspect fairly brief and concentrated on the substantive findings. We report results from all three techniques in this article, nevertheless, because observing substantively similar results using three different matching techniques provides some added assurance, analogous to replication.

The three methods described above each use different methods to match treated with untreated cases (but not 1:1, given their use of weights); thereafter, one uses these matched cases to estimate the effect of treatment on an outcome. Our first analyses report the estimated average treatment effect on the treated (ATT), which reflects the net association between each momentum treatment and graduation for those who received each treatment. In later analyses, we move beyond the ATT to consider heterogeneity across groups in the size of the treatment effect, asking whether the effect size differs for men and women, according to high school preparation, or comparing low-SES and higher SES students.

The ATTs in tables below should be interpreted as the average difference or gap in college graduation rates, measured in percentage points, between treated and untreated individuals (e.g., those who delay entry to college vs. those who do not delay). The ATT is measured after balancing the comparison groups on such characteristics as gender, race, marital status, parents' SES, parents' education, high school GPA, high school test scores, eighth grade school poverty level, eighth grade school minority composition, type of high school, academic rigor of high school program, degree aspirations, whether the student was ever held back a grade, and indexes of personal self-esteem and locus of control. (The exact model differs somewhat by treatment on the basis of balance criteria.)

A significance test is associated with each ATT value; it reports whether the observed ATT estimate is statistically different from zero.

When interpreting results of any matching analysis, it is important to consider how

TABLE 3
Results From Optimal Matching

	Matched Estimates		Balance Quality	
Variable	Average Treatment Effect on the Treated	Significance	Mean Absolute Standard Bias	Minimum p Value on Covariates
Four-year entrants				
Delay before entry	−9.0%	.000	0.022	.305
Part-time first term	−4.6%	.001	0.013	.448
High credits first term	0.6%	.552	0.013	.344
Attending first summer	4.3%	.007	0.013	.410
Two-year entrants				
Delay before entry	−7.9%	.000	0.012	.396
Part-time first term	−9.8%	.000	0.017	.314
High credits first term	−1.0%	.088	0.018	.496
Attending first summer	15.7%	.000	0.020	.414

Note. Dependent variable is earning a degree in 5 years. All other outcomes listed here are measured for those with no delay, and the outcome is a degree within 9 years. The minimum p value indicates the most significant t test on all the adjustment variables between treatment and matched control.

successful a match was achieved. Two measures are conventionally used to assess the quality of a match. One is known as a standard bias. This statistic measures, separately for each predictor or covariate, the difference in mean values on that predictor between the treated and untreated group, measured in standard deviation units. A good match should have low standard biases, implying that the treated and untreated groups are almost exactly balanced; they have near identical mean values on these covariates. Because matching algorithms often use many predictors, instead of reporting one standard bias for each and every predictor, it is common to report the mean standard bias across all predictors. The smaller the mean standard bias after matching, the better is the quality of the match.

An alternative way for reporting the quality of a match is to determine whether the mean value of the treated cases on a covariate is statistically significantly different from the mean value of the untreated cases on that covariate. For each covariate, these means can be compared using a t test and its associated p value. In this case, a good match is one that has no statistically significant differences on any covariate between treated and untreated. This is conventionally represented by reporting the minimum p value across all covariates. A good match will have a minimum p value well above .05, meaning that there are no covariates for which the treated and untreated are statistically significantly different. Both the mean standard bias and the minimum p value for covariates are reported in tables below, alongside the ATT.

For this article, we examined four treatments, for two types of college (2 and 4 year), with three matching methods: 24 analyses in all. Tables summarizing these 24 analyses are provided below.

Findings

Tables 3 to 5 summarize the results of each of the different matching techniques. We discuss the findings for each of the four aspects of momentum separately.

Delay Between High School and College

On average, students who delay between graduating high school and entry to college have weaker academic preparation and come from lower SES families (Horn, Cataldi, & Sikora, 2005). Matching analyses are intended to reduce the effects of these background differences and estimate the effect of delay per se on graduation rates. In most of the estimates, we find sizable shortfalls in graduation rates among students who delay entry to college, even after attempting to level the playing field by considering 5-year graduation rates relative to the student's starting date in postsecondary education, and even after statistically controlling for differences in family background and academic preparation via matching. Table 3 shows that delayed enrollment is

TABLE 4
Results From Predicted Odds of Treatment Weighting

	Matched Estimates		Balance Quality	
Variable	Average Treatment Effect on the Treated	Significance	Mean Absolute Standard Bias	Minimum p Value on Covariates
Four-year entrants				
Delay before entry	−8.9%	.145	0.028	.004
Part-time first term	−6.0%	.036	0.010	.312
High credits first term	−0.6%	.839	0.005	.683
Attending first summer	5.3%	.005	0.008	.451
Two-year entrants				
Delay before entry	−7.6%	.004	0.016	.290
Part-time first term	−13.2%	.000	0.020	.204
High credits first term	9.7%	.043	0.016	.322
Attending first summer	7.2%	.229	0.032	.108

Note. Dependent variable is earning a degree in 5 years. All other outcomes listed here are measured for those with no delay, and the outcome is a degree within 9 years. The minimum p value indicates the most significant t test on all the adjustment variables between treatment and matched control.

TABLE 5
Results From Local Linear Regression Weighting

	Matched Estimates		Balance Quality	
Variable	Average Treatment Effect on the Treated	Significance	Mean Absolute Standard Bias	Minimum p Value on Covariates
Four-year entrants				
Delay before entry	−13.7%	.001	0.033	.156
Part-time first term	−7.0%	.001	0.015	.234
High credits first term	7.1%	.121	0.026	.219
Attending first summer	10.7%	.001	0.029	.131
Two-year entrants				
Delay before entry	−12.5%	.000	0.021	.101
Part-time first term	−8.0%	.002	0.012	.423
High credits first term	6.7%	.186	0.020	.246
Attending first summer	11.0%	.001	0.017	.485

Note. Dependent variable is earning a degree in 5 years. All other outcomes listed here are measured for those with no delay, and the outcome is a degree within 9 years. The minimum p value indicates the most significant t test on all the adjustment variables between treatment and matched control.

associated with nearly a 9 percentage point gap in bachelor's degree attainment among 4-year college entrants and about an 8-point gap in attaining an associate or higher degree among 2-year entrants, using estimates based on optimal matching. Those findings are consistent with those of Horn et al. (2005), who also reported delay effects in a broader postsecondary sample, albeit without corrections for selection. The estimates in Table 4 of the negative effect of delayed entry to college on graduation using propensity matching were very close to the estimates using optimal matching and were similarly statistically significant. The estimates of the delayed entry effect were larger in Table 5, using local linear regression weighting, but again, the results showed a significant negative effect of delay on graduation probability.

The balance achieved in the 4-year college propensity model of delay in Table 4 was poor, even after attempting a variety of modeling strategies. The poor balance achieved in that model raises the more basic question of which students are likely to delay. For 4-year entrants, unmatched data show that students who delay are disadvantaged on virtually every covariate we test. They have lower incomes, lower parental education, weaker high school grades, less advanced high school curricula, greater rates of poverty in their schools, lower self-esteem, and longer working hours and are more likely to be Black or Hispanic. Given this distinctive profile and the fact that several terms in the propensity model related to disadvantage failed to balance well, it is very much in doubt whether this particular model in Table 4 should be considered an acceptable match. This is despite testing a much larger set of covariates, higher order terms, and interactions related to the most consistently imbalanced covariates. A model of some 100 theoretically and empirically promising covariates, transformations, and interactions failed to achieve balance better than the simpler one presented here, as did a stepwise model to select only relatively significant terms. But the matching for delay using the other two techniques (Tables 3 and 5) was much better, and all three methods yielded a consistent finding.

We caution that the NELS sample is a single cohort of students who were all in the eighth grade in 1988 and that the delayed entrants in the NELS were all in their late teens or early 20s. Readers should therefore not extrapolate from our findings about delay to much older students. Our findings suggest that delay between high school graduation and college is associated with lower graduation even among these relatively young undergraduates. Horn et al. (2005) reported effects for older long-delayed students, though their analyses did not address selection issues.

In addition, it should be noted that a 5-year graduation rate at the baccalaureate level, as is measured here, implies relatively rapid progress toward a degree. Our finding does not preclude the possibility that if we had data with a longer degree attainment time frame that the graduation differences associated with delay might differ.

Taking Few Credits in the First Semester

On average, students who begin college part-time and take lower than average course loads during their first semester have weaker academic preparation and come from lower SES families (Carroll, 1989; Chen & Carroll, 2005; Appendix B (available online at http:/epa.sagepub.com/supplemental)). Matching analyses are intended to reduce the effects of these background differences and estimate the effect of taking few credits per se on graduation rates. Much as the growth curves suggested, we found that college students who enroll part-time or take fewer than 12 credits in their first semester of college remain behind their college peers for a long duration. Statistical controls for selection still result in a finding that students who begin their college careers by enrolling part-time have significantly lower degree completion rates than full-time students. The three matching techniques (reported in Tables 3 to 5) balanced the covariates well and delivered similar estimates at the 4-year level, where the estimated graduation shortfall within 8.5 years ranged from 4.6 to 7 percentage points. A larger result was found among 2-year entrants, ranging from 8 to 13.2 percentage points lower probability of earning an associate or higher degree within 8.5 years. Although these models do not distinguish between attrition versus slow accumulation, it appears that part-time entry is associated with worse long-term degree outcomes, even after controlling for the types of student characteristics associated with part-time enrollment.

Taking High Credits in the First Semester

If enrolling part-time during one's first semester of college is associated with lower chances of graduation, one might expect an opposite and positive effect from taking high numbers of credits (18 or more) in one's first semester. In our sample approximately 10% of 2-year entrants and 15% of 4-year entrants took such a heavy course load.

However, our analyses provide little evidence for such a benefit. Among 4-year entrants, none of the three techniques found a statistically significant difference in graduation for students who enrolled in 18 or more credits. Among entrants to 2-year colleges, only one of the three techniques, predicted odds weighting in Table 4, found a significant 9.7 point advantage ($p = .04$). This single discrepancy raises the problem of possible model dependence of the results.

In sum, although there are negative associations between graduation and with low-momentum behaviors, there does not appear to be a gain associated with students taking many credits at the beginning of their college careers. Another reading of this finding, though, is that there is no measured disadvantage to taking more credits; the evidence does not suggest any "burnout" effect from overcommitment, for instance.

Coursework in the Summer After Freshman Year

The fourth type of momentum concerns coursework taken during the first summer after the conventional freshman year. We examined whether this was associated with a higher graduation rate, after controlling for covariates and for self-selection processes. Because this notion of momentum includes a timing and persistence component, the comparison we made is to students who remained in college at least one term into their 2nd academic year. (It is easy to find a much larger "summer effect" if care is not taken to qualify the comparison samples on the basis of persistence.) After controlling for these potential confounding conditions, we consistently find evidence of a summer effect, but the size of the effect varies. For 4-year entrants, the estimate ranges from as little as 4.3 percentage points to as much as a 10.7 percentage point difference in graduation rates, statistically significant in each case. Among 2-year entrants, who tend to take summer classes at a lower rate, the disparity of estimates is also large, from 7.2 percentage points to nearly 16 percentage points, as indicated in Tables 3 to 5. The lowest estimate did not reach conventional statistical significance, but the balance on covariates was also worse on that model.

The divergence in the size of estimates suggests either model dependency or lingering imbalances, but the consistent finding is that enrolling in college classes during the summer after one's freshman year is associated with a higher probability of graduation, after controlling for students' background characteristics and after balancing to minimize selection bias.

Heterogeneity of Effects

The analyses presented so far report the average effect of each treatment across a whole student population. Overall adjustments for selection bias do not preclude the possibility of different effect sizes of momentum across relevant subpopulations. In Table 6, we report findings for three types of potential heterogeneity: gender, SES, and high school preparation. For gender, we simply compare separate models for men and women. For SES, we divide the index of 8th grade family SES into halves to compare higher versus lower SES students; for high school preparation, we take the sum of four standardized NELS tests (math, reading, history, and science) measured in 12th grade and divide the combined score into halves to compare higher versus lower academic preparation.

Among undergraduates entering 4-year colleges, delay has a larger negative effect on women than on men and on lower SES students than on higher SES students (Table 6). The negative effects on graduation of starting college with a lower course load also appear greater for lower SES and for academically less well prepared students. There are some indications that attending summer school has a larger beneficial effect for women than men and for less academically well prepared students than among better prepared students. There was not a clear pattern of heterogeneity in effect sizes among entrants to 2-year colleges on the same dimensions, however.

Adelman's (1999, 2006) analyses of momentum found that race was not significantly associated with degree completion, after comprehensive controls were added for academic preparation and family socioeconomic background. We could not rigorously assess the heterogeneity of momentum effects across race in our matched models, because of sample size issues, but at first impression, we found a similar pattern to Adelman, a lack of racial differences. This contrasted with the clear heterogeneity associated with SES and academic preparation differences in 4-year colleges.

Discussion and Policy Implications

The academic momentum perspective suggests that the speed with which undergraduates progress during the early phase of college significantly affects their likelihood of completing a degree. Our analyses confirm that the momentum perspective is indeed worth serious consideration. Our growth model indicated that an undergraduate's momentum in his or her first semester predicts the student's trajectory in later years. Three of the four speed and timing issues we examined

TABLE 6

Effect Heterogeneity

	Estimated ATT for Women	Significance	Estimated ATT for Men	Significance	Estimated ATT for Lower SES	Significance	Estimated ATT for Higher SES	Significance	Estimated ATT for Lower Preparation	Significance	Estimated ATT for Higher Preparation	Significance
Four-year entrants												
Delay before entry	−15.2%	.000	−5.5%	.013	−24.1%	.000	−0.7%	.054	−9.7%	.000	−8.3%	.002
Part-time first term	−3.6%	.012	−5.7%	.006	−7.9%	.030	−4.4%	.007	−9.5%	.010	−4.9%	.031
High credits first term	3.4%	.204	−0.4%	.783	3.2%	.196	−0.9%	.771	6.7%	.296	0.4%	.710
Attending first summer	6.8%	.000	1.2%	.520	4.4%	.062	4.9%	.002	7.5%	.002	3.7%	.098
Two-year entrants												
Delay before entry	−7.6%	.001	−4.3%	.069	−7.4%	.001	−10.7%	.006	−5.2%	.004	−12.4%	.005
Part-time first term	−6.1%	.020	−11.3%	.000	−7.8%	.001	−9.9%	.005	−7.7%	.000	−12.1%	.002
High credits first term	−1.3%	.964	5.7%	.031	−2.9%	.487	9.3%	.280	1.3%	.307	11.1%	.068
Attending first summer	12.6%	.003	14.7%	.000	11.4%	.002	19.5%	.000	14.2%	.000	15.9%	.001

Note. ATT = Average Treatment Effect on the Treated.

had robust associations with long-term graduation, when estimated using different techniques attuned to selection effects. Our analyses found evidence both of a downside to low momentum and of an upside for one type of high-momentum behavior (summer school attendance at the end of the 1st year of college). However, simply taking 18 or more credits in the first term was not predictive of degree completion.

The pattern revealed by these momentum analyses suggests that academic momentum acts in such a way as to exacerbate previous social and educational inequalities. Lower SES and academically less well prepared students are overrepresented among undergraduates who delay entering college (Horn et al., 2005) and who attend college part-time (Chen, 2007), and our analyses documented that they subsequently suffer an additional disadvantage due to their delay or their part-time enrollment. Moreover, there were suggestive findings that lower SES students and less academically well prepared students were more strongly affected than privileged students by delay and by initial part-time enrollment at 4-year colleges.

Conversely, we found evidence that attending summer session is a positive force moving students toward the degree and that academically weaker students would benefit more than their fellows from attending summer session. However, in practice, better prepared students are the overrepresented group in summer session.

The challenge for policymakers is therefore to see whether already disadvantaged groups might be steered away from patterns of enrollment that appear harmful and toward patterns that tend to encourage completion, without undercutting college access for those students whose obligations and circumstances make it impossible for them to attend full-time or in the summer. Striking a balance may not be easy. Some students clearly attend part-time because they have employment or family obligations (Chen, 2007). Others avoid summer session because they need to earn money over the summer. But to some extent, students' decisions may be affected by organizational factors and incentives: Are courses priced in such a fashion as to encourage part-time enrollment among poorer students? Is summer session enrollment more expensive or cheaper than attending during the school year? Does financial aid cover summer courses?

Current federal and state policies concerning tuition costs and financial aid were not designed with academic momentum in mind and do not provide incentives to encourage high momentum. In some ways, they facilitate students who choose to attend college with low momentum. For example, at the federal level, the largest financial aid program for lower income students is the Pell Grant Program (U.S. Department of Education, 2011). The size of the grant is a complex function of student family resources and cost of attendance. However, Pell Grants pay part-time students proportionally. If an otherwise qualified student takes 6 credits in a semester rather than a normative load of 12 credits, he or she will receive half the full-time Pell aid amount; for 9 credits, the student will receive three quarters of the Pell amount. Some state aid programs follow the same pattern: New York State's Tuition Assistance Plan pays half tuition support for 6 credits and three quarters for 9 credits (New York State Higher Education Services Corporation, 2011). There have recently been recommendations to increase the threshold for full-time Pell Grants to 15 credits per semester (instead of the current 12) as a way of incentivizing higher momentum (Baum & McPherson, 2011).

The federal Pell Grant has a time limit of 18 semesters of enrollment; this is not a strong incentive to complete the degree quickly. Moreover, we have observed college counselors inform aid-eligible students to first use up their Pell Grants and then to shift to state programs such as the Tuition Assistance Plan, thus extending the time limit beyond 18 semesters. These relatively long time limits do not effectively incentivize rapid academic momentum toward the degree.

Our analyses also found that course taking in the summer session after the 1st year has a pay-off in terms of increased likelihood of graduation (cf. Sieben, 2011). Until recently, the federal Pell Grant did not provide aid to students to attend summer session. In a reauthorization that came into effect starting in the 2009–2010 academic year, Congress changed this, allowing qualified students to receive Pell aid while enrolled in the summer. This part of the program was recently terminated; there will be no summer Pell Grants for academic year 2012 and beyond (Kanter, 2011). Thus, a short-lived federal policy that encouraged students to take summer school will be replaced by a policy whereby low-income students face greater financial burdens if they attend summer school compared with attending during the regular school year.

Finally, colleges across the country have enormously varied policies in terms of the tuition cost charged per credit to students during the school year and also during the summer session. During the regular school year, many institutions report one cost for a normative full load of 12 credits in a semester but charge part-time students an amount based per credit taken. To take one institution as an example, a community college student pays \$140 per credit to attend part-time. A student who enrolls for 12 credits at that college enjoys a tiny discount, paying \$1,650 compared with \$1,680 if the student were to pay per credit. But if a full-time student took more than 12 credits in a semester (up to 18), his or her tuition would not increase. In sum, in terms of tuition, there is little disincentive to attending part-time compared with enrolling with a normal load. In this case, there is a financial incentive to taking a heavy course load (e.g., 18 credits), but our earlier analyses suggested no graduation benefit from this kind of extra-heavy load.

Tuition policies for undergraduate enrollment in summer session are even more varied than those during the regular school year, both nationwide and at individual institutions. They vary in part because some summer sessions have students attending for many hours per week but for few weeks, while others schedule more like a typical semester. Within one state system, some community colleges treat summer session as separate from the regular school year and charge additional tuition, while others classify the early summer as a second spring term, and attendance costs no additional tuition. Not surprisingly summer enrollment is much higher in the latter case.

In sum, at the national, state, and university levels, financial aid and tuition policies have not typically been used as instruments to affect graduation rates, via incentivizing the course load levels taken by students and their academic momentum. All policies regarding tuition and aid must balance goals of efficiency—encouraging students to complete their degrees and in good time—against goals of equity and access—making it possible for those low-income undergraduates who must attend part-time to nevertheless afford tuition and receive financial aid. Current policies have tended to address the access or equity agenda but arguably have the unintended cost of overlooking or undercutting incentives to increase momentum. The findings in this article suggest that it might prove fruitful for educational policymakers to consider aid, tuition, and summer session policies as instruments to encourage higher momentum, while taking care to protect access.

In our opinion, the next step in testing the academic momentum perspective and in translating its ideas into practice would be to undertake randomized controlled trials of interventions. Can one incentivize beginning undergraduates to enroll full-time rather than part-time and/or change the numbers of credits attempted, and would this "treatment" subsequently affect their progress toward the degree? If one increases enrollment in summer school, through a randomized controlled trial, will this improve those students' graduation rates? Experiments around these and other interventions for increasing academic momentum may also answer important practical questions for college administrators, such as, How many credits should undergraduates be encouraged to attempt? What type of schedule across semesters and summers is most effective in maximizing degree completion?

In conclusion, several applications of Clifford Adelman's theory of academic momentum hold considerable potential for improving undergraduate degree completion.

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