Three Essays in Higher Education Policy

By

Robert Kelchen

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This dissertation is approved by the following members of the Final Oral Committee:

Sara Goldrick-Rab, Associate Professor, Educational Policy Studies and Sociology

Douglas N. Harris, Associate Professor, Educational Policy Studies (now Department of Economics, Tulane University)

Christopher Taber, Professor, Economics

Adam Gamoran, Professor, Educational Policy Studies and Sociology

Xueli Wang, Assistant Professor, Educational Leadership and Policy Analysis

Abstract

This dissertation consists of three chapters examining issues relevant to current higher education policy debates. In the first chapter, I use surveys, in-depth interviews, and administrative records from a sample of Wisconsin Pell Grant recipients who chose among Wisconsin public colleges and universities to explore whether students' initial college choices affected their early college experiences and to examine how this was associated with their persistence and achievement in college. After controlling for a robust set of observed characteristics, students attending their first choice college have similar levels of early academic and social integration into college life and similar academic outcomes when compared to students who did not attend their first choice college.

In the second chapter, I use a form of cost-effectiveness analysis to estimate institutional performance and compare the results to popular college rankings, which generally reward colleges for attracting stronger students and spending more money. I use data from IPEDS, College InSight, and the Delta Cost Project for nearly 1,300 colleges and universities to estimate value-added to one important outcome: college graduation. I then adjust for two different types of costs for different audiences: the net price of attendance and per-student educational expenditures. All of the methods provide different results from the popular college rankings, suggesting that adjusting for costs and inputs yield a different set of high-performing institutions.

In the third chapter, I address concerns about the timing of the current financial aid system, in which students from low-income families receive concrete information about the cost of college too late to academically and financially prepare for college. Using data from the Panel Study of Income Dynamics, I conduct a simulation of the effects of using a simplified eligibility

process to make an early commitment of the full Pell Grant to eighth graders from needy families. The simulation of the estimated fiscal effects suggests that Pell program costs would grow by approximately $1.5 billion annually and the benefits would exceed the costs by approximately $600 million.

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Choosing State U: The Implications of Initial College Choice for Pell Grant Recipients

Keywords: College choice; academic and social integration; persistence; Pell Grant

Each year, millions of prospective college students go through the process of applying to college. This process differs among students, according to their prior academic achievement, family resources, and preferences, among other factors. While some students desiring to attend highly selective colleges may apply to ten or more colleges, other students are constrained to applying to only one or two institutions for financial or locational reasons. Students tend to place a great deal of importance on attending their "first choice" college, regardless of the number of applications they submit (e.g. McDonough, 1997; Roderick, Nagaoka, Coca, & Moeller, 2008). However, it is unclear whether the first choice really the better choice. Even students who are deemed well-informed about their college options often possess that information for only a small fraction of their available postsecondary opportunities, and it is common for students to apply to college with little or no information about the actual price of attendance (College Board and Art & Science Group, 2013) —a factor figuring prominently in whether they enjoy and succeed in completing college.

The meanings and implications of college choice may be different for students from lower-income families. While higher-income students may feel that they can apply to any college, lower-income students are often acutely aware of the financial barriers to attending certain types of institutions (McDonough, 1997; Supiano, 2011) or have little knowledge of the actual cost of attending college (Grodsky & Jones, 2007). Students from lower-income families

are more likely to have parents with less social and cultural capital (McDonough, 1994; 1997) and little knowledge of the college-going process, which is associated with a lower likelihood of attending college (Kim & Schneider, 2005). As a result, students from lower-income families are less likely to apply to more selective flagship public and selective private universities, even after controlling for standardized test scores (e.g. Pallais & Turner, 2006).

The patterns of college choices among more typical students from low-income families have received far less attention than the choices made by high-achieving low-income students. The latter group has been the subject of a sizable body of empirical research and philanthropic attention for nearly a decade (e.g. Wyner, Bridgeland, & Diiulio, 2007; Hill & Winston, 2010). Very recent work by Caroline Hoxby and her colleagues focused attention on high-achieving, low-income students from rural areas, who have the academic credentials to attend highly selective colleges, but often apply to few or no colleges (Hoxby & Avery, 2012; Hoxby & Turner, 2013). Their informational intervention increased enrollment at very selective colleges by six percentage points (Hoxby & Turner, 2013). But while there is reason to be concerned with this small group of exceptional students, their choices may shed little light on those of their more academically-average counterparts.

Most students from low-income families choose among public colleges and universities within their state of residence. Low-income students tend to be more concerned about cost than other students (Paulsen & St. John, 2002), which can affect the development of their college choice sets. Even among students who attend public universities, there are differences in the application processes by initial Pell Grant receipt; this is shown in data from the Beginning Postsecondary Students study, a nationally representative sample of first-time students in the 2003-04 academic year. Compared to their more advantaged peers, Pell recipients submit fewer

college applications (33% submit four or more applications, compared to 41% for non-Pell students), are less likely to attend very selective colleges (19% compared to 28%), and are more likely to attend an in-state university (89% compared to 77%) (author's calculations).

The implications of college choice may also differ for Pell recipients, especially given their lower likelihood of completing college and higher likelihood of being constrained to considering in-state public institutions. There is reason to be concerned about how their choices affect their college experiences; for example, Douglass and Thomson (2008) find that the self-reported academic and social integration levels of Pell Grant recipients are lower than those of students from higher-income families. Does this imply that it is more or less important for these students to place a premium on their first choice college? Does that choice matter at all?

To address these questions, I use data from the Wisconsin Scholars Longitudinal Study (WSLS), a mixed-methods study of first-time, full-time, Pell Grant recipients who enrolled at Wisconsin public colleges and universities in the fall of 2008, to describe the college choice processes of students from less advantaged backgrounds. I limit this sample to students whose first choice is a Wisconsin public university and top three choices are all Wisconsin public institutions of higher education so their entire application history can be observed. I begin by using interview data to illustrate how these students selected their first choice colleges and eventually enrolled at their state university. I then explore the implications of this choice for their perceptions of academic and social integration. After that, I leverage survey data from the same students to consider whether levels of academic and social integration during the first year of college differ for students who attended their first choice college when compared to (a) "rejected" students, who applied to their first choice college but were not accepted, and (b) "non-attending" students, who were accepted by their first choice college but did not attend.

I then control for student demographic, high school, and first choice college characteristics and examine whether initial college choice is associated with college enrollment, retention, and credit attainment using administrative data from the National Student Clearinghouse, the Wisconsin Technical College System, and the University of Wisconsin System. To preview my results, students who attended their first choice college have outcomes on average that are very similar to students who did not attend their first choice for either reason. However, the college choice process may still have more importance for students who aspire to attend highly selective institutions within a public university system.

Theoretical and Empirical Rationale

Nearly 77% of students attending four-year colleges and universities are accepted by their first choice college, a percentage which has remained steady over time. However, only 59% of students attended their first choice college in 2012, compared to 70% in 2005 (Pryor et al., 2012). The 18% of students who did not attend their first choice college after being accepted is the largest such group in three decades. This result is surprising since the number of spaces in the most selective colleges has remained constant during this period (Bound, Hershbein, & Long, 2009).

There are at least two theoretical rationales why students may place a premium on attending their first choice college. The first reason, consistent with rational choice theory from economics, is that students often make their first choice the most prestigious college to which they are applying, and there may be substantial monetary returns to attending a more prestigious college (e.g. Loury & Garman, 1995; Black & Smith, 2006; Hoekstra, 2009). Moreover, there is evidence that economically disadvantaged students benefit more from attending a more

prestigious college than do more advantaged students (Dale & Krueger, 2002; 2011). Prestige is closely related to institutional selectivity, and together these often serve as a proxy for college "quality" in the minds of families and students. Since the 1970s, college quality has become demonstrably more important to students as they develop their college choice preferences (Long, 2004). As a result, some studies indicate that students rank more selective colleges higher in their list of preferred institutions (Niu, Tienda, & Cortes, 2006; Niu & Tienda, 2008).

The second reason for preferring a first choice college is guided by status competition theory from sociology, which posits that students seek to attend the most prestigious college possible in order to obtain or retain a high spot in the social hierarchy (Haller & Portes, 1973; Jackson, 1978; Karen, 2002). Since many students from low-income families recognize that attending college is one of the best routes to a secure financial future, they may feel particular pressure to get into and succeed in the best possible college where they believe their chances of upward mobility stand the best chances of being realized (Armstrong & Hamilton, 2013).

Despite the reasons why students might care about attending their first choice college, few studies examine the effects associated with that action. The effects could arise through several mechanisms—better teaching or more resources, a strong academic peer culture, or perhaps simply a student's inherent satisfaction with the choice. Support for the latter mechanism comes from Tinto's (1975, 1993) model of student departure, which places high priority on the level of academic and social integration students achieve, and indicates that low levels of integration are associated with college departure (e.g., Wolf-Wendel, Ward, & Kinzie, 2009). Numerous correlational studies have provided support for this hypothesis for many different types of students and colleges (for example, Pascarella & Chapman, 1983; Stage, 1989; Bers & Smith, 1991; Cabrera, Nora, & Castaneda, 1993; Braxton, Shaw Sullivan, & Johnson,

1997; Thomas, 2000; Sorey & Duggan, 2008), but these studies do not consider how attending one's first choice college affects integration.

Among the most relevant studies to the current research is an empirical examination by Braxton, Vesper, and Hossler (1995), using a sample of 263 students attending Indiana universities that suggests (using a regression framework) that students who attend their first choice college are more likely to feel academically and socially integrated than students who are not attending their first choice college. They also found that students who attend their first choice college are more likely to indicate intent to return to that same college for a second year. Similar results are found by Pascarella, Terenzini, and Wolfle (1986) used survey data from a sample of 763 students beginning at a private university in the fall of 1976 to examine the predictors of retention to the second year of college. They find that institutional commitment (a measure of first choice college attendance) is associated with greater levels of academic and social integration, as well as higher rates of retention. Each of these studies has been limited in scope, as national datasets include measures of college choice or processes of integration, rarely combining the two with administrative data records to assess outcomes. Further, each of the prior studies failed to consider the effects of heterogeneity among the counterfactual group of students who did not attend their first choice.

An ideal experiment to test the importance of attending a first choice college would involve randomly assigning students to both treatment (attending the first choice college) and control (not attending) conditions; the control group should further be split into students who are randomly rejected by the first choice and those who are accepted but are randomly not allowed to attend that particular college. While the admission process may be somewhat random for students close to the margin, the counterfactual choices of students who are rejected are certainly

not random; neither is the decision not to attend a college after being accepted. This leaves researchers to rely on quasi-experimental or correlational (regression-based) analyses in order to examine the potential effects of first choice college attendance. While some national datasets have information on a student's college choice set, they lack information on the acceptance and attendance decision for each institution as well as detailed information on student outcomes and characteristics.

In this paper, I build on the existing literature by integrating interview, survey, and administrative data from a group of students attending Wisconsin public colleges and universities to explore the college choice process and its outcomes. I assess those outcomes by disaggregating the usual comparison group into students r*ejected* by their first choice college and students who *do not attend* their first choice college after being accepted. Finally, I qualitatively and quantitatively examine how that choice relates to college integration and retention among students from low-income families, affecting the odds that they will persist to graduation.

Methodology

Data for this study come from the Wisconsin Scholars Longitudinal Study (WSLS), which follows a sample of Pell Grant recipients who initially enrolled in college in the fall 2008 semester at a Wisconsin public university in the University of Wisconsin System. More information about the WSLS can be found in Goldrick-Rab, Harris, Kelchen, and Benson (2012).

Setting

Approximately 65,000 students graduate from Wisconsin public high schools each year, but only a select group of them transition to attending a four-year university the next fall. Data from the Wisconsin Department of Public Instruction suggests that approximately 50% of

graduating seniors plan on attending a university the next year. About 25,000 in-state students enroll as freshmen in the state's public universities each fall (University of Wisconsin System, 2012), suggesting that most students who attend a four-year university attend a UW System institution.

The four-year institutions in the UW System include two doctoral universities (the flagship at Madison as well as Milwaukee) and eleven regional comprehensive universities with varying levels of selectivity. According to IPEDS data for the 2008-09 academic year, admission rates varied from 63% (Madison) to 96% (Milwaukee) and median ACT scores ranging from 21 (Parkside and Stout) to 28 (Madison). The economic diversity of the campuses also varies substantially, with the percentage of students receiving Pell Grants ranging from 12% (Madison) to 38% (Superior) as WSLS students entered college.

Sample

I focus on a subsample of WSLS participants whose first choice college was one of the 13 public universities in the UW System and enrolled in a Wisconsin public college or university during the fall 2008 semester and completed a baseline survey during their first semester. . The initial sample of 1,759 students graduated from a Wisconsin public high school or received a Wisconsin High School Equivalency Diploma between 2005 and 2008, enrolled as first-time, full-time students in a Wisconsin public institution of higher education, and received a Pell Grant with at least one dollar in unmet financial need. My analytic sample is further narrowed to those 830 students named a four-year university as the first choice institution (since two-year colleges are open admission), (b) applied to that university, and (c) provided consent to allow administrative records to be accessed in order to assess educational outcomes). Finally, I limit the sample to students whose top choice was a Wisconsin public university and whose top three

choices were Wisconsin public institutions (two-year or four-year) so nearly all of their choice set can be observed.[1] This results in an analytic sample of 554 students. For exploratory purposes, I also include a small sample of 15 of these students who participated in in-depth interviews.[2] Table 1.1 describes the sample selection process and how many students met each of the inclusion criteria for the quantitative and interview samples.

Table 1.2 compares the quantitative sample to the group of students who applied to a four-year university as their first choice college, but listed a private or out-of-state university as one of their top three choices and applied to that school. Students who applied to colleges outside Wisconsin public higher education (and ended up attending a Wisconsin public college or university) are more likely to be black and less likely to be Southeast Asian than students whose entire choice sets were in-state public institutions. Students who applied outside Wisconsin public higher education also have more educated parents, higher educational aspirations of their own, but their household resources and concerns about the cost of college are relatively similar to students who limited their applications to in-state public colleges and universities.

Data

There are two sets of dependent variables in my analyses: academic and social integration in their initial college of attendance and academic outcomes. I measure the first using both quantitative and qualitative methods. First, I use measures from the survey, assessing academic

[1] Although I only observe the names of a student's top three choices, just sixteen percent of students in the analytic sample reported applying to more than three colleges. Only five percent of students applied to five or more colleges.
[2] A stratified random subsample of students who completed the baseline survey, were enrolled at one of four institutions in a region of the state of Wisconsin with higher-than-average enrollment of racial/ethnic minority students, and provided additional consent were selected to participate in a series of in-depth qualitative interviews. Forty-five students who were interviewed met my initial eligibility criteria, fifteen of whom satisfied my additional sample criteria. Nearly every student was interviewed each semester during the first three years of the study, regardless of whether the student transferred or left college altogether.

integration using a series of questions assessing a student's level of agreement or responses to the following statements:

1. My grades are lower than I'd expected.
2. I'm not getting as much help or support from my college as I'd expected.
3. How interesting are your classes?
4. Classes are more difficult than I'd expected.
5. How difficult is the material taught in your classes?
6. I make sure to have good grades.

The first five measures are on a five-point Likert scale and the last measure is binary (agree/disagree). I measure social integration using six questions regarding the student's level of agreement with the following statements about their happiness in college, number of friends, and enjoyment of his or her peers:

1. I'm not as happy in college as I'd expected.
2. I have fewer friends at college than I had expected.
3. How much do you enjoy the people you go to college with?
4. How much fun is college life?
5. I feel like I fit in with the other students at my college.
6. I have made many new friends in college.

The first four measures are on a five-point Likert scale and the last two measures are binary (agree/disagree).

I form scales for academic and social integration separately using factor analysis and then standardize the resulting scores to have a mean of zero and a standard deviation of one. The

correlations among the measures within the scales are generally acceptable (Cronbach's alpha=0.673 for academic integration and 0.796 for social integration) (Nunnally, 1978).

Next, I coded levels of academic and social integration for the interviewed students using the Dedoose software package. I began by identifying all passages in the first interview of college in which a student referred to academic or social integration. For example, a passage in which a student discusses their professors or friends that they made in college would receive a code. I then quantified the resulting codes (e.g. Creswell & Clark, 2007) by assigning each student a score on a three-point scale for academic and social integration separately. A score of one represents little evidence of successful integration into college, a two represents at least some evidence, and a three represents a great deal of evidence of successful integration. Table 1.3 further details my criteria for coding the academic and social integration scales and provides examples of each code.

To assess three years of enrollment and completion outcomes, I utilize administrative data from multiple sources. Enrollment data from the University of Wisconsin System and Wisconsin Technical College Systems are supplemented with data from the National Student Clearinghouse (NSC), a non-profit organization that tracks the enrollment of more than 92% of college students in the United States and nearly all students in the state of Wisconsin.[3] Measures for any enrollment during the second and third years of college is tracked as well as the cumulative number of semesters enrolled and whether a student ever transferred over three years. For students who remain within the University of Wisconsin System, I use the system's administrative records to track the number of GPA-bearing credits completed and the student's

[3] In the state of Wisconsin, Herzing University is the only college with more than 1,000 students which does not participate in the National Student Clearinghouse.

cumulative grade point average after three years.[4] These outcomes can be observed for all students who transfer within the UW System, which includes thirteen two-year branch campuses as well as the thirteen four-year universities discussed earlier.

To explore and measure characteristics of the students and the institutions they listed as both their first choice college and their college of attendance, I draw on survey and administrative data. On the initial survey, students were asked to list a first choice college, whether an application was submitted to that college, and whether they were accepted. This enables the construction of comparison groups. As a robustness check, I also use information from the ACT student survey regarding where test scores were sent.

I also utilize data on students' demographic characteristics, financial resources, and educational aspirations from the WSLS survey and the University of Wisconsin System's central records (race, gender, expected family contribution, and ACT scores) and students' FAFSA forms (parental income). Finally, I include several survey measures relevant to the college choice decision but not typically observed, including the student's self-reported perception of adulthood and the importance of cost in the college selection process. Young adults in their late teens, such as those in the WSLS sample, are at a key point on the path to adulthood (Montgomery & Cote, 2008) while also trying to make a successful transition to college (Terenzini et al., 1994). The students' exact places on the developmental pathway could influence the college choice decisions.

I also include characteristics of a student's high school of attendance from the Wisconsin Department of Public Instruction, such as the size and location of the school, the racial composition, and the percentage of students receiving free or reduced price lunches in the

[4] For students who are not enrolled in a given academic year, I use the GPA from the last term enrolled. This is the same method used by Scott-Clayton (2011) and Goldrick-Rab et al. (2012).

analytic models. Finally, I also draw on the characteristics of a student's first choice college from the Integrated Postsecondary Education Data System (IPEDS).[5] This includes whether the college is public or in the state of Wisconsin, the cost of attendance, the percentage of students receiving Pell Grants, the percentage of students admitted, and the median ACT score.[6] I use multiple imputation to estimate values for a small number of missing responses, including less than one percent of the academic and social integration measures. The results do not change substantially if listwise deletion is instead used.

Triangulation

One of the advantages of the WSLS dataset is its inclusion of multiple data sources on the same measure. For example, I have academic and social integration measures using both interview and survey data. However, the correlations between the survey and interview measures of academic and social integration are weakly positive or even negative. The correlation between the survey and interview measures of academic integration is -0.449 compared to 0.046 for social integration, suggesting that students' perceived integration in college varies substantially by measurement form and timing.

Analytic Strategy

Students enroll at a particular college as a result of two decisions, one made by the institution and the other made by the student. Once a student has applied, a college chooses whether or not to accept the student based on observed characteristics such as test scores, academic preparation, and unobserved (to the researcher) characteristics such as a personal

[5] I was unable to match approximately a dozen students' listed first choice colleges with an institution listed in IPEDS due to ambiguous abbreviations (such as MSU) or institutions that could not be found through an Internet search. These students were dropped from the analysis.

[6] The average ACT composite score is calculated by averaging the 25th and 75 percentile scores that are provided in IPEDS data, rounding up to the nearest whole number. For schools that provide only SAT scores, I averaged the 25th and 75th percentile scores on the SAT verbal and mathematics sections separately and then added the two numbers to get the median SAT composite score. The SAT composite score is then converted to an ACT composite score using the ACT/SAT concordance guide (ACT, Inc., 2008).

statement or other contact with the institution. Students who were accepted by their first choice then have to choose whether or not to attend. Theoretically, a student will choose to attend his or her first choice college if the decision results in a higher level of expected utility than either attending another college or not attending college at all, where utility is a function of cost, the expected academic and social fit between the student and the institution, the quality of a student's existing relationships with family and friends, and countless other factors. Because these two decisions are separate and made by different agents, I separately compare students who attended their first choice to those who were rejected or did not attend after being accepted.

I estimate the effects of attending one's first choice college by comparing students who attended their first choice and those who were rejected using the following OLS regression model:

$$Y_i = \beta_0 + \beta_1 AttRej_i + +\beta_2 D_i + \beta_3 F_i + \beta_4 H_i + \beta_5 F_i + \theta_i, \qquad (1)$$

where Y_i is the outcome of interest (integration or academic outcomes), $AttRej_i$ is a dummy variable equal to 1 if the student attended his or her first choice and 0 if he or she was rejected, $Prob(Acc_i)$ is the estimated probability of acceptance, D_i is a set of demographic characteristics, $C_i$is a set of characteristics about the student's first choice college, H_i is the set of characteristics of the student's high school, and F_i is a set of fixed effects for the initial college of attendance.[7]

I then estimate the effects of attending one's first choice college (by comparing students who attended their first choice and those who were accepted but did not attend) using a similar OLS model:

$$Y_i = \beta_0 + \beta_1 AttNot_i + \beta_2 Prob(A_i) + \beta_3 D_i + \beta_4 C_i + \theta_i, \qquad (2)$$

[7] For all outcomes, I also control for a student's financial aid receipt status.

where Y_i is the outcome of interest, $AttNot_i$ is a dummy variable equal to 1 if the student attended his or her first choice and 0 if he or she was accepted but did not attend, $Prob(A_i)$ consists of both the estimated probabilities of acceptance and attendance, and D_i and C_i are as in equation (1).

Limitations

The most significant limitation of my sample is that it only includes students who attended a Wisconsin public university. Because of this, I cannot observe two different groups of students: those who attended a college or university outside the Wisconsin public university system and those who had ambitions to attend college but did not attend. Although students at Wisconsin private colleges and universities are similar to those at public institutions on observable characteristics, my set of comparisons is limited. Although my data include students who listed a private and/or out-of-state college as their first choice, I exclude them from the analytic sample in order to observe a student's entire choice set. This is a significant limitation, but no other extant dataset has the combination of college choice information, survey data, and student outcomes.

Among the population of low-income students in the state of Wisconsin, the WSLS sample is a relatively advantaged group. WSLS students attended college on a full-time basis during their first semester of college, while many low-income students attend college part-time or are unable to attend college due to financial or family constraints. While approximately two-thirds of first-time, low-income college freshmen attend college full-time and nearly 80% of Pell Grant recipients attend full-time in their first year of college (U.S. Department of Education, 2006), the entire WSLS sample was enrolled full-time during the first semester. It is also worth emphasizing that all WSLS students completed the FAFSA in order to receive Pell Grants, which

suggests a certain amount of knowledge about the college-going process that may not be the case for all low-income students.

It should also be noted that the interviewed students are different from the full WSLS sample, which is partially a function of the sampling strategy (focusing on obtaining a racially and ethnically diverse sample) and also because of the types of students who agreed to be interviewed. Students who agreed to be contacted for an interview had higher levels of social integration on the survey measure ($p<.01$), suggesting differences on important characteristics between the two groups. This difference was equally pronounced for the four campuses at which students were interviewed. The higher level of social integration among students in the interview sample may also reduce the ability to detect variation in social integration levels between groups. It is also worth mentioning that all interviewed students were still enrolled in college three years later.

Measurement and Variation in College Choices

Given the focus on a student's "first choice" college in this analysis, I begin by examining the accuracy of this measure. For approximately 80% of students, I compare the listed first choice colleges in the student survey (from the fall of the student's first year of college) to where they sent their ACT scores during their junior or senior year of high school. The ACT asks students to list schools to which they would like to send their scores; since nearly all students listed four colleges, it is unclear whether students actually ordered their college choice preferences on the survey or selected the codes in the order they appeared in the registration booklet. The ACT data indicate that approximately 80% of students sent test scores to their first choice college, and in just over half of the cases the student's first choice college is listed first on

the list of colleges to which scores are sent. Most notably, there are no significant differences in the ACT score-sending behaviors based on the student's final attendance status; this suggests that preferences at the earlier point in time were relatively stable across groups. I also compared the first choice college based on interview and survey data. Of the students who mentioned the name of their first choice college in the interviews, all of them listed the same first choice college on the survey.

I next describe students in each of three groups based on whether they attended their first choice college, were rejected, or did not attend after being accepted. Table 1.4 contains summary statistics of the sample for the previously mentioned characteristics.

"Attending" Students

Seventy-one percent of students in the sample attended their first choice college, a rate which is substantially higher than the national average of 59% for all four-year students (Pryor et al., 2012). While this high rate can likely be explained by the sample restrictions, it is worth noting that no comparable data are available for Pell Grant recipients. Most (81%) of the students are white, 61% are female, and 31% have zero expected family contribution. They are fairly well-prepared for college, with an average ACT of 22.0, somewhat lower than their institution's median ACT of 22.9. Most attended college close to home, with an average distance of only 69 miles between their high school and college. Eight in ten students are from rural or suburban high schools with relatively low amounts of racial or ethnic minority students.

Although students in this group applied to an average of 2.1 colleges, one-third of students only applied to one college. For example, Melanie only applied to one school, but considered applying to others.[8] "Thank God I got in," she said. Bethany wanted to stay close to home for personal reasons. "I knew I wanted to be close to my mom and my brothers and sisters

[8] All names reported in this paper are pseudonyms that reflect the student's race/ethnicity and gender.

so I figured if I stayed [close to home] if something went wrong I could just go right back home," she explained. Other students who attended their first choice college applied to multiple institutions; most applied to a total of two or three colleges. Kia, an education major, selected a less-selective university as her first choice and attended that college; "[it] seemed like the perfect fit," she explained. She chose this university over the public university in her hometown where she was accepted. "I don't want to stay too close to home," she said while laughing. Kia also dismissed the flagship state university as being a "party school," although she did list it as her third choice and was rejected there.

"Rejected" Students

In this sample, 18% of students were denied admission to their first choice college. Men were significantly less likely than women to be accepted by their first choice college, while white students were significantly more likely to be accepted than targeted minority students. Not surprisingly, rejected students are academically weaker than attenders, with an ACT two points lower and a high school GPA one-quarter point lower. They applied to first choice colleges that were much more selective than attenders, with average admission rates of only 74% and median ACT scores five points higher than their own score; it seems these students were rejected by a 'reach' institution. These students aspired to attend a more expensive college, with an average sticker price of attendance of over $16,300 per year (compared to $15,300). Rejected students applied to institutions which have fewer economically disadvantaged students, with four percent fewer students receiving Pell Grants than at the institutions students attended as their first choice.

Isabelle was waitlisted by her first choice, a Wisconsin public university, and ended up attending her third choice, a different Wisconsin public university much closer to her home. In the initial interview, she discussed how being placed on the waitlist ended up being a positive

event in her mind: "When I visited the campus it wasn't, I don't know, it didn't feel right like I couldn't see myself going there." (This also shows that students selected and applied to their first choice college without having already visited the campus.) Matt's first choice was the flagship university, but he was rejected there and attended his second choice. He aspired to work in national security, and his college of attendance had a strong international studies program. However, he was concerned that attending a less-prestigious university would hurt him in a long run. "Trying to get to national security, you know, [my current college] isn't really recognized. Like if you go to [the flagship], it's more out there. People know it more than if you just say you graduated from [my college]," he stated. Although he expressed an interest in transferring to the flagship university, administrative data suggest that he never did so.

"Non-Attending" Students

Almost 11% of students accepted by their first choice university did not attend that institution, and these students are distributed across 12 of the 13 UW System universities. There are several significant differences between attenders and non-attenders. Southeast Asian and female students are overrepresented among non-attending students. Even though their families' financial strength was similar to that of attenders, non-attenders are notably more concerned about the cost of college (even though they applied to colleges with more expensive sticker prices) than those who attended their first choice.[9] Non-attenders were more likely to come from larger, urban high schools, have lower ACT scores, and apply to more selective colleges.

Emma faced a difficult choice between two selective universities and ended up attending her second choice college based on financial considerations. "I actually was going to go to [my first choice] because I got financial aid with them," she explained. "[My second choice] had to

[9] It is possible that students end up receiving more financial aid from the more expensive college, but I do not observe aid offers from colleges other than the institution of attendance.

match that financial aid or else I couldn't come here. And they ended up giving me more than [my first choice] did." Roberto was accepted by two nearby public universities and had a difficult time making his choice. "It was kind of like I want to go to [my first choice], I want to go to [my second choice] so they were kind of on the same level almost," he said. "But [my second choice] kind of weighted out the pros and cons and [it] kind of fit that so."

College Choice and Integration

A summary of the outcome measures for academic and social can be found in the first half of Table 1.5. Before adjusting for differences in student characteristics, students who attend their first choice college appear to have slightly higher levels of academic integration than those who did not, but the difference is not statistically significant. There are no differences in the interview measure of academic integration, with 13 of 14 students interviewed during their first year of college classified as having either a moderate or high level of academic integration.[10] Many students in the interview sample discussed how the academic transition to college was not as difficult as they had expected and that they were happy with their academic progress. For example, Isabelle, who attended her third choice college after not being accepted by her first choice, discussed how she liked some of her professors but not others. Although she had to retake a class, she enjoyed her university's academic atmosphere and the number of speakers who came to campus during the 2008 presidential race.

Table 1.5 does show some differences in the social integration levels of students who attended their first choice college compared to those who did not attend. Non-attending students had a social integration score 0.27 standard deviations below students who attended their first choice, which is significant at $p<.05$. There is no statistically significant difference in the social integration levels of students who attended their first choice and those who were rejected. Most

[10] It is worth noting that the least-integrated individuals do not attend college and are thus not in my sample.

students in the interview sample reported moderate levels of social integration, discussing the relative ease with which they made friends but also noted the stress of transitioning from high school to college. An example of this is Tou, who attended his first choice college. "You can just go up to someone, talk to them, and get their phone number, get together sometime, and like just talk," he said.

In Tables 1.6 and 1.7, I use the regressions from equations (1) and (2) to compare students who attended their first choice college to those in the other two groups (rejected and non-attending students) on self-reported academic and social integration into college and academic outcomes such as enrollment, retention, transfer, and credit attainment.[11] After controlling for student and institutional characteristics, there are no statistically significant differences in the overall academic or social integration levels between attending, rejected, and non-attending students. The coefficients on the academic and social integration measures suggest the possibility of slightly better outcomes for students who attend their first choice, but they are far from being statistically significant.

College Choice and Academic Outcomes

Before accounting for differences among the groups on baseline characteristics, the academic outcomes appear fairly similar between students who attended their first choice college and those who did not (see Table 1.5). About 85 percent of students who attended their first choice returned for their third year of college and had completed an average of 69.1 credits within the UW System during those three years. Not surprisingly, students who did not attend their first choice were approximately ten percentage points more likely to transfer than those who attended their first choice, with about ten percent of students transferring to their first choice

[11] For the sake of brevity, only the coefficients and standard errors for the outcome measures and selected student demographic characteristics are reported. The coefficients and standard errors for the other covariates are available upon request from the author.

college during the first three years of college. Students rejected by their first choice college remained enrolled at similar rates to students attending their first choice, while earning five more credits over three years.[12] Non-attending students earned four fewer credits than those attending their first choice and had a cumulative GPA one-quarter point lower ($p<.05$).

The adjusted outcomes indicate that after three years of college, students who attended their first choice college completed the same number of credits (within the University of Wisconsin System), had similar grade point averages, and were retained at similar rates as rejected students (Table 1.6). Next comparing students who attended their first choice with non-attending students (Table 1.7), the covariate-adjusted outcomes somewhat favor students who did not attend their first choice after being accepted. Students who attended their first choice were ten percentage points less likely to persist to the third year of college than non-attending students ($p<.10$), had cumulative GPAs 0.18 lower, but earned one credit more on average (the latter two measures are not significant). These covariate-adjusted differences suggest the importance of highlighting the differences in student demographics between students who attend their first choice and those who do not.

It is also possible that the null findings for the full sample may be because students perceived few differences between their first and second choice colleges. College choice may be more important for students who aspire to attend highly selective institutions. To explore this hypothesis, I examined the outcomes separately for the 154 students whose first choice college had a median ACT of 25 or higher. This includes three UW System universities, including the flagship university at Madison. In Table 1.8, I show the differences in outcomes by first choice college status, both with and without covariates.

[12] Students who began at a Wisconsin Technical College System campus are excluded from credit and GPA outcomes due to missing data.

Before adjusting for student characteristics, students who attended a highly selective college had much higher levels of academic and social integration, were much less likely to transfer, and earned significantly higher GPAs than those who were rejected by selective universities and attended a less selective Wisconsin public university (the "raw" columns). Students who attended their first choice also had slightly higher levels of academic integration than non-attending students, but they also had slightly lower levels of social integration than those who did not attend and similar academic outcomes. It should also be noted that only 17 students who were accepted by highly selective universities did not attend, severely limiting the statistical power of this comparison.

Adjusting for student characteristics shows a somewhat different picture (the "adjusted" columns of Table 1.8). Focusing on the comparison between students who attended their first choice and those who were rejected, it appears that students who attended their first choice may have higher levels of academic and social integration than those who do not—although the large standard errors and lack of statistical power limit the ability to draw conclusions. There is also some evidence that students rejected by their first choice academically outperform students who attended their first choice after adjusting for student characteristics, as evidenced by the coefficients on retention to the third year of college and cumulative GPA ($p<.10$).

Discussion

Much of the discussion on college choice has focused on students who aspire to attend the most elite colleges. In this paper, I discuss the college choice processes of Pell Grant recipients with a more constrained set of options and who ended up attending Wisconsin public universities. I examine whether attending one's first choice college (compared to attending

another college) affects the academic and social integration levels of Pell Grant recipients who attended Wisconsin public universities, as well as their persistence at any institution of higher education. After modeling the two processes through which selection occurs (the college's decision to accept the student, and the student's decision to attend after being accepted), I find no evidence among the full sample that attending one's first choice college significantly affects academic or social integration or continued enrollment and credit accumulation. These findings are largely confirmed by students' initial interviews during their first year of college.

Future research should explore the college choice mechanisms of Pell Grant recipients in more detail and include students who attended a broader range of institutions. This should include students who applied to colleges and universities across the selectivity spectrum, especially in light of my suggestive findings that college choice may matter differently for students attending more selective colleges. The issue of selectivity also merits additional discussion in light of work by Caroline Hoxby and colleagues (Hoxby & Avery, 2012; Hoxby & Turner, 2013) showing that more talented students from low-income families have the academic ability to be accepted by highly selective institutions.

Policymakers and institutions should consider ways in which to increase the academic and social integration levels of all students and particularly Pell Grant recipients, who are less likely to graduate from college than students from higher-income families. Student interviews with consenting students, who tend to be more academically and socially integrated than the typical student, may provide insight into programs and policies that are working for certain groups of students. More needs to be done to reach out to students who are more socially isolated and would not want to participate in an interview or focus group. Finally, colleges and high school guidance counselors should consider methods to counsel students who do not get into

their highly selective “reach” college, as it appears that these students may perform worse in college than students who attend their first choice.

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Table 1.1: Analytic sample criteria--quantitative and interview.

	Quantitative Sample	Interview Sample
Initial sample size	1759	45
Consented to administrative data linkage	1256	40
Applied to four-year university as first choice	830	30
Applied to WI public university as first choice	722	25
All applications to WI public institutions (analytic sample)	554	15

Table 1.2: Student characteristics by sample inclusion.

	Analytic Sample		Not in Sample	
Variable	Mean	(SE)	Mean	(SE)
Race/Ethnicity (percent)				
White	77.5	(1.9)	70.9**	(2.8)
Black	4.4	(0.8)	12.7***	(2.0)
Hispanic	4.8	(0.9)	4.1	(1.1)
Southeast Asian	8.5	(1.3)	4.9**	(1.3)
Native American	3.1	(0.8)	4.5	(1.3)
Gender (percent female)	59.8	(2.3)	64.1	(3.2)
Parental Education (percent)				
Did not complete HS	9.0	(1.3)	4.8**	(1.3)
High school diploma	33.1	(2.1)	28.3	(2.9)
Some college	39.0	(2.3)	38.2	(2.3)
Bachelor's or more	18.9	(1.8)	28.8***	(2.9)
Educational Aspirations (percent)				
Bachelor's degree	37.2	(2.2)	23.8***	(2.8)
Master's degree	35.1	(2.2)	42.9**	(3.2)
Professional degree	23.3	(1.9)	32.3**	(3.0)
Student Characteristics				
Number of siblings	3.2	(0.1)	3.1	(0.1)
Perception of adulthood (1-5)	3.5	(0.1)	3.5	(0.1)
Number of college applications	2.3	(0.1)	3.5***	(0.2)
College cost important (1-5)	3.4	(0.1)	3.6***	(0.1)
EFC ($)	1309	(93)	1536**	(60)
Zero EFC (percent)	31.8	(2.1)	31.2	(3.0)
Parent adjusted gross income ($)	28,411	(851)	30,809	(1247)
ACT composite score	21.4	(0.2)	22.1**	(0.2)
High school GPA	3.21	(0.02)	3.30**	(0.03)
Number of Observations	554		276	
Interview Sample	15		15	

Sources: UW System (race, gender, EFC), FAFSA (parental income) ACT (ACT score and high school GPA), WSLS baseline survey (all other measures).

Notes:

(1) * signifies p<.10, ** signifies p<.05, and *** signifies p<.01.

(2) Students not in the analytic sample applied to at least one college outside Wisconsin public higher education, but their first choice was a four-year college.

Table 1.3: Description of interview academic and social integration scores.

Value	N	Description	Example
Academic Integration			
1 (Low)	1	Little or no evidence shown of positive academic integration; evidence of a lack of integration	"I just feel like I can't really get help here as much, I don't get the individual attention that I need."
2 (Medium)	7	Mixed or weak evidence of positive or negative academic integration	"I don't need [academic advisors'] help. I mean they're just going to tell me what to take." "I don't want to say that [professors] don't care, but they are kind of like it's your own thing. If you're going to do your homework, you're going to do well. If you're not going to do it, you're not."
3 (High)	6	Strong evidence of positive academic integration; no evidence of negative integration	"I like the atmosphere. I like all the professors in [my] department; they're really nice. You can go up to their office and if they're in their office they'll help you out and they'll take time out of their day to help you out and make sure you're doing good." "Professors, tutors--everybody is just so willing to help."
Social Integration			
1 (Low)	0	Little or no evidence shown of positive social integration; evidence of a lack of integration	"I sat in my room a lot the first semester. I didn't really keep up with everything and kind of got in this weird niche where I just felt like everyone around me was living the college thing and I was kinda not." "I have to do some kind of job to help [my mom] out…all my free time will go there."
2 (Medium)	10	Mixed or weak evidence of positive or negative social integration	"I don't know if I'm going to like my [classmates] but the people here are really friendly. You can just go up to someone, talk to them and get their phone number, get together sometime and just talk." "I joined a few clubs…I can't remember at this time exactly what I joined."
3 (High)	4	Strong evidence of positive academic integration; no evidence of negative integration	"I met a lot of good longtime friends. Learned a lot of life lessons." "[My college] had a campaign where you could meet new incoming freshmen so a lot of [friends] I knew from there."

SOURCE: Initial interviews conducted during 2008-09 academic year.

Notes:

(1) One student was not interviewed during the first year of college.

(2) The low, medium, and high scores were developed using the full WSLS sample of 50 interviewed students. No students in this paper met the low integration threshold.

Table 1.4: Background characteristics by first choice college outcome.

	Attended first choice		Rejected by first choice		Accepted, did not attend	
Variable	Mean	(SE)	Mean	(SE)	Mean	(SE)
Student Demographics						
Race/Ethnicity (percent)						
White	80.6	(2.1)	71.5*	(4.8)	67.9*	(6.3)
Black	4.1	(1.0)	6.0	(2.3)	4.0	(2.5)
Hispanic	5.1	(1.1)	3.7	(1.9)	4.9	(2.8)
Southeast Asian	5.4	(1.2)	11.4	(3.5)	23.2***	(5.7)
Native American	3.4	(1.0)	4.0	(2.1)	0.0***	(0.0)
Gender (percent female)	60.8	(2.7)	48.0**	(5.8)	72.3	(7.2)
Parental Education (percent)						
Did not complete HS	7.9	(1.4)	11.2	(3.5)	12.5	(4.6)
High school diploma	35.1	(2.6)	30.6	(4.9)	24.5*	(5.8)
Some college	36.9	(2.6)	42.7	(5.4)	46.4	(6.9)
Bachelor's or more	20.1	(2.2)	15.5	(3.5)	16.7	(5.2)
Educational Aspirations (percent)						
Bachelor's degree	35.6	(2.6)	44.4	(5.3)	35.8	(6.6)
Master's degree	35.6	(2.6)	32.1	(5.0)	35.9	(6.7)
Professional degree	24.3	(2.4)	20.4	(4.5)	21.8	(5.5)
Student Characteristics						
Number of siblings	3.1	(0.1)	3.4	(0.3)	3.4	(0.4)
Perception of adulthood (1-5)	3.6	(0.1)	3.6	(0.1)	3.4	(0.1)
Number of college applications	2.1	(0.1)	3.1***	(0.1)	2.7***	(0.1)
College cost important (1-5)	3.3	(0.1)	3.5	(0.1)	3.6*	(0.2)
EFC ($)	1271	(71)	1371	(150)	1458	(182)
Zero EFC (percent)	31.0	(2.5)	33.9	(5.1)	33.1	(6.5)
Parent adjusted gross income ($)	27,765	(987)	29,937	(1854)	30,042	(3088)
ACT composite score	22.0	(0.2)	19.9***	(0.3)	20.5***	(0.5)
High school GPA	3.26	(0.03)	2.99***	(0.06)	3.27	(0.06)
First Choice College Characteristics						
Distance from HS (miles)	69.2	(3.0)	81.8*	(6.1)	79.1	(7.9)
Cost of attendance ($)	15,344	(73)	16,342***	(213)	15,839*	(253)
Percent receiving Pell Grants	19.6	(0.2)	15.1***	(0.4)	18.6	(0.7)
Selectivity						
Median ACT	22.9	(0.1)	24.8***	(0.3)	23.5*	(0.3)
Admit rate (percent)	83.9***	(0.5)	73.5***	(1.1)	81.3	(1.6)

Table 1.4: Background characteristics by first choice college outcome (continued).

	Attended first choice		Rejected by first choice		Accepted, did not attend	
Variable	Mean	(SE)	Mean	(SE)	Mean	(SE)
High School Characteristics						
Location (percent)						
Rural	40.3	(2.7)	33.2	(5.1)	30.3	(6.3)
Suburban/town	39.6	(2.7)	41.6	(5.3)	34.5	(6.5)
Urban	20.1	(2.1)	25.2	(4.7)	35.2**	(6.6)
Demographic Characteristics						
Title I school (pct)	40.0	(2.7)	35.3	(5.0)	37.6	(6.7)
Number of students	940	(32)	1045	(68)	1096*	(84)
Student-teacher ratio	15.3	(0.1)	15.4	(0.2)	15.2	(0.3)
White students (pct)	84.4	(1.1)	84.5	(1.9)	81.7	(2.6)
Black students (pct)	5.7	(0.8)	5.8	(1.3)	6.4	(2.1)
Hispanic students (pct)	4.7	(0.4)	4.8	(0.7)	5.0	(0.8)
Free/reduced lunch (pct)	26.4	(0.8)	25.3	(1.4)	29.5	(1.9)
Number of Observations	393		101		60	
Interview Sample	9		3		3	

Sources: UW System (race, gender, EFC), FAFSA (parental income) ACT (ACT score and high school GPA), IPEDS (first choice college measures), Wisconsin Department of Public Instruction (high school measures), WSLS baseline survey (all other measures)

Notes:

(1) * signifies p<.10, ** signifies p<.05, and *** signifies p<.01.

(2) Each group is compared to students attending their first choice college.

(3) There is a small amount of missing data on some measures. Multiple imputation is not used here.

Table 1.5: Integration and academic measures by first choice college outcome.

	Attended first choice		Rejected by first choice		Accepted, did not attend	
Variable	Mean	(SE)	Mean	(SE)	Mean	(SE)
Academic and Social Integration						
Academic integration						
Survey measure	0.07	(0.05)	-0.02	(0.14)	-0.08	(0.12)
Interview measure (1-3)	2.40	(0.17)	2.39	(0.63)	2.30	(0.27)
Social integration						
Survey measure	0.08	(0.06)	-0.06	(0.13)	-0.19**	(0.11)
Interview measure (1-3)	2.29	(0.15)	2.00*	(--)	2.30	(0.27)
Academic Outcomes						
Retention in college						
Year 2 (pct)	93.4	(1.3)	91.1	(3.9)	88.9	(3.3)
Year 3 (pct)	85.2	(2.0)	86.4	(4.9)	82.3	(4.3)
Any transfer (pct)	24.6	(2.3)	38.2*	(6.6)	35.0*	(5.1)
Total UW System credits earned	69.1	(1.4)	74.4	(4.1)	65.0	(3.1)
Cumulative GPA	2.66	(0.04)	2.79	(0.10)	2.43**	(0.10)
Number of Observations	393		60		101	
Interview Sample	9		3		3	

Sources: WSLS baseline survey and interview (academic and social integration), National Student Clearinghouse, University of Wisconsin System, and Wisconsin Technical College System (enrollment and transfer), UW System (all other academic outcomes)

Notes:

(1) * signifies p<.10, ** signifies p<.05, and *** signifies p<.01 (each group is compared to students attending their first choice college).

(2) The number of terms enrolled includes only fall and spring semesters.

(3) The survey measures of academic and social integration are standardized to have a mean of zero and standard deviation of one.

(4) Credit and GPA outcomes are only observed for students who start and remain within the University of Wisconsin System.

(5) All credit and GPA outcomes are trimmed to reflect unlikely or impossible reports.

Table 1.6: Estimated impact of first choice college attendance (vs. rejected students).

Measure	Academic integration	Social integration	Year 2 retention	Year 3 retention	Any transfer	Credits earned	Overall GPA
Attended first choice	0.134	-0.056	1.9	-2.3	17.2	-0.3	-0.061
(vs. rejected)	(0.249)	(0.274)	(6.6)	(10.4)	(12.1)	(6.1)	(0.168)
Race: Black	-0.304	0.069	2.2	11.8	3.4	-0.6	0.005
	(0.214)	(0.270)	(8.2)	(10.0)	(11.1)	(6.6)	(0.201)
Race: Hispanic	-0.155	-0.108	5.0	-2.5	11.0	-3.4	-0.141
	(0.223)	(0.279)	(4.5)	(10.1)	(10.9)	(5.5)	(0.132)
Race: Southeast Asian	0.072	0.020	10.4*	6.1	-19.6**	8.0	-0.017
	(0.213)	(0.249)	(5.3)	(8.1)	(9.3)	(5.7)	(0.164)
Race: Native American	0.556**	0.340	4.6	8.4	-8.2	-0.3	0.150
	(0.241)	(0.243)	(7.4)	(9.3)	(10.3)	(7.4)	(0.199)
Female	-0.081	-0.220**	3.4	0.4	7.7*	-0.8	0.121*
	(0.091)	(0.094)	(2.5)	(3.2)	(4.1)	(2.4)	(0.066)
Parent ed: No HS	-0.203	0.343*	3.8	2.0	7.0	0.9	0.033
	(0.228)	(0.195)	(3.9)	(6.2)	(8.4)	(4.8)	(0.125)
Parent ed: Some college	-0.106	0.203*	1.6	4.3	-0.7	1.5	-0.049
	(0.106)	(0.112)	(2.9)	(4.1)	(4.7)	(2.9)	(0.082)
Parent ed: BA or more	-0.189	0.249**	0.3	0.1	0.4	5.5*	0.087
	(0.128)	(0.126)	(3.5)	(4.9)	(6.0)	(3.3)	(0.091)
Ed asp: Master's degree	-0.416***	0.065	6.3**	6.9*	-7.2	4.9*	0.097
	(0.100)	(0.109)	(2.7)	(4.0)	(4.7)	(2.8)	(0.078)
Ed asp: Professional degree	-0.410***	-0.106	0.9	-0.4	-6.3	3.5	0.030
	(0.125)	(0.125)	(3.2)	(4.6)	(5.4)	(3.2)	(0.085)
College cost important (1-5)	-0.010	0.009	-1.8	-0.9	-3.6*	0.4	-0.007
	(0.041)	(0.044)	(1.1)	(1.5)	(1.9)	(1.1)	(0.031)
EFC ($1,000s)	0.003	-0.045	1.8	0.2	1.6	-0.0	0.004
	(0.051)	(0.053)	(1.2)	(1.7)	(2.3)	(1.3)	(0.036)
Parent AGI ($1,000s)	-0.003	0.004	-0.1	-0.1	-0.2	0.0	0.001
	(0.004)	(0.004)	(0.1)	(0.1)	(0.2)	(0.1)	(0.003)
ACT composite score	-0.040**	-0.007	0.5	0.3	-0.5	0.8*	0.031**
	(0.016)	(0.018)	(0.4)	(0.6)	(0.8)	(0.5)	(0.013)
High school GPA	-0.087	-0.087	5.6*	18.4***	-6.2	11.6***	0.602***
	(0.112)	(0.135)	(3.2)	(5.2)	(5.6)	(3.5)	(0.104)
Sample size	494	494	494	494	494	472	472

Sources: WSLS baseline survey (academic and social integration), National Student Clearinghouse, UW System, and WTCS (enrollment and transfer), UW System (all other academic outcomes).

Notes:

(1) * signifies p<.10, ** signifies p<.05, and *** signifies p<.01.

(2) A positive coefficient favors students who attended their first choice college.

(3) The models also include high school and college characteristics and college fixed effects, as well as survey measures of the perception of adulthood, the number of college applications submitted, and number of siblings. All covariates are available upon request.

(4) Outcomes for GPA and credits are only available for students who began in the UW System.

Table 1.7: Estimated impact of first choice college attendance (vs. non-attending students).

Measure	Academic integration	Social integration	Year 2 retention	Year 3 retention	Any transfer	Credits earned	Overall GPA
Attended first choice	0.045	0.110	3.1	-10.2*	-11.0	1.0	-0.180
(vs. accepted, not attended)	(0.194)	(0.204)	(5.2)	(5.5)	(9.9)	(4.7)	(0.122)
Race: Black	0.021	0.219	6.2	11.5	-1.3	0.8	0.047
	(0.232)	(0.267)	(7.6)	(10.4)	(10.9)	(7.3)	(0.197)
Race: Hispanic	-0.085	-0.192	3.8	-3.3	4.9	-2.9	-0.063
	(0.228)	(0.307)	(4.9)	(11.0)	(11.4)	(5.9)	(0.146)
Race: Southeast Asian	0.270	-0.109	5.8	1.7	-28.0***	12.7**	0.123
	(0.238)	(0.270)	(5.9)	(8.6)	(9.8)	(6.2)	(0.168)
Race: Native American	0.704***	0.258	-1.4	13.3	-16.3	4.2	0.209
	(0.266)	(0.280)	(9.2)	(8.1)	(10.0)	(8.1)	(0.221)
Female	0.010	-0.241**	3.3	-2.1	9.3**	-0.9	0.122*
	(0.095)	(0.099)	(2.4)	(3.5)	(4.4)	(2.5)	(0.069)
Parent ed: No HS	-0.168	0.290	0.9	-0.2	8.7	-3.8	-0.156
	(0.242)	(0.212)	(5.2)	(6.7)	(8.4)	(5.0)	(0.133)
Parent ed: Some college	-0.174	0.234**	2.5	1.1	3.9	2.1	-0.051
	(0.106)	(0.117)	(2.9)	(4.3)	(4.8)	(3.2)	(0.084)
Parent ed: BA or more	-0.171	0.225*	0.2	-5.0	5.2	4.9	0.040
	(0.131)	(0.132)	(3.6)	(5.3)	(6.2)	(3.6)	(0.093)
Ed asp: Master's degree	-0.332***	0.108	5.8**	8.7**	-4.5	6.6**	0.093
	(0.1050	(0.112)	(2.7)	(3.9)	(5.0)	(2.9)	(0.078)
Ed asp: Professional degree	-0.242*	-0.026	3.3	0.5	-6.0	4.7	0.075
	(0.123)	(0.125)	(3.4)	(4.7)	(5.4)	(3.4)	(0.090)
College cost important (1-5)	-0.083**	-0.012	-2.1*	-0.3	-2.8	0.3	0.004
	(0.041)	(0.046)	(1.1)	(1.6)	(2.1)	(1.2)	(0.031)
EFC ($1,000s)	0.027	-0.103*	0.1	0.0	0.9	-0.0	-0.027
	(0.058)	(0.056)	(1.2)	(1.9)	(2.6)	(1.5)	(0.038)
Parent AGI ($1,000s)	-0.003	0.006	0.0	-0.1	-0.1	0.0	0.002
	(0.004)	(0.004)	(0.1)	(0.1)	(0.2)	(0.1)	(0.003)
ACT composite score	-0.031*	-0.005	0.4	0.6	-0.9	0.7	0.036**
	(0.017)	(0.019)	(0.5)	(0.6)	(0.8)	(0.5)	(0.014)
High school GPA	-0.079	-0.038	5.2	14.7***	-6.8	12.8***	0.614***
	(0.122)	(0.140)	(3.4)	(5.6)	(5.8)	(3.6)	(0.114)
Sample size	453	453	453	453	453	432	432

Sources: WSLS baseline survey (academic and social integration), National Student Clearinghouse, UW System, and WTCS (enrollment and transfer), UW System (all other academic outcomes).

Notes:

(1) * signifies p<.10, ** signifies p<.05, and *** signifies p<.01.

(2) A positive coefficient favors students who attended their first choice college.

(3) The models also include high school and college characteristics and college fixed effects, as well as survey measures of the perception of adulthood, the number of college applications submitted, and number of siblings. All covariates are available upon request.

(4) Outcomes for GPA and credits are only available for students who began in the UW System.

Table 1.8--Regression estimates of attendance on integration and academic outcomes (highly selective first choice).

Attended vs…	Rejected by first choice		Accepted, did not attend	
Outcome	Raw	Adjusted	Raw	Adjusted
Academic and Social Integration				
Academic integration	0.432**	0.222	0.231	0.065
	(0.190)	(0.646)	(0.309)	(0.350)
Social integration	0.445**	1.041	-0.204	-0.201
	(0.176)	(0.638)	(0.185)	(0.271)
Academic Outcomes				
Retention in college				
Year 2 (pct)	0.7	-6.5	6.8	-0.5
	(2.3)	(4.9)	(7.7)	(1.4)
Year 3 (pct)	-2.2	-12.4*	4.6	-7.2
	(5.8)	(7.3)	(10.5)	(6.0)
Any transfer (pct)	-24.2***	-4.3	-7.4	17.5*
	(8.4)	(18.3)	(11.4)	(9.6)
Total UW System credits earned	5.1	-0.5	0.7	-1.1
	(4.0)	(5.5)	(7.8)	(7.3)
Cumulative GPA	0.30**	-0.26*	0.09	-0.05
	(0.12)	(0.15)	(0.20)	(0.14)
Number of Observations	137		95	

Sources: WSLS baseline survey and interview (academic and social integration), National Student Clearinghouse, University of Wisconsin System, and Wisconsin Technical College System (enrollment and transfer), UW System (all other academic outcomes).

Notes:

(1) * signifies p<.10, ** signifies p<.05, and *** signifies p<.01.

(2) Each group of students is compared to those attending their first choice college.

(3) The raw model includes no covariates, and the adjusted model includes all student characteristics and college fixed effects.

(4) Positive coefficients mean that students who attended their first choice are favored; negative coefficients mean the opposite.

(5) Outcomes for GPA and credits are only available for students who began in the UW System.

(6) A college is defined as being highly selective if its median ACT score is 25 or higher. This includes three of 13 UW System universities.

A Proposed Cost-Adjusted Value-Added Measure to Address the Limitations of Popular College Rankings

(with Douglas N. Harris, Tulane University)

Keywords: College rankings; value-added; cost-effectiveness

Information plays an important role in the higher education marketplace and serves as a de facto accountability mechanism. Students, their families, and policymakers have access to a wide range of sources regarding college performance, the provision of which may affect students' college choice preferences (Kelly & Schneider, 2011). Information sources include college websites, insights from family members or friends who attended a particular college, and federally required information about graduation rates and other outcomes.

Various college rankings and ratings, such as *U.S. News and World Report*, *Barron's*, *Washington Monthly* and *Fiske's Guide*, among others, are also influential information sources which affect the actions of colleges and prospective students alike (e.g., McDonough, Antonio, Walpole, & Perez, 1998). Students from higher-income families and those considering out-of-state institutions are more likely to be influenced by the rankings, as are higher education administrators (Bastedo & Bowman, 2011). A small change in the rankings, especially at top-tier universities, can significantly change the composition of the student body (Monks & Ehrenberg, 1999; Meredith, 2004; Bowman & Bastedo, 2009).

The way in which the majority of college rankings incorporate inputs and outcomes is problematic for two primary reasons. First, most rankings evaluate colleges based more on the characteristics of the students they attract instead of what colleges contribute to student learning and other outcomes. This provides an incentive for colleges to recruit students who are the most

likely to succeed regardless of what colleges do once students arrive on campus. Additionally, most college rankings either do not take cost into account or actually reward colleges for spending more money. The main thing we learn from these ratings is that highly-regarded colleges charge more, spend more, attract students with stronger academic backgrounds and build stronger reputations. Reputation, in turn, is largely driven by historical circumstances and other factors that are already part of the rankings, such as students' academic background. Although attending a more prestigious college may guarantee a student better peers, it by no means guarantees better instructional quality and program performance.

The proliferation of college rankings complicates policymakers' efforts to increase college completion rates during a period of limited resources. Completion rates among students enrolled at four-year institutions have stagnated at around 60 percent (Bound, Lovenheim, & Turner, 2010), while the cost per degree has risen faster than inflation. Research suggests that many of the instructional and non-instructional programs in wide use in higher education are reducing rather than increasing productivity in terms of degree completion (Harris & Goldrick-Rab, 2010). The rising cost of college has also been attributed to factors other than the colleges and their performance (e.g., Baumol & Blackman, 1995; Breneman, 2001; Ehrenberg, 2000; Vedder, 2004; Weisbrod, Ballou, & Asch, 2008; Harris & Goldrick-Rab, 2010).[13] The fact that more resources are required to generate similar educational outcomes gives policymakers, parents, and students good reasons to be concerned about what they are getting from—and giving to—colleges and universities. Some states and the federal government are considering addressing these problems with more rigorous accountability systems in higher education (e.g. U.S. Department of Education, 2006; National Research Council, 2012), which would also

[13] Jacob, McCall, and Stange (2013) provide evidence that some of the additional spending is a function of student demand for amenities such as better housing and recreational facilities, which are not classified as core educational expenditures.

require some type of college performance measure.

One possible way to improve accountability systems could be to use "value-added" or "input-adjusted" measures, which explicitly account for differences in the students who attend schools and colleges. Value-added measures, although somewhat controversial, have rapidly expanded in K-12 education along with increased standardized testing (Glazerman et al., 2010; Harris, 2009a, 2011). With annual student outcome measures, it is possible to estimate gains after accounting for student characteristics.

Value-added work is in its infancy in higher education, in part due to a lack of repeated outcome measures. Most prior studies that have estimated education production functions have focused on the coefficients rather than on the residual value-added (e.g. Astin, 1997; Bailey, Calcagno, Jenkins, Leinbach, & Kienzl, 2006; Scott et al., 2006). Some researchers have estimated value-added to degree completion for a range of institutions. Two examples of this work are by Mortenson (2011), who compares actual and predicted graduation rates for the top 197 national universities and top 189 liberal arts colleges in the 2011 *U.S. News* rankings, and Archibald and Feldman (2008), who estimate technical efficiency scores with respect to graduation rates and rank 187 research universities.

By including per-student educational expenditures as a covariate, however, it is not clear that the Archibald and Feldman (2008) approach yields a complete measure of college performance. The reason is that the education production function displays diminishing returns to scale, in which providing additional resources yields progressively smaller improvements in outcomes. This makes sense for understanding how educational outcomes are produced, but not for designing performance measures that aim to encourage the efficient use of resources. Instead, we incorporate cost by taking more of a cost-effectiveness approach. Specifically, we start by

estimating a value-added measure, but then divide this by the amount of resources used. Most other prior studies that have estimated education production functions have focused on the coefficients rather than on the residual value-added (e.g. Mortenson, 1997; Astin, 1997; Hamrick, Schuh, & Shelley, 2004; Scott et al., 2006; Bailey, Calcagno, Jenkins, Kienzl, & Leinbach, 2005; Yunker, 2005; Bailey, Calcagno, Jenkins, Leinbach, & Kienzl, 2006).[14]

While value-added estimates have been generated in previous research, there has been no work estimating a college's cost-effectiveness. In this paper, we expand on prior research by adjusting a college's estimated performance in graduation students for two different measures of resources. Students and their families care about how much a college contributes to the probability of graduation, given the cost they have to pay. Alternatively, policymakers are primarily concerned with the total amount of resources required to educate an additional student.[15] This approach of incorporating costs into the value-added estimate does have its limitations, particularly as students attending the same college face different prices, but we contend that an imperfect measure of costs is better than no measure at all.

We use publicly available data from the Integrated Postsecondary Education Data System (IPEDS), the Delta Cost Project, and The Institute for College Access and Success (College InSight) to estimate the predicted six-year graduation rates of first-time, full-time students at 1,286 four-year colleges and universities and then compare the predicted rates to the actual rates. These value-added measures are then adjusted for the two different types of costs. We find a negative correlation between popular college rankings and our cost-adjusted value-added estimates, suggesting that college rankings may not be capturing cost-effectiveness.

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[14] See Bailey and Xu (2012) for a complete review of the literature on research regarding input-adjusted graduation rates.

[15] An earlier version of this paper was funded as part of a larger project on measuring college performance, funded by the Gates Foundation. Other papers in this series also take a value-added approach (Cunha & Miller, 2012; Pryor & Hurtado, 2012; Wright, Fox, Murray, Carruthers, & Thrall, 2012).

Introduction to *U.S. News* and Other Rankings

U.S. News and World Report has been ranking colleges since 1983 and has become an influential agent in higher education. In 2009 (the rankings examined in this paper), *U.S. News* created its rankings using the following measures and weights: peer assessment (25%), six-year graduation rate (16-20%), first-year retention rate (4-5%), faculty resources (20%), student selectivity (15%), financial resources (10%), alumni giving rate (5%), and graduation rate performance (0-5%) (Morse & Flanigan, 2009).[16] Faculty resources, financial resources, and student selectivity collectively represent the "inputs" that colleges have to work with, while the six-year graduation rate and first-year retention represent a measure of student "outputs," although those outputs are highly correlated with inputs.[17] Notably, the financial resources measure means that *U.S. News* rewards colleges directly for using more resources, regardless of whether they are used efficiently.

The peer assessment component is arguably even more problematic because it involves college leaders ranking other colleges, which invites gaming. Clemson University offers a striking example, as its president ranked almost all other schools as being below average in the peer assessment, directed potential first-time freshmen in the lower two-thirds of their high school class to technical colleges for one year, and strategically lowered some class sizes that counted in the rankings while raising other class sizes that do not count (Watt, 2009).

The *Washington Monthly* college rankings provide a somewhat better estimate of college value-added by using three equally-weighted outcomes that are not as strongly correlated with

[16] At national universities and liberal arts colleges, the combined graduation and retention rates are worth 20% and graduation rate performance is worth 5%. At master's and baccalaureate universities, the combined graduation and retention rates are worth 25% and no weight is given to graduation rate performance.

[17] Whether the peer assessment measure reflects inputs or outputs is more ambiguous. We discuss the "graduation rate performance" measure later in this paper.

inputs: social mobility, research, and service. Specifically, these rankings include the following measures: the percentage of students receiving Pell Grants and the predicted graduation rate (social mobility), research expenditures, the number of students earning Ph.D.'s, faculty receiving significant awards, and faculty elected to national academies (research), and the percentage of graduates in the Peace Corps and ROTC as well as the percentage of work-study funds spent on service (service). However, even in this case, resources still comprise approximately one-third of the information used to make the rankings.

Barron's Profiles of American Colleges, published annually since 1991, rates colleges based on their level of admissions selectivity. There are six primary levels of selectivity, ranging from noncompetitive to most competitive (Barron's Educational Series, 2009). These ratings are based solely on the percent of students admitted and the academic characteristics of the incoming class.

All three of these rankings reflect an underlying assumption that good colleges are those that attract academically talented and wealthy students, rather than those whose instructional and other programs are of high quality and help students graduate. There is some merit to this argument. From students' and parents' perspectives, having peers with higher academic abilities can be directly beneficial for academic success (Stinebrickner & Stinebrickner, 2006; Carrell, Fullerton, & West, 2009). Fellow students also provide important social networks and some employers restrict their recruiting to colleges with high percentages of strong students—both of which open up useful career opportunities and provide social status (Brand & Halaby, 2006). However, some research also suggests that most students do not receive additional benefits from attending a highly prestigious college (Dale & Krueger, 2011). Either way, evaluating colleges based primarily on peer quality gives colleges little incentive to use resources efficiently.

The more a ranking system focuses on student background, the more colleges will focus on attracting students with a high probability of graduation (Bowman & Bastedo, 2009) and there is no guarantee they will focus on program quality. Further, if performance rankings focus more on inputs, there is a larger incentive to raise tuition and use additional resources. As a drop in rankings can be financially devastating, universities are forced to compete, often in unproductive ways, for the type of student that would help them receive a better ranking (Mause, 2009).

The problem is that none of these ranking systems really have anything to do with what might reasonably be defined as "performance." We argue that any reasonable definition has to include the quality of course offerings and instruction and how well non-instructional programs facilitate graduation and long-term success. This is especially true today with the growing challenge that colleges face in helping freshmen become graduates (Bound, Lovenheim, & Turner, 2010; Harris & Goldrick-Rab, 2010; Bailey & Dynarski, 2011).

Value-Added Components in College Rankings

As researchers have begun to apply value-added principles to both K-12 and higher education, college ranking systems have adopted small value-added components. In 1997, *U.S. News* began to apply value-added-like techniques to create a "graduation rate performance" measure that now comprises a small portion of its rankings. Specifically, *U.S. News* predicts graduation rates using per-student academic expenditures, standardized test scores, and the percentage of students who are receiving Pell Grants. The difference between predicted and actual graduation rates (the residual) then becomes the value-added measure. There are some problems with the specifics of the *U.S News* approach to value-added, as we discuss later, but the key point is that graduation rate performance constitutes only a small part of the ranking at

research and liberal arts institutions (five percent in 2009 and 7.5 percent in following years) and is excluded for other institutions.

A less well known system, College Results Online, produced by The Education Trust, also carries out value-added-like calculations.[18] The website does not yield a fixed set of comparisons but rather identifies colleges "similar" to a selected college based on an index calculated from measures like those we use to estimate value-added. The graduation rates and other information from these similar colleges are then reported. One problem, however, is that students do not always limit their searches to "similar" institutions, especially as many students apply to "reach" and "safety" colleges. Policymakers also need to know which types of colleges are most efficient to make wise decisions about funding and other higher education policies, which necessarily requires comparing *dissimilar* colleges.

In 2005, *Washington Monthly* followed the lead of *U.S. News* by including an input-adjusted graduation rate performance measure in their college rankings. Prior to 2012, the magazine estimated value-added through an average of two regressions, taking the percentage of students who receive Pell Grants and the 25th and 75th percentile scores on a standardized test (SAT or ACT) into account. The graduation rate performance score is approximately one-sixth of the total score for all institutions. In 2012, as a result of earlier versions of this article, the magazine adopted a simplified version of our methodology, which we will discuss next.[19]

Data and Methods

Data

To estimate value-added based on graduation rates, we use three years of publicly

[18] More information can be found at http://www.collegeresults.org/aboutthedata.aspx.
[19] The first author was the primary analyst for *Washington Monthly*'s 2012 college rankings. The magazine and its leaders had no influence over the content of this article.

available data (2006-2007 through 2008-2009) from three different data sources. Most of our measures of institutional and student characteristics come from IPEDS, to which colleges and universities that receive any federal funding are required to provide data. Data on education and related expenditures come from the Delta Cost Project, and supplementary data on the percentage of students receiving types of financial aid are from The Institute for College Access and Success's College InSight dataset.[20]

We started with the 1,637 four-year institutions in IPEDS that primarily offer four-year degrees, and we eliminated all institutions that do not report a six-year graduation rate in the 2008–2009 academic year; that primarily do not offer bachelor's degrees; or that do not report either (a) ACT or SAT scores or (b) the percentage of students receiving Pell Grants in at least one of the three academic years from 2006–2007 through 2008–2009. This results in 1,295 institutions. Dropping the nine for-profit institutions gives us the final sample of 1,286 colleges and universities in all 50 states. By Carnegie classification, there are 253 research universities, 528 master's universities, and 505 bachelor's universities in the full sample. The descriptive statistics of the dataset are shown in Table 2.1.

Value-Added Model

We use Ordinary Least Squares (OLS), regressing graduation rates on a large number of factors describing student characteristics, as listed in Table 2.1. A typical education production function in K-12 would include school resources like class size, though we exclude these in our baseline model because we account for costs in a different way.

We separate the estimation into two steps. In the first step, we adjust the graduation rate

[20] Some data for the percentage of students receiving Pell Grants, financial aid, and student loans is provided courtesy of The Institute for College Access & Success. College InSight, http://college-insight.org. Student debt and undergraduate financial aid data are licensed from Peterson's Undergraduate Financial Aid and Undergraduate Databases, © 2010 Peterson's, a Nelnet company, all rights reserved.

based on the characteristics of students attending the college. We know, for example, that higher-income students are more likely to persist and graduate than students from low-income families (as measured through the percentage of students receiving Pell Grants). Specifically, we estimate the predicted graduation rate for college *j* in year *t* ($Grad_{jt}$) using OLS regression with panel data and adding the variables in blocks:

$$Grad_{jt} = \beta_{0jt} + \beta_{1jt}StuAcad_{jt} + \beta_{2jt}StuFin_{jt} + \beta_{3jt}StuDem_{jt} + \beta_{4jt}Inst_{jt} + u_j + e_{jt} \quad (1)$$

Model (1) includes students' academic background, as measured through average ACT/SAT scores ($StuAcad_{jt}$) and the percentage of students receiving Pell Grants, as these are the two most commonly used characteristics in other graduation rate predictions. Model (2) adds additional student financial information, as measured by the percentage of students receiving any financial aid or student loans ($StuFin_{jt}$) and students' demographic characteristics such as race, gender, and full-time enrollment status ($StuDem_{jt}$). These first two models include those variables that pertain to student characteristics, only a few of which can be controlled through the admissions process.

Finally, Model (3) adds institutional characteristics, including (the number of undergraduate students, whether a college is primarily residential, and the percentage of students admitted ($Inst_{jt}$). A case can be made to include additional institutional characteristics, particularly whether a college is public or private or a historically black college or university, in our models. This depends on the objectives of different stakeholders. If policymakers are seeking to maximize the number of graduates, then these institutional characteristics should be omitted. But if policymakers want to reward institutional leaders who are efficient in the short term, then

we would want to include institutional characteristics that are difficult to change (e.g., public/private). The situation is similarly ambiguous from a family's perspective. If students and their families are simply trying to identify the college where they have the best chance of graduating, then the institutional characteristics should not matter and therefore should not be included as covariates. But institutional characteristics have been shown to capture some aspect of student performance that the other student characteristics do not (e.g., Monks, 2000), so they could be included for the same reason student characteristics are included in Models (1) and (2). Although we follow Archibald and Feldman (2008) and exclude measures for public/private control and HBCU status in our preferred model, we do include these characteristics as robustness checks. We also impute a small amount of data (less than five percent of observations) for missing covariates.

Our estimate of value-added is u_j, or what is, in econometric terms, the institutional effect. Below, we call this the (unadjusted) value-added measure for college j ($VA(Unadj)$). The term e_{jt} is then the residual and reflects the annual deviation of the predicted and actual outcomes. We do not necessarily because that u_j, reflects the causal influence of each institution in the way the word "effect" might imply, but use this term anyway because this is what we intend to measure. For this to be an actual causal effect, it would have to be the case that the institutional effect reflects differences in college performance but not other differences between students that affect graduation that we cannot measure. For example, it seems likely that students who score 1400 on the SAT and attend Western Kentucky University are probably different from students with the same scores who attend Princeton. How different are these students? There is no way to know for sure with the available data. Even after accounting for student backgrounds and other factors in the equation, there are probably differences among students that we cannot

measure. This means the residual likely captures part of what we do want—college performance—as well as differences in students that are unrelated to college performance. If we were estimating this model with a single year of data, this term would drop out and we could refer to u_j simply as the "residual," or the difference between actual and predicted outcomes.

We estimate value-added models using panel data from the 2006-07 to 2008-09 academic years, as well as separately by year. Our preferred estimates are panel models using random effects and maximum likelihood estimation, although we do estimate models with fixed effects as a sensitivity check and briefly discuss those results. Random effects are arguably more appropriate than fixed effects in this case because of the difficulties of identifying the coefficients of the fixed effect covariates (Ehrenberg & Zhang, 2005). Although the results of a Hausman test suggest that the use of fixed effects is appropriate in this context, the random effects coefficients are more in line with what theory would suggest (i.e. that a higher percentage of students who come from groups which are more likely to graduate should result in higher predicted graduation rates), while the fixed effects coefficients often have the opposite sign.

Cost adjustments

The above calculations do not account for any type of costs and therefore fall short of a true value-added measure in our view. Some prior studies account for cost within the value-added model, generally by including expenditures or some similar measure in equation (1). But that approach assumes that college leaders have no control over both the available amount of resources (through tuition and fees, endowment funds, and/or state appropriations) and how these resources are then allocated. If high-spending colleges typically use their additional resources less effectively, then such an approach improves their apparent performance by reducing the influence of high spending levels on predicted outcomes. Finally, this approach never rewards

colleges that generate the same outcomes for a lower cost even though keeping costs low is an objective for both families and policymakers. A simple example illustrates: Suppose two colleges have the same outcomes, but College A spends twice as much as B. A reasonable metric of institutional effectiveness will rank B above A, but the standard model where costs are included in equation (1) often does not yield that verdict.

Instead, we take the lead of basic cost-effectiveness analysis and divide the value-added measure by various measures of costs. This provides a natural metric for "bang for the buck" and is essentially the same as the effectiveness-cost ratio, common in program evaluation (Levin & McEwan, 2001; Harris, 2009b). Like cost-adjusted value-added, the effectiveness-cost ratio divides the estimated impact of a program by the cost of resources involved in generating that impact. This is helpful for policymakers who are making decisions about how to invest in higher education. Implicitly, state legislators make decisions every year about which institutions warrant the most public resources, and they ultimately choose what they see as an optimal mix. Effectiveness-cost ratios are also useful to students and their families, although they face a different set of costs than do policymakers. The decision to attend a particular college still depends on both costs and benefits, and families may find both unadjusted and cost-adjusted value-added measures useful.

We first normalize the value-added estimate so the lowest-performing institution has an estimate of zero in order to make meaningful comparisons across institutions. Otherwise, an extremely inexpensive college with a slightly negative unadjusted value-added would have lower value-added than one with a slightly positive unadjusted value-added that uses three times the resources. Without this normalization, the negative unadjusted figures could give the false impression that many colleges are harming students—a "negative effect"—which could be

misleading.

We use two different measures of costs because policymakers and students/families care about different types of costs. Policymakers are concerned about the costs to society; therefore, this cost adjustment (CA) is made based on total educational expenses (in thousands):

$$VA_{CA}(Policy)_j = \frac{VA(Unadj)_j}{EdExp_j} \quad (2)$$

where educational expenses consist of the education and related expenses category as defined by the Delta Cost Project; this includes instructional and student services expenditures as well as a portion of expenditures for academic and administrative support and operations and maintenance. It excludes research and public service expenditures, among other types of expenditures which do not directly benefit undergraduate students.

In the student/family model, we divide by tuition less the average grant aid received by students at the college (in thousands) ($NetPrice_j$):

$$VA_{CA}(Family)_j = \frac{VA(Unadj)_j}{NetPrice_j} \quad (3)$$

The numerator is identical in both models, while the cost adjustment differs. An important implication of our approach is that some colleges with below-average value-added have high cost-adjusted value-added because they use so few resources.

These cost-adjusted measures reflect the estimated institutional effect with respect to graduation rates for each $1,000 in per-student costs (the denominator is either educational expenses or the net price of attendance). More intuitive are the properties of the measure: other things equal, producing more graduates improves the measure, while increasing cost reduces it. Consider the following example. Institution A has an unadjusted value-added of 0.10 and spends $10,000 per student, while Institution B has an unadjusted value-added of 0.15 and spends $30,000 per student. Institution A therefore has a cost-adjusted value-added of 0.01 (recall that

we measure costs in thousands), while Institution B has a cost-adjusted value-added of 0.005. In order to be as cost-effective as A, Institution B would have to either increase its graduation rate by 15 percentage points or cut its costs in half.

But in other respects, the cost-adjusted value-added measure is hard to interpret. In our view, it is best to view cost-adjusted value-added as an index, much like the Dow Jones Industrial Average. What does it mean when the Dow drops by 100 points? There is no simple interpretation, yet it is still viewed as a useful measure of the health of the stock market as a whole. Popular college rankings such as *U.S. News* are also based on indices. As we described in the second section, each component (e.g., peer assessment and average ACT/SAT scores) is given a certain amount of weight and scaled in a way that boils down all the information into a single number that is used to rank colleges. We are doing the same here.

Replicating the U.S. News Rankings

A key purpose of this study is to compare popular rankings to value-added and cost-adjusted value-added, based on the same data used to create the *U.S. News* rankings. To show that the differences in results are due to methodology rather than the data, we start by replicating the *U.S. News* rankings with our data (252 research universities and 225 liberal arts colleges). The results of our predicted graduation rate replications can be found in Appendix 2.1.

The correlation coefficients between the magazines' graduation rate predictions and our replications are around 0.92.[21] Three elite universities (the California Institute of Technology, Washington University in St. Louis, and Harvey Mudd College) have predicted graduation rates of greater than 100 percent. *U.S. News* apparently did not limit the maximum predicted

[21] We cannot perfectly match the value-added-like measures from *U.S. News* because they use a slightly larger number of colleges in their national rankings than we have in our dataset because we are missing data on a small percentage of colleges.

graduation rate to 100 percent, and we follow this convention throughout this paper.

Results

Value-Added Calculations

In Table 2.2, we present the results from three different panel regression models for each Carnegie classification (research, master's, and bachelor's). In the first model, we use only the percentage of students receiving Pell Grants, and the average ACT composite score, as these are the primary measures used in the *U.S. News* model. The second model adds key student demographic and financial measures, while the third model adds institutional factors that are difficult for a college to control. Although the coefficients are generally as hypothesized, we are less focused on the role of any given factor than on the value-added estimate, represented by the error term. One intuitive test of the validity of the unadjusted measures is whether the factors we account for are associated with graduation in the way we would expect. The vast majority of the coefficients are in the expected direction; for example, more students receiving Pell Grants is associated with a lower graduation rate, and higher ACT/SAT scores are associated with a higher graduation rate. The statistical significance of certain coefficients varies across Carnegie classifications (such as institutional size and race/ethnicity), but the general pattern of results is consistent.[22]

The model might not work well for schools with very high actual graduation rates because the maximum predicted outcome is restricted to 100%, which will result in value-added estimates of close to zero. We therefore took two steps. First, we checked the estimated value-added for colleges with an average graduation rate over the three year period of either 80 or 90

[22] It should be noted that some of the coefficients for the percentage of Hispanic students are positive (for master's-level institutions). This is likely because of the characteristics of the omitted group (primarily Asian students), as well as the extent to which Hispanic ethnicity is correlated with other measures such as Pell Grant receipt.

percent (122 and 36 colleges, respectively). The unadjusted value-added estimates are several percentage points higher for the high graduation rate institutions ($p<.01$), but the estimates for the student and family cost-adjusted models are lower for these colleges because of the higher costs of these institutions ($p<.05$). Second, because the high graduation rate institutions could have a different production function than others, we re-estimated the unadjusted value-added models, excluding the top 30 institutions in the *U.S. News* research university and liberal arts college rankings. This exclusion did not meaningfully affect the rankings of the institutions ranked lower by *U.S. News*. The correlations in rankings between these two approaches for the colleges that are not in the top 30 are 0.92 for research universities and 0.89 for liberal arts colleges across our models. This shows that the model performs adequately for colleges with high graduation rates, although those institutions are not our primary focus in this paper. In Appendix 2.2, we present the results for model (3) by Carnegie classification and year as a robustness check. Although the regression coefficients are similar using one or three years of data, the precision of our value-added estimates is significantly improved by using multiple years of data, especially for smaller colleges.

To highlight the potential significance of pooling the data, Table 2.3 reports the correlation in value-added measures across years, or "stability," by Carnegie classification. The correlation in value-added just two years apart (2006-07 and 2008-09) is 0.75 for research institutions, but below 0.60 for bachelor's and master's-level colleges. One possible reason for the apparently low correlation is random error. Value-added measures improve systematic error by reducing the disadvantages of institutions serving disadvantaged students, but at a cost of increasing random error (Glazerman et al., 2010; Harris, 2009a, 2011). The correlations between rankings estimated separately for each year (from Appendix 2.2) and the panel rankings are all

around 0.8; the correlations between rankings estimated for individual years are around 0.6 for bachelor's-level colleges and 0.8 for research universities. Since the variation over time most likely reflects random error, as opposed to true differences in performance, this reinforces the importance of pooling data across years.[23]

Figure 1 plots the distribution of the unadjusted value-added measures ($VA(Unadj)$), using the panel regressions in Table 2.2 to predict the average six-year graduation rate for all three academic years (estimated separately by Carnegie classification). The distribution is centered at zero, with a standard deviation of approximately eight percentage points. This means if we could take these measures literally as being college effects, colleges can improve their graduation performance by one standard deviation in the college value-added distribution, the average graduation rate would increase by eight percentage points. The distribution of the value-added estimates is more dispersed for bachelor's-level institutions (a standard deviation of 9.0%) and master's-level institutions (8.3%) than research institutions (6.6%); part of this is probably because research institutions tend to have more students and therefore less random error than other colleges.

The distribution of $VA(Unadj)$ is quite similar for public and private institutions, although there are more private institutions at the extreme ends of the distribution (likely because of the smaller size of these institutions). We tested for differences in means and differences in the overall shape of the distribution using a Kolmogorov-Smirnov test and found no statistically significant differences between public and private institutions.

The results from the unadjusted value-added model are not inconsistent with the

[23] Although standard errors are not reported in popular rankings, Dichev (2001) shows that between seventy and eighty percent of the annual *change* in rankings is a result of random and systemic error that have nothing to do with changes in college quality. As a result, Clarke (2002) contended that placing colleges in similar groups would be more accurate than a rank order when college rankings are not statistically significant from each other, though even this would still mean that colleges near the cut-off scores would be frequently placed in the wrong groups.

hypothesis that attending a college with more resources results in a higher value-added estimate. Figure 2 tests this hypothesis by showing the distribution of unadjusted value-added scores by per-FTE educational expenditures. There is only a weak relationship between per-FTE spending (the best proxy available for institutional resources) and our unadjusted value-added estimates, with a correlation of only 0.19 between the two measures. An increase in the value-added estimate of five percentage points is associated with a per-FTE spending on educational expenses of nearly $1,400 per student. A nearly identical relationship exists when shifting from per-FTE expenditures to the net price of attendance.

Cost-Adjusted Value-Added

In the family model, we divided $VA(Unadj)$ by the overall cost of attendance less the average amount of need-based aid received by students at a given college (see equation (3) above) to get $VA_{CA}(Family)$. This model rewards colleges that either charge low sticker prices or charge higher prices but provide large amounts of grant aid to their students. As a result, we would expect public colleges to appear better in cost-adjusted value-added estimates because state subsidies reduce the net prices that students must pay. Our findings back up this hypothesis, with public colleges having substantially higher levels of cost-adjusted value-added than private colleges. Figure 3 shows the distribution of cost-adjusted value-added scores by type of institutional control. Both the means and the distributions as a whole are significantly different from each other across the public and private sectors ($p<.01$, based on a t-test and a Kolmogorov-Smirnov test, respectively).

While students and their families care about the value-added with respect to the net price of attendance, policymakers are interested in the total cost of educating a student.[24] As a result,

[24] It is theoretically unclear whether room and board expenses should be included in the total cost of educating a student. Although students at many institutions are required to live on campus for at least one year, these students

we divide $VA(Unadj)$ by per-student educational expenses to get the policymaker value-added estimate ($VA_{CA}(Policy)$). Figure 4 shows the distribution of cost-adjusted policymaker value-added scores by type of institutional control. The mean public institution has a higher value-added coefficient than the mean private institution (p<.001) and the distributions of the educational expense-adjusted value-added estimates also vary between public and private institutions (p<.001, based on a Kolmogorov-Smirnov test).

Comparing Value-Added with Popular Ranking Systems

We compare our value-added measures with the *U.S. News* rankings and *Barron's* selectivity ratings using Spearman rank order correlations separately for research universities and liberal arts colleges and present the results in Table 2.4. The correlations between our unadjusted value-added rankings and the two magazines' rankings are between 0.3 and 0.5 for both research universities and liberal arts colleges. The *U.S. News* and *Barron's* rankings are very highly correlated with each other, with correlations around 0.83 for both types of institutions.

The correlations drop considerably when we consider cost-adjusted value-added. The rank-order correlations between $VA_{CA}(Policy)$ and the two national rankings are negative. The negative correlations between $VA_{CA}(Policy)$ and the *U.S. News* and *Barron's* rankings are between -0.40 and -0.55 for both types of colleges, while $VA_{CA}(Policy)$ and $VA(Unadj)$are somewhat positively correlated (around 0.3). $VA_{CA}(Family)$ has a weak negative correlation with the two national rankings and is positively correlated with $VA(Unadj)$ and $VA_{CA}(Policy)$. The negative correlations are not surprising given that increased costs lower value-added estimates, while higher costs are implicitly viewed as a positive in college rankings.

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would still have to make housing arrangements even if they were no longer enrolled. Additionally, university housing revenues become part of an auxiliary budget, which is separate from the educational budget, at most institutions. This means that direct appropriations to institutions do not pay for housing. For these reasons, we decided to exclude housing expenses from the educational expenses category.

To provide a more concrete sense of how these alternative measures affect performance rankings, we list in Table 2.5 the top ten colleges by Carnegie classification and model type. In the unadjusted value-added model, there is a mix of public and private colleges in each category. Although the top ten lists contain some colleges that are highly regarded (such as UC-San Diego, Berea College, and the University of Florida), the lists are dominated by lesser-known institutions (such as South Carolina State University, CUNY Baruch College, and Ohio Christian University).

The top ten lists change substantially for $VA_{CA}(Family)$ and $VA_{CA}(Policy)$ compared to $VA(Unadj)$. There are more public institutions in the top ten lists for $VA_{CA}(Family)$ than $VA_{CA}(Policy)$, which comes as little surprise since students and parents pay a smaller portion of total costs in public colleges versus private ones. While many institutions make two of the three top ten lists, only South Carolina State University (research) appears on all three lists. Although these institutions could be considered exemplary, we know little about the reasons behind the high-value-added measures. They should be studied in more detail to see whether their high value-added measures are a result of institutional practices or some type of statistical error.

It is worth emphasizing the differences between our top ten lists and the results from the popular college rankings. For example, South Carolina State University (ranked seventh in the unadjusted model, eighth in the policymaker's model, and sixth in the family model for research universities) languishes in the third tier (ranked 134-190) in the *U.S. News* national university rankings and is classified as "less competitive" by *Barron's*, the second-lowest ranking.

Sensitivity Checks

We conducted multiple sensitivity checks to make sure that our results are relatively robust to different specifications and assumptions. We first compared the results of models (2)

and (3) from equation (1) to examine the effects of additional institutional characteristics (the number of students enrolled, whether the college is primarily residential, and the percentage of students admitted). The rank-order correlations between the two models are between 0.92 and 0.98 across the Carnegie classifications for both the unadjusted and cost-adjusted models.

Although we prefer our panel estimates with random effects, we also estimated the panel regressions with fixed effects (Appendix 2.3). The rank-order correlations between the random and fixed effects models generally range from 0.4 to 0.7, with somewhat higher correlations in the policymaker model than either of the other two models. Although the results of the Hausman test ($p<.01$) suggest that fixed effects should be used, the coefficients of the covariates in the fixed effects models often do not make intuitive sense, which is likely because they are being estimated on a small amount of within-institution variation.

In his critique of the *U.S. News* rankings, Porter (1999) noted that value-added estimates of graduation rates should be examined to see whether the estimate is significantly different from zero. He found that only five percent of research institutions had graduation rates that are outside their confidence interval using a somewhat smaller set of research universities than what we use. We used the panel regression estimates separately by Carnegie classification (model (3) from Table 2.2) and estimated 95% confidence intervals for the unadjusted model. With the additional years of data, we are able to estimate more precise confidence intervals and hence more institutions have statistically significant value-added. Across each category of colleges, between 35 and 40 percent of colleges have statistically significant positive value-added, with a similar proportion having significantly negative value-added.

Figure 5 presents the 95 percent confidence intervals for all research institutions. This suggests that institutions ranked within approximately 30 to 100 places of each other have

unadjusted value-added estimates that are not significantly different from each other. An example of how a top ten institution could be affected by measurement error is the University of Florida, which ranks eighth on the unadjusted model for research institutions. Its confidence interval encompasses the confidence intervals of institutions ranked from third to 34th. This suggests that an institution's precise ranking is inexact, but its general position on the distribution is only somewhat affected by the presence of measurement error.

Discussion and Limitations

The current popular college rankings clearly yield very different results compared with value-added measures. While we argue that value-added measures provide information about performance that is important and largely ignored by the existing rankings, there are four main limitations of these data: (1) we have only one student outcome at one time point; (2) we have data only at the college level; (3) we have somewhat limited information about student background; and (4) the measures are potentially problematic for institutions with extremely high graduation rates.

First, unlike in K-12 value-added research, which involves annual measures of student test scores for each student, we do not have annual measures for individual students. Students can go only from "not graduated" to "graduated." Focusing on graduation at the exclusion of educational quality not only provides a potentially misleading picture but also opens up the possibility of perverse incentives. If colleges are given incentives to produce more degrees, they can easily do so by simply making graduation easier through lowering standards or steering students toward certain "easy" majors. It would also be helpful to have data on employment, earnings, health and other longer-term outcomes (Carnevale, Melton, & Meyer, 2010); researchers such as Cunha and Miller (2012) and Wright et al. (2012) are among the first to use

state-level datasets for estimating institutional effectiveness. More states are building the capacity for such an analysis. But this cannot be done on anything close to a national level, nor for the wide variety of noneconomic benefits that are affected by education (Wolfe & Haveman, 2002). We are also unable to directly examine the contribution of individual instructional and non-instructional programs.

Second, using institutional-level instead of student-level data does have certain disadvantages. The most notable concern with institutional-level data is that, due to the substantial amount of price discrimination in higher education, students may face very different net prices. IPEDS does include the average net price for students in five household income brackets (ranging from less than $30,000 per year to more than $110,000 per year), but the number of students in each of these categories may be quite small at many universities and using multiple net prices further complicates the analysis. In an ideal world, this measure would be tailored to individuals through the use of a net price calculator. Having data on individual students would also allow us to identify effects on subgroups (e.g., adding part-time and non-first-time students whom we are forced to exclude by using IPEDS) and allow us to define outcomes in different ways (e.g., treating lateral transfers differently from dropouts). Aggregated data tend to yield a false sense of precision by ignoring the fact that certain types of students end up in certain colleges; this selection can be modeled in individual-level data.

Some research suggests that aggregated data lead to upwardly biased estimates of the role of school inputs, although this may or may not apply to higher education or the effects of entire institutions (Hanushek, Rivkin, & Taylor, 1996). Aggregated data incorporate both the effects of certain background characteristics for each individual student and the ways in which groupings of students affect overall institutional performance. For example, while student ability (as

measured by standardized test scores) affects each individual student's likelihood of graduating, high concentrations of lower-ability students may further reduce a student's likelihood of graduation (Carrell, Fullerton, & West, 2009; Zimmerman, 2003).

A third and possibly more important issue is the richness of the variables intended to capture students' backgrounds, but this issue is separate from data aggregation. The IPEDS data, with information about ACT/SAT scores and financial aid receipt, may be richer than some student-level data sets created by state governments. Ideally, we would have student-level data with rich covariates and we could test the sensitivity of results to data aggregation and the availability of student background factors.

Fourth, our measures are potentially problematic for institutions with extremely high graduation rates, even though we have shown that the results do not change when the models are re-estimated without the colleges with the highest graduation rates. None of the top ten colleges in our value-added measures are also among the top 10 in the *U.S. News* rankings. In fact, among the top ten research institutions in the *U.S. News* rankings, the highest-ranked in our ratings is Harvard, which ties for first in the *U.S. News* rankings but ranks 12th of 253 in the unadjusted value-added model, 230th in the policymaker model and 109th in the family model. This is not a surprising result because by privileging resources and prestige over institutional effectiveness, popular college rankings create a systematic disadvantage for colleges serving lower-income and modestly-prepared students.

Policy Implications and Conclusion

Different stakeholders necessarily mean different things by "college performance," but our results suggest that the current college rankings are inadequate for any stakeholder.

Students, parents, and policymakers have been taught to believe that the best colleges are those with high rankings which in turn are those with a wealth of resources, high tuition, and the most academically prepared students. Rather than encouraging efficiency, this motivates colleges to attract better students and continually raise costs and tuition—but not to use resources more wisely. This is one of the major causes behind both the college cost spiral and the stagnant graduation rates over the past two decades (Harris & Goldrick-Rab, 2010).

It is doubtful that all of the existing college ranking producers will make changes to incorporate the major flaws in their systems. However, in response to some of the concerns discussed in this paper, *Washington Monthly* did incorporate a simplified version of our methodology into their 2012 college rankings, with the first author serving as the consulting methodologist for the rankings.[25] While our value-added estimate still only makes up one-sixth of the total ranking, it is an improvement over previous measures. Any system that relied solely on a value-added measure would have to heavily emphasize that the measures are intended only as information and that no single metric could possibly account for the vast array of outcomes that students and policymakers hope to achieve. The idea of tying the measures to high-stakes decisions is questionable at this point in time, given that we know so little about them at this stage and that only one outcome (graduation) is measured.

It is only a matter of time until it will be possible to estimate value-added to longer-term outcomes such as employment for large numbers of colleges nationwide. These additional outcomes could easily be incorporated into the framework we have presented. The unadjusted value-added would become a weighted average of the value-added estimated using different outcome measures, and these separate measures could be combined and then adjusted for costs.

[25] More information on the methodology used in the *Washington Monthly* rankings can be found at http://www.washingtonmonthly.com/magazine/septemberoctober_2012/features/a_note_on_methodology_4year_co_1039359.php.

It will also be increasingly feasible to estimate value-added for different subgroups. We argue that different families have different preferences, constraints, and goals for what a college should do. The more we can target information to specific families, the more useful it is likely to be. Compared with college rankings, the basic value-added approach is likely to be more useful to a much broader range of constituencies. Low- and middle-income families often do not have the luxury of worrying about prestige and instead simply want to make sure they will be able to get a degree and a quality education. High-income families may still be more interested in prestige-based rankings, as they face fewer constraints than other families.

Value-added measures might also be a basis for learning about successful programs and practices. What distinguishes low- and high-value-added colleges? The answer to this question might be more important to improving student success than a formal accountability system. Moving from the 50^{th} to the 84^{th} percentile on our value-added measure is associated with an increase of eight percentage points in the graduation rate, *ceteris paribus*. If those differences reflect different strategies, and those strategies could be identified and scaled up, the graduation rate might be improved noticeably.

We can never measure college success perfectly, and failing to recognize performance measurement problems can have serious consequences. But, compared with current practice, there are better ways to use information and improve performance. Although we fully acknowledge the limitations of our rankings, we argue that using value-added to estimate the cost-effectiveness of colleges is a step forward.

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Table 2.1: Summary statistics of the variables.

Variable	N	Mean	St. Dev.	Min	Max
Six-year graduation rate (%)	1286	55.1	18.0	1	98
Percent receiving Pell Grants	1282	29.2	14.9	0	93
Average ACT composite	1251	22.9	3.3	12	34
Education and related expenses/FTE	1214	$17,891	$12,027	$4,773	$141,309
Receiving financial aid (%)	1286	87.8	13.6	0	100
Receiving student loans (%)	1282	60.9	19.1	2	99
Male (%)	1286	42.6	12.2	0	100
Full-time (%)	1286	85.0	12.9	8.9	100
White (%)	1286	66.5	22.1	0	97.9
Black (%)	1286	12.5	19.3	0	99.5
Hispanic (%)	1286	6.2	8.7	0	92.7
Undergraduate enrollment	1286	5,607	6,857	63	53,298
Carnegie research (%)	1286	19.7	39.8	0	100
Carnegie master's (%)	1286	41.1	49.2	0	100
Mainly residential (%)	1286	76.0	42.7	0	100
Admit rate (%)	1275	65.1	18.3	8	100

Sources: Education and related expenses is from the Delta Cost Project. All other measures are from IPEDS.

Notes:

(1) The summary statistics presented are from the 2008-09 academic year for all variables.

(2) The natural logarithm of endowment funds per FTE and undergraduate enrollment are used in the analysis.

(3) The six-year graduation rate listed is for first-time, full-time students who finish at the same institution at which they start college. We use IPEDS's revised cohort minus exclusions to generate the graduation rate.

(4) The average ACT composite score is calculated by averaging the 25th and 75 percentile scores that are provided in the IPEDS data. The average score is then rounded up to the nearest whole number. For schools that provide only SAT scores, we averaged the 25th and 75th percentile scores on the SAT verbal and mathematics sections separately and then added the two numbers to get the median SAT composite score. The SAT composite score is then converted to an ACT composite score using the ACT/SAT concordance guide (ACT, Inc., 2008).

(5) We define mainly residential campuses as the Carnegie classifications of either primarily or highly residential.

Table 2.2: Panel regression results by Carnegie classification.

Panel 1: Research institutions (n=253)

	Model (1)		Model (2)		Model (3)	
Variable	Coeff	(SE)	Coeff	(SE)	Coeff	(SE)
Percent Pell	-0.242***	(0.043)	-0.274***	(0.045)	-0.249***	(0.043)
Average ACT composite	0.028***	(0.002)	0.024***	(0.002)	0.019***	(0.002)
Percent receiving financial aid	--	--	-0.038*	(0.023)	-0.034	(0.022)
Percent receiving student loan	--	--	0.011	(0.020)	0.001	(0.019)
Percent male students	--	--	-0.169***	(0.052)	-0.131***	(0.050)
Percent full-time students	--	--	0.538***	(0.043)	0.442***	(0.045)
Percent white students	--	--	-0.038	(0.027)	-0.008	(0.026)
Percent black students	--	--	-0.043	(0.039)	-0.077**	(0.037)
Percent Hispanic students	--	--	-0.056	(0.057)	0.067	(0.055)
Log enrollment	--	--	--	--	0.011*	(0.006)
Mainly residential	--	--	--	--	0.061***	(0.011)
Admit rate	--	--	--	--	-0.104***	(0.019)
Constant	0.007	(0.057)	-0.242***	(0.061)	-0.155*	(0.084)

Panel 2: Master's institutions (n=528)

	Model (1)		Model (2)		Model (3)	
Variable	Coeff	(SE)	Coeff	(SE)	Coeff	(SE)
Percent Pell	-0.248***	(0.030)	-0.271***	(0.034)	-0.232***	(0.034)
Average ACT composite	0.021***	(0.002)	0.020***	(0.002)	0.018***	(0.002)
Percent receiving financial aid	--	--	-0.022	(0.027)	-0.040	(0.027)
Percent receiving student loan	--	--	0.090***	(0.018)	0.080***	(0.019)
Percent male students	--	--	-0.153***	(0.037)	-0.154***	(0.037)
Percent full-time students	--	--	0.306***	(0.029)	0.213***	(0.030)
Percent white students	--	--	0.022	(0.033)	0.011	(0.032)
Percent black students	--	--	-0.034	(0.038)	-0.079**	(0.038)
Percent Hispanic students	--	--	0.092*	(0.052)	0.087*	(0.051)
Log enrollment	--	--	--	--	0.007	(0.005)
Mainly residential	--	--	--	--	0.069***	(0.010)
Admit rate	--	--	--	--	-0.072***	(0.016)
Constant	0.126***	(0.044)	-0.079	(0.054)	-0.010	(0.072)

Table 2.2: Panel regression results by Carnegie classification (continued).

Panel 3: Bachelor's institutions (n=505)

	Model (1)		Model (2)		Model (3)	
Variable	Coeff	(SE)	Coeff	(SE)	Coeff	(SE)
Percent Pell	-0.290***	(0.031)	-0.337***	(0.034)	-0.308***	(0.035)
Average ACT composite	0.028***	(0.002)	0.025***	(0.002)	0.024***	(0.002)
Percent receiving financial aid	--	--	-0.041	(0.029)	-0.023	(0.030)
Percent receiving student loan	--	--	0.019	(0.020)	0.016	(0.020)
Percent male students	--	--	-0.107***	(0.030)	-0.110***	(0.029)
Percent full-time students	--	--	0.325***	(0.035)	0.285***	(0.039)
Percent white students	--	--	0.002	(0.038)	0.016	(0.038)
Percent black students	--	--	0.043	(0.044)	0.026	(0.044)
Percent Hispanic students	--	--	-0.195*	(0.110)	-0.173	(0.109)
Log enrollment	--	--	--	--	0.018***	(0.006)
Mainly residential	--	--	--	--	0.049***	(0.015)
Admit rate	--	--	--	--	-0.045**	(0.019)
Constant	-0.005	(0.042)	-0.137**	(0.064)	-0.251***	(0.086)

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01. Standard errors are in parentheses.

(2) We used multiple imputation on a small number of observations.

Table 2.3: Stability of value-added measures over time
(Spearman rank-order correlations).

Panel 1: Research institutions (n=253)

Model	Panel	2008-2009	2007-2008	2006-2007
Panel	1	--	--	--
2008-2009	0.861	1	--	--
2007-2008	0.856	0.837	1	--
2006-2007	0.830	0.747	0.795	1

Panel 2: Master's institutions (n=528)

Model	Panel	2008-2009	2007-2008	2006-2007
Panel	1	--	--	--
2008-2009	0.843	1	--	--
2007-2008	0.835	0.668	1	--
2006-2007	0.803	0.593	0.771	1

Panel 3: Bachelor's institutions (n=505)

Model	Panel	2008-2009	2007-2008	2006-2007
Panel	1	--	--	--
2008-2009	0.817	1	--	--
2007-2008	0.828	0.588	1	--
2006-2007	0.797	0.546	0.639	1

Notes:
(1) All correlations are statistically significant from zero at $p<.01$.

Table 2.4: Spearman rank-order correlations among ranking systems.

Research universities (n=251)

Model	*U.S. News*	*Barron's*	Policymaker	Family	Overall
U.S. News	1	--	--	--	--
Barron's	0.836	1	--	--	--
Policymaker's model	-0.345	-0.443	1	--	--
Family model	-0.108^	-0.270	0.735	1	--
Unadjusted model	0.526	0.366	0.326	0.383	1

Liberal arts colleges (n=222)

Model	*U.S. News*	*Barron's*	Policymaker	Family	Overall
U.S. News	1	--	--	--	--
Barron's	0.833	1	--	--	--
Policymaker's model	-0.454	-0.516	1	--	--
Family model	-0.274	-0.366	0.663	1	--
Unadjusted model	0.505	0.317	0.338	0.380	1

Notes:
(1) Rank-order correlations were only calculated for institutions which were ranked by both *U.S. News* and *Barron's*.
(2) Institutions that were tied in the *U.S. News* rankings were assigned the median value of all the tied schools. For example, if there was a five-way tie for first place, all five institutions would be assigned a ranking of 3.
(3) All correlations are statistically significant from zero at $p<.01$ except those marked by ^.

Table 2.5: Top ten institutions by Carnegie classification and model.

Carnegie research (n=253)

Rank	Unadjusted Model	Policymaker's Model	Family Model
1	Immaculata Univ.	Immaculata Univ.	Univ. of Florida
2	Univ. of Cal.--Davis	Univ. of Central Florida	North Carolina A&T
3	Univ. of La Verne	Mississippi State Univ.	Univ. of Georgia
4	Saint Louis Univ.	Oregon State Univ.	Univ. of Washington
5	Harvard Univ.	St. Mary's Univ. of MN	Tennessee State Univ.
6	Univ. of Washington	FL International Univ.	South Carolina State Univ.
7	South Carolina State Univ.	Univ. of La Verne	North Carolina State Univ.
8	University of Florida	South Carolina State Univ.	Univ. of Wyoming
9	Univ. of Cal.--San Diego	Colorado State Univ.	Florida State Univ.
10	Univ. of Texas--Austin	Old Dominion Univ.	Univ. of Cal.--Davis

Carnegie master's (n=528)

Rank	Unadjusted Model	Policymaker's Model	Family Model
1	Gwynedd-Mercy Coll.	Indiana Wesleyan Univ.	CUNY Brooklyn Coll.
2	Coll. of Notre Dame (MD)	Park Univ.	NC Central Univ.
3	The Citadel	Missouri Baptist Univ.	CUNY John Jay Coll.
4	Penn State--Harrisburg	Benedictine Univ.	CUNY Staten Island
5	Regis Univ.	Cardinal Stritch Univ.	CUNY Queens Coll.
6	Coll. of Saint Elizabeth	Wayland Baptist Univ.	The Citadel
7	Bentley Univ.	College of St. Joseph	Albany State Univ.
8	Providence Coll.	William Carey Univ.	CUNY Baruch Coll.
9	King's College	Gwynedd-Mercy Coll.	Fayetteville State Univ.
10	Saint Joseph's College (ME)	Towson Univ.	Univ. of TX--Pan American

Carnegie bachelor's (n=505)

Rank	Unadjusted Model	Policymaker's Model	Family Model
1	Martin Methodist Coll.	Ohio Christian Univ.	Berea Coll.
2	Saint Joseph's Coll. (NY)	Penn State--Shenango	Elizabeth City State Univ.
3	Coll. of Our Lady of the Elms	Martin Methodist Coll.	OK Panhandle State Univ.
4	Coll. of St. Thomas More	Central Baptist Coll.	US Air Force Academy
5	Penn State--Dubois	Penn State--Eberly	US Naval Academy
6	Ohio Christian Univ.	Penn State--Dubois	Coll. of St. Thomas More
7	Moravian Coll.	Saint Joseph's Coll. (NY)	US Coast Guard Academy
8	Berea Coll.	Rust Coll.	Am. Ind. Coll. of Assem. God
9	Am. Ind. Coll. of Assem. God	McKendree Univ.	Rust Coll.
10	St. Francis Coll.	Keystone Coll.	VA Military Institute

Figure 1--Unadjusted Value-Added Panel Estimates for Public vs. Private Institutions

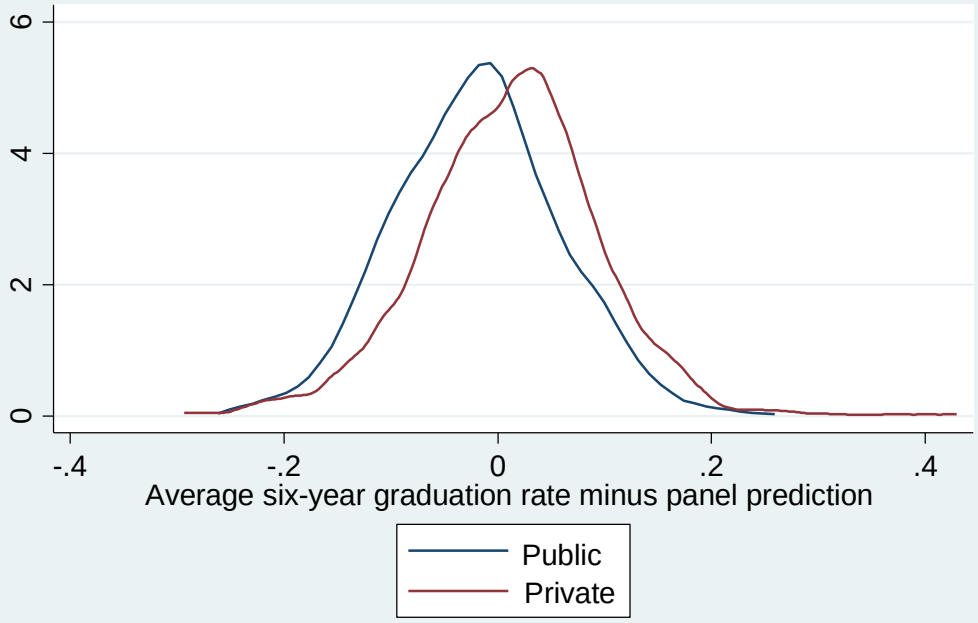


Figure 2--Distribution of Unadjusted Value-Added Estimates by Per-FTE Educational Expenditures

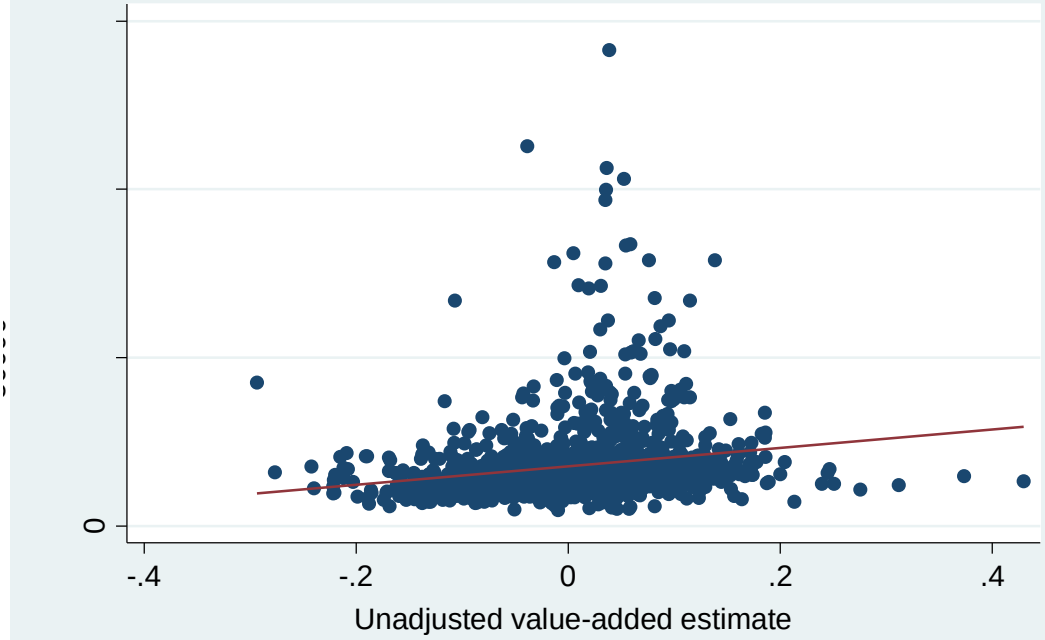


Figure 3--Families' Cost-Adjusted Value-Added for Public vs. Private Institutions

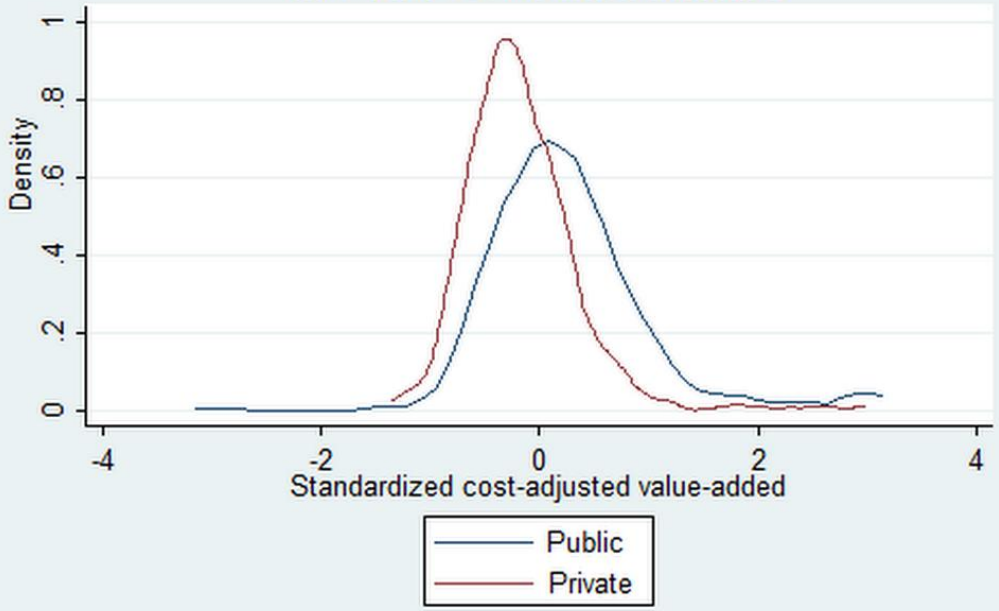
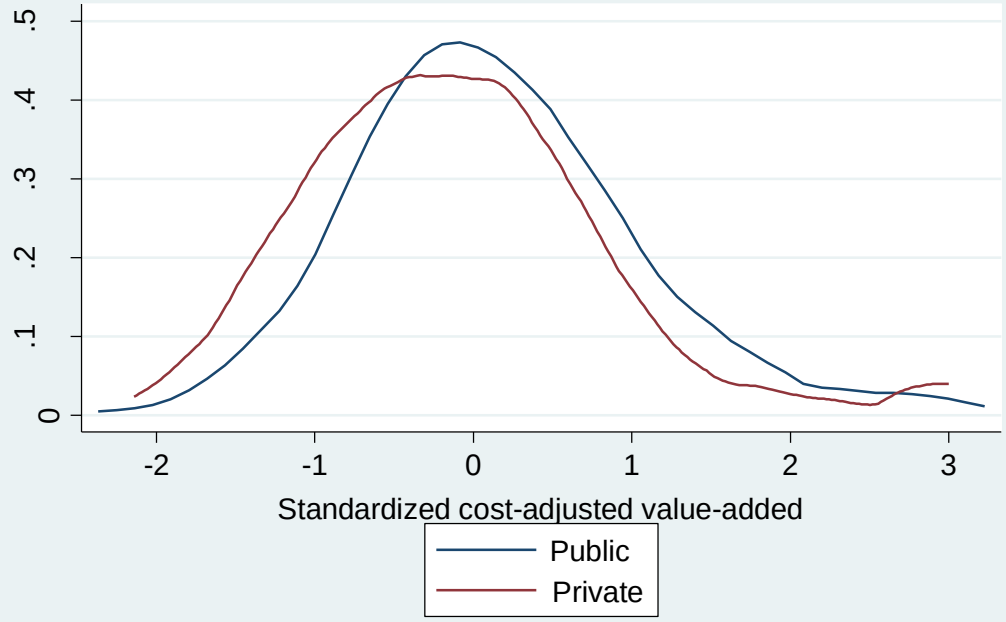
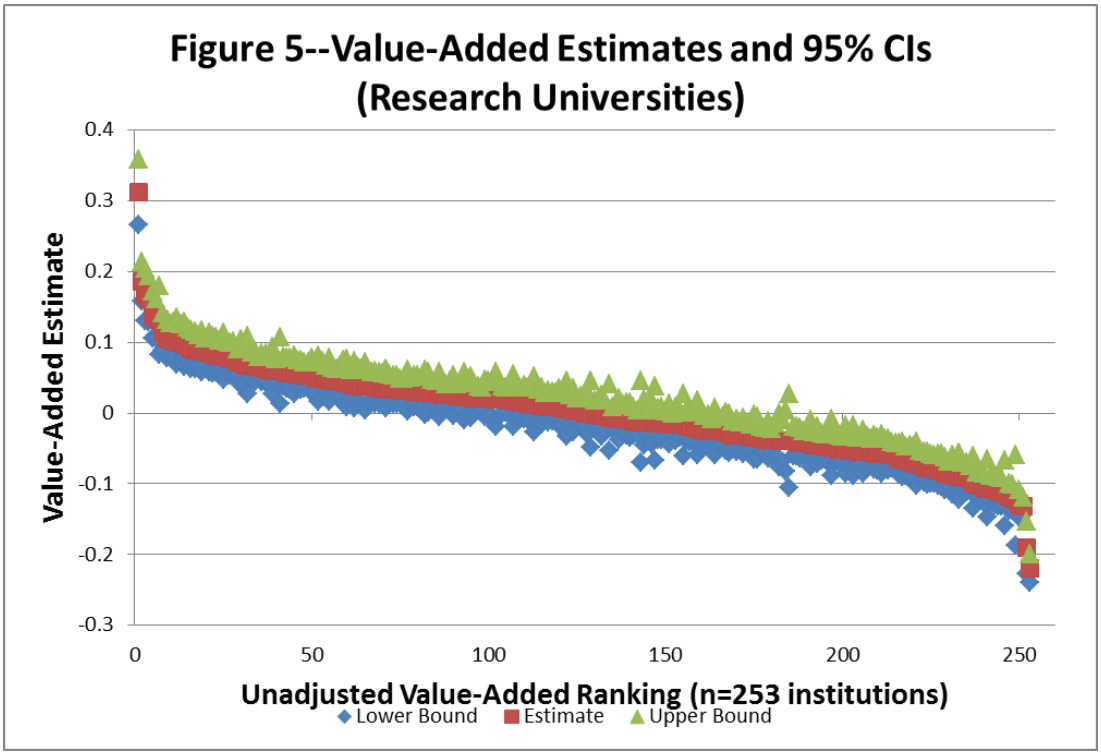


Figure 4--Policymakers' Cost-Adjusted Value-Added for Public vs. Private Institutions





Appendix 2.1: Replicating the *U.S. News* graduation rate predictions.

	All universities		Research		Liberal Arts	
Variable	Coeff.	(SE)	Coeff.	(SE)	Coeff.	(SE)
Percent Pell	-0.261***	(0.031)	-0.215***	(0.073)	-0.284***	(0.059)
ACT	0.031***	(0.002)	0.033***	(0.003)	0.031***	(0.003)
Log education expenses per FTE	0.053**	(0.019)	0.047***	(0.016)	0.054***	(0.020)
Constant	-0.593***	(0.145)	-0.598***	(0.114)	-0.597***	(0.168)
Number of colleges	1286		252		220	
Adjusted R-squared	0.668		0.758		0.748	

Notes:
(1) * represents p<.10, ** represents p<.05, and *** represents p<.01. Standard errors are in parentheses.
(2) We used multiple imputation on a small number of observations.

Appendix 2.2: Regression results by year and Carnegie classification.

Panel 1: Research institutions (n=253)

	2008-09		2007-08		2006-07	
Variable	Coeff	(SE)	Coeff	(SE)	Coeff	(SE)
Percent Pell	-0.249***	(0.093)	-0.311***	(0.071)	-0.382***	(0.073)
Average ACT composite	0.029***	(0.003)	0.030***	(0.003)	0.029***	(0.003)
Percent receiving financial aid	-0.108**	(0.047)	-0.096**	(0.038)	-0.033	(0.037)
Percent receiving student loan	0.107***	(0.040)	0.118***	(0.035)	0.103***	(0.036)
Percent male students	-0.246***	(0.058)	-0.199***	(0.050)	-0.179***	(0.054)
Percent full-time students	0.393***	(0.055)	0.481***	(0.049)	0.386***	(0.052)
Percent white students	-0.065	(0.042)	-0.038	(0.036)	-0.074*	(0.041)
Percent black students	-0.095*	(0.056)	-0.027	(0.045)	-0.021	(0.051)
Percent Hispanic students	0.019	(0.073)	0.135**	(0.062)	0.080	(0.073)
Log enrollment	0.008	(0.007)	0.016***	(0.006)	0.019***	(0.007)
Mainly residential	0.038***	(0.012)	0.040***	(0.011)	0.044***	(0.012)
Admit rate	-0.060*	(0.036)	-0.060*	(0.032)	-0.064*	(0.034)
Constant	-0.242*	(0.132)	-0.464***	(0.109)	-0.396***	(0.125)
Adjusted R-squared	0.842		0.873		0.854	

Panel 2: Master's institutions (n=528)

	2008-09		2007-08		2006-07	
Variable	Coeff	(SE)	Coeff	(SE)	Coeff	(SE)
Percent Pell	-0.216***	(0.056)	-0.462***	(0.050)	-0.505***	(0.048)
Average ACT composite	0.030***	(0.003)	0.023***	(0.002)	0.023***	(0.002)
Percent receiving financial aid	-0.123**	(0.052)	-0.017	(0.043)	0.010	(0.038)
Percent receiving student loan	0.217***	(0.032)	0.164***	(0.029)	0.142***	(0.028)
Percent male students	-0.126***	(0.044)	-0.154***	(0.041)	-0.232***	(0.040)
Percent full-time students	0.196***	(0.038)	0.275***	(0.036)	0.271***	(0.035)
Percent white students	-0.108**	(0.044)	-0.063	(0.039)	-0.045	(0.036)
Percent black students	-0.158***	(0.051)	-0.037	(0.048)	0.041	(0.045)
Percent Hispanic students	0.034	(0.066)	0.121**	(0.059)	0.110*	(0.057)
Log enrollment	0.010	(0.007)	0.013**	(0.006)	0.010	(0.006)
Mainly residential	0.059***	(0.011)	0.039***	(0.010)	0.036***	(0.010)
Admit rate	-0.101***	(0.028)	-0.083***	(0.027)	-0.052**	(0.027)
Constant	-0.180*	(0.108)	-0.156	(0.097)	-0.122	(0.092)
Adjusted R-squared	0.602		0.638		0.634	

Appendix 2.2: Regression results by year and Carnegie classification (continued).
Panel 3: Bachelor's institutions (n=505)

	2008-09		2007-08		2006-07	
Variable	Coeff	(SE)	Coeff	(SE)	Coeff	(SE)
Percent Pell	-0.320***	(0.055)	-0.403***	(0.055)	-0.430***	(0.056)
Average ACT composite	0.031***	(0.002)	0.032***	(0.002)	0.027***	(0.002)
Percent receiving financial aid	-0.030	(0.047)	0.004	(0.045)	-0.023	(0.042)
Percent receiving student loan	0.096***	(0.036)	0.081**	(0.032)	0.065**	(0.032)
Percent male students	-0.067*	(0.036)	-0.128***	(0.035)	-0.109***	(0.035)
Percent full-time students	0.181***	(0.050)	0.222***	(0.049)	0.355***	(0.047)
Percent white students	0.062	(0.050)	0.035	(0.050)	-0.026	(0.051)
Percent black students	0.058	(0.057)	0.151***	(0.056)	0.102*	(0.057)
Percent Hispanic students	-0.097	(0.133)	-0.013	(0.138)	-0.159	(0.146)
Log enrollment	0.006	(0.008)	0.013*	(0.008)	0.011	(0.008)
Mainly residential	0.029	(0.018)	0.044**	(0.018)	-0.004	(0.018)
Admit rate	-0.111***	(0.031)	-0.064**	(0.032)	-0.044	(0.031)
Constant	-0.280**	(0.119)	-0.389***	(0.118)	-0.257**	(0.115)
Adjusted R-squared	0.711		0.722		0.698	

Notes:
(1) * represents p<.10, ** represents p<.05, and *** represents p<.01. Standard errors are in parentheses.
(2) We used multiple imputation on a small number of observations.

Appendix 2.3: Fixed effect panel regression results by Carnegie classification.

	Research		Master's		Bachelor's	
Variable	Coeff	(SE)	Coeff	(SE)	Coeff	(SE)
Percent Pell	-0.126***	(0.045)	0.210	(0.038)	-0.055	(0.044)
Average ACT composite	-0.002	(0.002)	0.000	(0.002)	-0.001	(0.002)
Percent receiving financial aid	0.025	(0.022)	-0.055*	(0.033)	0.072*	(0.043)
Percent receiving student loan	-0.026	(0.019)	0.000	(0.021)	-0.014	(0.024)
Percent male students	0.516***	(0.165)	-0.017	(0.156)	0.211	(0.143)
Percent full-time students	0.088	(0.089)	-0.111	(0.073)	-0.145	(0.096)
Log enrollment	0.120***	(0.035)	0.114***	(0.035)	0.051*	(0.028)
Percent white students	0.031	(0.043)	0.088	(0.062)	0.077	(0.073)
Percent black students	0.192	(0.126)	0.845***	(0.168)	-0.165	(0.164)
Percent Hispanic students	0.263	(0.245)	1.262***	(0.261)	-0.531**	(0.264)
Admit rate	-0.048**	(0.020)	-0.054***	(0.018)	0.022	(0.023)
Constant	-0.746**	(0.372)	-0.529*	(0.307)	0.178	(0.260)
Number of colleges	253		528		505	

Notes:

(1) * represents p<.10, ** represents p<.05, and *** represents p<.01. Standard errors are in parentheses.

(2) We used multiple imputation on a small number of observations.

(3) The dichotomous institutional characteristic (primarily residential) is excluded from the fixed effects regression because it does not vary within institutions.

Accelerating College Knowledge:

Examining the Feasibility of a Targeted Early Commitment Pell Grant Program

(with Sara Goldrick-Rab, University of Wisconsin-Madison)

Keywords: Pell Grant program; College attendance; Financial aid

Despite decades of public and private investment in financial aid, just 30 percent of children born to families in the bottom income quartile can expect to enroll in college, compared to 80 percent from the top income quartile (Bailey & Dynarski, 2011). Even among high school graduates, the college enrollment gap by family income is 30 percentage points (Aud et al., 2012). The college completion gap is more substantial; students from high-income families are six times more likely than those from low-income families to complete a bachelor's degree by age 25 (Bailey & Dynarski, 2011). There is growing concern that the talent loss among students from low-income families who forgo college or attend less selective colleges may be substantial, affecting the nation's economy (e.g. Lee, Jr., Edwards, Menson, & Rawls, 2011; Hoxby & Avery, 2012).

Research suggests that insufficient academic and financial preparation for college, partly attributable to the common perception that college is unaffordable and out of reach, is one reason students from low-income families under-enroll in college and often fail to complete degrees (Ellwood & Kane, 2000; Heller, 2006; Goldrick-Rab, Harris, & Trostel, 2009). Specific and accurate information about college costs is provided to students during their junior or senior year of high school, far into the college choice process (Cabrera & La Nasa, 2000). The lateness of

this intervention is most consequential for price-sensitive students, overrepresented among low-income families with less "college knowledge" and larger errors in their estimates of college costs (Horn, Chen, & Chapman, 2003; Luna de la Rosa, 2006; Grodsky & Jones, 2007; Rowan-Kenyon, Bell, & Perna, 2008; Bell, Rowan-Kenyon, & Perna, 2009).[26]

The failure to plan for college enrollment from an early point in K-12 schooling is also detrimental because the academic and financial pathways to college (especially a four-year college) are structured and sequential (Cabrera & La Nasa, 2001; Klasik, 2012). For example, the track to college-level math begins in middle school and fewer students from low-income families engage at that time, even though they disproportionately receive the benefits of early engagement in such coursework (Lucas & Berends, 2002; Long, Conger, & Iatarola, 2012). Studies also show that families who start saving for college from an early age are more likely to exhibit strong college expectations for their children and place them into appropriate academic courses (Destin & Oyserman, 2009; Elliott, Choi, Destin, & Kim, 2011). This information needs to reach students as early as possible: impacts on postsecondary enrollment are detectable for interventions as late as tenth grade (Ford et al., 2012), but are not statistically significant for information provided in twelfth grade (Bettinger, Long, Oreopoulos, & Sanbonmatsu, 2012).

The issue of the *timing* of financial aid has received relatively little attention in discussions about reforming its design and delivery. Most efforts are directed at simplifying the process for applying for aid, since Dynarski and Scott-Clayton (2008) and Dynarski, Scott-Clayton, and Wiederspan (2013) contend that the complexity of the existing financial aid

[26] Net price calculators can give students an earlier estimate of their aid packages, but these have yet to be universally implemented (Cheng et al., 2012) and still target high school juniors and seniors. The federal government's "FAFSA4caster" (http://www.mymoney.gov/content/fafsa4caster.html) is also designed to give students an estimate of their aid packages as early as middle school, but knowledge of this website appears to be very low.

application process reduces the program's efficiency even as it promotes targeting. Awareness of the aid application process is also demonstrably problematic, and early awareness may be key to ensuring that more students engage in the process even once it is simplified (Dynarski & Scott-Clayton, 2013; Dynarski & Wiederspan, 2012).[27]

For these reasons, this paper examines the feasibility of committing to provide a maximum Pell Grant (currently $5,550) to a targeted group of eighth grade students from economically disadvantaged families compared to committing in grade 12, which is the case under current law. In particular, we consider whether the program could effectively increase college enrollment rates without greatly inflating program costs or otherwise hampering efficiency. To summarize our results, we estimate that an early commitment program is likely to increase the enrollment rates of Pell Grant recipients by approximately four percentage points. The expansion of the Pell program results in an estimated additional cost of $1.5 billion per cohort to the federal government in our median simulation, but is more than offset by an additional $2.1 billion in net discounted federal tax revenues resulting from increased enrollment and completion rates.

Federal Financial Aid Timing and Eligibility

The federal system for distributing financial aid has received much scrutiny. Administrators of large and expensive programs often struggle with issues of efficiency and targeting, and federal student aid is no exception. An early commitment of the Pell Grant is intended to address two particular concerns: the timing of when aid notification is provided, and

[27]Estimates suggest that the number of Pell Grant-eligible students who fail to file for financial aid range from at least 500,000 students (Novak & McKinney, 2011) to as many as 1.5 million students annually (King, 2006). At community colleges, at least one-fifth of all students in the lowest income categories (below $20,000 per year) do not file the FAFSA (Advisory Committee on Student Financial Assistance, 2008a), and many file late because they think the FAFSA is complicated and takes too much time to fill out (LaManque, 2009).

the eligibility requirements that must be satisfied for a student to receive financial aid. This section reviews the status quo regarding each issue.

Timing and Eligibility in the Current Financial Aid System

To be eligible for federal financial aid in a given academic year, a student must complete the 105-question Free Application for Federal Student Aid (FAFSA), which includes standard income information that is found on a W-2, but also items on student and parent investments and assets that are not a part of a tax return. This information is used to calculate an expected family contribution (EFC) for the upcoming academic year, representing a measure of a family's *short-term* financial ability to pay for college. Eligibility for the Pell Grant and many other grant and loan programs is determined by the EFC. This process is repeated each year that a student wishes to apply for financial aid. Therefore, the fact that eligibility calculations for financial aid currently utilize data from families' tax returns from the calendar year prior to expected college enrollment, along with a desire to target resources to the neediest students, means that students do not learn about their financial aid eligibility until the year of their college enrollment.

Students with family income below $50,000 can complete a simplified FAFSA if they (1) did not have to file the IRS 1040 long tax form, (2) meet dislocated worker criteria, or (3) received a means-tested federal benefit. In addition, if family income is below $23,000, students qualify for an *automatic* zero EFC (and thus the maximum Pell Grant) if they participate in at least one federal means-tested benefit program, by far the largest of which is the free and reduced price lunch program (FRL).[28]. The automatic zero EFC provision affects about 4.2 million students (45 percent of Pell recipients) (U.S. Department of Education, 2012). But

[28] The other programs are Supplemental Security Income, Supplemental Nutrition Assistance Program (food stamps), Temporary Assistance to Needy Families, special supplemental nutrition programs, and Women, Infants, and Children (WIC).

qualification for the auto zero EFC does not occur until the FAFSA is filed, at which point students are usually on the brink of the college enrollment decision and have little time left to prepare.

Theory and Research on the Effects of Early Intervention

Since the effects of interventions earlier in a child's life have the potential to compound over time (e.g., Heckman & Masterov, 2007), we expect early interventions to improve student and family financial literacy to be more successful than later interventions. A growing body of literature suggests that this is the case. Some studies, such as those by Sherraden, Johnson, Gao, and Elliott (2011) and Go, Varcoe, Eng, Pho, and Choi (2012), indicate that financial literacy interventions are effective for younger students. Mandell (2006) finds that middle school students exposed to a financial literacy seminar received substantial benefits, with the largest gains in financial knowledge accruing among the youngest students. But the effects of financial literacy programs in high school are less positive; for example, Peng, Bartholomae, Fox, and Cravener (2007) and Mandell and Klein (2009) find no long-term effects of taking a financial literacy course in high school. However, relatively few financial interventions target students before high school, which concerns both researchers and policymakers (McCormick, 2009).

Correlational research on the effects of child savings accounts suggests that interventions designed to help students and their families save a small amount toward the cost of college, even an amount less than the cost of a single year of tuition, can help increase educational expectations and aspirations. Elliott (2009) analyzed the Panel Study of Income Dynamics and concluded that after controlling for observable characteristics, children with a savings account were twice as likely to expect to attend college and also had higher levels of academic achievement in school than students without a savings account. Other studies suggest that

families who start saving for college from an early age are more likely to exhibit strong college expectations for their children and place them into appropriate academic courses (Destin & Oyserman, 2009; Elliott, Choi, Destin, & Kim, 2011).

The impacts of early interventions that increase knowledge of the costs and benefits of college attendance might also be more effective for younger students because of the large benefits accruing to academic and financial preparation for college. A recent experimental program providing information about the actual cost of college (tuition and fees less financial aid) to parents of middle school students identified substantial increases in their knowledge of the cost of college. Parents provided with the additional information were much more likely to know that students from low-income families could attend college at little or no cost (College Board and College Foundation of North Carolina, 2012). Similarly, using random assignment, Oreopoulos and Dunn (2012) find that an intervention consisting of a short video providing information about the costs and benefits of college attendance combined with a financial aid calculator significantly increased low-income Canadian high school students' aspirations. But it is unknown whether increasing aspirations at such a late point will result in an increase in college enrollment rates.

The federal government recognizes the importance of providing students with information about the cost of college as early as sixth grade (Advisory Committee on Student Financial Assistance, 2008b), but has made only modest efforts to do so. The primary federal effort has been the Gaining Early Awareness and Readiness for Undergraduate Programs (GEAR UP) program, which serves students in high-poverty middle and high schools and provides early

information about college and additional financial aid to students upon entering college.[29] Preliminary results suggest that treatment students have improved levels of academic achievement and greater educational aspirations than control students (ACT, Inc., 2007); however, the decentralized nature of the program and a lack of rigorous evaluations make estimating the effects of the early information component difficult.

Past and Ongoing Efforts to Improve Timing of and Eligibility for Federal Aid

Over the last decade, several states and communities have tried to ensure earlier notification of financial aid through (often private) early commitment programs. Three states (Indiana, Oklahoma, and Washington) adopted broad early commitment programs targeted to students from lower-income families.[30] These programs provide middle school and early high school students with the knowledge that college will be affordable if they meet a relatively modest GPA requirement in high school, stay out of significant trouble, and attend an in-state college or university while filing the FAFSA each year. St. John and colleagues (2004) conclude that the Indiana program may have induced students to enroll in college at somewhat higher rates.

Dozens of cities and towns have adopted their own version of promise programs in an effort to induce families to stay or relocate to their community.[31] For example, the Kalamazoo Promise guarantees that students living in the school district and attending public schools from elementary through high school would receive a grant equivalent to the cost of tuition and fees at in-state public institutions. Emerging evidence suggests that students who know they will receive a large Kalamazoo Promise scholarship to attend college work harder in high school, and

[29] There is currently a rigorous experimental evaluation of GEAR UP in progress; this paper's second author is on the evaluation's technical working group.
[30] More information on these early commitment programs can be found in Harnisch (2009).
[31] See Vaade (2009) for a list of these programs.

teachers have higher expectations for them (Bartik & Lachowska, 2012; Jones, Miron, & Kelaher-Young, 2012). These causal claims cannot be fully supported with the kinds of research designs currently used; it is difficult to find appropriate comparison groups to estimate impacts.

In lieu of early commitment programs, some have advocated for simplifying the existing FAFSA process but populating the calculation with tax information from two years prior to college enrollment, rather than one year (e.g., Advisory Committee on Student Financial Assistance, 2005; Dynarski & Wiederspan, 2012). This "prior-prior year" approach would make high school juniors aware of available federal financial aid, which may induce them to consider enrolling in college. However, it would not reach students who do not complete the FAFSA and could only affect the university enrollment decisions of students who are capable of being admitted. If the goal is to induce the most price-sensitive students to consider college *and* prepare for it so that they can gain admission, they need to know much earlier about the likelihood of receiving financial aid. Thus, we consider the feasibility of a program targeting students in eighth grade, far earlier than what is being currently discussed.

A Targeted Early Commitment Pell Grant Program

National college attainment goals and growing concerns about college affordability, coupled with recent changes to aid eligibility requirements that simplify the process for needy families, set the stage for a federal effort to target an early commitment Pell Grant program to students in eighth grade (Advisory Committee on Student Financial Assistance, 2005, 2008b; Heller, 2006). We therefore undertake an examination of the economic feasibility of such a program using a fiscal analysis. The current federal needs analysis automatically awards students a full Pell Grant if their family receives a federal means-tested benefit in grade 12 and they file the FAFSA. We examine the costs and benefits of advancing that timeline from twelfth to eighth

grade, and waiving the requirement of FAFSA completion for students receiving free or reduced price lunch. This is consistent with proposals offered by others, albeit prior to the revision of aid eligibility rules (Fitzgerald, 2006; Schwartz, 2008). Figure 1 provides a summary of our proposed system compared to current law.

Program Timing

Advancing the determination for Pell eligibility from twelfth to eighth grade, even for some students, creates the potential for greater program inefficiency. If the intent is to compensate students for *short-term* financial constraints (low family income) close to beginning college, then any aid awarded to students who are not as constrained at that time might be inefficiently targeted. Evidence, primarily from the Panel Study of Income Dynamics, suggests that income volatility (both up and down) is increasing, especially toward the bottom of the income distribution (e.g., Dynan, Elmendorf, & Sichel, 2007; Gottschalk & Moffitt, 2009; Kopczuk, Saez, & Song, 2010), and this is especially common during recessions (Celik, Juhn, McCue, & Thompson, 2012; Shin & Solon, 2011).[32] Additionally, Wagmiller and Smith (2012) show that income volatility has increased sharply among low-income families with children.

However, trends suggest that poor families remain persistently poor while their children are in school. For example, Heller (2006) estimated that 77 percent of seventh-graders eligible to receive free or reduced price lunch (a proxy for low income) in 1987 were still eligible for FRL as eleventh-graders. He also examined a cohort of entering college students in 2004, finding that 80 percent of families who were FRL-eligible as eleventh-graders got the Pell Grant upon enrolling in college in fall 2003. Dynarski and Wiederspan (2012) used data from the 2006 and

[32] Using administrative earnings records from the Social Security Administration, Dahl, DeLeire, & Schwabish (2011) found no evidence of increased income volatility since the 1980s.

2007 tax years to examine eligibility over a shorter timeframe and found that for 77 percent of continuing undergraduates, using income data from two years prior would result in a Pell Grant award within $500 of the award based on income one year prior. This paper revisits these estimates in order to assess the potential that an early commitment would "over-award" some students.

On the other hand, if the intent of the Pell Grant is to compensate students for longer-term financial constraints—and a lack of family wealth rather than income—there is far less risk of increased inefficiency via an early commitment program. Wealth is quite persistent (Keister & Moller, 2000), and does not substantially increase as poverty decreases (Caner & Wolff, 2004).

Program Targeting

Determining program eligibility using a proxy for family income is far more desirable than introducing an additional application process, which is likely to reduce the accessibility of the early commitment program. Using the FRL program for targeting an early commitment of the Pell has benefits and drawbacks. FRL receipt is a reasonable way of measuring childhood poverty because it is a means-tested entitlement program that enjoys strong take-up rates (particularly in elementary school). A student's household income must be less than 130 percent of the federal poverty line to receive a free lunch, while the cutoff is 185 percent of poverty for reduced price lunch receipt. Moreover, all students who have a family member receiving TANF or food stamps automatically receive FRL. But while 87 percent of students who are income-eligible for FRL participate in the program, participation rates decline to approximately 70 percent in middle school and 60 percent in high school (Gordon & Fox, 2007), and certain high-poverty schools are authorized to offer free lunches to all students. Take-up rates may decline in later grades due to social stigma associated with receiving government benefits and the increased

availability of outside food options for students (Mirtcheva & Powell, 2009). Thus, as Robert Hauser notes, "a free or reduced-price lunch is a treatment, not merely an indicator" of poverty and thus must still be considered a rough measure (2010, p. 4).

Another consideration is that tying an early commitment program to FRL participation might provide students and their families with a stronger incentive to participate in that program. While increased participation is a positive outcome since students receive the food they need, it could create incentives for cheating (e.g., families who shift income from one tax year to the next) and might drive up program costs.[33] Efforts to minimize this behavior, for example by increasing the complexity of the FRL application, would likely reduce the efficacy of both programs by limiting the number of qualified individuals served. In this case, the unintended consequence would be an expansion of FRL program costs and a loss of efficiency to both that program and the early commitment program.

Nevertheless, these tradeoffs may be tolerable given that an early commitment program based on FRL receipt would reach millions of students. In the 2009-2010 academic year, 31.7 million children received FRL through the National School Lunch Program (Young, Diakova, Earley, Carnage, Krome, & Root, 2012), even though approximately five percent of schools do not participate in the program.

Implementation

This program could be straightforward to implement. An initial demonstration program would be advisable, to assess implementation challenges and examine program effectiveness across the spectrum of implementation (e.g., are effects stronger where information is more fully

[33] Another possibility is to use multiple years of means-tested program participation data to determine a student's eligibility. While it does reduce a family's ability to shift income around to meet the participation threshold, the cost is increased complexity to both the family and the government.

disseminated?). Information about the program could be distributed in schools, public assistance offices, libraries, and through the media—many of the same sources that are currently used to provide information about the FAFSA. While eligibility for the program would be based on a family's financial circumstances in eighth grade, it is critical that students and their families know about the program well before then.

Specific Research Questions

To assess the feasibility of this program, we addressed the following questions: (1) To what extent does receipt of federal assistance programs in eighth grade predict receipt of federal assistance programs in twelfth grade (the year currently used for Pell eligibility for on-time college goers)? (2) How would the distribution of auto-zero EFCs change if eighth grade program receipt was used in the federal needs analysis instead of using twelfth grade receipt? How many students would be over-awarded (e.g., receiving a full rather than partial Pell)? Correspondingly, how would Pell expenditures change? (3) To what extent might college enrollment rates respond to this change to early notification for a targeted group of students? How would this affect the costs and benefits of the Pell Grant program with respect to the federal government?

Data and Methods

We used a sample broadly representative of American adolescents to examine our research questions using probit models. The resulting coefficients from these models are then used to estimate the costs of the possible early commitment program.

Data

To examine the extent to which early commitment programs would appropriately and efficiently notify students from needy families about their eligibility for the federal Pell Grant, we used data from the Panel Study of Income Dynamics (PSID) from 1999 through 2009. The biennial survey includes questions on demographics, income and assets, and participation in federal programs such as TANF/AFDC, food stamps, FRL, and Women, Infants, and Children nutrition program (WIC). The PSID includes a nationally representative sample, along with an oversample of low-income families, and we focus on a subsample of families in the core/immigrant sample. This dataset is often used to examine trends in income volatility (as evidenced by the numerous studies previously referenced), but no previous research has focused on the income and program receipt dynamics of our population of interest—families with adolescent children. We included families with at least one biological or adopted child between the ages of seven and 14 in 1999. This results in a sample size of 2,240 children in 1,503 households. With the use of survey weights, the sample is generally representative of the American population in 1999 (Gouskova, Heeringa, McGonagle, & Schoeni, 2008).[34] Nearly three-fourths of the students are white and 18 percent are black; only ten percent of the students are Hispanic. Nearly half of the parents in the sample attended at least some college, and 27 percent hold bachelor's degrees.

Since the PSID does not provide information on a child's grade in school on a regular basis, we used a student's age to estimate his or her grade. Students ages 13 and 14 are estimated to be in eighth grade, ages 15 and 16 are estimated to be in tenth grade, and 17 and 18 are

[34] All estimates in this paper are reported using survey weights and clustered at the household level.

estimated to be in twelfth grade. There are four cohorts of eighth grade students: 1999, 2001, 2003, and 2005. Table 3.1 provides summary statistics of the PSID sample in eighth grade.[35]

When in eighth grade, 33 percent of students in the sample received at least one of four types of public assistance; over 96 percent of those students received free or reduced price meals at school.[36] At the time, six percent of students had a family member receiving WIC assistance and 10.5 percent received food stamps, but fewer than three percent of students had a family member receiving assistance through TANF. Appendix 1 shows information on federal program receipt in eighth grade, by cohort. Receipt rates are consistent across the cohorts, suggesting that they can be combined for estimation purposes.

Table 3.2 illustrates rates of public assistance receipt in tenth and twelfth grades, family income in twelfth grade, and educational attainment levels by eighth grade public assistance receipt. The results indicate that 81 percent of students receiving means-tested benefits in eighth grade received them again in tenth grade, and 69 percent of eighth grade recipients were still receiving benefits in twelfth grade (which would automatically qualify them for the maximum Pell Grant under current rules). The decline in benefit receipt rates during high school is likely attributable to three factors: reduced take-up among income-eligible students, students who drop out from high school before twelfth grade, and increased family income. The last factor appears to be driving some, but not most, of the decline in benefit receipt rates. Just 26.7 percent of students receiving assistance in eighth grade had a family income of more than 185 percent of the poverty line when in twelfth grade (which would currently qualify them for the automatic

[35] We use complete cases in the analyses. This excludes three to four percent of students with eighth grade information, as sample attrition from the PSID is very low.

[36] Free and reduced price lunch receipt are combined in the PSID data. We combine free/reduced breakfast with the lunch program because very few children participate in the breakfast program without participating in the lunch program. We thus refer to the programs as free/reduced lunch.

zero EFC), and only 7.7 percent had a family income of more than 300 percent of poverty at that time (which would likely make them ineligible for a Pell Grant). Only 18 percent of students who did not receive benefits in eighth grade had a family income of less than 185 percent of the poverty line in twelfth grade.

There is a sharp disparity in college enrollment rates according to likely Pell eligibility. Only 29.6 percent of students who received federal benefits in eighth grade enrolled in college by 2009 (ages 19-24), compared to 44.0 percent of students who did not receive benefits. If knowledge of likely aid eligibility plays a role in that disparity, an early commitment to Pell receipt has the potential to narrow that gap.

Methodology

We used several methods to examine the feasibility of an early commitment program based on federal means-tested program receipt. We first predicted public assistance receipt for student *i* in tenth or twelfth grade based on eighth grade receipt and student demographic characteristics using a probit model:

$$\Pr(Asst_{gi} = 1) = \Phi(\beta_0 + \beta_1 Asst_{8i} + \beta_2 StuDem_i + \beta_3 Cohort_i), \quad (1)$$

where Φ is the standard normal distribution, $Asst_{gi}$ represents having received assistance in grade g, $StuDem_i$ represents demographic characteristics (race, gender, number of siblings, and parental education), and $Cohort_i$ represents the student's cohort.

The ability of an early commitment program to reach students from low-income families depends on the extent to which families receive means-tested programs if they are income-

eligible. To explore this concern, we regressed public assistance receipt for student *i* in a given grade *g* on the income cutoffs for FRL receipt:

$$\Pr(Asst_{gi} = 1) = \Phi(\beta_0 + \beta_1 Income_{gi} + \beta_2 StuDem_i + \beta_3 Cohort_i), \quad (2)$$

where $Income_{gi}$ represents whether a student's family income is less than 130 percent (free lunch) or 185 percent of the poverty threshold (reduced price lunch) and the rest of the measures are as before. If fewer students are taking up the FRL program, then the relationship between public assistance receipt and income should grow weaker between eighth and twelfth grade.

A key concern with early commitment programs is that some students who are eligible in eighth grade are no longer financially needy upon reaching college age, leading to an over-award of financial aid.[37] Among students who received any public assistance in eighth grade, we regressed having a tenth or twelfth grade household income of at least 200 percent or 300 percent of the poverty line on being below 130 percent of the poverty line in eighth grade (our best estimate of whether someone was eligible for free lunches) and a vector of other student characteristics:

$$\Pr(Income_{gi} = 1) = \Phi(\beta_0 + \beta_1 Poverty_{8i} + \beta_2 StuDem_i + \beta_3 Cohort_i), \quad (3)$$

where $Income_{gi}$represents whether a family has taxable income over 200 percent or 300 percent of the poverty threshold and $Poverty_{8i}$ is an estimate of whether a student received FRL in eighth grade. This allows us to examine student characteristics associated with large upward income swings before reaching college-going age.

[37] The opposite case, in which a student's family income drops between eighth and twelfth grades, is not a concern because he or she could still receive Pell Grants through the traditional financial aid disbursement system.

We then examined the relationships between receiving public assistance in eighth to twelfth grades and later educational attainment:

$$\Pr(EdAttain_i = 1) = \Phi(\beta_0 + \beta_1 Asst_{8i} + \beta_2 StuDem_i + \beta_3 Cohort_i), \quad (4)$$

where $EdAttain_i$ is an indicator in separate regressions for either graduating high school or attending any college (the categories are not mutually exclusive). We focus on the eighth grade public assistance receipt measure for the regression on having attended college, as this would be the theoretical upper-bound for the effectiveness of an early commitment program.

Limitations

There are several limitations of using the PSID for this purpose. The primary concern is that we cannot perfectly observe means-tested program receipt in this dataset, and as such we are likely understating the rate of program participation by using survey data. Meyer, Mok, and Sullivan (2009) estimate that only about 70 percent of families receiving FRL (who make up the vast majority of means-tested benefit recipients) actually report it in the PSID. This underreporting is true for most other means-tested programs and introduces error into our estimates. Additionally, the measure of educational attainment (years of education completed) is crude, but it does provide an indicator of postsecondary enrollment.

Results

We first examined the extent to which public assistance receipt in tenth and twelfth grade is a function of eighth grade receipt and student and demographic characteristics (Table 3.3). Later receipt of federal assistance is highly correlated with eighth grade receipt, with coefficients around 0.6 in tenth grade and 0.4 in twelfth grade. However, this relationship weakens between tenth and twelfth grade ($p<.01$); this matches with the higher FRL takeup rates among eligible

students in elementary and middle school. Racial/ethnic minority children and those whose parents who did not complete high school were much more likely to continue receiving public assistance in later grades compared to white children or those whose parents completed some college. The results are similar when examining any form of public assistance receipt or FRL receipt only.

Table 3.4 shows the relationship between public assistance receipt and household income by grade, examining both the 130 percent of poverty line (free lunch) and 185 percent of poverty line (reduce price lunch) thresholds. The relationship between low household income and receiving public assistance weakens somewhat over time, with coefficients declining from 0.48 to 0.40 between eighth and twelfth grade for the free lunch income threshold. This suggests the importance of starting an early notification program in eighth grade when participation in public assistance programs is more common. Again, minority students and those with less-educated parents were more likely to continue to take up the programs than white students or those with college-educated parents, which may be a function of universal FRL eligibility at high-poverty schools.

Next, we examined family income volatility among students who initially received public assistance in eighth grade, using thresholds of 200 percent and 300 percent of the poverty line (Table 3.5). Only 20 percent of students who received assistance in eighth grade had a family income of over 200 percent of poverty by tenth grade, increasing to 25 percent by twelfth grade. Fewer than 10 percent ever had a family income of over 300 percent of poverty in high school, suggesting that few poor families become well-off while their children go through high school. The multivariate regressions also show that free lunch eligibility continued to act as a strong predictor of continued low-income status in tenth grade (a coefficient of 0.451), but was

somewhat less effective at predicting twelfth grade eligibility (a coefficient of 0.209). In other words, the current system, which relies on twelfth grade program receipt, is likely under-awarding some students (or at least subjecting to unnecessary additional needs analyses) who experience childhood poverty and who may still be quite poor, but are not receiving FRL.

Table 3.6 illustrates the likelihood of educational attainment (high school graduate or above and any college attendance) based on public assistance receipt. Students who received assistance in eighth grade were nearly ten percentage points less likely to attend college than those who did not, net of other demographic characteristics.[38] This differential increases over time, but this could be due to changes in the composition of program participants in later grades; thus ten percentage points may be viewed as an upper-bound estimate of the potential effect of early commitment on college enrollment.

We next used a range of possible enrollment effects to estimate the cost of this early commitment program, assuming that the cost of providing an early commitment is negligible (for example, if it simply became part of the FRL award process). Given that nearly 32 million students participate in the National School Lunch Program, we assume that approximately one-thirteenth of the students, or 2.5 million, are in eighth grade each year. This means that an effective early commitment program could result in a substantial increase in the approximately three million new freshmen who enroll in college each year (Aud, KewalRamani, & Frohlich, 2011).

[38] In Appendix 2, we estimate the likelihood of educational attainment by being income-eligible for FRL (185 percent of the poverty line). The gap between students from poor and nonpoor families is even larger, although the estimates are on a smaller number of cohorts.

Fiscal Analysis

We used a Monte Carlo simulation with 10,000 trials to estimate the net fiscal effects of the proposed early commitment program, assuming a 30 percent initial enrollment rate of FRL students and an average estimated impact on enrollment of four percentage points.[39] This estimated enrollment effect is based on the findings of prior research examining the effects of college access programs. Bettinger et al. (2012) found a 4.8 percentage point increase in any college enrollment over a three-year period for dependent students in their test of a FAFSA assistance intervention. A meta-analysis conducted by Harvill, Nguyen, Robertson-Kraft, Tognatta, and Maynard (2011) examined the effects of college access programs on college enrollment rates. Among studies using random assignment, they estimated an impact of approximately four percentage points.[40] All costs and benefits are discounted back to age 19 (a student's first year in college) using a 3.5 percent discount rate with sensitivity checks at 2 percent and 5 percent (Moore, Boardman, Vining, Weimer, & Greenberg, 2004). Table 3.7 contains the distribution of each of the parameters used in the simulation.

Cost Estimates

To estimate the cost of the additional enrollment to the federal Pell Grant program, we used data from the Beginning Postsecondary Students (BPS) study, a nationally representative sample of first-time college students enrolled in the fall of 2003. There are two ways in which the cost to the program would increase: through increased enrollment rates (Case 1) and the over-awarding of aid to students who would not have been eligible for a full Pell Grant under the current aid system (Case 2). We used the distribution of part-time and full-time students for

[39] All binary variables are estimated using a binomial distribution with 100 draws, while continuous variables are estimated with specified standard deviations.

[40] They estimate much larger effects (13 percentage points) when including quasi-experimental studies, but many of these programs target more narrow groups than the federal Pell Grant program. As such, we prefer the more conservative estimates from the random assignment programs.

initial full and partial Pell recipients, as well as the average amount of Pell Grant funds received over six years by enrollment status and initial Pell receipt, from the BPS in our estimates. [41] We adjusted the estimates to current dollars by multiplying by the percentage increase in the maximum Pell Grant between 2003 and 2012 ($4,050 vs. $5,550).

The program might be less cost-effective if many students who received an early commitment of a maximum Pell Grant then experienced increases in their family income (Case 2). In the prior analysis, we estimated that 26.7 percent of students who were income-eligible for FRL in eighth grade were no longer income-eligible in twelfth grade. However, most of these students likely remained Pell-eligible based on income, as just 29 percent of students who were no longer income-eligible had family incomes of over 300 percent of the poverty line by twelfth grade. We assumed that everyone between 185 percent and 300 percent of the poverty line is receiving the average Pell Grant for non-zero EFC Pell recipients and nobody above 300 percent of the poverty line receives a Pell Grant.[42] To estimate the net increase in Pell expenditures, we subtracted the partial Pell awards that would currently be given to students between 186 percent and 300 percent of the poverty line.

We then combined these two cost drivers (increased enrollment of zero-EFC students and over-awarding of some students who would not qualify for Pell Grants under current rules) to estimate the total costs of the early commitment program. Our preferred assumption of a four percentage point increase in enrollment resulted in a $1.5 billion increase in expenditures per

[41] It is difficult to estimate the number of years for which Pell recipients stay enrolled in the public-use datasets. We use the number of years of Pell receipt as a proxy for the number of years of enrollment, although this may slightly understate enrollment. However, it is likely that the additional students induced into attending college by this program may remain enrolled for shorter periods of time, overstating the number of years enrolled.

[42] Depending on household size, 300 percent of household income is approximately $60,000-$75,000 per year. In the 2010-2011 academic year, only three percent of all Pell Grant recipients had household incomes of over $60,000 per year (U.S. Department of Education, 2012).

cohort in our simulation. This is a small fraction of the current Pell Grant expenditures of approximately $36 billion (United States Department of Education, 2012). A program that is effective in reaching students in earlier grades may encourage students to prepare more for college, which could also result in lower remediation costs for students who currently enroll in college.

Benefit Estimates

Estimating the fiscal benefits of this proposed program requires a series of assumptions regarding increased educational attainment and the resulting labor market outcomes as well as labor force participation and tax rates. Some students may be induced to attend college who would have not completed high school in the counterfactual case; we estimated that ten percent of the enrollment increase is from this category, with the other 90 percent coming from students who would have otherwise graduated from high school.[43] It is likely that the students who attend college as a result of the early commitment program are less academically prepared than their peers and are less likely to complete a degree. Our preferred estimate is that 30 percent of students induced to enroll in college complete an associate's degree and 20 percent complete a bachelor's degree, with the remaining students completing some college.

The educational benefits of the early commitment program are likely not limited to the students who are induced to enroll in college; the additional financial aid received by students who could be considered "over-awarded" is likely to have some benefits on the persistence and completion margins. The average student who would not have previously qualified for a full Pell Grant is estimated to receive an additional $4,200 in Pell aid. Some of this additional aid will

[43] Alternatively, some students may be induced not to attend college after receiving information about the cost of college through the early commitment program. This could be a net benefit to the federal government if they would have briefly attended college, taken out loans, and then defaulted on those loans in the counterfactual scenario. We exclude this group of students from our cost or benefit estimates.

likely supplant other types of financial aid, so we estimate the additional increase in aid to be approximately $2,000. Prior quasi-experimental work suggests that an additional $1,000 in total financial aid received results in a 2.8 percentage point increase in retention rates among Pell recipients (Goldrick-Rab, Harris, Kelchen, & Benson, 2012). Assuming that the average student receives the Pell Grant for approximately two years, a three percentage point increase in retention and completion seems reasonable.

We used the estimated present discounted value of lifetime earnings by education category (less than a high school diploma, a high school diploma, some college but no degree, an associate's degree, or a bachelor's degree) from Carnevale, Rose, and Cheah (2011) to estimate the returns to receiving additional education. The distributions are estimated using a standard deviation equal to one-third of the mean; this resulted in a slightly narrower interquartile range than is reported in their analysis, but yielded a normal distribution with few implausibly low values. The earnings distributions were jointly estimated to preserve the relative returns to education.

The estimates of the labor market returns to education are for full-time workers, so we multiplied the estimated (discounted) lifetime earnings by the average labor force participation rate for 25- to 64-year-olds from the Bureau of Labor Statistics (Toossi, 2012). This resulted in an average labor force participation rate of 78 percent. We then estimated the amount of tax revenue received by multiplying this number by the average effective federal tax rate paid by individuals in the median income bracket between 1993 and 2009 (Harris, 2012), resulting in our preferred estimate of a 15 percent tax rate.[44] Benefit estimates are discounted by an additional

[44] This is more appropriate than the average effective tax rate for the median quintile in 2009 (11 percent) because this tax rate was temporarily depressed by two percentage points due to a reduced Social Security payroll tax rate and because effective tax rates are likely to increase given a stronger economy and the current fiscal climate.

0.2 percent to account for mortality during adults' prime earning years (Office of the Chief Actuary, 2012).

Net Fiscal Impacts

We estimated the net fiscal impacts of the potential early commitment program using the assumptions detailed above and in Table 3.7, allowing the parameter values to vary across 10,000 simulations. We report net benefit and cost-benefit ratio estimates in Table 8 and estimated costs of approximately $1.5 billion per cohort and benefits of $2.2 billion in the median simulation. This resulted in an estimated net benefit of over $600 million and a benefit-cost ratio of 1.41.[45] Figure 2 provides a distribution of the estimated net fiscal benefits across 10,000 simulations with the preferred discount rate of 3.5 percent. The estimated net benefit was positive in 68.8 percent of the simulations with the preferred discount rate, compared to 82.1 percent of simulations with a 2 percent discount rate and 52.9 percent of simulations with a 5 percent discount rate. These analyses suggest that the proposed early commitment program is likely to provide positive net fiscal benefits under reasonable assumptions. Because such a wide variety of program effects and assumptions are plausible, we created an interactive spreadsheet to test different assumptions. We also provide our Stata code for the Monte Carlo simulation if readers wish to modify either the means or distributions of any measure used in our analyses.[46]

We did not model several important components of the fiscal analysis. On the benefit side, we excluded the nonmarket benefits of education, such as better health and lower rates of incarceration, that have been shown to significantly increase the returns to education (Wolfe &

[45] We report the net fiscal impact from the median instead of the mean simulation because the distribution of estimated effects (as shown in Figure 2) is skewed to the right. For example, the mean fiscal impact is approximately $900 million with a 3.5 percent discount rate, compared to a median impact of about $600 million.
[46] The Stata code is available at http://finaidstudy.org/documents/monte_carlo_simulation.txt and the fiscal analysis spreadsheet is at http://finaidstudy.org/documents/fiscal_analysis_spreadsheet.xlsx.

Haveman, 2002). We also excluded the reduced rate of future means-tested program receipt for more-educated adults. On the cost side, we did not estimate the costs of providing additional financial aid to disadvantaged college students, which is contingent on Pell Grant receipt, such as student loan subsidies or through grant programs such as the Supplemental Educational Opportunity Grant. We view our estimated cost-benefit ratios as conservative estimates of the effectiveness of the program, as the omitted benefits are likely larger than the omitted costs.

Discussion

There are substantial income disparities in college enrollment and completion rates and evidence exists that some students from low-income families may not be academically preparing for college because they perceive college to be unaffordable. In this paper, we evaluate the feasibility of a targeted early commitment program that would guarantee full Pell Grants to eighth-grade students from families receiving public assistance programs. Changing the timing of financial aid notification for the neediest students would be reasonably well-targeted, as nearly seven in ten students who would receive the maximum Pell Grant under this new approach are already receiving it under the current system. The difference is that instead of waiting until twelfth grade to learn that college is affordable, they would learn this information in eighth grade. The level of inefficiency would be low—our estimates suggest that fewer than three in ten students would receive a larger Pell Grant under the new system. Since the current needs analysis would remain intact for all students not involved in the early commitment program, there would be no "losers" in the new system.

The results of our Monte Carlo simulation suggest that such a program is likely to have positive net fiscal benefits under a fairly conservative and robust set of estimates. Given an average estimated program impact of four percentage points (in line with other similar

interventions) and a discount rate of 3.5 percent, we estimate a median net benefit of about $600 million per year. Federal Pell expenditures would increase by approximately $1.5 billion per cohort of students. This would represent a four percent increase in Pell expenditures, but might be partially offset by reduced costs if students are induced to prepare for college at an earlier age and this diminishes the need for remediation or shortens time-to-degree. The estimated benefits of the program are at least $2.1 billion per cohort, suggesting that the program should be cost-effective under the majority of assumptions.

Would the program overlook needy students? It would not if the early commitment program supplemented rather than supplanted the existing needs analysis. Family income could decline during high school, rendering new students eligible. However, in this study we find that only seven percent of students who did not receive federal assistance in eighth grade later received it in tenth or twelfth grade. Such students would not be informed of Pell eligibility early on, but would receive it when they filed a FAFSA in twelfth grade.

More research should be done on the potential general equilibrium effects of an early commitment program. Currently, many state and institutional need-based grants use Pell eligibility as their eligibility requirements, and thus this might expand their service populations as well. If this program were implemented, providing consistency across programs would mean that ideally states and colleges would also give targeted students automatic eligibility for their need-based grants. It is also possible that the number of additional students induced to attend college by this program (estimated to be approximately 100,000 per year based on the effects of other college access programs, such as Bettinger et al., 2012 and Harvill et al., 2011) could result in a decline in the returns to education or increased tuition resulting from colleges wishing to capture additional Pell Grant revenues.

In addition to considering a demonstration early commitment program, researchers and policymakers should consider an information-only intervention. Students and their families receiving means-tested benefits in eighth grade could be sent information stating that they would qualify for the maximum Pell Grant upon enrolling college if their family circumstances remained unchanged, along with information about the net price of attendance for local colleges and universities. While this would not represent an early commitment of Pell funds or provide students with certainty regarding the cost of college, it would give many students a better idea of what college will cost and give them time to prepare.

It may not be important for an early commitment program to have strict income checks at later grades unless evidence shows that many families are gaming the system. Even if family income rises somewhat while a student is in high school (which is perfectly consistent with the life cycle trajectory of earnings), increased income does not mean that a family has the level of wealth or discretionary income required to make college truly affordable (Conley, 2001). Ensuring that students do not forgo college opportunities due to short-term income constraints is the express purpose of need-based financial aid, and the current program is far from achieving that goal.

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Figure 3.1: Summary of the proposed early commitment system.

Grade/FRL status	Pell eligibility under current rules	Pell eligibility under early commitment
8th grade, no FRL	No notification	No notification
8th grade, FRL	No notification	Guaranteed maximum Pell Grant for 12 semesters
FRL in 8th and 12th grades	Maximum Pell first year of college if the FAFSA is filed; must refile and meet income requirements each year	Guaranteed maximum Pell Grant for 12 semesters
No FRL in 8th grade, FRL in 12th grade	Maximum Pell first year of college if the FAFSA is filed; must refile and meet income requirements each year	Maximum Pell first year of college if the FAFSA is filed; must refile and meet income requirements each year

Table 3.1: Baseline characteristics (grade 8) of the PSID sample.

Measure	Mean	(SE)
Race (percent)		
White	72.0	(1.6)
Black	17.8	(1.4)
Hispanic	9.5	(1.0)
Asian	2.2	(0.4)
Native American	1.1	(0.4)
Gender (percent female)	49.7	(1.4)
Number of siblings age 0-17	1.39	(0.04)
Parental education (percent)		
Less than high school	15.9	(1.4)
High school	36.9	(1.8)
Some college or AA	20.5	(1.4)
BA or higher	26.8	(1.6)
Family taxable income ($)	$64,087	($1,929)
At or below 100% of poverty (pct)	18.6	(1.3)
At or below 200% of poverty (pct)	37.1	(1.7)
Received public assistance (percent)		
Any assistance	33.0	(1.7)
WIC	6.1	(0.8)
Free/reduced price lunch	31.9	(1.7)
TANF	2.6	(0.4)
Food stamps	10.5	(1.0)
Cohort (percent)		
1 (8th grade in 1999)	25.7	(1.1)
2 (8th grade in 2001)	27.0	(1.1)
3 (8th grade in 2003)	24.3	(1.0)
4 (8th grade in 2005)	23.0	(1.0)
Number of children	2240	
Number of households	1503	

Notes:
(1) Family income is trimmed to the 1st and 99th percentiles.
(2) Parental education is for the head of household. In the rare case of multiple households, the highest level of parental education was selected.
(3) Observations are weighted to account for the study's design. Standard errors are clustered at the family level.
(4) The components of assistance add up to more than the overall percentage of families receiving assistance because multiple types of assistance can be simultaneously received.

Table 3.2: Income dynamics and educational attainment by initial public assistance receipt.

	8th grade public assistance receipt?			
	Yes		No	
Measure	Mean	(SE)	Mean	(SE)
Public assistance receipt (percent)				
10th grade	81.3	(2.0)	6.6	(0.8)
12th grade	69.3	(2.7)	7.8	(1.0)
12th grade income (pct of poverty)				
Below 130%	54.7	(2.9)	12.9	(1.3)
131%-185%	18.6	(2.0)	5.1	(0.8)
186%-300%	19.0	(2.3)	17.1	(1.4)
301% or higher	7.7	(1.3)	64.9	(1.8)
Educational attainment (percent)				
Did not complete HS	31.2	(2.5)	21.1	(1.4)
High school diploma	39.1	(2.4)	34.9	(1.6)
Any college enrollment	29.6	(2.5)	44.0	(1.8)
Sample Size	913		1248	

Notes:
(1) Public assistance receipt includes FRL, WIC, TANF, and food stamp receipt in the prior year.
(2) 8th grade includes children ages 13 and 14 in the listed year.
(3) Observations are weighted to account for the study's design. Standard errors are clustered at the family level.
(4) Poverty is defined as the ratio of taxable income to the federal need threshold, which takes into account household size.
(5) 130% of the poverty line is the threshold for free lunches and 185% is the threshold for reduced price lunches.

Table 3.3: Predicting public assistance receipt by 8th grade characteristics.

	Any assistance		FRL receipt	
	Grade 10	Grade 12	Grade 10	Grade 12
Grade 8 receipt	0.598***	0.413***	0.576***	0.382***
	(0.038)	(0.041)	(0.039)	(0.043)
Female	0.037	0.023	0.027	-0.007
	(0.026)	(0.025)	(0.025)	(0.024)
Black	0.232***	0.236***	0.202***	0.203***
	(0.046)	(0.040)	(0.043)	(0.041)
Hispanic	0.260***	0.324***	0.240***	0.333***
	(0.083)	(0.078)	(0.077)	(0.079)
Asian	0.363**	0.117	0.191	0.112
	(0.153)	(0.124)	(0.139)	(0.124)
Native American	0.417**	0.288*	0.483***	-0.112***
	(0.176)	(0.158)	(0.167)	(0.016)
Other race	-0.018	-0.006	0.103	-0.004
	(0.064)	(0.080)	(0.064)	(0.073)
Number of siblings	0.033**	0.062***	0.025**	0.061***
	(0.013)	(0.014)	(0.011)	(0.013)
Parent ed: Less than HS	0.237***	0.054	0.236***	0.067
	(0.070)	(0.057)	(0.068)	(0.055)
Parent ed: HS	0.061	0.045	0.069*	0.052
	(0.039)	(0.038)	(0.037)	(0.036)
Parent ed: BA or higher	-0.135***	-0.125***	-0.102***	-0.103***
	(0.037)	(0.034)	(0.036)	(0.033)
Number of observations	1911	1893	1892	1745

Notes:
(1) Coefficients are marginal effects from a probit model. Standard errors appear below the regression coefficients and are clustered at the family level.
(2) Regressions also include cohort fixed effects.
(3) "Any assistance" includes FRL, food stamps, TANF, and WIC.
(4) * p<0.10, ** p<0.05, *** p<0.01.

Table 3.4: Predicting public assistance receipt by household income.

	Grade 8		Grade 10		Grade 12	
130% of poverty line	0.478***	--	0.413***	--	0.398***	--
	(0.044)	--	(0.045)	--	(0.039)	--
185% of poverty line	--	0.469***	--	0.386***	--	0.424***
	--	(0.037)	--	(0.039)	--	(0.032)
Female	-0.018	-0.035	0.017	0.005	0.016	0.008
	(0.030)	(0.028)	(0.027)	(0.026)	(0.025)	(0.023)
Black	0.361***	0.331***	0.368***	0.350***	0.310***	0.279***
	(0.045)	(0.044)	(0.048)	(0.049)	(0.041)	(0.040)
Hispanic	0.307***	0.272***	0.347***	0.315***	0.445***	0.405***
	(0.085)	(0.088)	(0.081)	(0.077)	(0.072)	(0.073)
Asian	-0.099	-0.103	0.138	0.118	-0.010	-0.015
	(0.079)	(0.075)	(0.190)	(0.177)	(0.130)	(0.101)
Native American	0.051	0.077	0.387**	0.408**	0.327**	0.252**
	(0.167)	(0.166)	(0.165)	(0.162)	(0.148)	(0.120)
Other race	0.037	-0.011	-0.052	-0.042	-0.011	-0.014
	(0.100)	(0.098)	(0.086)	(0.091)	(0.086)	(0.084)
Number of siblings	0.073***	0.072***	0.055***	0.061***	0.073***	0.069***
	(0.017)	(.017)	(0.015)	(0.014)	(0.013)	(0.012)
Parent ed: Less than HS	0.327***	0.254***	0.326***	0.276***	0.118**	0.062
	(0.082)	(0.077)	(0.076)	(0.073)	(0.057)	(0.051)
Parent ed: HS	0.134***	0.103**	0.087**	0.059	0.049	0.018
	(0.045)	(0.044)	(0.039)	(0.039)	(0.036)	(0.032)
Parent ed: BA or higher	-0.194***	-0.173***	-0.205***	-0.184***	-0.166***	-0.144***
	(0.037)	(0.037)	(0.031)	(0.032)	(0.030)	(0.028)
Number of observations	1959	1959	1911	1911	1877	1877

Notes:
(1) Coefficients are marginal effects from a probit model. Standard errors appear below the regression coefficients and are clustered at the family level.
(2) Regressions also include cohort fixed effects.
(3) "Any assistance" includes FRL, food stamps, TANF, and WIC.
(4) Poverty is defined as the ratio of taxable income to the federal need threshold, which takes into account household size.
(5) 130% of the poverty line is the threshold for free lunches and 185% is the threshold for reduced price lunches.
(6) * p<0.10, ** p<0.05, *** p<0.01.

Table 3.5: Predicting family income for 8th grade assistance recipients.

	Above poverty threshold			
	200 percent		300 percent	
	Grade 10	Grade 12	Grade 10	Grade 12
Below 130% of poverty in grade 8	-0.451***	-0.209***	-0.121***	-0.079***
	(0.047)	(0.052)	(0.031)	(0.025)
Female	-0.054	0.005	-0.038**	-0.010
	(0.042)	(0.046)	(0.015)	(0.018)
Black	-0.087**	-0.148***	-0.028*	-0.050***
	(0.044)	(0.048)	(0.017)	(0.019)
Hispanic	-0.092*	-0.028	0.005	-0.005
	(0.048)	(0.063)	(0.020)	(0.023)
Asian	--	--	--	--
	--	--	--	--
Native American	--	0.013	--	--
	--	(0.203)	--	--
Other race	0.205	0.159	-0.028***	-0.032**
	(0.172)	(0.234)	(0.011)	(0.015)
Number of siblings	-0.000	-0.033*	-0.005	-0.013
	(0.017)	(0.018)	(0.007)	(0.010)
Parent ed: Less than HS	-0.170***	-0.234***	-0.063***	-0.088***
	(0.052)	(0.052)	(0.021)	(0.022)
Parent ed: HS	-0.123**	-0.141***	-0.012	-0.053**
	(0.052)	(0.053)	(0.019)	(0.023)
Parent ed: BA or higher	-0.110**	-0.159***	-0.012	-0.030*
	(0.043)	(0.040)	(0.022)	(0.016)
Above poverty threshold (percent)	22.4	24.2	7.1	7.8
Number of observations	769	762	769	762

Notes:
(1) Coefficients are marginal effects from a probit model. Standard errors appear below the regression coefficients and are clustered at the family level.
(2) Regressions also include cohort fixed effects.
(3) "Any assistance" includes FRL, food stamps, TANF, and WIC.
(4) This table is limited to those receiving any assistance in grade 8.
(5) Poverty is defined as the ratio of taxable income to the federal need threshold, which takes into account household size.
(6) Some racial groups are omitted due to a lack of variation on the outcome measures.
(7) * p<0.10, ** p<0.05, *** p<0.01.

Table 3.6: Educational attainment by public assistance receipt.

	Grade 8		Grade 10		Grade 12	
	HS graduate	Any college	HS graduate	Any college	HS graduate	Any college
Received public assistance	-0.055*	-0.102**	-0.032	-0.196***	-0.020	-0.240***
	(0.031)	(0.052)	(0.029)	(0.050)	(0.026)	(0.048)
Female	0.018	0.097***	0.020	0.103***	0.016	0.093***
	(0.022)	(0.035)	(0.022)	(0.035)	(0.022)	(0.035)
Black	-0.020	-0.026	-0.036	-0.018	-0.025	0.037
	(0.029)	(0.049)	(0.032)	(0.050)	(0.029)	(0.050)
Hispanic	-0.001	0.233***	-0.007	0.279***	-0.002	0.312***
	(0.050)	(0.081)	(0.052)	(0.075)	(0.053)	(0.074)
Asian	-0.003	0.248**	0.001	0.277**	-0.020	0.347***
	(0.074)	(0.120)	(0.074)	(0.110)	(0.086)	(0.100)
Native American	-0.369*	-0.441***	-0.362*	-0.419***	-0.372*	-0.436***
	(0.203)	(0.051)	(0.204)	(0.058)	(0.212)	(0.051)
Other race	-0.070	-0.032	-0.070	-0.043	-0.062	-0.035
	(0.082)	(0.123)	(0.082)	(0.128)	(0.079)	(0.134)
Number of siblings	-0.013	-0.001	-0.016	-0.004	-0.014	0.009
	(0.010)	(0.018)	(0.010)	(0.018)	(0.010)	(0.018)
Parent ed: Less than HS	-0.088	-0.296***	-0.094	-0.270***	-0.111*	-0.291***
	(0.058)	(0.055)	(0.059)	(0.058)	(0.061)	(0.057)
Parent ed: HS	-0.061*	-0.170***	-0.066*	-0.172***	-0.064*	-0.170***
	(0.035)	(0.049)	(0.036)	(0.049)	(0.035)	(0.049)
Parent ed: BA or higher	-0.049	0.025	-0.046	0.009	-0.045	0.003
	(0.038)	(0.054)	(0.038)	(0.054)	(0.037)	(0.054)
Number of observations	1421	1421	1401	1401	1398	1398

Notes:

(1) Coefficients are marginal effects from a probit model. Standard errors appear below the regression coefficients and are clustered at the family level.

(2) Regressions also include cohort fixed effects.

(3) "Any assistance" includes FRL, food stamps, TANF, and WIC.

(4) Educational attainment is measured by the total years of completed education.

(5) This table measures cumulative educational attainment through 2009. If observations were missing, the most recent post-high school observation was used.

(6) Only the first three cohorts are included because cohort 4 was in 12th grade in 2009.

(7) The high school graduate and any college categories are not mutually exclusive.

(8) * p<0.10, ** p<0.05, *** p<0.01.

Table 3.7: Parameters for the Monte Carlo simulation.

Invariant assumptions

(1) 2.5 million students receive FRL in grade 8, and 30% enroll in college.
(2) 26.7% of FRL recipients who enroll in college would not have received the maximum Pell Grant under current rules. 19% would receive a partial Pell and 7.7% would not receive a Pell
(3) All costs and benefits are discounted to age 19 at 3.5%, with sensitivity checks at 2% and 5%.
(4) Benefits are discounted by an additional 0.2% to account for mortality rates.

Enrollment and attainment assumptions

Variable	Mean	10th %ile	25th %ile	50th %ile	75th %ile	90th %ile
Case 1: Increased enrollment resulting from the early commitment program.						
Increased enrollment (pct)	4.0	2.0	3.0	4.0	5.0	7.0
Counterfactual attainment						
High school diploma	10.0	6.0	8.0	10.0	12.0	14.0
No high school diploma	90.0	94.0	92.0	90.0	88.0	86.0
Educational attainment (pct)						
Some college	50.0	44.0	47.0	50.0	53.0	56.0
Associate's degree	30.0	25.1	27.3	29.9	32.5	35.0
Bachelor's degree	20.0	16.0	17.8	19.8	22.1	24.0
Case 2: Increased attainment by previously enrolled students.						
Educational attainment (pct)						
Some college to AA	3.0	1.0	2.0	3.0	4.0	5.0
AA to BA	3.0	1.0	2.0	3.0	4.0	5.0

Cost assumptions

Variable	Mean	10th %ile	25th %ile	50th %ile	75th %ile	90th %ile
Case 1: Increased enrollment resulting from the early commitment program.						
Enrollment status (pct)						
Full-time	61.0	55.0	58.0	61.0	64.0	67.0
Part-time	39.0	45.0	42.0	39.0	36.0	33.0
Years of Pell receipt						
Full-time	2.50	1.69	2.07	2.49	2.91	3.29
Part-time	1.60	1.07	1.32	1.60	1.87	2.12
Average Pell (undiscounted)						
Full-time	4326.94	2923.05	3577.32	4305.86	5037.35	5690.05
Part-time	1445.43	980.37	1209.75	1450.20	1697.61	1914.36

Table 3.7: Parameters for the Monte Carlo simulation (continued).

Variable	Mean	10th %ile	25th %ile	50th %ile	75th %ile	90th %ile
Case 2: Increased funding for previously enrolled students not receiving full Pell.						
Enrollment status (pct)						
Full-time	57.0	51.0	54.0	57.0	60.0	63.0
Part-time	43.0	49.0	46.0	43.0	40.0	37.0
Years of Pell receipt						
Full-time	2.30	1.56	1.90	2.30	2.68	3.05
Part-time	1.50	1.03	1.25	1.51	1.76	1.99
Average Pell (undiscounted)						
Full-time	2644.27	1803.41	2208.09	2648.39	3089.89	3482.35
Part-time	873.61	593.87	726.25	877.83	1019.45	1151.19
Benefit assumptions						
Variable	Mean	10th %ile	25th %ile	50th %ile	75th %ile	90th %ile
Lifetime earnings (undiscounted)						
No high school diploma	969,000	554,324	749,997	968,280	1,184,523	1,380,629
High school diploma	1,304,000	742,897	1,005,135	1,297,675	1,587,480	1,850,298
Some college	1,547,000	881,335	1,192,442	1,539,496	1,883,307	2,195,100
Associate's degree	1,727,000	983,883	1,331,187	1,718,623	2,102,438	2,450,510
Bachelor's degree	2,268,000	1,292,094	1,748,195	2,256,999	2,761,047	3,218,156
Labor force particiption rate (pct)	78.0	73.0	75.0	78.0	81.0	83.0
Effective federal tax rate (pct)	15.0	10.0	12.0	15.0	17.0	20.0

Table 3.8: Estimated fiscal impacts of the early commitment program.

Cost estimates ($mil)	10th %ile	25th %ile	50th %ile	75th %ile	90th %ile
Increased enrollment (case 1)	232.7	404.8	655.3	969.9	1331.4
Additional awards (case 2)	324.7	570.4	857.3	1195.0	1523.9
Total	707.3	1066.0	1523.5	2103.7	2687.3

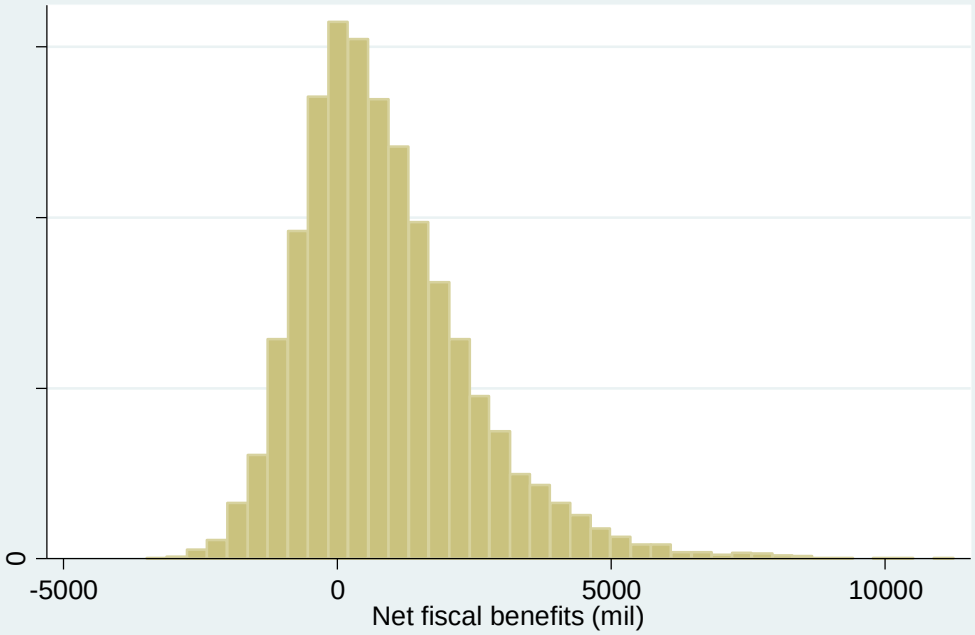
Benefit estimates ($mil)	10th %ile	25th %ile	50th %ile	75th %ile	90th %ile
Increased enrollment (case 1)	630.3	1153.4	1968.4	3068.4	4357.4
Additional awards (case 2)	66.3	111.1	181.4	278.3	393.8
Total	777.8	1321.7	2175.2	3310.7	4641.9

Net fiscal benefit by discount rate ($mil)	10th %ile	25th %ile	50th %ile	75th %ile	90th %ile
Low (2 percent)	-444.4	326.0	1418.4	2947.1	4628.7
Preferred (3.5 percent)	-832.6	-201.5	609.1	1682.4	2897.9
High (5 percent)	-1123.3	-562.5	78.0	888.5	1770.3

Benefit-cost ratio by discount rate	10th %ile	25th %ile	50th %ile	75th %ile	90th %ile
Low (2.0%)	0.73	1.20	1.94	3.15	4.93
Preferred (3.5%)	0.53	0.87	1.41	2.28	3.56
High (5.0%)	0.39	0.64	1.05	1.70	2.65

Note:
(1) All estimates come from a Monte Carlo simulation with 10,000 trials.

Figure 2: Distribution of Estimated Net Benefits



Result of 10,000 simulations with the parameters from Table 2.

Appendix 3.1: Federal program receipt by 8th grade cohort.

Cohort 1 (1999)	Mean	(SE)
Any public assistance (pct)	32.0	(2.7)
WIC	6.2	(1.1)
Free/reduced price lunch	31.1	(2.7)
TANF	4.1	(1.0)
Food stamps	11.3	(1.7)
Number of children	569	

Cohort 2 (2001)	Mean	(SE)
Any public assistance (pct)	31.9	(2.7)
WIC	5.3	(1.4)
Free/reduced price lunch	30.7	(2.6)
TANF	1.4	(0.4)
Food stamps	6.9	(1.3)
Number of children	565	

Cohort 3 (2003)	Mean	(SE)
Any public assistance (pct)	30.1	(2.6)
WIC	5.4	(1.6)
Free/reduced price lunch	29.1	(2.6)
TANF	3.1	(1.0)
Food stamps	11.2	(1.9)
Number of children	546	

Cohort 4 (2005)	Mean	(SE)
Any public assistance (pct)	38.7	(2.8)
WIC	7.7	(1.8)
Free/reduced price lunch	37.4	(2.8)
TANF	1.9	(0.6)
Food stamps	13.2	(2.1)
Number of children	560	

Notes:
(1) Any aid includes FRL, WIC, TANF, and food stamp receipt in the prior year.
(2) 8th grade includes children ages 13 and 14 in the listed year.
(3) FRL includes both free/reduced lunch and breakfast programs.
(4) Observations are weighted to account for the study's design. Standard errors are clustered at the family level.

Appendix 3.2: Educational attainment by family income.

	Grade 8		Grade 10		Grade 12	
	HS graduate	Any college	HS graduate	Any college	HS graduate	Any college
Below 185% of poverty line	-0.099***	-0.224***	-0.042	-0.205***	-0.075***	-0.198***
	(0.032)	(0.040)	(0.029)	(0.043)	(0.028)	(0.040)
Female	0.020	0.104***	0.021	0.103***	0.018	0.092***
	(0.021)	(0.035)	(0.022)	(0.035)	(0.022)	(0.035)
Black	-0.012	0.000	-0.035	-0.035	-0.010	0.002
	(0.028)	(0.045)	(0.031)	(0.049)	(0.027)	(0.048)
Hispanic	0.010	0.269***	-0.004	0.273***	0.010	0.265***
	(0.046)	(0.071)	(0.051)	(0.072)	(0.048)	(0.073)
Asian	0.004	0.265**	0.007	0.296**	-0.010	0.354***
	(0.070)	(0.116)	(0.073)	(0.118)	(0.082)	(0.103)
Native American	-0.331*	-0.425***	-0.368*	-0.429***	-0.352*	-0.444***
	(0.185)	(0.057)	(0.202)	(0.051)	(0.207)	(0.047)
Other race	-0.068	-0.019	-0.067	-0.021	-0.064	-0.027
	(0.083)	(0.122)	(0.082)	(0.123)	(0.082)	(0.121)
Number of siblings	-0.013	-0.001	-0.016	-0.010	-0.012	-0.001
	(0.010)	(0.017)	(0.010)	(0.018)	(0.010)	(0.018)
Parent ed: Less than HS	-0.069	-0.260***	-0.090	-0.266***	-0.090	-0.279***
	(0.054)	(0.058)	(0.058)	(0.058)	(0.058)	(0.057)
Parent ed: HS	-0.056	-0.164***	-0.062*	-0.165***	-0.053	-0.155***
	(0.034)	(0.049)	(0.036)	(0.050)	(0.034)	(0.049)
Parent ed: BA or higher	-0.058	0.003	-0.047	0.005	-0.052	0.017
	(0.039)	(0.054)	(0.038)	(0.054)	(0.038)	(0.053)
Number of observations	1421	1421	1401	1401	1398	1398

Notes:
(1) Coefficients are marginal effects from a probit model. Standard errors appear below the regression coefficients and are clustered at the family level.
(2) Regressions also include cohort fixed effects.
(3) 185% of the poverty line is the threshold for FRL eligibility.
(4) Educational attainment is measured by the total years of completed education.
(5) This table measures cumulative educational attainment through 2009. If observations were missing, the most recent post-high school observation was used.
(6) Only the first three cohorts are included because cohort 4 was in 12th grade in 2009.
(7) The high school graduate and any college categories are not mutually exclusive.
(8) * p<0.10, ** p<0.05, *** p<0.01.