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6. THE PELL PROGRAM AT THIRTY YEARS

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THE PELL PROGRAM AT THIRTY YEARS

For more than 30 years the Pell program has provided a voucher-like subsidy, for low-income students who apply for financial aid, to any qualifying college or university in the country. In 2005, the Pell program provided over \$12 billion in grants to more than a fifth of all college students. However, despite the significant resources spent on need-based financial aid in the United States, the gap between low- and high-income students' matriculation rates into post-secondary education has not only persisted but has widened in the last three decades (e.g., Ellwood & Kane, 2000). Disparate college attainment across socio-economic status is of particular concern to policy makers and university administrators because the percentage difference in earnings between college and high-school graduates has grown dramatically over the same period – from 19 percent in 1980 to 50 percent in 1997, for 25 to 34-year-old males. Combined with above-inflation increases in the cost of college and an increasing proportion of college-age students attending colleges these factors have contributed to mounting pressure by consumers, providers, and overseers of higher education to reform the Pell program and other components of the U.S. financial aid system (e.g., McPherson & Schapiro, 1997; Ehrenberg, 2000). To the extent that potential shortcomings of the system may be mitigated by a serious account of the related academic literature, our analysis aims to inform future policy proposals on the quantifiable outcomes of Pell on the access, choice, and persistence of low-income students.

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Curs, Singell, and Waddell: The Pell Program at Thirty Years

01 data for 1979; and (c) the High School and Beyond (HSB) survey for the
02 senior class of 1980. However, similar to Kane (1994), an examination
03 of the growth in enrollment rates for those from families in the lowest
04 income quartile (generally eligible students) in comparison to those from
05 the top three quartiles (who are increasingly unlikely to be eligible) is
06 made using CPS data for the pre-versus post-Pell period (i.e., 1970...1972
07 versus 1973...1979). However, in this case, a distinction is made between
08 any college enrollment, private college enrollment, and public two-
09 year college enrollment. The results indicate that total (private) college
10 enrollment grew 2.6 (2.8) percent less for the lowest income quartile over
11 the period. On the other hand, college enrollment grew between 2.4 and
12 3.4 percent more quickly for the lowest income quartile at public two-
13 year colleges. This result provides the first evidence that the Pell program
14 may yield significantly different effects across institutions of different
15 selectivity. In other words, student concerns that the Pell program is not
16 sufficiently generous to provide access to more selective institutions and
17 institutional concerns that the Pell program differentially benefits certain
18 institutions could well be warranted.

19 The most dramatic change in enrollment behavior over the quarter
20 century has been the rise in the participation of students over the age
21 of 30 in undergraduate education, which rose from 15 to 30 percent of
22 the total of all undergraduates between 1970 and 2000. An important
23 question is the extent to which the availability of federal aid in general
24 and the Pell grants in particular accounts for the greater participation of
25 older students in higher education. Seftor and Turner (2002) explicitly
26 examine this issue by again making use of a structural change brought
27 about by; (1) the introduction of the Pell program and; (2) a 1986
28 reauthorization rule change that redefined independent student status
29 thereby restricting this group's access to Pell funds.

30 Two separate analyses use CPS data for the period between 1969 and
31 1974 that span the period of the Pell program's introduction and between
32 1984 and 1990 that span the period of the 1986 reauthorization. The
33 results from the first analysis show that, unlike for the broader population
34 of college students, the introduction of the Pell program increased the
35 enrollment of male (female) independent students by 1.5 (1.3) percent.
36 The relatively greater responsiveness of older, independent students
37 versus their younger dependent counterparts suggests that older students
38 are somewhat less daunted by the complexity of applying for federal aid or
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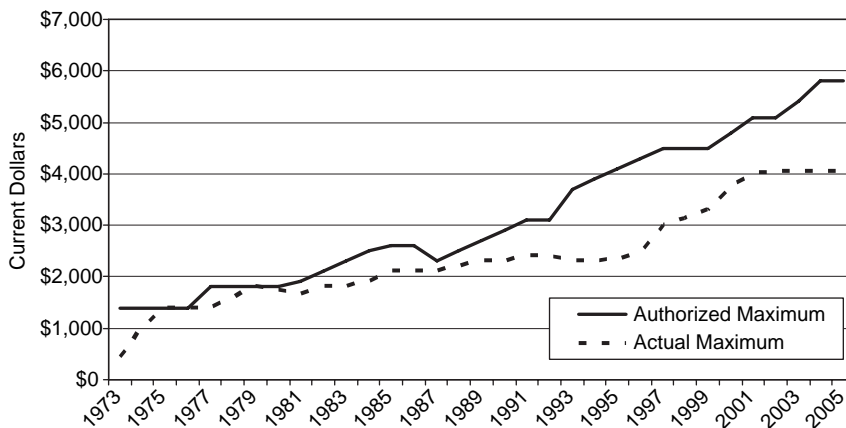
40 ⁶ The 1986 amendment to the Higher Education Act required, for the first time, a Pell applicant
41 to be at least 24 years old, married, or with children to qualify as an independent student.

01 value to be less than the authorized maximum determined by the
 02 reauthorization process.

03 In practice, the restrictions placed on Pell funding by the appro-
 04 priations process has meant that the appropriated maximum rarely
 05 equals the authorized maximum. Thus, as Figure 6.1 shows, the appro-
 06 priated maximum has equaled the authorized maximum in only
 07 three years of its history and last occurred in 1979. The appropriated
 08 maximum is the amount that the neediest students are likely to receive
 09 and is often used as a measure of the generosity of the Pell program.
 10 Consequently, there are often heated debates during the appropriation
 11 process regarding the setting of the maximum award. Nonetheless, the
 12 gap between the appropriated and authorized maximum grant grew
 13 through the mid-1990s, with only a small abatement of this trend with
 14 the 1998 reauthorization.

15 The logic behind enacting education appropriations at least nine
 16 months in advance of the relevant academic year is so that students
 17 can plan for college with some reasonable expectation regarding the
 18 level of financial aid that will be available to them. However, because
 19 the annual appropriation is determined on the basis of estimates of
 20 the programs costs that are expected to occur at the chosen maximum
 21 grant, it has not been too uncommon that there is either a surplus or
 22 a shortfall of funds to pay students the award value for which they
 23 qualify. A surplus is a potential problem for the DOE because it has
 24 relatively limited carryover authority. Nonetheless, shortfalls have been
 25

26 Figure 6.1: Authorized and Actual Maximum Pell Grant.



38 Source: College Board, Trends in Student Aid 2004, Table 8.

01 the more common occurrence, particularly in recent years. The recent
02 response of the DOE to these shortfalls has been of particular policy
03 concern.

04 Prior to the 1992 HEA, the Secretary of Education had the
05 statutory authority to reduce awards to respond to a shortfall in appro-
06 priated funds. A reduction of awards was made eight times between
07 1973 and the last reduction in 1991. Although the 1992 HEA repealed
08 the Secretary of Education's authority to reduce awards, the appro-
09 priations legislation between 1994 and 2001 technically restored this
10 authority (although it was never used). Moreover, since 2002, the
11 appropriation legislation has not included the authority to lower grants.
12 Instead, the Secretary of Education has utilized the relatively unique
13 attribute of the Pell grant appropriation to cover the shortfall: the DOE
14 can and has borrowed from next year's appropriation because program
15 funds are available for obligation immediately upon enactment and
16 remain available for a full two years. In other words, the DOE uses
17 funds from the 2nd (overlapping) fiscal years' appropriation to meet
18 the current award year costs.

19 According to a 2004 Congressional Research Service report
20 (Congressional Research Service [CRS] Report RL 31668, CRS-11), the
21 shortfall problem began in FY2001, when under-funding led to the
22 borrowing of almost \$1 billion in future funds. Specifically, appropria-
23 tions legislation for FY2001 set the maximum Pell Grant at \$3,750 and
24 appropriated \$8.756 billion. In January of 2001, the DOE estimated
25 that the program costs for FY2001 at the specified maximum grant level
26 would be \$9.115 billion, and that the difference between the appropria-
27 tion and the program costs would be made up by the \$359 million
28 in surplus funds from the prior year. However, the actual program
29 costs were \$9.998 billion (a 10 percent higher program cost), while
30 the surplus from the prior year was \$40 million less than had been
31 estimated. Consequently, there was a \$923 million shortfall for the
32 FY 2001 Pell Grant program and the DOE funded the shortfall by
33 borrowing from FY2002 appropriation, which became available for
34 obligation during the 2001–2002 award year.

35 These shortfalls have continued to accumulate, and the budget
36 shortfall had reached \$4.3 billion as of 2005. The DOE attributes
37 these shortfalls to the recent growth in the maximum appropriated
38 Pell Grant and the unexpected growth in the number of Pell applicants
39 and recipients (U.S. Department of Education, 2004). Regardless, the
40 combined effect of the Pell program changes brought about by the
41 reauthorization process and the increasing demand pressure for Pell

01 funds have had a dramatic impact on who received Pell aid and the
02 level of funding provided to Pell recipients, which is the topic we turn
03 to next.¹

04

05 THE PELL AWARD: WHO QUALIFIES AND FOR HOW MUCH?

06

07 All information needed to determine a student's eligibility, and the size
08 of any Pell grant to be awarded, is provided to financial aid adminis-
09 trators through the completion and submission of a Free Application
10 for Federal Student Aid (FAFSA) form, which can be submitted as
11 early as the first of January of the year preceding projected enrollment.
12 While there is no formal application deadline for the FAFSA, there are
13 a number of states that impose their own deadline, some as early as the
14 first of March. Likewise, many institutions impose their own deadline
15 to submit the FAFSA to better facilitate the determination of their own
16 financial aid offers.

17

18 Beyond broad-based requirements (e.g., qualified to enroll in
19 postsecondary education, working toward a degree in an eligible
20 program, U.S. citizen or eligible non-citizen, maintain satisfactory
21 academic progress once in school, and no major convictions for the
22 selling or possessing of illegal drugs), the Pell Grant further limits
23 eligibility to students with the greatest amount of need. Although
24 there are some exceptions, Pell recipients must have a high-school
25 diploma (or equivalent) and be enrolled at an eligible institution as an
26 undergraduate with the purpose of obtaining a degree or certificate.
27 Pell funds, although available for the completion of more than one
28 vocational/certificate or non-degree program, cannot be received by
29 persons who have already earned a baccalaureate or professional degree.
30 By design, then, the Pell program is not intended to facilitate retraining
31 associated with career changes, for example. Formally, the eligibility
32 requirements for federal student aid are contained in Title IV of
33 the HEA.

34

35 To determine the level of the Pell grant, FAFSA-reported data
36 are used to calculate two key measures: a cost of attendance (COA)

37

¹ Fiscal pressures affect other sources of federal, state, and institutional aid beyond Pell awards. Thus, although only briefly explored here, it is important to emphasize that broad-based budgeting issues are important for understanding the conclusions of the research with regard to the efficacy of the Pell program, because frequently changes in Pell awards are taking place in the context of broader changes in funding for financial aid that might well confound its effects (St. John, 2003). For example, the Social Security Benefits program that provided grant aid to college students whose parents died or experienced a disability was eliminated in the early 1980s at the same time that there were significant changes in the Pell program (Dynarski, 2002).

41

01 (which varies across both institutions and students) and an expected
 02 family contribution (EFC) (which varies across students). The COA is a
 03 measure of the expected educational expenses a student will undertake.
 04 Individual institutions set the COA for a given student, based on the
 05 attributes of the institution and the student. For full-time students,
 06 their COA includes such costs as tuition and fees, books, supplies,
 07 transportation, other personal education related expenses, and room
 08 and board. For part-time students and those enrolled in correspondence
 09 courses COA expenses are more limited.

10 Determination of a student's EFC is much more complex an
 11 undertaking. In general, the EFC is constructed to represent the
 12 amount the applicant-student and/or family can be expected to
 13 contribute toward financing the degree being sought. The student's
 14 contribution is estimated from information regarding their income,
 15 allowances against their income, number of children and their assets.
 16 However, the formula differs depending on whether the student is
 17 dependent, independent without dependents other than their spouse,
 18 or independent with dependents. To qualify as an independent student,
 19 and thus not have contribution from their parents be counted in their
 20 EFC, a person must meet one of the following criteria: be 23 years
 21 of age, a veteran of the U.S. Armed Forces, working on a master's or
 22 doctorate degree, married, being or having been a ward of the court,
 23 or having legal dependents other than a spouse. For dependent appli-
 24 cants, parental contributions are calculated over the same attributes,
 25 the value of which being divided by the number of dependents the
 26 parent's currently have in college. For independent students, there is
 27 no parent's contribution.

28 The EFC calculation includes a few *exceptions* in its formula.
 29 For example, a student can qualify to have their EFC calculated
 30 through a simplified formula, one which does not take assets into
 31 account. To qualify, they (for an independent student) or their parents
 32 (for dependent) must have an income below \$49,999 and be eligible
 33 to file the IRS 1040A or 1040EZ tax form. Further, students may
 34 automatically qualify for an EFC of zero if their (for independent)
 35 or their parent's (for dependent) adjusted gross income was less than
 36 \$15,000 the previous year and they are eligible to file the IRS 1040A
 37 or 1040EZ tax form.

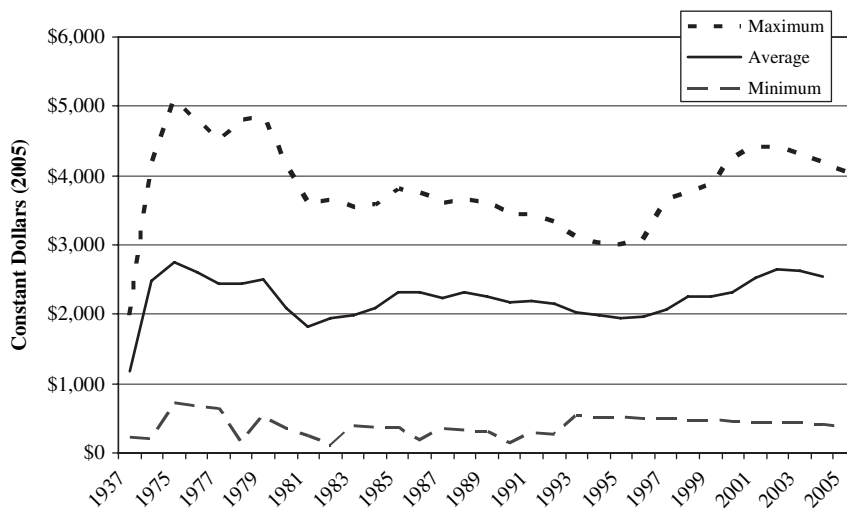
38 Once the COA and EFC have been calculated, the value of the
 39 Pell award is formulaic. At present, conditional on being above the
 40 federally-mandated minimum grant (currently \$400), the level of an
 41 individual student's grant in a given year is the minimum of: (a)

01 with very low tuition levels. Thus, in 2006, a student would have
 02 had to face tuition of less than \$675 for the tuition sensitivity amount
 03 to the binding factor in their Pell award.

04 The original intent of the Pell Grant program was to provide an
 05 award that, when combined with other sources of aid and a reasonable
 06 family or student contribution, covered no less than 75% of the
 07 student's cost of attendance. However, while the average nominal award
 08 value has increased from \$270 when the program started in 1973 to
 09 \$2,466 in 2004, the real value of the Pell grant has decreased. In fact,
 10 in real dollars, the average Pell peaked shortly after the program's
 11 introduction – in 1975 at \$2,602. Figure 6.3 reports the time series of
 12 2005-dollar Pell awards since the inception of the program in 1973,
 13 illustrating the decline in the average award value to \$1,718 in 1981,
 14 rising again to \$2,550 in 2004.

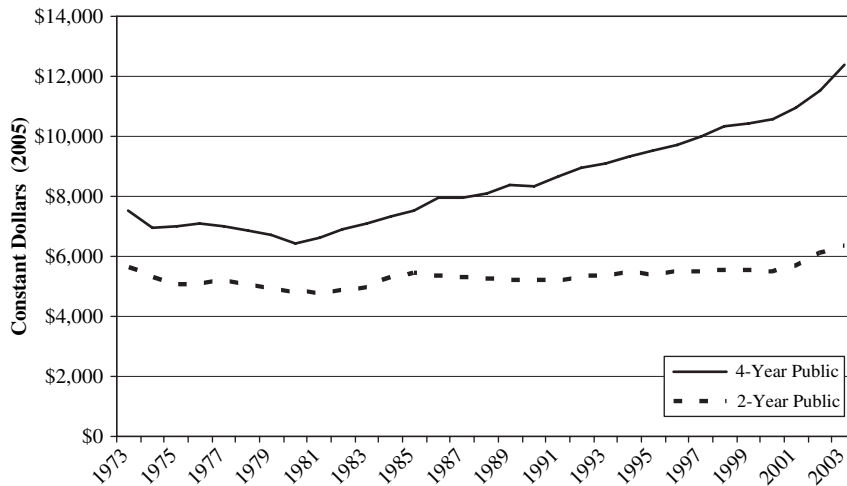
15 Of course, in relation to the costs of college, inflation-adjusted
 16 Pell awards still overstate their purchasing power over this time series
 17 (particularly in recent years). In fact, the cost of college has increased
 18 at nominal rates of between 5 and 8 percent since the 1980s, which
 19 has outpaced both the growth in award values and the overall rate of
 20 inflation, more generally. Figure 6.4 show a particularly strong trend
 21 for four-year public institutions, where the average cost of attendance in
 22

23 **Figure 6.3: Maximum, Average, and Minimum Pell Award Values.**



38 Source: U.S. Department of Education, 2003–2004 Title IV/Federal Pell grant Program End-of-Year Report, Table 1.

Figure 6.4: Cost of Attendance at Public four-year and two-year Institutions.



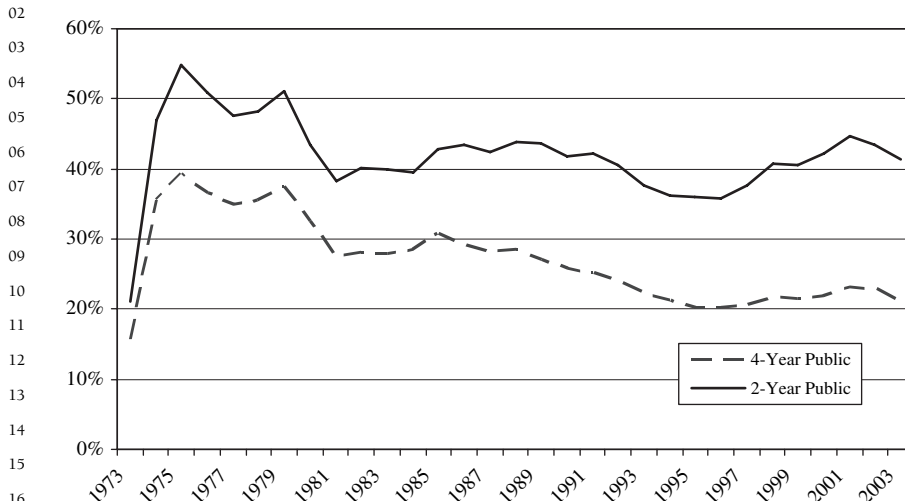
Source: National Center for Education Statistics, U.S. Department of Education, *Digest of Education Statistics*, 2004, Table 313.

2003 was nearly 93 percent higher (in 2005 dollars) than in 1981 (when costs were at their minimum). Figure 6.4 also indicates that the trend in real costs of attending two-year public institutions, although positive, is less pronounced than at four-year institutions with an increase in real costs of about 24 percent over this same period.

Figure 6.5 illustrates the changing purchasing power of an average Pell grant as a percentage of the cost of attendance at four-year and two-year public institutions of higher education. The plots show that, at its height of purchasing power in 1975, the average Pell award covered 39 percent of the cost of a four-year public university and 55 percent of a two-year public college. In 1995, the maximum award hit its lowest point, covering only 20 percent of the average cost of a four-year public university and 36 percent of the average cost of a two-year public college.

The descriptive evidence shows that there have been significant changes in the value of the Pell program over the last 30 years, which have resulted from both explicit changes in the parameters of the Pell formula and due to intermittent interest by Congress in funding the Pell program. This variation in the real value of the Pell award has been exploited in the empirical literature to evaluate whether changes in the generosity of the program do, in fact, influence the college outcomes low-income students.

01 **Figure 6.5:** The Average Pell Grant as a Percentage of the Average Cost of Attendance.



17 Sources: U.S. Department of Education, 2003–2004 Title IV/Federal Pell grant Program End-of-Year Report, and
 18 National Center for Education Statistics, U.S. Department of Education, *Digest of Education Statistics*, 2004.

21
 22 **EMPIRICAL ANALYSIS OF THE PELL PROGRAM**

23
 24 **NATURAL EXPERIMENTS: TESTING THE EFFICACY OF THE PELL**
 25 **PROGRAM**

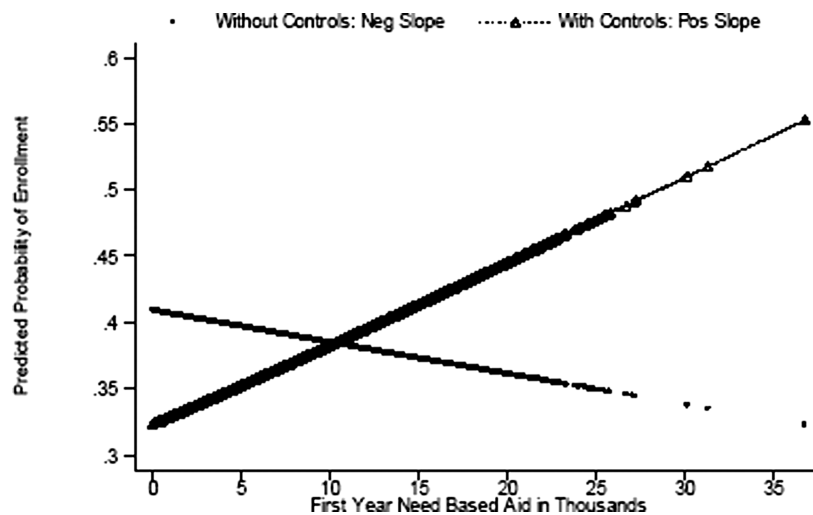
26
 27 With the introduction of federal aid to students in 1973, the earliest
 28 analyses in higher education were motivated in an attempt to quantify
 29 simple student choice models which predicted that financial aid should
 30 affect both student access and enrollment choice. For example, Jackson
 31 (1978) posits that the college-entry decision depends on both socio-
 32 logical factors such as family background, high-school peers, and the
 33 desire to interact with similar persons and economic factors such as
 34 the investment (human capital) and consumption value of college.
 35 Such a framework suggests that empirical analyses can potentially
 36 identify both the predisposition of students to attend college and how
 37 financial aid affects college attendance controlling for this predispo-
 38 sition. However, the expectation that college outcomes depend on
 39 a relatively complex set of student attributes suggests that empiri-
 40 cally testing the efficacy of the Pell program is not straightforward.
 41 In particular, Pell awards – that by design are provided to a select

01 set of low-income students – are likely to be correlated with omitted
02 (unobserved) student attributes that directly affect college choice.

03 The potential bias arising from student heterogeneity on the
04 predicted impact of aid on college outcomes can be demonstrated by a
05 simple empirical exercise. Specifically, we use data detailed in Singell
06 and Stater (2006) for freshman applicants to three large public univer-
07 sities (i.e., Indiana University, University of Colorado, and University
08 of Oregon) to estimate a probit model of enrollment on need-based
09 aid alone (i.e., the unconditioned need-based aid effect) and a probit
10 model of enrollment on need-based aid conditioned on a detailed list of
11 individual and institutional controls (i.e., the conditioned need-based
12 aid effect).³ The predicted probability of enrolling in one of the three
13 universities is plotted in Figure 6.6, which maps the unconditioned
14 and conditioned effect of need-based aid.

15 The unconditioned aid effect shows a downward-sloping
16 relationship between the amount of need-based aid and the probability
17 of enrollment, suggesting that increases in need based aid actually
18 reduce the likelihood of student enrollment. However, the conditioned
19 relationship between need-based aid and the probability of enrollment

20
21 Figure 6.6: Relationship between Probability of Enrolling and Need-Based Aid.



37 Source: Singell and Stater, 2006.

38
39 ³ The control variables include a list of personal attributes (e.g., age, gender, race, high-school
40 GPA) and a set institutional dummies. For a full list of controls, see Table 2 in Singell and Stater
41 (2006).

01 exhibits a positive relationship, which indicates that students who
02 receive relatively large amounts of need-based aid have other attributes
03 that make them less likely to enroll than the typical enrollee. In other
04 words, a failure to control for these attributes can yield a downward
05 bias on the impact of need-based aid. For example, there need be only
06 one covariate that relates negatively to one's propensity to enroll and
07 positively to one's propensity to receive need-based aid (e.g., family
08 income) in order that omitting such a variable bias the observed
09 relationship between aid and enrollment so much so that it yields the
10 misleading conclusion that aid lowers one's enrollment probability.

11 However, just as inadequate controls for observable attributes may
12 taint estimates of the effects of different types of aid, failure to account for
13 the influence of unobservable attributes may have similarly undesirable
14 effects. For instance, although it is possible to control for the relatively
15 low socio-economic status of Pell recipients, these students may yet have
16 some difficult-to-measure attributes (e.g., having a low personal taste
17 for higher education or originating from backgrounds without highly
18 developed academic support networks) that make them less likely to
19 enroll or graduate than students who do not receive a Pell award.

20 The estimated effects of financial aid can be biased when an
21 empirical analysis fails to control for unobserved attributes related
22 to a student's likelihood of enrollment or graduation. In a critique
23 of the techniques of the National Center for Educational Statistics,
24 Heller (2004) documents how a failure to model unobserved attributes
25 may understate the importance of a student's financial attributes in
26 their college-going decisions. In the same volume, Becker (2004) goes
27 further in the critique of education literature and outlines the various
28 biases that arise through omitted variable bias and sample selection,
29 which could confound the identification of the "true effects" of financial
30 aid. To obtain unbiased estimates of the effect of Pell aid, one must
31 find a source of exogenous variation in aid that is uncorrelated with
32 unobserved student attributes that affect educational outcomes.

33 The concern with regard to identifying exogenous variation in aid
34 has led to a wider embrace of experimental methods into the field and
35 study of economics. Laboratories now provide a proving ground for
36 controlled experimentation with economic incentives. For example, the
37 laboratory often makes economic incentives observable, where they may
38 not be outside of the laboratory, and provides the researcher greater
39 control over economic variables of interest. As such, those particular
40 variables that theory may relate to behavior are both observable and able
41 to be controlled by the researcher within laboratory environments.

01 In many cases, this ability to control the initial event to which
02 theory predicts a response becomes an important element in achieving a
03 legitimate test of the theory. Further, without the knowledge (or, more
04 weakly, the assumption) that the event was not due to the behavior of
05 agents, associating any observed change in behavior to the event in a
06 causal way would be in error.

07 For example, consider the economist's standard model of demand
08 and supply, where quantities demanded by consumers are believed to
09 fall with price while quantities supplied by firms are believed to rise.
10 The quantity demanded at a given price is also thought to respond to
11 economic factors, such as it would to a new advertising campaign, for
12 example. Likewise, the quantity supplied to a market at a given price also
13 responds to economic factors. Even in such a simple model, measuring the
14 responsiveness of consumers to a change in price becomes a non-trivial
15 task, as the researcher must isolate the effect of changes in price on sales,
16 holding constant other factors that can influence the level of demand. As
17 a price change can occur precisely because of a change in one of the factors
18 affecting the level of demand (e.g., advertising), it becomes difficult to
19 assure that observed price changes are occurring independently.

20 What is a natural experiment? Arguably, the fundamental
21 advantage offered through the advent of experimental methods in
22 economics is the ability to better isolate a change in prices and therefore
23 incentives. However, while the laboratory will often provide a cleaner
24 environment, such isolation need not be absent from the world beyond
25 the lab. Natural experiments, then, can offer the same ability for the
26 researcher to test theory or measure behavioral responses to events.
27 In a natural experiment, with the event which one is interested in
28 measuring, a response occurs as a natural product of the economic
29 system more generally. Outside of the particular period of analysis,
30 the economic system driving the laboratory environment is specifically
31 designed by the researcher. Therefore, while the laboratory researcher
32 knows that the event of interest is exogenous to the economic agents
33 under analysis (because he instigated it himself as part of the experi-
34 mental design), the exploitation of naturally occurring events (such as
35 the introduction of the Pell Grant) must be assumed by the researcher
36 to be exogenous to the economic agents under analysis. Often times,
37 evidence will suggest this as the case, making such an assumption
38 quite reasonable. Given our particular interest in the efficacy of the Pell
39 program, researchers must assume that the particular change in policy
40 to which the response of economic agents is to be measured arose in a
41 way that is exogenous to the economics agents themselves.

01 It is important to note that the comfort-level of researchers differs
 02 in regards to their wiliness to assume that government actions are
 03 independent of the behavior of the agents affected by the policy change.
 04 For example, one would naturally be more inclined to question the
 05 cause of a policy change at a particular institution (e.g., the intro-
 06 duction of a merit-based scholarship at Agnes Scott College) than if the
 07 policy change were to apply to a larger sample of institutions (e.g., all
 08 Georgia institutions). More germane to our point, one may question
 09 whether the behavior thought to be in response to the policy change
 10 (e.g., a stronger entering class) was actually caused by something else
 11 altogether – something that may have also caused the introduction of the
 12 merit-aid in the first place and therefore would spuriously assign some
 13 degree of predictive power to the introduction of merit aid. In general,
 14 by appealing to scale, one tends to be more comfortable arguing that the
 15 event be treated as exogenous to the economic agents under analysis as
 16 the size of the affected group becomes larger.

17 Of course, having the policy applied very broadly also comes at some
 18 cost. For example, testing for the efficacy of the naturally occurring intro-
 19 duction of the Pell Grant in 1973 is facilitated by the discriminating nature
 20 of the award. That is, by excluding certain observable types of students
 21 (e.g., the wealthy), such a group of students can act as a control group
 22 against which one might measure the effect of providing low-income
 23 students with financial assistance. It is a necessary assumption then (made
 24 either with support or not) that the control group is appropriate for such a
 25 role. Such analyses are commonly referred to as difference-in-difference.
 26 That is, when an isolated change occurs in one aspect of the economic
 27 environment, one can measure the difference in the behavioral changes
 28 within two distinct groups of economic agents before and after the change.
 29 Thus, the best studies of need-based grants are often those that exploit the
 30 unique attributes of the authorization process for the Pell program (e.g.,
 31 changes in the maximum award) that can be argued are exogenous to the
 32 student and his or her home institution.

33 34 IDENTIFYING THE ENROLLMENT EFFECT OF PELL PROGRAM

35
36 The understanding that college outcomes depend on personal attributes,
 37 ability, family background, and a host of other factors led early Pell
 38 studies to use large national surveys that included a relatively high level of
 39 individual detail. In particular, early work utilized three nationally repre-
 40 sentative surveys – the National Longitudinal Study of the High School
 41 Class of 1972 (NLS72), the High School and Beyond survey of 1980

01 for the calculation of the ratio of below-median-income enrollment
02 rates to above-median-income enrollment rates. This descriptive analysis
03 indicated that this ratio of enrollment rates actually decreased over this
04 period for each of four population groups (i.e., men, women, white and
05 black).

06 Second, the NLS72 data include the percentage of high school seniors
07 that were expected to enroll in some form of postsecondary education by
08 socioeconomic status between 1972 and 1980. A descriptive summary
09 of the college-attendance expectations in the NLS72 indicated that the
10 expected enrollment rate of below-median-income students declined
11 relative to the above-median-income group between 1972 and 1980 for
12 white college students. This ratio increased slightly for blacks. Collec-
13 tively, these findings provide suggestive evidence that the introduction
14 of the Pell program and the availability of federal aid did little to increase
15 the access to higher education for the poor.

16 However, by failing to control for other factors that affect the choice
17 to attend college that potentially vary by income (e.g., the unemployment
18 rate, the return to education), the analysis may simply indicate that federal
19 aid was not sufficient to offset other factors. Moreover, Hansen (1983) also
20 concedes that lack of support for Pell improving access may result from
21 the value of the Pell award being insufficient to overcome the liquidity
22 constraints facing lower-income students. At the same time, the impact
23 of Pell was likely blunted by changes in the program over the period that
24 permitted middle- and upper-income students greater access to federal
25 aid. Moreover, St. John (2003) also notes that the Pell grant replaced other
26 federal grant programs such that percentage of total federal aid distributed
27 as grants declined over the period from 55 to 47 percent. Thus, the decline
28 in low-income enrollment found by Hansen might simply reflect that the
29 Pell funds might have better been spent on other grant programs.

30 Manski and Wise (1983) exploit the dynamics of Pell generosity
31 generated by the authorization process. In particular, in the late 1970s
32 the Pell program expanded beyond its original intent to exclusively
33 service low-income students such that middle- and high-income students
34 received a growing proportion of awards by the end of the decade.
35 Expanding access to middle-income students at the expense of funding
36 low-income students could reduce overall college access if financial aid
37 influences the college-going behavior of low-income students and higher-
38 income students plan to attend college regardless of their eligibility for
39 the program. The paper develops a model of college-going behavior that
40 is used to forecast if and how a given student admitted to a given set of
41 colleges would react to changes in the cost of enrollment at those schools.

01 Specifically, data from the first wave of NLS72 are used to estimate
 02 a model of college-going behavior where a weighting procedure is used
 03 to account for the stratification of the NLS72 that over samples low-
 04 income and non-white students. This empirical model is then used to
 05 predict the distribution of postsecondary activity choice (i.e., four-year
 06 college, two-year college, vocational/technical school, or labor force) and
 07 the distribution of Pell awards in the period between 1977 and 1979
 08 after the generosity of the Pell program had changed. By comparing the
 09 predicted distributions with their actual distributions, the analysis is able
 10 to simulate the potential impact of the program changes. The results
 11 indicate that the number of awards and the percent of the budget given
 12 to low income students dropped from 86 to 49 percent and from 90 to 60
 13 percent, respectively. Moreover, by comparing the predicted and actual
 14 enrollment of Pell recipients, the results show that 41 percent of low-
 15 income enrollees are *induced enrollees* that would not have been predicted
 16 to attend in the absence of aid, dropping to 16 and 6 percent for middle-
 17 and high-income students, respectively.

18 Collectively, the evidence with regard to the efficacy of the Pell
 19 program in increasing college enrollment is relatively mixed. These
 20 findings, although perhaps surprising, are consistent with early demand
 21 studies such as Jackson and Weathersby (1975) that suggested that
 22 college students are generally insensitive to variation in the net price
 23 of college. For example, Leslie and Brinkman (1987) conduct a meta-
 24 analysis using elasticity estimates from twenty-five college demand
 25 studies. In their analysis, variation in net price depends on both the tuition
 26 price and the level of need-based or non-need-based aid. The results
 27 confirmed the theoretical expectation of a downward-sloping demand
 28 curve for college, but also indicate an inelastic price responsiveness in the
 29 range of 0.5 to 0.8 percent.⁴ Thus, much of the early empirical evidence
 30 suggested that pulling students over the threshold from non-enrollment
 31 to enrollment may not be an easy task, particularly for the low-income
 32 students serviced by the Pell program.

33 ⁴ Broadly, empirical studies that have estimated student responsiveness in higher education markets
 34 report elasticities of demand that are less than one despite substantial variation in both the degree of
 35 aggregation and the time period analyzed. In particular, such is the case using time-series variation
 36 in aggregate prices and enrollments for broad sets of universities (e.g., Campbell & Siegel, 1967) or
 37 single institutions (e.g., Seneca & Taussig, 1987), individual variation in net prices and decisions
 38 to enroll for a random cross-section of college-age persons (e.g., Tierney, 1982) and for applicants
 39 to a specific university (e.g., Ehrenberg & Sherman, 1984). In addition, more recent work also
 40 finds an inelastic price response for public and private universities (i.e., Dolye & Cicarelli, 1980;
 41 Parker & Summers, 1993), for in-state and out-of-state students at public universities (i.e., Curs
 & Singell, 2002), and across different racial and income groups (i.e., Blakemore & Low, 1983;
 Wetzel et al., 1998).

01 HETEROGENEITY IN THE ENROLLMENT IMPACT OF FEDERAL AID

02
03 More recent work has sought to examine whether the mixed evidence
04 regarding the impact of the Pell program on college access might be
05 attributed to either heterogeneity in the responsiveness to different type
06 of aid or for different subgroups of the population. Jackson (1990) uses
07 the HSB80 data to examine how different demographic groups respond
08 to financial aid. An important contribution of this paper is a distinction
09 between scholarships/grants and loans. Scholarships are found to have a
10 positive effect on college entry, while the presence of loans in a financial
11 aid package has little enrollment effect. Interestingly, the largest grant
12 effect is found for minority students, although this differential aid effect is
13 not significantly different between Hispanics and whites and disappears
14 completely when a control for the tendency to attend college is introduced
15 into the model. These findings suggest that financial issues may not be the
16 deciding factor in whether a Hispanic student chooses to go to college.⁵

17 Following Jackson's model of student choice, St. John and Noell
18 (1989) investigate the impacts of various financial aid packages on the
19 enrollment of high school seniors. The analysis extends the literature
20 by focusing on the type of financial aid package offered, not just the
21 availability of aid. While evidence is found that all types of aid (grants,
22 loans and work-study) have a positive impact on college attendance, the
23 analysis does not find significant differences between the various types
24 of aid. In addition, St. John (1990a) uses the sophomore class of the HSB
25 database and finds that an increase in each type of financial aid (i.e.,
26 grants, loans, work-study) alters behavior more than a similar reduction
27 in tuition. Interestingly, grants appear to have the largest impact on the
28 lowest income group, while loans only are effective in changing behavior

29 ⁵ Studies based on individual-level data at specific institutions have found similar evidence that
30 minority students and students from lower socio-economic status respond differently to financial
31 aid. For example, Ehrenberg and Sherman (1984) model how financial aid can be used to obtain
32 the optimal mix of students in a selective university (Cornell University) that faces a larger number
33 of applicants than it has capacity. They find that the enrollment yield from aid is significantly
34 lower for minority and low-income students. In addition, individual-level, institution specific
35 studies have also continued the disaggregation of the aid package into its separate components.
36 For example, exploiting variation at a large public university (University of Oregon), Singell and
37 Stone (2002) find that enrollment responses not only differ between increases in tuition versus
38 aid, but that merit-based aid has a larger impact than need-based aid. Moreover, less generous
39 forms of need-based aid (e.g., unsubsidized loans) have a larger enrollment impact than more
40 generous forms of need-based aid (e.g., grants or subsidized loans). In addition, non-need-based
41 and merit-based aid, while improving the access of all students, is found to increase the relative
opportunities of well-to-do students, even with merit held constant. Thus, individual level studies
of college choice suggest that needy students are less responsive to financial aid both at public
and private universities.

01 for the middle class. Although this result is consistent with low-income
02 students being relatively risk averse with regard to debt, it also suggests
03 the potential presence of unobserved heterogeneity that is jointly corre-
04 lated with need and the level and type of aid.

05 Thus, a number of studies have continued the tradition in
06 the higher education literature of employing the natural experiment
07 methodology that (at least potentially) can identify exogenous variation
08 in aid. For example, Kane (1994) uses CPS data from 1973 through
09 1988 which includes time series and cross-sectional variation in public
10 tuition levels, financial aid, family background, local economic condi-
11 tions, and the returns to education to investigate the differential trends
12 in college enrollment for 18–19 year old white and black students.
13 Specifically, the analysis makes use of CPS information on home
14 ownership, family income, number of siblings in college, and the
15 employment status of the spouse and of the head to simulate for each
16 sample member the expected Pell grant using the Pell grant rules in
17 each year. Thus, the impact of the Pell grant is identified by nonlin-
18 earities in the Pell grant formula as well as changes in the Pell grant
19 formula over time.

20 The results for the fully specified model that includes controls for
21 state and year effects indicates that the Pell grant had no significant impact
22 on college enrollment of black youth, but does significantly increase
23 enrollment of white youth by approximately 9 percent for each \$1000
24 of aid. However, an analysis comparing changes in enrollment rates for
25 eligible and ineligible students before and after the establishment of the
26 Pell program in 1973 yields little evidence that those targeted by the
27 Pell program (white or black) experienced relatively greater increases in
28 enrollment. Interestingly, the results for tuition are consistently negative
29 and significant with generally larger magnitudes than those found for
30 the Pell award. The differential response of students to equal offsetting
31 changes in tuition and financial aid is a common finding in the higher
32 education literature (e.g., Curs & Singell, 2002). Such differences might
33 arise because there are genuine differences in the value of a dollar of
34 tuition and a dollar of aid. For example, students may be more uncertain
35 about the actual amount of their Pell grant eligibility than they are about
36 the level of tuition (e.g., Orfield, 1992). Alternatively, in this instance,
37 measurement error in the simulated Pell grant variable might also bias
38 the impact of the estimated impact of the Pell award toward zero.

39 Kane (1995) extends the enrollment analysis conducted in his 1994
40 paper by exploiting the unique information available in three different
41 data sources: (a) the October CPS survey from 1977 to 1993; (b) NLS

01 data for 1979; and (c) the High School and Beyond (HSB) survey for the
02 senior class of 1980. However, similar to Kane (1994), an examination
03 of the growth in enrollment rates for those from families in the lowest
04 income quartile (generally eligible students) in comparison to those from
05 the top three quartiles (who are increasingly unlikely to be eligible) is
06 made using CPS data for the pre-versus post-Pell period (i.e., 1970–1972
07 versus 1973–1979). However, in this case, a distinction is made between
08 any college enrollment, private college enrollment, and public two-
09 year college enrollment. The results indicate that total (private) college
10 enrollment grew 2.6 (2.8) percent less for the lowest income quartile over
11 the period. On the other hand, college enrollment grew between 2.4 and
12 3.4 percent more quickly for the lowest income quartile at public two-
13 year colleges. This result provides the first evidence that the Pell program
14 may yield significantly different effects across institutions of different
15 selectivity. In other words, student concerns that the Pell program is not
16 sufficiently generous to provide access to more selective institutions and
17 institutional concerns that the Pell program differentially benefits certain
18 institutions could well be warranted.

19 The most dramatic change in enrollment behavior over the quarter
20 century has been the rise in the participation of students over the age
21 of 30 in undergraduate education, which rose from 15 to 30 percent of
22 the total of all undergraduates between 1970 and 2000. An important
23 question is the extent to which the availability of federal aid in general
24 and the Pell grants in particular accounts for the greater participation of
25 older students in higher education. Seftor and Turner (2002) explicitly
26 examine this issue by again making use of a structural change brought
27 about by; (1) the introduction of the Pell program and; (2) a 1986
28 reauthorization rule change that redefined independent student status
29 thereby restricting this group's access to Pell funds.⁶

30 Two separate analyses use CPS data for the period between 1969 and
31 1974 that span the period of the Pell program's introduction and between
32 1984 and 1990 that span the period of the 1986 reauthorization. The
33 results from the first analysis show that, unlike for the broader population
34 of college students, the introduction of the Pell program increased the
35 enrollment of male (female) independent students by 1.5 (1.3) percent.
36 The relatively greater responsiveness of older, independent students
37 versus their younger dependent counterparts suggests that older students
38 are somewhat less daunted by the complexity of applying for federal aid or
39

40 ⁶ The 1986 amendment to the Higher Education Act required, for the first time, a Pell applicant
41 to be at least 24 years old, married, or with children to qualify as an independent student.

01 that credit constraints are relatively more binding on older, independent
02 students. In addition, a second analysis suggests that narrowing the
03 definition of an independent student reduced the probability of college
04 enrollment by between 3.9% and 4.4% relative to single students with no
05 children. Thus, these results again suggest that the college-going behavior
06 of nontraditional students is responsive to the generosity of federal aid.
07 Nonetheless, the authors are careful not to suggest that providing greater
08 aid to this population is worthwhile from a policy perspective. Specifi-
09 cally, to identify the merits of public subsidies for older students requires
10 a credible estimate of the impact of additional schooling on their earnings
11 that is not presently available and that is likely to differ from the return
12 to education of younger students who make a direct transition from high
13 school to college.⁷

14 One concern regarding the effectiveness of need-based programs,
15 such as the Pell grant, to influence college access has been the growing
16 use of merit-based programs by states and institutions to attract and
17 retain the best students. For example, McPherson and Schapiro (1998)
18 document the declining portion of subsidized need-based aid in the
19 total financial aid package. One prime example of these merit-programs
20 is the Georgia HOPE scholarship that, starting in 1993, provided a
21 full tuition subsidy to attend any public university in the state to any
22 Georgia resident who graduate high school with a B average or better
23 along with a generous subsidy for any private university in the state.
24 Using data on annual Pell enrollments by institution that span the 1993-
25 introduction of Georgia's HOPE Scholarship, Singell, Waddell and Curs
26 (2006) documents differential responses to the generous merit schol-
27 arship based on need. Contrary to that implied by other work, they find
28 that the number of Pell recipients increased at Georgia institutions after
29 HOPE, when compared to other southern universities, which they argue
30 is consistent with broad merit-based scholarship programs improving
31 college access for needy students. However, as they also document that
32 the average Pell award in Georgia falls after HOPE's introduction, they
33 suggest that HOPE drew students of lesser need into the Pell program.
34 Total Pell revenues increased in Georgia relative to other southern insti-
35 tutions after HOPE, which also implies that broad merit-aid programs are
36

37 ⁷ Other work has examined the impact of other benefit restrictions brought about by acts of
38 Congress. For example, Tewksbury, Erickson, and Taylor (2001) examine the enrollment impact
39 of The Violent Crime Control and Law Enforcement Act of 1994 that precluded all prisoners in
40 federal or state penal institutions from receiving Pell Grants. This Act furthered the restrictions
41 instituted by the 1992 reauthorization that limited awards to incarcerated persons not under a
death sentence and not serving a life sentence without the possibility of parole.

01 effective at leveraging scholarships with greater Federal funding paid to
02 needy students who may have not otherwise attended college.

03 Collectively, the results of these more recent studies suggest that
04 the findings in prior work that the Pell program does not yield signif-
05 icant broad based college enrollment effects may not necessarily apply
06 to particular groups of students or to specific types of institutions. For
07 example, the college access of independent students appeared to be
08 harmed by the reduced generosity of the Pell program toward this class
09 of students in the 1992 reauthorization (Seftor & Turner, 2002), while
10 the Pell grant appeared to increase enrollment at two-year schools but
11 not at four-year institutions (Kane, 1995). Thus, these studies highlight
12 the importance of understanding how exogenous changes in federal aid
13 programs intentionally or unintentionally target particular actors in the
14 higher education market.

15

16

17 THE EFFICACY OF PELL AID ON PERSISTENCE

18

19 Beyond considerations of access to college, there is a reasonable expecta-
20 tion that financial aid should improve students' ability to remain in
21 college through to graduation. However, the effects of financial aid on
22 enrollment may well differ from its effect on graduation, especially since
23 we know only 60 percent of enrollees at four-year public universities
24 graduate (Singell, 2004). In general there are relatively few studies of the
25 effect on financial aid on retention and graduation simply because there
26 are less data detailing persistence in college than on enrollment (Hu &
27 St. John, 2001). Moreover, graduating students are a self-selected sample
28 of enrollees who choose to first enroll and then complete a degree at a
29 particular school versus a number of often unobserved alternatives such
30 as enrolling at a competing school, transferring schools, or completing a
31 degree at a later time, which present empirical issues regarding sample
32 selection and the correlation of aid with unobserved factors the relate to
33 graduation. For example, a first generation college student or one with
34 unobserved family or health issues are more likely to fail to complete
35 federal aid applications (i.e., a FAFSA form) and graduate. Such poten-
36 tially unobserved student attributes can yield a negative association
37 between the Pell grant and graduation when, in fact, the lack of need-based
38 aid is not the root cause of the drop-out decision but is simply inversely
39 correlated with the unobserved attribute or negative shock that ultimately
40 leads to the student not persisting in college. Thus, it is not surprising that
41 studies that do not account for the self-selection of Pell students find little

01 or no effect of the Pell grant on the persistence of recipients in comparison
02 to non-recipients (Wei & Horn, 2002).

03 Institution level studies of persistence have tried to minimize the
04 correlation of aid with *unobservables* by including relatively detailed
05 lists of controls for personal attributes and for different types of aid
06 that comprise the financial aid package (i.e., loans, scholarships, grants,
07 work-study, etc.), which are generally not available in national-level
08 data sources (e.g., Wetzel, O'Toole, & Peterson, 1999). For example,
09 Metz (2001) uses detailed student-level data from a two-year technical
10 college and exploits a change in the 1992 HEA which required two-year
11 colleges to report degree completion rates to qualify for federal aid in
12 order to examine the impact of various components of financial aid on
13 degree completion. The results indicate that Pell grants do not signifi-
14 cantly influence degree completion, while loans and work-study improve
15 degree completion. A relative small retention effect of Pell grants is not
16 uncommon in institution-level studies (e.g., Singell & Stone, 2002).

17 The finding that Pell awards have relatively small (insignificant)
18 retention effect may reflect that federal grants are perceived by students
19 as entitlements, but may also reflect the difficulty in finding suffi-
20 ciently detailed data to control for the fact that grants are systemati-
21 cally provided to the neediest students whose unmeasured attributes are
22 correlated with persistence. For example, DesJardins, Ahlburg & McCall
23 (2000a & 2002b) use detailed data on students enrolled at the University
24 of Minnesota to estimate a hazard model, which is used to simulate how
25 changes in financial-aid packaging affect students' departure decisions.
26 Collectively, the simulations indicate that scholarships significantly
27 reduce stop outs, whereas grants yield insignificant effects. However,
28 income data are missing and excluded from the hazard model specifi-
29 cation. Thus, if low-income students are more likely to receive a grant
30 and to stop-out for a term to work, the impact of grants on persistence
31 may well be biased downwards if these students eventually return and
32 complete a degree.

33 In general, there is a fair amount of heterogeneity in the empirical
34 evidence regarding the persistence effects of Pell grants. Specifically,
35 some work finds that grants do improve persistence (e.g., Thomas, 1981;
36 St. John, 1990b), but other analyses find insignificant (e.g., Braunstein,
37 McGrath & Pescatrice, 2001) or even negative effects (e.g., St. John &
38 Starkey, 1995). Moreover, there is no clear consensus with regard to the
39 types of aid that most effectively induce higher persistence, with some
40 articles pointing to on-campus employment (e.g., DesJardins, Ahlburg &
41 McCall, 1999), others to merit aid (e.g., Singell, 2004) and still others

01 to grants (e.g., Carroll, 1987; St. John, 1990b). This lack of consistent
02 evidence regarding the graduation effect of aid in general and Pell grants
03 in particular may well arise from the particular importance of endogeneity
04 in regards to the receipt of Pell awards and persistence.

05 Thus, similar to empirical modeling issues related to enrollment,
06 pinpointing exogenous sources of aid variation is also important in
07 the study of persistence. A handful of studies have sought exogenous
08 sources of variation in support to identify the effect of aid on post-
09 enrollment outcomes. For example, Singell (2004) uses data on appli-
10 cants and enrollees to the University of Oregon to estimate a bivariate
11 probit specification that models the retention decision jointly with the
12 decision to enroll. The results of a univariate probit model for graduation
13 indicate that grants do not significantly increase the retention probability.
14 However, a bivariate-probit specification that is conditioned on both the
15 observed attributes that relate to graduation and unobserved attributes
16 that determine enrollment (i.e., the error structure for enrollment)
17 indicate that a \$1000 increase in grants raises the probability of remaining
18 in school by 1.3 percent. These findings suggest that the unobserved
19 attributes of needy students that determine the enrollment decision
20 are inversely related to their retention probability such that there is a
21 downward bias on the retention effect of grant aid.

22 Likewise, similar to studies of the impact of financial aid on access,
23 some graduation studies have exploited changes in the aid assignment
24 rules in the Pell program that yield different levels of support to similar
25 students in order to identify variation in aid that is uncorrelated with the
26 underlying propensity to graduate. The best example of this approach
27 is Bettinger (2004), which is the only paper to directly study the affect
28 of Pell grants on retention (as opposed to grant aid in general). The
29 empirical analysis uses unique Ohio Board of Regents (OBR) data that
30 permit transfer behavior of students to be tracked (at least within the
31 state of Ohio). The OBR data provide information for all public univer-
32 sities in the state of Ohio for 1999 and 2000 and include detailed student
33 demographics information along with financial information that track
34 whether a student stops out from college as opposed to transferring to
35 another school within the state. To isolate the exogenous variation of Pell
36 grants independent from a students' stop-out behavior, the Pell grant is
37 imputed for each student in the 2000–2001 school year holding constant
38 family characteristics. The imputed Pell grants vary due solely to changes
39 in the Pell program and tuition and also provide Pell award values for
40 non-filers.
41

01 To examine the impact of the Pell grant on retention, the empirical
02 analysis makes use of the discontinuity in the value of the Pell award,
03 which arise from Pell rules regarding family size. Specifically, by
04 assuming that the differences in family size are unrelated to a student's
05 success in college, the analysis makes comparisons between different-
06 sized families who have the same number of children in college. The
07 stop-out behavior of these similar groups are compared using the Wald
08 estimator developed by Angrist (1991), which is simply the regression
09 of the instrumental variables estimate of stop-out behavior on size of
10 the Pell grant. The results show that a \$1000 increase in Pell grants
11 stemming from differences in family size corresponds to a 3 to 4 percent
12 decrease in the probability of dropping out. However, this retention
13 effect declines in magnitude (i.e., to approximately 1.2 percent) and
14 becomes insignificant if the sample is restricted to those students
15 for which the ACT exam is available or if additional campus level
16 controls are included. The decline in the magnitude of the retention
17 effect when individual or campus-wide ability differences are included
18 highlights the potential importance of student self-selection, which
19 appears to be correlated with the effect of need-based grants on college
20 outcomes.

21 Overall, the broad findings with regard to the persistence effects
22 of Pell grants are decidedly mixed. A generous assessment of the
23 efficacy of the Pell grant on retention would suggest that it improves
24 retention by a relatively small amount – on the order of a 1
25 percent increase in the probably of graduation per \$1000 of aid.
26 These findings combined with the findings of a small and generally
27 insignificant impact of the Pell grant on access imply that the
28 cumulative impact of the Pell grant on college outcomes is at best
29 modest.⁸

30
31
32 ⁸ Some evidence has been found that grant aid can improve persistence. For example, Dynarski
33 (2002), in addition to issues of access discussed above, exploits the natural experiment brought
34 about by the elimination of the Social Security Benefits Program to study how the reduction
35 in grant aid affects college completion. Using death of a parent to proxy for qualifying for
36 Social Security Beneficiary status in the CPS data, she finds a \$1000 increase in the offering
37 of grant aid raises educational attainment by 0.16 years, suggesting that grants improve
38 retention and the likelihood of clearing the graduation threshold. Consistent with the insti-
39 tutional level studies, the retention effect of grant aid appears to be relatively small. On the
40 other hand, the analysis can only identify whether the individual potentially qualifies for aid
41 and not whether the person actually receives aid. It follows that the aid results are likely
to be attenuated due to measurement error, which would bias the coefficient on grant aid
toward zero.

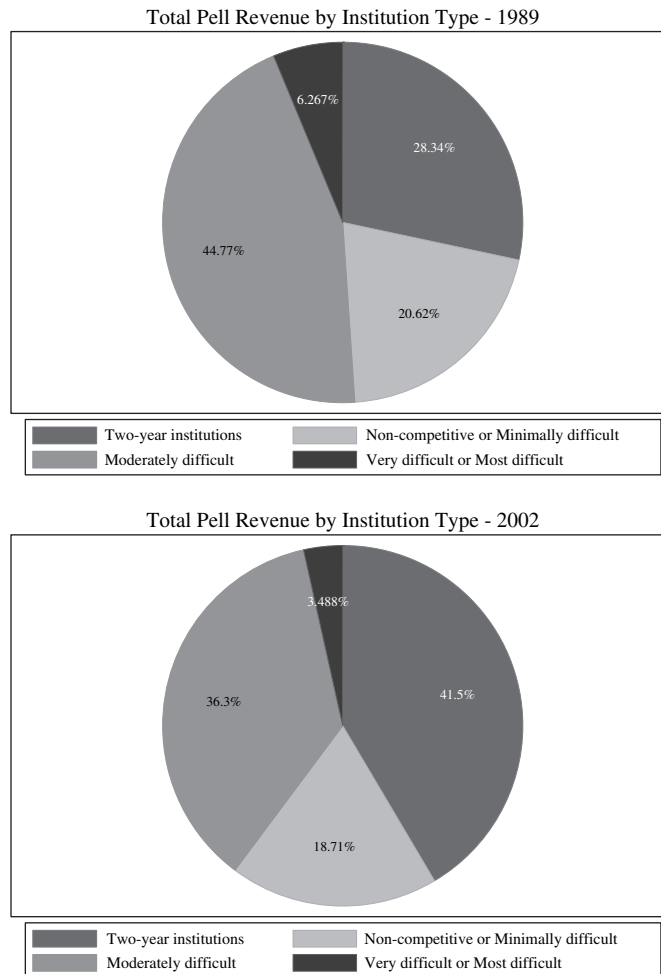
01 In many cases, this ability to control the initial event to which
02 theory predicts a response becomes an important element in achieving a
03 legitimate test of the theory. Further, without the knowledge (or, more
04 weakly, the assumption) that the event was not due to the behavior of
05 agents, associating any observed change in behavior to the event in a
06 causal way would be in error.

07 For example, consider the economist's standard model of demand
08 and supply, where quantities demanded by consumers are believed to
09 fall with price while quantities supplied by firms are believed to rise.
10 The quantity demanded at a given price is also thought to respond to
11 economic factors, such as it would to a new advertising campaign, for
12 example. Likewise, the quantity supplied to a market at a given price also
13 responds to economic factors. Even in such a simple model, measuring the
14 responsiveness of consumers to a change in price becomes a non-trivial
15 task, as the researcher must isolate the effect of changes in price on sales,
16 holding constant other factors that can influence the level of demand. As
17 a price change can occur precisely because of a change in one of the factors
18 affecting the level of demand (e.g., advertising), it becomes difficult to
19 assure that observed price changes are occurring independently.

20 What is a natural experiment? Arguably, the fundamental
21 advantage offered through the advent of experimental methods in
22 economics is the ability to better isolate a change in prices and therefore
23 incentives. However, while the laboratory will often provide a cleaner
24 environment, such isolation need not be absent from the world beyond
25 the lab. Natural experiments, then, can offer the same ability for the
26 researcher to test theory or measure behavioral responses to events.
27 In a natural experiment, with the event which one is interested in
28 measuring, a response occurs as a natural product of the economic
29 system more generally. Outside of the particular period of analysis,
30 the economic system driving the laboratory environment is specifically
31 designed by the researcher. Therefore, while the laboratory researcher
32 knows that the event of interest is exogenous to the economic agents
33 under analysis (because he instigated it himself as part of the experi-
34 mental design), the exploitation of naturally occurring events (such as
35 the introduction of the Pell Grant) must be assumed by the researcher
36 to be exogenous to the economic agents under analysis. Often times,
37 evidence will suggest this as the case, making such an assumption
38 quite reasonable. Given our particular interest in the efficacy of the Pell
39 program, researchers must assume that the particular change in policy
40 to which the response of economic agents is to be measured arose in a
41 way that is exogenous to the economics agents themselves.

Figure 6.7: Distribution of Pell Revenue by Institution Type, 1989 and 2002.

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Source: Curs, Singell and Waddell, 2006.

(e.g., the half-cost rule) might well benefit some institutions at the expense of others. Indeed, Singell (2002) shows that composition of the financial aid package depends on student attributes, institutional factors, as well as external pressures. Likewise, Turner (1998) contends that institutions with large financial aid budgets before the introduction of the Pell grant program had the capacity to undo the targeting of the federal grants. It follows that, the change in the net cost to low-income students who qualify for a Pell award might well be much smaller at well-funded universities as opposed to community colleges.

01 Curs, Singell and Waddell (2006) explicitly examine whether
02 changes in the generosity of the Pell program affect the distribution of
03 Pell revenues across the quality spectrum of universities. In particular,
04 they analyze how generosity correlates with institutional Pell revenues
05 by utilizing exogenous variation in the federally-determined maximum
06 Pell Grant and federal appropriation levels, as well as the annual
07 variation in the total number of students who are deemed Pell eligible
08 following the application of federally-determined criteria. Broadly, Pell
09 revenues depend on the pool of students applying for aid and their
10 institutional choices. Thus, they also explore how the institutional
11 Pell-revenue response relates to changes in the average Pell award per
12 student and enrollment at each institution, which provides some of the
13 first formal evidence of the efficacy of the Pell program in influencing
14 the composition and net distribution of needy students across U.S.
15 universities.

16 Overall, they find significant increases in institutional Pell
17 revenues with increased generosity. Nonetheless, the magnitude (and
18 even the direction) of the revenue response depends on the channel
19 (i.e., the maximum Pell value versus federal Pell funds) and the selec-
20 tivity of the institution. In particular, they report that revenue data
21 show that the fraction of Pell revenues going to two-year institutions
22 rose from just over a quarter of the total disbursements in 1989 to over
23 40 percent in 2002, suggesting that Pell has expanded access at less
24 selective institutions.

25 Curs, Singell, and Waddell (2006) also investigates the aggregate
26 enrollment patterns around the 1992 Higher Education Amendments
27 (HEA) that removed tuition-based caps on maximum Pell awards.
28 Measured against a group of slightly higher-cost but otherwise similar
29 institutions, they report a significant increase in the enrollment of
30 low-income students at low-cost institutions that experienced this
31 exogenous increase in Pell generosity. In short, results are suggestive
32 that student enrollment does respond to aid. Further, although prior
33 evidence suggests that Pell grants do not move students over the
34 threshold from non-enrollment to enrollment, they find that low-
35 income students appear sensitive to the level of aid conditioned on the
36 decision to enroll.

37 38 THE BENNETT HYPOTHESIS

39
40 Rather than argue for greater emphasis on need-based aid, some critics
41 have argued instead that federally subsidized aid may be part of the

01 problem, and have even proposed federal caps on tuition increases.
02 Several former Secretaries of Education, beginning prominently with
03 William Bennett, have expressed concern that increases in federal
04 support, rather than lowering college expenses for students, are instead
05 appropriated by universities through increases in tuition (Bennett,
06 1987). This view has come to be known as the Bennett hypothesis.

07 Even without turning to idiosyncratic organizational models of
08 university behavior (as in Hoenack & Pierro, 1990; or Netz, 1999),
09 one can offer three basic interpretations or explanations for the Bennett
10 hypothesis. The simplest is provided by the standard competitive
11 model. In this case, increases in student demand for enrollment arising
12 from increases in financial aid are met with a relatively inelastic
13 supply response from universities, so that increases in aid are trans-
14 lated into proportionately large increases in tuition. In the extreme
15 case of perfectly inelastic supply, tuition increases by the full amount
16 of the increased aid. This result would be counter to the intent of
17 the Pell program, which seeks solely to increase enrollment through
18 improved access to college. Pell grants could yield a pure enrollment
19 effect in the case of perfectly elastic supply, in which case enrollments
20 would increase but not tuition. With public universities, tuition may
21 be regulated directly or indirectly by the state, possibly limiting tuition
22 responses to enrollment pressures, at least for in-state students.

23 A second explanation relies on imperfect competition, possibly
24 enabling universities to appropriate an even higher proportion of aid
25 via tuition increases. In fact, universities are highly differentiated:
26 public and private, exclusive and nonexclusive, liberal arts and compre-
27 hensive, large and small, close and far, and so on. In this case, the
28 demand for enrollment at many universities is likely to be downward
29 sloping, providing an opportunity for universities to exert market
30 power in setting tuition and exaggerating increases in tuition beyond
31 competitive levels. As in the competitive case, though, tuition increases
32 at most by the full amount of the increased Pell aid. It is this expla-
33 nation that appears to most closely match the rhetorical arguments of
34 former Secretary Bennett and other critics. Indeed, there is evidence
35 not only that presidents and provosts of public colleges and univer-
36 sities have a significant effect on enrollment supply (e.g., Coates &
37 Humphreys, 2002), but also that the total compensation of presidents
38 of private colleges and universities is related to the level of tuition,
39 even with expenditures, type of institution, reputation rankings, and
40 other factors held constant (Tang, Tang, & Tang, 2000).

01 A third explanation also relies on imperfect competition, but with
 02 price-discriminating behavior by universities. In this case, the Bennett
 03 hypothesis might hold if an increase in aid to needy students with
 04 relatively elastic demand induces an even greater increase in tuition for
 05 other students with relatively less elastic demand. With price discrim-
 06 ination, the price charged to each type of student is set, via discounts
 07 or internal scholarships, to equate marginal revenue in each case to
 08 the common marginal cost (where there are no cost differences).¹⁰
 09 In this case, the price increase for students with less elastic demand
 10 is *not* limited to the increased aid amount to needy students. With
 11 sufficiently steep marginal cost curves, relatively elastic demands by
 12 aid recipients, and relatively inelastic demands by other students, the
 13 increase in price for the market with relatively less elastic demand can
 14 *exceed* the increased aid amount.¹¹

15

16 EMPIRICAL EVIDENCE OF THE BENNETT HYPOTHESIS

17

18 Early empirical examinations of the Bennett hypothesis are suggestive.
 19 McPherson and Shapiro (1991), Turner (1997), and Li (1999) find
 20 evidence that tuition rises for at least some segments of the higher
 21 education market with the generosity of federal aid, but the segments
 22 where effects are significant and the magnitude of the effects vary substan-
 23 tially across the three studies. Such inconsistencies may arise from
 24 unobserved heterogeneity among universities, which is addressed in the
 25 subsequent literature by introducing institution-specific fixed effects.

26 Singell and Stone (in press) estimate a tuition regression with
 27 the average Pell award per recipient as the key explanatory variable,
 28 which the Bennett hypothesis suggests should be positively related to
 29 tuition. Theory suggests that the coefficient is expected to be between
 30 zero and one, reflecting the extent to which federal aid support is
 31 passed on to the student in terms of higher tuition. However, the cost
 32 characteristics of the Pell program and the selection decisions of needy
 33

34 ¹⁰ Netz (1999) finds evidence of this kind of price discriminating behavior for need-based aid
 35 and tuition for the schools that coordinate criteria for awarding need-based aid in the Ivy Overlap
 36 Group. Internally provided need-based aid substantially increases tuition for non-needy students,
 as well as for students who receive financial aid.

37 ¹¹ Hill and Winston (2006), for example, using data for Williams College, find remarkably similar
 38 shares of income paid for a year of college for aided students across the five income quintiles.
 39 Specifically, the shares of pretax family incomes range from 6% to 20% – the lowest income
 40 quintile paying the smallest share and those at the 95th and 99th percentiles, paying full price,
 41 spending 22% and 9% of their family incomes, respectively. Thus, there is some evidence that
 the best private schools do price discriminate, typically via need-blind admissions policies.

01 students could bias the coefficient on average Pell awards. Specifically,
02 because the Pell grant formula uses cost of attendance to calculate a
03 student's award, the tuition of a school may be positively correlated
04 with the level of the Pell grant, which would yield an upward bias on
05 the coefficient for the Pell grants. The potential bias is limited, though,
06 because the formula only depends in part on costs, of which tuition
07 is only a part, and the allowable tuition has been subject to various
08 maximums in the formula, all typically well below the relevant student
09 costs. Alternatively, Pell grant recipients may be less likely to enroll in
10 universities where tuition is rising more rapidly than average because
11 they are relatively needy students, which would yield a negative bias
12 to the coefficient for Pell grants.

13 A concern with potential endogeneity requires the use of instru-
14 mental variables that include both a set of binary variables that identify
15 changes in the Pell program parameters and the lagged value of Pell grants
16 to instrument for the current value of the average Pell.¹² Fixed-effect
17 tuition regressions are estimated for in-state and out-of-state students at
18 public universities and for students attending private universities using a
19 panel of 1554 colleges and universities from 1988 to 1996 drawn primarily
20 from the IPED data source. Hausman tests, in fact, indicate rejection of
21 the null hypothesis of exogeneity for the average Pell grant at no less than
22 the five percent level in each of the specifications.

23 The fixed-effect instrumental variable specifications indicate little
24 evidence of the Bennett hypothesis for in-state tuition at public univer-
25 sities, but indicate nearly a one-to-one relationship between Pell
26 awards and out-of-state tuition and tuition charged at private univer-
27 sities. Thus, while in-state students appear to be insulated from price
28 responses to federal aid (perhaps because of the explicit mission of
29 public universities to serve in-state students or because of agents
30 such as Regents or legislators that represent them), public universities
31 behave similarly to their private counterparts with regard to tuition
32 charged to out-of-state students. These results suggests that intra-state
33 political factors are particularly strong, especially since prior evidence
34 on demand elasticities indicate that demand, if anything, is less elastic
35 for in-state versus out-of-state students (Curs & Singell, 2002). Collec-
36 tively, the results in Singell and Stone (in press) suggest that the pricing

37
38 ¹² The binary variables for changes in parameters of the Pell program reflect exogenous government
39 changes in the program in particular years: the percent cost rule, which mandated the maximum
40 percentage of tuition costs that could be covered by Pell grants, was raised from 60 to 100 percent
41 in 1993; and budget shortfalls led the Office of Postsecondary Education to decrease the grants of
all but the neediest students by a linear formula in 1990.

01 private colleges differs distinctly across income. For example, high-
 02 income students pay \$2.75 more in tuition for every dollar increase
 03 in grant aid, middle-income students pay an additional \$1.51, and
 04 low-income students have net tuition lowered by \$4.09. Thus, the
 05 analysis, while finding support for the Bennett hypothesis at private
 06 universities, suggests that tuition increases in response to federal aid
 07 are used to price discriminate in favor of providing access to needy
 08 students.¹³

09 Overall, then, there is evidence both for and against the Bennett
 10 hypothesis. Specifically, the evidence for in-state tuition charged by
 11 public universities tends to reject any substantial or significant effect;
 12 alternatively, the evidence for out-of-state public and private tuition
 13 tends to support the Bennett hypothesis. Collectively, the results
 14 suggest that the pricing behavior of higher education institutions is
 15 sensitive to both political and market interests, as well as, perhaps,
 16 to individual institutional objectives with regard to access for needy
 17 students. Given recent evidence suggesting that some private univer-
 18 sities compete and manage enrollments with financial aid (McMillen,
 19 Singell & Waddell, 2006), it is paramount to understand how tuition
 20 responds to the provision of Pell aid in evaluating whether improved
 21 generosity of the Pell program will affect access of needy students to a
 22 college education.

23
 24 THE EFFICACY OF THE GI BILL AND OTHER GRANT AID
 25 ON ENROLLMENT
 26

27 The overarching conclusion of the Pell research is that the demand-side
 28 effects of federal aid are relatively small (if not zero). Even worse, the
 29 dramatic rise in tuition may be, in part, due to the presence of federal
 30

31
 32 ¹³ Supply-side responses to the provision of aid have also been found for the provision of
 33 state level aid. For example, Long (2004) studies a time-series of Georgia-institutions spanning
 34 the introduction of Hope using a difference-in-difference approach to identify the exogenous
 35 introduction of scholarship aid. She finds that public institutions, while not responding
 36 directly in terms of tuition increases that are controlled centrally by the state, did increase
 37 room and board fees by 5% on average. On the other hand, private universities in Georgia
 38 (with a significant number of HOPE recipients) reduced institutional financial aid by approx-
 39 imately 19%. Overall, the findings suggest that while public institutions recouped nearly
 40 10% of the value of the scholarship by increasing room and board fees, private institu-
 41 tions recouped nearly 30% of the value of the scholarship by increasing tuition and reducing
 institutional financial aid. Thus, the institutions most affected by the HOPE scholarship
 responded strategically so as to extract rents created by the program consistent with the Bennett
 hypothesis.

01 aid that in the words of William Bennett (1987) have allowed institu-
02 tions to “blithely raise tuition” (p. A31). Nonetheless, there is still room
03 for optimism that federal grant programs can improve college outcomes.
04 Specifically, while there is little evidence of broad-based effects of the Pell
05 grant on enrollment, there are a number of natural experiment studies
06 that imply that the precursor and inspiration for the Pell program, the GI
07 Bill (and other federal grant programs), did affect the college outcomes
08 of needy students.

09 The earliest such study by Angrist (1993) examines the extent to
10 which the presence of veteran’s benefits affected the level of education and
11 subsequent earning of veterans. The analysis uses the Survey of Veterans
12 data for discharged military personnel from the Vietnam era and the early
13 periods of the All-Voluntary Forces (AVF). Most Vietnam veterans were
14 eligible for the GI Bill, but a majority of those entering under the period
15 of AVF were eligible for the Veterans Educational Assistance Program
16 (VEAP). The VEAP is a contribution based program where contributions
17 were matched by the government at a rate of 2 to 1, which induced a
18 significant fraction of Vietnam veterans not to use the VA program. The
19 analysis restricts the sample to men who are 30–54 years old and who have
20 1–15 years of service, which permits these service men to reenter into the
21 civilian work force after discharge. An OLS regression of education on a
22 vector of control variables including individual specific dummy variables
23 indicates that the availability of benefits increase schooling by 1.6 years.
24 If the individual fixed effect is correlated with the use of the program
25 (students with more education pre-entry were more likely to obtain
26 education post military), a separate aid effect cannot be identified. Thus,
27 a first-difference approach is used that distinguishes between pre- and
28 post-recruitment returns that are found to be 9.6% versus 4.3%, respec-
29 tively. Moreover, although specification tests indicate that initial levels
30 of schooling are likely to be correlated with the error terms, an instru-
31 mental variables regression using period of service interacted with the
32 entry-level schooling yields similar findings. Thus, grant aid associated
33 with various veterans’ programs appear to have increased both education
34 and earnings.¹⁴

35
36 ¹⁴ Other studies have found the GI Bill increased earnings. For example, Card and Lemieux (2001)
37 use 1971 Canadian Census data and 1973 Canadian Job Mobility Survey data to identify the
38 effects of the Veteran’s Rehabilitation Act (VRA) upon the educational attainment of Canadian
39 men. Specifically, the analysis uses a sample of approximately 21,000 English speaking men from
40 Ontario and French speaking men from Quebec. The analysis exploits the fact that, due to a failure
41 to participate in WWII, most French speaking men from Quebec were not eligible for the VRA
and, thus, form a valid control group. An instrumental variable approach uses an Ontario specific

01 Bound and Turner (2002) examine whether the combined effect
02 of military service and the availability of subsidies through the GI
03 Bill increased educational attainment of World War II veterans. This
04 analysis again highlights the potential problem that treatment effects
05 are often not randomly assigned. In this case, Census data are used to
06 show that, because physical and mental fitness were prerequisites for
07 military service, comparisons of the educational attainment of veterans
08 and non-veterans from the same birth cohort are likely to overstate
09 the causal effect of military service and the availability of postwar
10 benefits. Nonetheless, the analysis exploits differences between birth
11 cohorts in the likelihood of military conscription generated by changing
12 manpower requirements in the armed forces during the World War
13 II to identify the separate effects of conscription and GI benefits.
14 Specifically, by aggregating data within birth cohorts and using the
15 between-cohort variation in veteran status, the analysis identifies the
16 independent effects of the availability of GI grant aid on the colle-
17 giate attainment net of the participation in WWII. The within cohort
18 comparisons of educational attainment between veterans and non-
19 veterans show that those who served in World War II received about
20 0.4–0.5 years more collegiate training and were eight percent more
21 likely to graduate than those who did not serve. However, conditional
22 on high school graduation and the fraction of veterans who have a
23 high-school diploma, the difference between veterans and non-veterans
24 in terms of average number of years of college completed (gradu-
25 ation rate) declines to 0.2 (4 percent). Nonetheless, overall, the results
26 again suggest that veteran-specific grants improve college access and
27 completion.

28 Stanley (2003) extends the work of Bound and Turner (2002)
29 on grant-aid effects by exploiting a unique natural experiment arising
30 from differences in the Korean War GI Bill versus the WWII GI Bill.
31 Specifically, Korean War era veterans were eligible for an education
32 subsidy through the GI bill provided they entered the military on or
33 before January 31, 1955, but not after. Thus, the empirical analysis
34 compares the educational outcomes of a sample of veterans who
35 entered the military within a year prior to the cutoff date to those
36 from a sample of veterans who entered within a year after the
37 cutoff date using a difference-in-difference approach. Exploiting data
38

39 dummy variable to measure the potential eligibility for VRA benefits as an exogenous determinant
40 of schooling, which yields a return to education for men from Ontario at 15% using an instrumental
41 variables approach.

01 from the 1973 Survey of Occupational Change in a Generation, the
02 difference-in-difference analysis indicates nearly a 20 percent increase
03 in educational attainment for eligible Korean War veterans or an
04 elasticity of educational attainment of about 0.4 (based on estimated
05 subsidy of approximately 50 percent). Moreover, the estimated effect
06 is larger for younger veterans and those with higher socioeconomic
07 status scores. Overall, while the empirical evidence regarding the
08 efficacy of the Pell grant on access is fairly modest, the results
09 regarding the GI Bill indicate significant and large impacts on college
10 attainment. It follows that understanding the differences between
11 the GI Bill and the Pell program (e.g., entitlement versus not, size
12 of subsidy, group targeted by subsidy) may be critical to identi-
13 fying the apparent differences in their impact on observed college
14 outcomes.

15 Other grant aid programs have also been found to improve college
16 outcomes. For example, Dynarski (2002) exploits a natural exper-
17 iment arising from the elimination of the Social Security Benefit
18 (SSB) program in 1982, which had provided an average of \$6,700
19 to college-age students who had experienced the death of a parent.
20 The analysis uses three years of data surrounding the elimination of
21 the SSB drawn from the NLSY cross-sectional and poverty samples to
22 estimate a difference-in-difference analysis that compares the educa-
23 tional outcomes of eligible versus non-eligible high-school seniors,
24 before the elimination of SSB versus after. A dummy variable for a
25 deceased father is used to determine eligibility, which accounted for
26 90 percent of the eligible beneficiaries. The difference-in-difference
27 coefficients indicate that about 22 percent more students enter college
28 under SSB by age 28, with \$1000 in grant aid estimated to increase
29 the probability of attending college by 3.6 percent. Although the SSB
30 program was not directly comparable to the Pell program because the
31 benefits rose with earnings of the deceased parent, the finding of a
32 significant impact of grant aid on college access even for a student
33 who has lost a parent suggests that a sufficiently generous grant can
34 improve college outcomes. Moreover, the finding of a significant impact
35 of the SSB suggests that the elimination of other federal grant programs
36 (including the SSB) and modifications to state grant programs that
37 occurred concurrently with studied changes in the Pell program should
38 have been considered in evaluating the efficacy of the Pell program
39 (Kane, 1995).

40 Two papers by Abraham and Clark (2003) and Kane (2004)
41 use natural experiment methodology to analyze the District of

01 Columbia's Tuition Assistance Grant Program that was instituted in
02 1999 and allows DC residents to attend public colleges and univer-
03 sities throughout the country at rates considerably lower than out-of-
04 state tuition. Both studies use samples of unaffected college students
05 (e.g., students in nearby cities) as a control group, and find that the
06 number of freshman attending (particularly four-year) colleges outside
07 of DC increased substantially. Interestingly, however, the impact on
08 total enrollment of DC residents is actually quite modest, suggesting
09 that the subsidy had a greater impact on where students went to
10 college as compared to whether they choose to attend college at
11 all. Thus, these studies again suggest that it is easier to influence
12 college choice than it is to influence the choice of attending college
13 or not.

14 Overall, studies of the GI Bill and other federal grant programs
15 consistently indicate that the college-going behavior of veterans and
16 other targeted groups of students are positively influenced by the
17 generosity of federal grant aid. This evidence combined with the
18 findings that the Pell program can affect the college going behavior
19 of (at least) particular types of students highlights the importance of
20 understanding the nuances in various federal aid programs and how
21 they target federal aid. Thus, the final question to be examined is
22 whether there is a consistent pattern to where federal grants have been
23 found to improve college outcomes, which then can speak to how the
24 Pell program might be altered to improve its effect?

25 26 POLICY CONCLUSIONS: WHAT DO WE KNOW? 27

28 The Pell program has provided fertile ground for testing whether the
29 introduction of a higher-education voucher and marginal adjustments
30 to its generosity (i.e., through the reauthorization process) affects the
31 college outcomes of low-income students. Federal adjustments to the
32 Pell program provide a useful foil for testing the efficacy of need-based
33 aid because it yields variation in the access and level of financial aid that
34 can be legitimately assumed to be exogenous to unobserved student
35 attributes that also relate to the level of aid awards (e.g., student health
36 status or parent's educational background). The econometric advan-
37 tages of the Pell program combined with its size, breadth of student
38 coverage, and longevity have led it to be the focus of considerable
39 academic interest. Thus, the Pell program is the source of some of the
40 best and most thoroughly researched analysis of financial aid in the
41 higher-education literature.

01 It is, therefore, regrettable that the preponderance of evidence
02 suggests that even the relatively large increase in the availability (and
03 generosity) of need-based aid brought about by the Pell program and its
04 reauthorizations appear to have had less-than-a-broad-based influence
05 on the college going behavior of low-income students. In other words,
06 research suggests that enticing an otherwise non-college bound, low-
07 income student to matriculate to college with federal aid is not easily
08 accomplished (Kane, 2001). While perhaps disappointing, the results
09 should not necessarily be surprising given that the implicit costs of
10 preparing for college may be quite socially and economically high for
11 the low-income student (e.g., taking and succeeding in college-prep
12 courses or forming the social networks necessary to be informed about
13 the matriculation process), and that these costs are incurred far before
14 the arrival of financial aid upon matriculation.

15 On the other hand, the research also suggests that the Pell program
16 can be successful at influencing access for narrower populations of
17 college students such as independent students for whom the benefits
18 of enrolling in college may be relatively more apparent – e.g., persons
19 who have entered a career and discovered ex post that the lack of a
20 college degree may limit their opportunities in their chosen occupation.
21 Moreover, some research has found that the generosity of the Pell
22 program, while not necessarily directly influencing *access*, per se, has
23 appeared to affect *choice* of college for low-income students. Thus,
24 while the enrollment threshold may be difficult to clear for non-college
25 inclined students, the college-choice threshold and the quality of the
26 match may well be influenced by financial aid. Overall, these findings
27 may indicate that the Pell program has important economic efficiencies
28 by providing low-income students the opportunity to upgrade their
29 skills or their college.

30 Ultimately, the characteristics of the Pell program that account for
31 its longevity and political success may also have limited its economic
32 success. For example, the Pell program by being student-based (as
33 opposed to institution-based) yields its most direct economic benefits
34 to students who are the least likely agent within the higher education
35 system to politically organize and argue for the program. Regular and
36 consistent lobbying of Congress is essential for a ‘non-entitlement’
37 Pell program, where the funding must continually be reauthorized.
38 In addition, the interests of students and institutions are not neces-
39 sarily aligned. For example, universities have pushed for Pell program
40 restrictions, such as the half-cost rule, that clearly protect institutional
41 interests at the expense of students. Even worse, the literature testing

01 the Bennett hypothesis suggests that federal aid might well encourage
02 rent-seeking behavior on the part of universities. Thus, it is not wholly
03 surprising that the history of the reauthorization process shows a steady
04 erosion of the real value of the Pell awards at a time when more
05 politically expedient aid programs such as deferred tax college savings
06 plans at the federal and state level (e.g., 529 plans) and merit-based
07 aid programs at the state and institutional level have received growing
08 support (Dynarski, 2000, 2004).

09 However, the research suggest that perhaps the potentially greatest
10 weakness of the Pell program is the reauthorization design itself that
11 has led to a focus on marginally adjusting the pre-existing Pell param-
12 eters as opposed to more significant and creative adjustments that
13 may be necessary to yield a real lasting effect. In particular, unlike
14 the evidence surrounding the Pell program, studies of the GI Bill, the
15 Social Security Benefits Program (SSB), and the DC Tuition Assistance
16 (DCTA) Program find strong evidence that federal aid can yield signif-
17 icant and economically meaningful changes in college-going behavior
18 (e.g., Abraham & Clark, 2003; Bound & Turner, 2002; Dynarski,
19 2002). Moreover, merit-based aid programs (e.g., the HOPE scholarship
20 in Georgia), which might well be expected to favor the well-to-do
21 student, have also been found to increase the enrollment propensities
22 of needy students (e.g., Cornwell, Mustard, & Sridhar, in press; Singell,
23 Waddell, & Curs, 2006). Thus, it is reasonable to ask what these
24 programs do that the Pell program does not.

25 The programs that yield significant effects on college-going
26 behavior are, first and foremost, entitlements. Among the related need-
27 based aid programs, the GI Bill was broadly available to all veterans,
28 the SSB Programs was available to all persons who experience a death
29 in the family, and the DCTA Program is available to all DC residents.
30 Likewise, the Georgia HOPE Scholarship is an example of a merit-based
31 entitlement where all Georgia residents with a high-school average of
32 "B" or better qualify for assistance toward Georgia post-secondary insti-
33 tutions. Thus, these programs entitle qualified students to aid, which
34 reduces uncertainty with regard to the sources of funding and permits
35 students to plan (prepare) for college. Uncertainty regarding funding
36 may be the greatest barrier to college access because needy students
37 (particularly first-generation students) may not have the social capital
38 necessary to fully evaluate whether they have the sufficient resources to
39 attend college and may greatly underestimate their access to financial
40 aid (e.g., Singell & Stater, 2006).

01 Second, in a related point, most programs that have been found
02 to successfully entice previously non-enrolling students to matriculate
03 have clear and simple rules that determine whether a student qualifies
04 for aid. The Pell program has a myriad of complex (regularly changing)
05 rules that make it hard for a student to know, a priori, the level of
06 federal grant support they will receive. This fact, combined with the
07 non-entitlement status of the Pell grant, means that a student must first
08 apply for college with the confidence that they have the wherewithal
09 to enroll independent of their potential grant aid. Such confidence
10 is likely to be lacking for relatively needy students who may require
11 significant financial support to attend college (e.g., St. John, 2003).

12 Finally, the most successful programs entitle a student to grant
13 funds that cover a well-specified and significant portion of the cost
14 of college. At the time of their inception, the GI Bill, SSB, DCTA
15 programs, and the HOPE Scholarship all covered most, if not all, of the
16 costs of college, entitling students to both well-defined and generous
17 aid packages that left the student with relatively little debt burden from
18 college and little uncertainty. If needy students are relatively more
19 uncertain about their ability to complete college and less certain about
20 their earning capacity when they complete a degree, they are less likely
21 to take on the necessary debt to go. Risk aversion combined with
22 the rising cost of college and the increasing share of non-subsidized
23 aid in the financial aid package may go along way toward explaining
24 the growing gap of college attendance between needy and non-needy
25 students.

26 In the end, good aid policy must weigh the costs and benefits
27 of any program and must compare the net benefit of government
28 funds spent in a given use versus its next best alternative. Thus, an
29 important question to ask is whether the federal government should
30 be subsidizing student college access. Driven by a growing return to a
31 college education, a significant and increasing portion of the college-
32 age population (the needy included) find it worthwhile to attend
33 college. Thus, a relevant question might well be whether the additional
34 resources necessary to induce the marginal needy student to enroll in
35 college can justify the expenditure (e.g., Dynarski, 2002). The policy
36 pundits that have been pushing for greater funding for the Pell program
37 have done little to answer such questions.

38 Nonetheless, from a social perspective, it is unlikely to be optimal
39 to permit a growing educational divide between the income classes
40 and it is here where the evening hand of government is likely to be
41 required to equalize opportunity. Federal courts have already insisted

01 that we have a constitutional obligation to fund K-12 equally and
02 equitably, and the growing importance of college education in the
03 labor market may well suggest that this principle should be applied
04 to K-16. However, the growing use of merit aid and other non-need-
05 based aid programs by both institutions and states to leverage limited
06 federal aid dollars and influence the choice of the marginal (able)
07 student is evidence that these levels of government are unlikely to
08 have the financial wherewithal or the self-interest to effectively pursue
09 need-blind admissions. Thus, given that the Pell program is the largest
10 federal attempt to level the playing field, it is important to know what
11 modifications to the program will best make use of the federal purse.
12 The body of research to date suggests that the current Pell program is
13 unlikely to be optimal.

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Curs, Singell, and Waddell: *The Pell Program at Thirty Years*

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