MONEY FOR NOTHING? THE IMPACT OF CHANGES IN THE PELL GRANT PROGRAM ON INSTITUTIONAL REVENUES AND THE PLACEMENT OF NEEDY STUDENTS

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Abstract

Using new institutional-level data, we assess the impact of changing federal aid levels on institutional-level Pell revenues. Using various policy instruments associated with Pell generosity, we quantify the sensitivity of institutional Pell revenues to the generosity of the Pell Grant program. In general, we find an elastic response of institutional Pell revenues with respect to the maximum Pell award, where other policy instruments associated with Pell generosity are found to have an inelastic or zero impact. We also document significant asymmetries across institutional selectivity, both in magnitude and in terms of which channel accounts for the measured sensitivity—award values directly or institutional enrollment. In the end, exogenous changes in the federal Pell Grant program are found to correlate strongly with changes in the distribution of needy students and revenues across institutional quality.

1. INTRODUCTION

The Higher Education Act of 1965 authorized the creation of the Pell Grant that first provided financial aid in 1973. From its inception, Pell has been the largest need-based grant program in the United States, allocating over $13 billion in assistance to roughly one-quarter of all U.S. undergraduates in 2006. Despite the size of the Pell program, however, the effect of its available policy instruments on postsecondary educational institutions is relatively unknown. This article empirically examines whether changes in the generosity of the Pell program affect the distribution of Pell revenues across the quality spectrum of higher education institutions, which speaks to a primary interest of U.S. financial aid policy to facilitate the match of students to institutions based on ability. In particular, we analyze how generosity correlates with institutional Pell revenues by utilizing exogenous variation in federally determined maximum Pell Grant and federal appropriation levels, as well as annual variation in the total number of students who are deemed Pell eligible following the application of federally determined criteria. Broadly, Pell revenues depend on the pool of students applying for aid and their institutional choices. Thus we also explore how institutional Pell revenues relate to changes in the average Pell award per student and enrollment at each institution, which provides some of the first formal evidence of the Pell program's efficacy in influencing the composition and net distribution of needy students across U.S. universities.

Although a stated objective of the Pell program is to increase the accessibility of higher educational opportunities for low-income students, there is little existing evidence that the Pell Grant has significantly affected the college-going behavior of needy students. In particular, in response to the introduction of the Pell program, enrollment effects in general populations of students are weak (e.g., Hansen 1983; Kane 1994, 1995; Heller 1997; McPherson and Schapiro 1998). This is particularly noteworthy in light of other forms of aid found to have significant enrollment effects (e.g., Bound and Turner 2002; Dynarski 2003). The lack of a clear and consistent enrollment effect generally suggests that Pell aid might not be sufficient for the broad population of needy students to surmount the enrollment hurdle.

On the other hand, there is evidence of Pell influencing more narrowly defined groups of institutions or students. For example, Kane (1995) finds that the Pell program increases overall enrollment at public two-year colleges, which suggests that aggregate enrollment effects may vary with the selectivity of the institution. However, Kane (1995) does not separate needy from nonneedy students. Similarly, Seftor and Turner (2002) exploit variation in the Pell eligibility formula in the late 1980s that decreased the generosity of the program for financially independent students to document decreased access for nontraditional students.

In the existing literature, student-level studies generally do not observe whether a high school graduate receives a Pell grant or whether the student even applies. Instead, they focus on low-income students who would potentially qualify for the Pell award. On this note, while our analysis will remain at the institutional level due to data limitations, we do build on prior work by focusing on the institutional outcomes of participating students. Overall, we find significant increases in institutional Pell revenues with increased generosity. For example, our estimates suggest that a 10 percent increase in the maximum Pell award is associated with a 16 percent increase in revenues received at the average institution. Nonetheless, the magnitude (and even the direction) of the revenue response depends on the channel (i.e., the maximum Pell value versus federal Pell funds) and the selectivity of the institution. Thus, for example, the revenue elasticity point estimate for the maximum Pell award is not statistically different from one for two-year institutions, whereas this elasticity is found to be nearly two for the least exclusive four-year institutions. Overall, we show that changes in Pell generosity can yield significant distributional effects in terms of both Pell revenue received and Pell students enrolled in institutions of different selectivity.

From a political economy perspective, reporting the potential for differential gains across institutions in response to changes in Pell distribution rules highlights the importance of understanding allocation mechanisms. In particular, our revenue data also show that the fraction of Pell revenues going to two-year institutions rose from just over a quarter of the total disbursements in 1989 to over 40 percent in 2002, suggesting that Pell aid has expanded access at less selective institutions. While institutions may not have direct preferences about where their revenues arise—from Pell or non-Pell sources—the issue clearly remains of social concern, given the distribution of federal expenditures across institutions. Our empirical analysis examines whether changes in policy instruments contribute to or mitigate the movement of funds for needy students toward two-year degree programs and away from four-year schools, which highlights the political economy factors that underlie federal aid allocation mechanisms.

The aggregate nature of the data available on Pell enrollees implies that one cannot separate the collective impact of student- and institutional-level decisions on observed outcomes. Nonetheless, consistent with Pell awards being made to individuals rather than institutions, we also find evidence through dissecting institutional revenue into separate analyses of Pell student enrollment and average award values reported at institutions that revenue specifications alone do not reveal the true nature of the underlying allocation of Pell funding across institutions. Thus our analysis offers additional support for the conjecture that individual student

enrollments respond to aid net of potential institutional responses to the program.

Our findings suggest that changes in Pell generosity may affect the margin determining who among the needy apply for federal aid and where they enroll. Thus, while institutional-level data keep us from making strict inferences about the behavior of particular groups of students (i.e., needy students), the ability to separate Pell enrollment patterns complements existing efforts to document the efficacy of Pell. In particular, we follow the enrollment decisions of low-income youth around the 1992 Higher Education Amendments (HEA) that removed tuition-based caps on maximum Pell awards to identify how exogenous granting practices affect institutional choice. Measured against a group of slightly more expensive but otherwise similar institutions, we find a 5.3 percent increase in the enrollment of low-income students at low-cost institutions that experienced this exogenous increase in Pell generosity. In short, results suggest that student enrollment does respond to aid.

After describing some of the mechanics of the federal Pell Grant program, the following section of this article assesses the impact of changing federal aid levels on institutional-level Pell revenues over the 1989–2002 period. Section 3 contains the dissection of institutional revenue into enrollment and award value, offering estimates of the relative strength of these contributing factors. In section 4, we separately test the efficacy of change to the Pell program in the 1992 HEA using a subsample of relatively low-cost institutions. We summarize the results in section 5 and offer some additional discussion and concluding remarks regarding enrollment effects and average award values.

2. TOTAL INSTITUTIONAL PELL REVENUE AND VARIATION IN PELL GENEROSITY

Data

Our primary data source is the Integrated Post-Secondary Education Data System (IPEDS), which provides detailed institutional data. Our sample consists of IPEDS data for 1989–2002. Where related research has relied on indirect measures for the number of low-income students, such as minority enrollments or other student background measures that are correlated with income (e.g., Kane 1994; Dynarski 2004), our analysis exploits unique institutional-level Pell-related data from the Department of Education to directly examine the effects of changes in the Pell Grant program on low-income students and the associated revenues they bring to institutions. In particular, these data provide information on the number of Pell recipients and revenues for each institution for the 1989–2002 period. We supplement these data with information drawn from the state-specific labor market and economic measures acquired from

standard sources. In addition, we adopt *Peterson's Guide to Four-Year Colleges* (1990) as our metric for an institution's selectivity in 1989, which allows us to separate two-year institutions from four-year institutions classified as noncompetitive, minimally difficult, moderately difficult, very difficult, and most difficult. To focus on a well-defined set of colleges with a common academic mission, we restrict the sample to nonprofit institutions that offer at least an associate's degree, excluding for-profit and trade schools.[1]

Pell Generosity and Sample Selection

To receive federal aid, a student must first complete a Free Application for Federal Student Aid (FAFSA) form, which provides financial aid administrators with the information needed to determine the size of an applicant's Pell Grant. The award value is formulaic, determined by the student's expected family contribution (EFC) and the institution-specific costs of attendance (COA) such as tuition, room, board, and other expenses such as books and travel. For dependent students, the EFC is a function of parental income and wealth and the number of siblings in college. Conditional on being above the federally mandated minimum grant, the level of an individual student's grant in any given year is the minimum of (1) the difference between the federal maximum Pell Grant and the student's EFC; (2) the difference between the institution's COA and the student's EFC; and (3) prior to 1993, 60 percent of the institution's COA.

There are three margins on which the federal government can effectively change the generosity of the Pell program that we exploit in our empirical analysis. First, in terms of measuring the influence of Pell generosity on institutional revenues, the maximum potential Pell award any student might receive at institution i in year t, $MaxPell_{it}$, is arguably of most interest. Second, given that we can also control for variation in the typical student-age population, the number of Pell applicants deemed eligible based on family income, $Eligibles_t$, can also speak to the generosity of the Pell program. Finally, we include federal appropriations for Pell grants, $FedApprop_t$, in an attempt to capture the intended generosity of the Pell program in aggregate. While not

1. Our sample period is restricted to 1989–2002 because IPEDS is not reliably reported prior to 1989 and Pell data are unavailable after the 2002–2003 academic year. Degree-granting two-year and four-year institutions account for the majority of participants in the Pell program. For example, in 1989, for-profit colleges and trade schools account for about 16 percent of the Pell recipients prior to their exclusion from the sample. The 1992 reauthorization placed some limits on Pell students matriculating at for-profit colleges and trade schools such that the proportion of Pell recipients attending these institution types in the full universe of higher education institutions was approximately 11 percent in 1994 (12 percent in 2002). Thus the proportion of Pell recipients attending for-profit colleges and trade schools is small and declines slightly over the sample period.

the actual sum of award values in a given year, such a measure may in fact better capture the expected or intended generosity in aggregate.[2]

Of course, each of these generosity measures should be positively correlated with Pell revenues for the average institution. However, investigating the empirical patterns around changes in these policy instruments is of interest because they may well be expected to yield different institutional effects. It follows that including all three measures of generosity is important. Specifically, as a given increase in *MaxPell* can provide additional assistance to those already receiving relatively large Pell awards and can also induce small awards to those who would previously not have qualified (i.e., changing the number of eligible students), including *Eligibles* allows one to measure the effect of *MaxPell* holding constant the number of eligible students. Further, having the ability to measure the effect of *Eligibles* while holding *MaxPell* and the size of the potential student population constant may indirectly illustrate the effect of changes to the calculation of EFC.[3] Likewise, including both *MaxPell* and *FedApprop* may allow one to separately identify the effects of changing the upper tail of award values as measured by its maximum and changing where in the distribution of awards the mass falls that relates to the total provision of awards, holding the maximum constant.

Nonetheless, given the potential contribution of an institution's COA to the grant determination as it relates to the maximum Pell award, the tuition responses could well be endogenous to changes in Pell generosity for low-cost institutions. To alleviate this concern, we restrict the sample to institutions with COA sufficiently high such that $MaxPell_{it} = MaxPell_t$ for all years in the sample period. In so doing, we retain over 71 percent of the larger sample of institutions that, after discarding low-COA institutions, enroll 60 percent of all Pell recipients. We do not discard these low-COA institutions, but subsequently examine their responsiveness to changes in the generosity of federal aid in section 4. In particular, we exploit a change in Pell funding brought about by the 1992 HEA that exogenously affected Pell generosity for some (but not all) of these low-COA institutions.[4] Thus the analysis examines the impact of

2. Further, the sum of actual award values would be prone to simultaneity bias, whereas federal appropriations would not.
3. With both *MaxPell* and *FedApprop* depending on congressional authorization, the determination of *FedApprop* may in practice be the simple multiplying of *MaxPell* by the anticipated number of eligible students. Thus the inclusion of the actual number of *Eligibles* introduces new information not contained in the other measures, variation that may contribute to explaining revenue allocations. Including a measure of the number of eligible Pell applicants in a given year may also alleviate any concern that one's propensity to complete a FAFSA depends on *MaxPell*, which would otherwise bias the estimated effect of maximum Pell awards without its inclusion.
4. There were two major changes to the Pell Grant program brought about by the 1992 reauthorization. First, the reauthorization abolished the percentage cap on Pell Grants, which provides the exogenous variation that forms the basis of the natural experiment analyzed in section 4. Second, it removed mortgage costs from the calculation of EFC, which was designed to protect low-income families

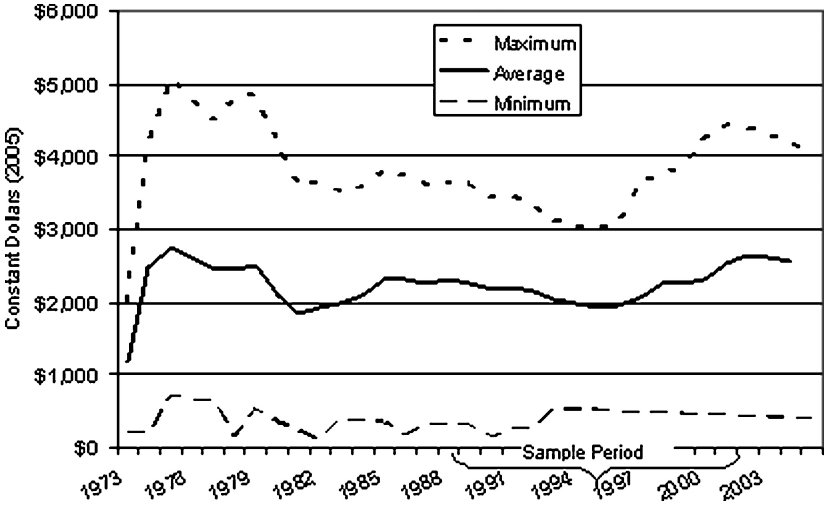

Figure 1. Maximum, Average, and Minimum Pell Grant Awards over Time. Source: U.S. Department of Education 2003–2004, table 1.

changes in the Pell program on institutions that are constrained and those that are unconstrained by the COA rule.

The fact that such a large proportion of institutions have a sufficiently high COA as to not affect individual Pell award values may explain the mixed support for the Bennett hypothesis, which postulates that universities might respond to more generous aid by raising their COA (e.g., Long 2004; Rizzo and Ehrenberg 2004; Singell and Stone 2007). Of course, if Pell award values for the remaining observations are actually independent of institutions' COA, one would not expect that the COA would rise with Pell at these institutions. Because only Congress can change the maximum Pell Grant each year, we exploit all variations in $MaxPell_t$ within the remaining sample as exogenous to institutions. While there has been a significant amount of variation in the real generosity of the Pell program over time, as demonstrated in figure 1, the qualitative findings drawn from the analyses are not sensitive to the exclusion of low-cost institutions as discussed subsequently.[5] Summary statistics for

from appreciation of non-liquid assets that cannot readily be used to pay for college. Acosta (2001) shows this change increased the number of students participating in the Pell program that had higher family wealth.

5. The original intent of the Pell Grant program was to provide an award that, when combined with other sources of aid and a reasonable family or student contribution, covered no less than 75 percent of the student's cost of attendance. However, while the average nominal award value has increased from $270 when the program started in 1973 to $2,466 in 2004, the real value of the Pell Grant has decreased. Figure 1 shows that when measured in real 2005 dollars, the maximum Pell award peaked at roughly $6,000 shortly after the program's introduction in 1975. However, the value of the maximum Pell award steadily declined until 1997, when it reached a low of just over $3,000. The 1998 HEA provided a yearly increase in the maximum Pell award through 2003, but there has

the sample of 1,784 institutions that are never restricted by the COA rule are provided in table 1.

In general, we anticipate that the empirical regularities revealed through our analysis will demonstrate a revenue-increasing effect of generosity, in general. However, there may be different distributional effects across different types of institutions. As this is fundamentally an empirical question, we now define our specification.

Empirical Specification

With all three measures being exogenous to individual institutions, we regress institution-specific Pell revenues on these Pell generosity measures and a set of controls. Throughout the analysis, we adopt the *Peterson's Guide to Four-Year Colleges* (1990) ranking of institutions as our metric of institutional selectivity. However, given the small cell size of the noncompetitive and minimally difficult classes of institution, we combine these into a single category. Likewise, we combine very difficult and most difficult four-year institutions into a single category. A distinction by selectivity is preferred to alternative classifications such as tuition that might also be expected to restrict needy student access, because a primary goal of U.S. federal financial aid policy is to facilitate the match between needy students by ability. Nonetheless, selectivity and tuition are generally correlated such that the conclusions based on broad categories of selectivity relate to those based on broad tuition categories.

Relaxing the constraint that the set of controls influences Pell revenues similarly across institutional selectivity, we therefore estimate the following fixed effect specification separately for each of these selectivity categories:

$$\log(TR_{it}) = \alpha_i + \beta_1 \log(MaxPell_t) + \beta_2 \log(FedApprop_t) + \beta_3 \log(Eligibles_t) + \gamma' \mathrm{X}_{it} + \varepsilon_{it}, \quad (1)$$

where TR_{it} is the total revenue received by institution i from all Pell grants associated with students enrolling in year t.

Because total revenue is a simple product of Pell enrollment and individual award values, X_{it} is a vector of controls that are expected to correlate with enrollment and need. In particular, we follow prior work (e.g., Leslie and Brinkman 1987; Heller 1997) in allowing enrollments and award values to vary with prices, institutional characteristics, and local market conditions. Specifically, we capture variation at the institutional level by including a measure of the direct cost of attendance (in-state tuition) and a size measure (the

not been an increase in the level of the maximum Pell award since then (Curs, Singell, and Waddell 2007).

Table 1. Sample Characteristics by Institution Selectivity

Variables	Two-Year Institutions (1)	Noncompetitive or Minimally Difficult (2)	Moderately Difficult (3)	Very Difficult or Most Difficult (4)	Full Sample (5)
MaxPell	-	-	-	-	$2,324 (257)
FedApprop[a]	-	-	-	-	$5,560 (943)
Eligibles	-	-	-	-	5,043,570 (459,320)
Lagged enrollment less Pell students	3,080 (4,013)	1,291 (2,230)	3,030 (4,373)	3,660 (4,089)	2,770 (4,004)
In-state tuition	$2,816 (2,153)	$5,192 (2,655)	$7,860 (3,998)	$13,343 (5,327)	$6,558 (4,594)
State-based need aid expenditures[a]	$121 (144)	$67 (106)	$104 (130)	$162 (170)	$106 (136)
State-based merit aid expenditures[a]	$12 (23)	$16 (36)	$11 (26)	$13 (31)	$13 (28)
Per capita disposable income in state	$21,553 (3,041)	$19,718 (2,769)	$20,710 (2,908)	$23,985 (3,018)	$20,844 (3,008)
Lagged unemployment rate in state	5.3 (1.5)	5.4 (1.5)	5.3 (1.5)	5.5 (1.5)	5.3 (1.5)

Table 1. Continued

Variables	Two-Year Institutions (1)	Noncompetitive or Minimally Difficult (2)	Moderately Difficult (3)	Very Difficult or Most Difficult (4)	Full Sample (5)
Mean weekly manufacturing earnings in state	$462 (54)	$441 (57)	$452 (51)	$450 (40)	$452 (53)
Median home value in state	$90,619 (23,604)	$80,481 (26,170)	$92,743 (74,538)	$97,461 (27,678)	$87,668 (27,408)
Number of high school graduates in state	89,044 (57,417)	81,763 (70,547)	92,743 (74,538)	114,807 (79,418)	91,631 (70,772)
Number of 18–19-year-olds in state	243,650 (165,980)	234,548 (206,639)	259,143 (219,184)	322,802 (23,658)	256,230 (207,530)
Observations/number of institutions[b]	5,890/503	4,402/339	11,129/817	1,998/148	23,419/1,784

Notes: Sample means (in 1990 dollars) are reported for the sample used in the estimation procedures reported in table 2, table 3, table 4, and table 5. Standard deviations are in parentheses.
[a]Millions of dollars.
[b]The sum of the institutions across the selectivity types is greater than the total number of institutions (i.e., 1,784), because 23 institutions change from two-year to four-year institutions over the course of the sample. In other words, the change in status of these 23 institutions, while not affecting the number of observations, yields double counting in regard to the number of institutions.

lag of total enrollment minus Pell enrollment), as reported in IPEDS.[6] State-level measures of financial aid generosity (state expenditures on need-based and merit-based aid), employment opportunities (unemployment rate), demographic conditions (per capita disposable income, median home values, and mean weekly manufacturing earning), and demographics (the number of high school graduates and the 18–19-year-old population) are also included.[7] A quadratic trend is also included in this vector of controls to pick up any unobserved time dependence. The selectivity of an institution, and other sources of time-invariant heterogeneity, is absorbed into the error treatment through α_i.

In all specifications, we capture any time-invariant unobserved heterogeneity specific to institutions by including an institution-specific fixed effect. We therefore assume that the factors that affect the decisions of Pell recipients are captured in X_{it} or are time invariant. For example, unobserved preference for one institution over another is captured through the error structure as long as such preferences are persistent.[8] The non-aid-related control variables and the use of fixed effects follow related higher education studies that examine similar college outcomes (e.g., Cornwell, Mustard, and Sridhar 2006; Singell, Waddell, and Curs 2006). It is important to emphasize that, to the extent Pell recipients choose a particular institution due to the institution's generosity, a concern for omitting variation across institutions in aid or generosity is

6. The fraction of students paying in-state tuition varies across institutional types. For example, 92 percent of students who attend two-year institutions are in-state as compared with 42 percent at the most selective private schools. However, the coefficient on a control for the proportion of out-of-state students is generally insignificant and does not affect the qualitative conclusions presented. The tuition costs exclude actual room and board measures, which are incomplete in IPEDS for many schools. However, the COA at institution i could be approximated by i's in-state tuition plus the average statewide room and board for public or private institutions, for example. Nonetheless, the subsequent qualitative conclusions of the analysis are not affected by alternative treatments of room and board (e.g., including room and board in the price or by including tuition separate from room and board), which likely results from the fact that the majority of the cross-institutional variation in price is due to tuition. Of course, if variation in room and board is largely cross sectional, it is absorbed into our error structure as time-invariant heterogeneity between institutions.

7. State expenditures on need-based and merit-based aid are available from the National Association of State Student Grant and Aid Programs. Unemployment rate and mean weekly manufacturing earning made are available from the Bureau of Labor Statistics. Per capita disposable income is available from the Bureau of Economic Analysis. Median home values are available from the U.S. Bureau of the Census and Freddie Mac. The number of high school graduates is available from the Southern Regional Education Board. The population of 18–19-year-olds is available from the U.S. Bureau of the Census.

8. Of course, one potential concern in explaining institution-specific variation in Pell revenues with few institution-specific controls that vary only over the time series dimension of the sample is that the standard errors might be biased, possibly downward, due to aggregation. Nonetheless, the inclusion of institutional fixed effects controls for time-invariant differences in Pell revenues between institutions, whereas time-varying differences in Pell revenues that can be attributed to institutional behavior are likely to be reflected (fully) in tuition and lagged enrollment. It follows that with an abundance of state-specific controls and a few key institutional-level controls, the inclusion of institutional fixed-effects alleviates our concern.

mitigated by our inclusion of institutional fixed effects. That is, on this issue of fully capturing aid availability, the only real omission from the specified model is the potential variation in the generosity of a given institution over time.[9] With our inclusion of state-level need-based and merit-based aid expenditure, as well as quadratic time trends in the sample more generally, any remaining omission would be anticipated to have only a minor effect on the qualitative results.

Results

The results of estimating equation 1 are presented in table 2 for different samples of institutions, by selectivity. Overall, the empirical relationships with regard to the non-aid-related controls mostly confirm our prior expectations. For example, both need- and merit-based state aid expenditures have a positive impact on the Pell revenue generated at noncompetitive, minimally difficult, and moderately difficult four-year institutions and a negative impact at the very and most difficult institutions. Merit-aid expenditures also have a positive impact on Pell revenues at two-year institutions, possibly due to many state merit programs having vocational grants in addition to merit (e.g., Georgia). As expected, total Pell revenues at an institution generally decline as income increases in the state, suggesting that there are fewer Pell-eligible students. Total Pell revenue increases with the number of high school graduates in the state, potentially indicating a larger pool of potential Pell recipients. The coefficient on mean weekly manufacturing wage is significantly negative at two-year institutions and (where significant) is positive at four-year institutions. This may suggest that the manufacturing wage better represents the opportunity cost of attending two-year schools. For brevity, the remainder of the discussion focuses on the measures of Pell generosity that are of primary interest.[10]

In general, the estimation results demonstrate that institutional Pell revenues are increasing in generosity. In particular, however, there are three strong regularities revealed through the analysis. First, for all levels of

9. While IPEDS contains information on financial aid expenditures, unreported data in this area are extensive. Further, there is a two-year hole where the National Center for Education Statistics (NCES) collected financial data but did not release final data files (1997 and 1998). Two-year and for-profit schools were undergoing a change in accounting standards, which apparently produced such a number of erroneous reports that NCES never did complete its final data production.

10. In sensitivity analyses, we interact the state-level economic controls with a measure of the scope of the market from which the institution draws (i.e., the average proportion of in-state students over the period). With the level effect of such a control absorbed in an institutional fixed effect, the interactions are largely insignificant. We report the more parsimonious specification, because both specifications yield the same qualitative conclusions and in-state student counts are missing for fifty-seven observations. In a subsequent sensitivity test, we also included annual U.S. gross domestic product (GDP), which also reveals the robustness of the patterns in our variables of interest.

Table 2. Effect of Changes in Pell Generosity on Institutional Pell Revenue

Independent Variable	Two-Year Institutions (1)	Noncompetitive or Minimally Difficult (2)	Moderately Difficult (3)	Very Difficult or Most Difficult (4)	Full Sample (5)
Log(*MaxPell*)	1.050 (0.128)***	1.825 (0.141)***	1.566 (0.071)***	1.119 (0.127)***	1.516 (0.056)***
Log(*FedApprop*)	0.076 (0.041)*	−0.113 (0.046)**	−0.015 (0.023)	0.041 (0.040)	−0.015 (0.018)
Log(*Eligibles*)	1.422 (0.103)***	0.844 (0.106)***	0.533 (0.055)***	0.442 (0.101)***	0.745 (0.043)***
Log(Lagged enrollment less Pell students)	0.101 (0.011)***	0.081 (0.012)***	0.135 (0.010)***	−0.018 (0.031)	0.108 (0.006)***
Log(in-state tuition)	−0.021 (0.016)	0.071 (0.028)**	0.006 (0.014)	−0.036 (0.020)*	−0.007 (0.009)
Log(state-based need aid expenditures)	0.011 (0.012)	0.037 (0.010)***	0.035 (0.006)***	−0.033 (0.010)***	0.026 (0.005)***
Log(state-based merit aid expenditures)	0.053 (0.010)***	0.029 (0.007)***	0.009 (0.004)**	−0.019 (0.008)**	0.018 (0.003)***
Log(per capita disposable income in state)	−0.705 (0.223)***	−0.130 (0.240)	−0.399 (0.120)***	−0.401 (0.215)*	−0.491 (0.095)***

Table 2. Continued

Independent Variable	Two-Year Institutions (1)	Noncompetitive or Minimally Difficult (2)	Moderately Difficult (3)	Very Difficult or Most Difficult (4)	Full Sample (5)
Lagged unemployment rate in state	0.026 (0.006)***	0.010 (0.006)	0.005 (0.003)*	−0.000 (0.006)	0.015 (0.002)***
Log(mean weekly manufacturing earnings in state)	−0.633 (0.158)***	0.323 (0.154)**	−0.122 (0.083)	0.649 (0.162)***	−0.053 (0.065)
Log(median home value in state)	−0.955 (0.082)***	−0.268 (0.088)***	−0.883 (0.044)***	−0.520 (0.076)***	−0.715 (0.034)***
Log(number of high school graduates in state)	0.387 (0.057)***	0.212 (0.068)***	0.389 (0.030)***	0.395 (0.054)***	0.319 (0.024)***
Log(number of 18–19-year-olds in state)	0.126 (0.095)	−0.206 (0.112)*	−0.464 (0.052)***	−0.448 (0.088)***	−0.282 (0.041)***
$t(1989 = 1)$	0.030 (0.011)***	0.041 (0.012)***	0.029 (0.006)***	0.040 (0.011)***	0.036 (0.005)***
t^2	−0.001 (0.001)	−0.003 (0.001)***	−0.001 (0.000)***	−0.002 (0.001)**	−0.002 (0.000)***
Constant	−19.380 (3.109)***	−21.485 (3.227)***	−7.159 (1.674)***	−8.362 (3.086)***	−11.565 (1.314)***
Observation/number of institutions	5,890/503	4,402/339	11,129/817	1,998/148	23,419/1,784
R^2	0.38	0.34	0.41	0.43	0.36

Notes: In all specifications, the dependent variable is Log(total institutional Pell revenue). Coefficients are estimated while controlling for time-invariant unobserved heterogeneity specific to institutions. Standard errors are in parentheses. Dependent variables means for columns 1–5 are $1,203,585, $1,070,222, $1,438,984, $1,262,457, and $1,295,404, respectively. *significant at 10%; **significant at 5%; ***significant at 1%

selectivity, while institutional Pell revenues respond only moderately to federal appropriations, changes in the maximum available Pell award are associated with elasticity measures in excess of one in all cases.[11] For example, pooled-sample estimates (in column 5) suggest that a 10 percent increase in the maximum Pell award is associated with a 15 percent increase in revenues received at the average institution in the sample. This is not the case for changes in federal appropriations, where pooled-sample point estimates suggest that federal appropriations explain little, if any, variation in institutional Pell revenues. Estimates from the pooled sample do suggest, however, that Pell revenues respond significantly to the number of eligible students, with an estimated elasticity of 0.7.

The second regularity evident in the results is the systematic nature by which the effect of generosity differs across institutional selectivity, in terms of both *MaxPell* and *FedApprop*. As selectivity increases among four-year institutions in our sample, point estimates suggest a monotonic decrease in the elasticity of institutional Pell revenues with respect to changes in *MaxPell*. However, the estimated sensitivity of revenues to *MaxPell* is lowest at two-year institutions, where the elasticity is not statistically different from one. Interestingly, the largest relative difference revealed in the elasticity point estimates is the difference between two-year and noncompetitive or minimally difficult four-year institutions. Holding constant the size of the applicant pool and federal appropriations for the program, this may suggest that the maximum grant available has the strongest effect on the margin of enabling some two-year enrollees to access the less selective four-year institutions. Alternatively, it may also indicate that increases in the maximum Pell award relatively benefit those students already enrolled in less selective four-year institutions.

As selectivity increases among four-year institutions, point estimates suggest a monotonic increase in the elasticity of institutional Pell revenues with respect to changes in *FedApprop*. However, two-year institutions are, in fact, the most sensitive to *FedApprop*. Again, the largest relative difference revealed in the elasticity point estimates is the difference between two-year and noncompetitive or minimally difficult four-year institutions.

The third empirical regularity demonstrated in the results of table 2 is that, holding constant the maximum Pell award and controlling for the characteristics of the student body (e.g., the population of 18–19-year-olds, high school

11. Of course, one might expect such elasticity measures to be greater than one, since not all students at an institution receive the maximum available Pell award. For example, if the maximum grant is not received by all students, a given dollar increase in *MaxPell* necessarily amounts to a larger proportional increase in the average Pell award than in *MaxPell* itself. Note that if we do not discard institutions for which *MaxPell* is endogenous to the institution's own costs of attendance, the results are predictably that variation in *MaxPell* is less associated with variation in the institution's total revenue collected through Pell awards. In particular, because many of the low-cost institutions are two-year or less selective four-year institutions, this effect is more pronounced in such specifications.

graduating class, and family income), increases in Pell generosity measured through changes in the number of Pell students in a given year appear to have significant explanatory power in predicting institutional Pell revenues. Further, the effect of *Eligibles* is monotonically decreasing in selectivity, with the highest point estimate at two-year institutions (i.e., an elasticity of 1.4). To the extent that other controls leave changes in how expected family income is determined as the primary factor systematically contributing to the variation in the number of eligibles, this is consistent with any such changes in generosity being sufficiently small as to be overcome by other costs associated with a student accessing more selective institutions.[12]

Before continuing, note that the above estimates afford the opportunity to address broader policy issues surrounding proposed changes in the available instruments of the Pell program. For example, the Government Accountability Office (GAO; 2005, p. 2) estimates that the Education Department's proposed changes to the calculation of expected family contribution will reduce the number of Pell-eligible students by 1.5 percent. Taking these estimates as given, our analysis suggests that were such numbers to be realized in aggregate, Pell revenue to institutions would decrease by 1.1 percent at the average school in our sample. The asymmetries reported in our analysis, however, also suggest that decreases may be in the order of 2.1 percent at two-year institutions but only 0.7 percent at the most selective institutions in the country. Although these asymmetries are of primary interest from a broad policy perspective of the Pell program's ability to influence the distribution of needy students across the quality spectrum of higher educational institutions, a significant redistribution of Pell revenues may well be an important source of financial aid funds to particular institutions. This is particularly true for public universities that suffered significant reductions in state support and are often below capacity. Moreover, even the most selective private universities may nonetheless care about the composition of their student body in terms of need and whether reduced Pell generosity might require more institutionally provided grants.

3. PELL STUDENT ENROLLMENT AND AVERAGE AWARD VALUE

As suggested earlier, any systematic relationship between institutional Pell revenues and the generosity of the Pell program is necessarily due to the sensitivity of enrollment of Pell students, individual award values, or some combination of both. In the following analysis, we therefore examine the

12. Replication of the specifications in table 2 that reintroduce the low-cost institutions previously dropped from the data does not differ significantly from those presented. As expected, the coefficient estimates for the two-year institutions experience the largest magnitude changes from including low-cost institutions; specifically, coefficients on *MaxPell*, *FedApprop*, and *Eligibles* change from those presented (i.e., 1.050, 0.076, and 1.422, respectively) to 0.662, 0.129, and 1.425, which yield the same qualitative conclusions as drawn from specifications that exclude the low-cost schools.

proportional breakdown of the total revenue estimates of table 2 into these two contributing factors. In considering the underlying factors, the sensitivity of enrollment to changing generosity is itself recognized as nontrivial. In fact, as the generosity of the federal Pell program changes, there are four margins around which the number of Pell students enrolling in postsecondary institutions may change, as well as their distribution across institutions.

First, conditional on the granting of support, increases in overall generosity will tend to decrease the expected cost of college and may increase overall needy student enrollment rates. Second, as the expected value of grants increases, it may become in the best interest of a student previously on the margin of filing a FAFSA to now do so, increasing the number of applicants and potentially the number of students meeting Pell's minimum eligibility requirement.[13] Third, certain marginally needy students who filed a FAFSA and who would have been denied Pell prior to an increase in generosity may now receive a small Pell award. Each of these margins may be expected to increase Pell student enrollments generally and, where higher-quality education is valued, mitigate credit constraints that might otherwise limit needy students' choice over higher-quality institutions.

A fourth margin, however, works against the first three. That is, given significant cross-sectional variation in costs of attendance, increases in generosity will change institutions' relative prices. If prices rise with selectivity, a general increase in the funds available to needy students may be expected to change the distribution of needy students across selectivity, because added generosity will tend to increase the relative price of more selective institutions. As such, some Pell students may switch away from more expensive, more selective four-year schools. Of course, added generosity may also enable Pell students to switch from two-year to four-year schools, for example, with no additional out-of-pocket costs. The overall ambiguity in predicting the direction of the effect of generosity is therefore to be determined empirically.

The discussion of enrollment margins also implies that the sensitivity of Pell award values to generosity is not separate from similar considerations. In fact, it need not be the case that an increase in the maximum Pell award increases the average award value, for such an increase in generosity implies an enrollment response that could offset the direct effect of increasing the maximum award. For example, an increase in *MaxPell* would trigger the contemporaneous granting of new small Pell awards to those previously on the margin of qualifying for Pell.

13. Of course, year-to-year changes in congressional funding are hard to anticipate and occur after the student applies for college. As such, this effect is not likely to be significant.

Empirical Specification

Without an obvious reason for specifying correlates differently across the specification of average award values and enrollments, we propose the following specifications:

$$\log(PellEnroll_{it}) = \alpha_i + \beta_{PE} Z_{it} + \varepsilon_{it} \tag{2}$$

and

$$\log(AR_{it}) = \alpha_i + \beta_{AR} Z_{it} + \varepsilon_{it}, \tag{3}$$

where AR_{it} is the average revenue received by institution i from all Pell grants associated with the number of Pell students, $PellEnroll_{it}$, enrolling at the institution in year t. The vector Z_{it} captures all correlates previously included in equation 1. Rewriting equation 1 as $\log(TR_{it}) = \alpha_i + \beta_{TR} Z_{it} + \varepsilon_{it}$, it can easily be shown that, given the log specification, if $\hat{\beta}_{TR}$ is an unbiased estimate of β_{TR}, then $\hat{\beta}_{AR} + \hat{\beta}_{PE}$ is also unbiased in predicting β_{TR}. This property is also made clear in the estimated coefficients reported in tables 3 and 4.

When considering the estimation of equations 2 and 3 separately, recall that we have discarded all institutions with costs of attendance sufficiently low as to have Pell eligibility or award value depend on institution-specific costs. Thus, at the underlying disaggregated level, the award values of the individual students, which then contribute to the observed institutional-level average, are in fact exogenous to the particular institution in which the student enrolls. That is, the students represented by our sample of institutions would have received awards of equivalent value at any and all institutions in the sample. Further, within our sample of institutions, if an individual student was eligible to enroll as a Pell student anywhere, he or she would be eligible everywhere.[14] Therefore, in terms of our interest in separating the correlation of generosity with total revenue into that associated with average Pell award values and that associated with Pell enrollments, questioning the potential for simultaneity on causal grounds is unfounded. We do note that there is some validity to the question of, for example, omitted variables correlating cross sectionally with AR_{it} and $PellEnroll_{it}$. With our extensive list of controls, we believe it is instructive to consider the proportional breakdown of the revenue sensitivities of section 2 into that derived from award values and that derived from enrollments. In the following sections, we therefore document separately

14. Of course, in an unrestricted sample, some low-cost institutions could potentially influence the Pell award values and therefore the number of Pell students (i.e., if their COA was below *MaxPell*, which would imply that the Pell award would be determined by COA − EFC). Even if we did not discard low-cost institutions from the analysis, less than one percent of all institutions had COA less than *MaxPell* in 1992 (see Li 1999).

Table 3. Effect of Changes in Pell Generosity on Institutional Pell Enrollment

Independent Variable	Two-Year Institutions (1)	Noncompetitive or Minimally Difficult (2)	Moderately Difficult (3)	Very Difficult or Most Difficult (4)	Full Sample (5)
Log(*MaxPell*)	0.341 (0.119)***	0.894 (0.135)***	0.680 (0.068)***	0.300 (0.122)**	0.669 (0.053)***
Log(*FedApprop*)	0.028 (0.038)	−0.099 (0.044)**	−0.011 (0.022)	0.032 (0.038)	−0.022 (0.017)
Log(*Eligibles*)	1.395 (0.095)***	0.797 (0.101)***	0.502 (0.052)***	0.364 (0.097)***	0.716 (0.041)***
Log(lagged enrollment less Pell students)	0.117 (0.010)***	0.090 (0.011)***	0.157 (0.010)***	0.005 (0.030)	0.124 (0.006)***
Log(in-state tuition)	−0.036 (0.015)**	0.083 (0.027)***	0.004 (0.013)	−0.054 (0.019)***	−0.014 (0.009)*
Log(state-based need aid expenditures)	0.009 (0.011)	0.037 (0.009)***	0.035 (0.006)***	−0.030 (0.010)***	0.025 (0.004)***
Log(state-based merit aid expenditures)	0.036 (0.010)***	0.025 (0.007)***	0.005 (0.004)	−0.019 (0.007)**	0.013 (0.003)***
Log(per capita disposable income in state)	−0.378 (0.207)*	−0.118 (0.230)	−0.316 (0.115)***	−0.521 (0.207)**	−0.388 (0.090)***
Lagged unemployment rate in state	0.019 (0.005)***	0.010 (0.006)*	0.002 (0.003)	−0.002 (0.005)	0.012 (0.002)***
Log(mean weekly manufacturing earnings in state)	−0.564 (0.147)***	0.224 (0.147)	−0.067 (0.079)	0.664 (0.155)***	−0.032 (0.061)

Table 3. Continued

Independent Variable	Two-Year Institutions (1)	Noncompetitive or Minimally Difficult (2)	Moderately Difficult (3)	Very Difficult or Most Difficult (4)	Full Sample (5)
Log(median home value in state)	−0.917 (0.076)***	−0.188 (0.084)**	−0.843 (0.042)***	−0.487 (0.073)***	−0.674 (0.033)***
Log(number of high school graduates in state)	0.384 (0.053)***	0.209 (0.065)***	0.389 (0.029)***	0.435 (0.052)***	0.331 (0.023)***
Log(number of 18–19-year-olds in state)	0.016 (0.089)	−0.123 (0.107)	−0.429 (0.050)***	−0.430 (0.084)***	−0.263 (0.039)***
$t(1989 = 1)$	0.022 (0.010)**	0.047 (0.011)***	0.035 (0.006)***	0.046 (0.010)***	0.040 (0.004)***
t^2	−0.001 (0.001)	−0.004 (0.001)***	−0.002 (0.000)***	−0.002 (0.001)***	−0.002 (0.000)***
Constant	−12.664 (2.888)***	−12.981 (3.082)***	−0.111 (1.602)	1.507 (2.962)	−4.153 (1.244)***
Observation/number of institutions	5,890/503	4,402/339	11,129/817	1,998/148	23,419/1,784
R^2	0.27	0.15	0.23	0.27	0.19

Notes: In all specifications, the dependent variable is Log(total institutional Pell enrollment). Coefficients are estimated while controlling for time-invariant unobserved heterogeneity specific to institutions. Standard errors are in parentheses. Dependent variables means for columns 1–5 are 926, 699, 961, 820, and 891, respectively. *significant at 10%; **significant at 5%; ***significant at 1%

Table 4. Effect of Changes in Pell Generosity on Mean Institutional Pell Awards

Independent Variable	Two-Year Institutions (1)	Noncompetitive or Minimally Difficult (2)	Moderately Difficult (3)	Very Difficult or Most Difficult (4)	Full Sample (5)
Log(*MaxPell*)	0.710 (0.042)***	0.931 (0.038)***	0.886 (0.018)***	0.819 (0.039)***	0.847 (0.016)***
Log(*FedApprop*)	0.049 (0.013)***	−0.015 (0.012)	−0.005 (0.006)	0.009 (0.012)	0.008 (0.005)
Log(*Eligibles*)	0.028 (0.034)	0.047 (0.028)*	0.031 (0.013)**	0.077 (0.031)**	0.029 (0.012)**
Log(lagged enrollment less Pell students)	−0.016 (0.004)***	−0.009 (0.003)***	−0.022 (0.002)***	−0.023 (0.009)**	−0.016 (0.002)***
Log(in-state tuition)	0.015 (0.005)***	−0.012 (0.007)	0.002 (0.003)	0.018 (0.006)***	0.007 (0.003)***
Log(state-based need aid expenditures)	0.003 (0.004)	−0.000 (0.003)	−0.000 (0.001)	−0.003 (0.003)	0.001 (0.001)
Log(state-based merit aid expenditures)	0.017 (0.003)***	0.004 (0.002)*	0.004 (0.001)***	−0.001 (0.002)	0.005 (0.001)***
Log(per capita disposable income in state)	−0.327 (0.073)***	−0.012 (0.064)	−0.083 (0.030)***	0.120 (0.066)*	−0.103 (0.027)***
Lagged unemployment rate in state	0.006 (0.002)***	−0.001 (0.002)	0.003 (0.001)***	0.002 (0.002)	0.003 (0.001)***
Log(mean weekly manufacturing earnings in state)	−0.069 (0.052)	0.099 (0.041)**	−0.055 (0.020)***	−0.015 (0.049)	−0.021 (0.018)

Table 4. Continued

Independent Variable	Two-Year Institutions (1)	Noncompetitive or Minimally Difficult (2)	Moderately Difficult (3)	Very Difficult or Most Difficult (4)	Full Sample (5)
Log(median home value in state)	−0.037 (0.027)	−0.079 (0.023)***	−0.040 (0.011)***	−0.033 (0.023)	−0.041 (0.010)***
Log(number of high school graduates in state)	0.002 (0.019)	0.003 (0.018)	−0.000 (0.007)	−0.040 (0.017)**	−0.011 (0.007)*
Log(number of 18–19-year-olds in state)	0.111 (0.031)***	−0.083 (0.030)***	−0.035 (0.013)***	−0.019 (0.027)	−0.019 (0.012)
$t(1989 = 1)$	0.008 (0.003)**	−0.006 (0.003)**	−0.006 (0.001)***	−0.006 (0.003)*	−0.004 (0.001)***
t^2	0.000 (0.000)	0.000 (0.000)*	0.001 (0.000)***	0.000 (0.000)*	0.000 (0.000)***
Constant	2.494 (1.021)**	0.706 (0.858)	1.940 (0.412)***	−0.659 (0.944)	1.799 (0.370)***
Observation/number of institutions	5,890/503	4,402/339	11,129/817	1,998/148	23,419/1,784
R^2	0.54	0.70	0.81	0.84	0.71

Notes: In all specifications, the dependent variable is Log(mean institutional Pell revenue). Coefficients are estimated while controlling for time-invariant unobserved heterogeneity specific to institutions. Standard errors are in parentheses. Dependent variables means for columns 1–5 are $1,311, $1,509, $1,484, $1,528, and $1,449, respectively. *significant at 10%; **significant at 5%; ***significant at 1%

the sensitivity of institutional-level Pell enrollments and average award values to the time-varying measures of generosity: *MaxPell*, *FedApprop*, and *Eligibles*.

Results

Tables 3 and 4 report the results of estimating equations 2 and 3 on institutional-level Pell enrollments and institutional-level mean Pell award values. First, we note that proportionately, increases in *MaxPell* expand Pell enrollments most at the less selective among four-year schools, which could occur for two reasons. In terms of explanation, it could be that students are being made newly eligible for small Pell grants in greater proportion at the less selective four-year schools than at other institutions. Alternatively, such regularity is consistent with increases in the maximum award providing marginally greater access to four-year schools for low-income students who might otherwise attend two-year institutions.

Given the consideration of need in determining award values, these two potential margins can be informed by an analysis of average award values. In particular, consider three potential student types at a more disaggregate level than is afforded by our data. First, for a student receiving a Pell award below the maximum, a 1 percent increase in *MaxPell* would necessarily increase the award value by more than 1 percent. Second, for a student already receiving the maximum Pell award, a 1 percent increase in *MaxPell* would increase the award value by 1 percent. Third, an increase in *MaxPell* may allow previously ineligible students to qualify for small Pell awards, which would tend to yield a coefficient less than one on *MaxPell*.

At the institutional level of disaggregation permitted by the data, the point estimates across all levels of selectivity are in fact less than one. However, the less selective four-year institutions again stand out as different from two-year and more selective four-year institutions, with a point estimate on *MaxPell* not significantly different from one. Thus, while the results may be most consistently interpreted as higher *MaxPell* allowing previously ineligible students to qualify for small Pell awards, this seems most probable at two-year and the most selective four-year institutions.

As would be expected from specifications that have captured the systematic patterns in overall Pell generosity, with respect to the value-based and student-based measures of Pell generosity, institutional-level Pell enrollments are more sensitive to the number of eligibles (table 3), and institutional-level average award values are more sensitive to federal appropriations (table 4). Table 5 provides a summary of the proportional breakdown of the total revenue estimates of table 2 into the two underlying factors of number of Pell students and average Pell awards, which provides the estimated strength of

Table 5. Proportional Breakdown of the Sensitivity of Revenue to Generosity

Variables	Two-Year Institutions (1)	Noncompetitive or Minimally Difficult (2)	Moderately Difficult (3)	Very Difficult or Most Difficult (4)	Full Sample (5)
Log(*MaxPell*)	1.050***	1.825***	1.566***	1.119***	1.516***
Estimated contribution of average Pell award	.710 (68%)	.931 (51%)	.886 (57%)	.819 (73%)	.847 (56%)
Estimated contribution of Pell enrollment	.341 (32%)	.894 (49%)	.680 (43%)	.300 (27%)	.669 (44%)
Log(*FedApprop*)	0.076*	−0.113**	−0.015	0.041	−0.015
Estimated contribution of average Pell award	.049 (65%)	−.015 (13%)	−.005 (33%)	.009 (22%)	.008 (−46%)
Estimated contribution of Pell enrollment	.028 (35%)	−.099 (87%)	−.011 (67%)	.032 (78%)	−.022 (146%)
Log(*Eligibles*)	1.422***	0.844***	0.533***	0.442**	0.745***
Estimated contribution of average Pell award	.028 (2%)	.047 (6%)	.031 (6%)	.077 (17%)	.029 (4%)
Estimated contribution of Pell enrollment	1.395 (98%)	.797 (94%)	.502 (94%)	.364 (83%)	.716 (96%)
Observation/number of institutions	5,890/503	4,402/339	11,129/817	1,998/148	23,419/1,784

*significant at 10%; **significant at 5%; ***significant at 1%

each contributing factor. In considering the general empirical patterns in this way, the sensitivity of institutional Pell revenue due to changes in *MaxPell* appears more strongly associated with average Pell awards than with enrollments. While this is true across all selectivity levels, the two channels are much more similar in strength at four-year institutions than they are at two-year institutions. In this regard, one may also note that the most selective institutions (column 4) are more like two-year institutions than other less selective four-year institutions.[15] However, federal appropriations contribute largely through enrollment at four-year institutions while working largely through average award values at two-year institutions.

Clearly, not only are there significant asymmetries across schools of different selectivity in their sensitivity to Pell generosity, but scrutinizing the overall influence in this way reveals further empirical regularities that in particular set two-year institutions apart. In short, our analysis reveals that, in addition to changes in maximum award values and overall appropriations affecting institutions differently according to selectivity, these effects need not even materialize through the two channels in like fashion. As might be expected, holding constant the population, the maximum award value, and federal appropriations, increasing the generosity of the Pell program measured by increases in the number of eligible Pell students is almost entirely through enrollment effects. However, at the most selective four-year institutions, increasing the eligible population increases total Pell revenues through increases in average Pell awards, which also suggests that needier students may have accessed these selective schools in response to this dimension of increased generosity.

From a policy perspective, the above analysis again affords us the ability to comment on the potential outcomes of currently proposed changes to Pell administration procedures. That is, in terms of the enrollment of needy students in response to the 81,000 decrease in the number of Pell-eligible students estimated by the GAO, our results point to the potential for larger proportional decreases at two-year (2.2 percent) and less selective four-year institutions (1.2 percent) than at the most selective four-year schools (0.4 percent). Moreover, proposed changes to the maximum Pell award ranging from $100 to $500 suggest that the enrollment of needy students would be most

15. As elite private institutions may be thought to have both the objective to enroll students independent of need and the resources to do this through alleviating a particular student's need where the Pell program is not sufficiently generous to that student, it may be that enrollment at such schools is less dependent on Pell generosity, as measured. Using Consortium on Financing Higher Education (COFHE) membership as a proxy for the type of institution where such practice may be most evident in the data, there is evidence (not reported) that from among the most selective category of institution, total Pell revenues at institutions with COFHE membership are less responsive to *MaxPell*. Further, consistent with the practice of backfilling aid, all three measures of Pell generosity fail to significantly predict Pell enrollments at COFHE schools. Thus all revenue gains at these elite institutions in response to increased Pell generosity seem to operate through award values.

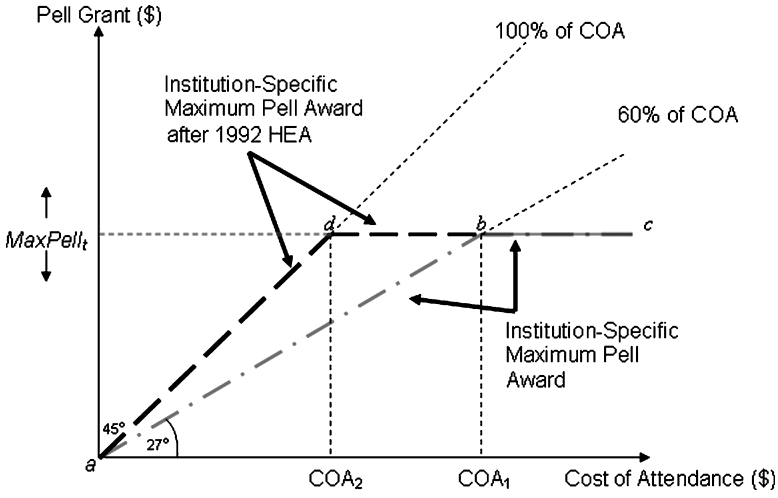

Figure 2. Relationship between Maximum Institutional Pell Grant and Costs of Attendance. Source: U.S. Department of Education 1994–1995, table 4.

responsive at four-year institutions, with responses up to 3.2 times larger than at two-year institutions. Thus our model suggests that these policy changes would jointly improve the relative support provided by the Pell program at four-year institutions.

4. THE 1992 HIGHER EDUCATION AMENDMENTS

In the previous section, we discarded the set of low-cost institutions for which institution-specific maximum Pell awards were determined by institution COA (i.e., those with COA sufficiently low such that $MaxPell_{it} < MaxPell_t$). Specifically, this mitigated the potential endogeneity of Pell award values in the sample as institutions with sufficiently low COA may have found that increases in *MaxPell* afforded them the opportunity to increase their tuition. While the potential endogeneity of *MaxPell* at these institutions made this sample restriction appropriate in the preceding analysis, this same sample of low-cost institutions may, in fact, contribute to our understanding of enrollment effects. For example, it is this sample of institutions for which the 1993 removal of the COA cap exogenously raised Pell aid. In this section, we therefore analyze this set of low-cost institutions that we had initially discarded from the sample around the exogenous removal of institution-specific Pell caps.

Figure 2 illustrates how the institution-specific Pell award, $MaxPell_{it}$, changes with the federally determined maximum, $MaxPell_t$, before and after the 1992 HEA. In particular, figure 2 demonstrates that, prior to 1993, maximum Pell grants at institutions with relatively low COA (i.e., specifically, less than COA_1) were constrained to be a maximum of 60 percent of the

institution's cost of attendance. With the 1992 HEA, these low-cost institutions therefore experienced a one-time increase in the maximum Pell from their institution-specific value determined by the binding percent-of-cost rule. At institutions with higher costs of attendance (i.e., above COA_1), the binding constraint on maximum Pell Grants is merely the federal maximum ($MaxPell_t$). The 1992 HEA would therefore not directly affect net costs of attending such institutions.

The previous analysis uses exogenous increases in the maximum Pell Grant to assess whether equal absolute changes in the level of need-based aid alter the distribution of low-income students across institutional selectivity. Given its federally induced change in the COA rule in 1993, the HEA provides a unique natural experiment to study whether exogenous variation in the level of Pell aid affects the choice of students among these low-cost institutions. In other words, whereas the previous analysis examined whether a given increase in the Pell award affects the distribution of students across a hierarchy of institutions, this section examines whether variation in the level of Pell award affects the enrollment choice of students across a similar set of low-cost institutions.

Empirical Model and Data

Following related work (e.g., Dynarksi 2004; Cornwell, Mustard, and Sridhar 2006), we employ a difference-in-difference strategy around the natural experiment that brought about a federal change in aid policy. Specifically, the following institutional-level fixed effect model is estimated:

$$\log(PellEnroll_{it}) = \alpha_i + \beta_1(HEA1992_t \times Treatment_i) + \beta_2 HEA1992_t + \gamma' X_{it} + \varepsilon_{it}, \quad (4)$$

where $HEA1992_t = 1$ for years after the 1992 HEA (i.e., $t \geq 1993$) and $Treatment_i = 1$ for those institutions with maximum Pell awards that were restricted by the COA rule in 1992, the year prior to the rule change.

Equation 4 is estimated using the same institutional-level data set as in earlier sections, where the sample is restricted to contain all institutions that had maximum Pell awards that were restricted at any time between 1989 and 1992, not by the federal maximum but by the COA rule (i.e., institutions with a cost of attendance less than COA_1 in figure 2). Given the definition of the treatment group above, the control group is all institutions that were constrained by the cost-of-attendance rule at any time between 1989 and 1991 but were not constrained by the rule in 1992. Thus we have defined the control group of institutions in such a way that they are the set of institutions that were

recently constrained by the COA rule similar to the treatment group but that had crossed the tuition-constraint threshold just prior to the 1993 rule change, such that they did not experience an increase in their maximum allowable Pell award with the 1993 change. We adopt the same set of controls in X as in the previous empirical specifications. The descriptive statistics for the 357 treatment and 367 control institutions over the sample period 1989–97 are provided in table 6.[16]

Empirical Results

Table 7 provides estimates of equation 4, which generally yield significant coefficient estimates that are qualitatively similar to the prior findings with regard to the control variables. Overall, the difference-in-difference results are consistent with the raw difference-in-difference and confirm the prior finding that Pell enrollments increase by a smaller amount at low-cost institutions coincident with increasing generosity.[17] However, all else equal, the removal of the percent-of-cost rule in 1993 provides a one-time exogenous increase in the maximum Pell awards for institutions in the treatment group, which would be expected to raise the number of Pell recipients at the treated versus the control institutions (i.e., $\beta_1 > 0$). This expectation is confirmed in the results presented in column 1 for the full sample of low-cost institutions. Specifically, institutions that were restricted by the percent-of-cost rule in 1992 saw a 5.3 percent higher increase in the enrollment of Pell recipients after the removal of the rule than did those institutions in the control group.[18]

The ability of the difference-in-difference approach to identify the exogenous impact of a change in financial aid depends on whether the control and treatment groups represent comparable institutions. In this particular case, the higher average tuition level at four-year versus two-year institutions implies that most of the institutions constrained by the percentage cost rule were two-year institutions. Nonetheless, although the removal of the percent-of-cost rule predominantly affects two-year institutions, 40 percent of the treatment group is composed of four-year institutions (see table 6) while accounting for

16. Given amendments to the Higher Education Act in 1998, this sample is restricted to annual observations before 1998.
17. The raw difference-in-difference suggests a 5.34 percent relative increase in Pell student enrollment at treated institutions.
18. A common concern with difference-in-difference analysis is that serial correlation in the error term may understate standard errors and increase the probability that the null hypothesis of no treatment effect is rejected. In our particular analysis, Bertrand, Duflo, and Mullainathan (2004) would imply ignoring the time series component in the estimation by first calculating an average before and after the 1992 HEA and then estimating the earlier equations on this averaged outcome variable as a panel of length two. Results are robust to this alternative specification with the null hypothesis of no treatment effect rejected at traditional levels. Moreover, a specification that excludes the year of treatment in 1993 yields a difference-in-difference coefficient of 0.048, versus 0.053 in the specification presented that included data from 1993.

Table 6. Sample Characteristics: Treatment and Control Groups

	Treatment Group[a]		Control Group	
	Pre-1992	Post-1992	Pre-1992	Post-1992
Variables	**HEA**	**HEA**	**HEA**	**HEA**
Number of Pell recipients	927 (1,068)	1,013 (1,171)	1,614 (1,576)	1,850 (1,747)
Lagged enrollment less Pell students	2,479 (3,467)	2,568 (3,441)	6,279 (5,897)	6,221 (5,609)
In-state tuition	$3,186 (296)	$3,372 (421)	$3,970 (399)	$4,317 (464)
State-based need aid expenditures[b]	$11 (13)	$15 (16)	$52 (64)	$68 (83)
State-based merit aid expenditures[b]	$7 (9)	$12 (27)	$8 (13)	$14 (30)
Per capita disposable income in state	$16,838 (1,441)	$17,667 (1,434)	$18,572 (23,970)	$18,982 (1,902)
Unemployment rate in state	5.7 (1.5)	5.6 (1.2)	5.9 (1.3)	6.5 (1.7)
Mean weekly manufacturing earnings in state	$403 (44)	$404 (38)	$427 (51)	$422 (47)
Median home value in state	$63,434 (29,480)	$65,856 (27,673)	$90,332 (42,967)	$85,666 (31,291)
Number of high school graduates in state	62,839 (51,005)	64,640 (57,864)	113,012 (99,994)	119,041 (107,644)
Number of 18–19-year-olds in state	198,471 (169,557)	195,640 (174,736)	360,741 (327,587)	347,722 (310,030)
Four-year institution	0.12 (0.33)	0.12 (0.32)	0.40 (0.49)	0.40 (0.49)
Observations/number of institutions	1,356/357	1,742/357	1,453/367	1,826/367

Notes: Sample means are reported in the estimation procedures reported in table 7. Standard deviations are in parentheses.

[a]The treatment group consists of all institutions with maximum Pell awards that were restricted by the cost-of-attendance rule in 1992, the year prior to the Higher Education Amendments (HEA). The control group consists of all institutions that were constrained by the cost-of-attendance rule at any time between 1989 and 1991 but were not constrained by the rule in 1992. Thus the control group of institutions did not experience an increase in their maximum allowable Pell award due to the 1992 rule change.

[b]Millions of dollars.

only 12 percent of the control group. Thus four-year institutions appear to be overrepresented in the control group, and the difference-in-difference effect may be identifying time-varying differences between two- and four-year institutions in addition to the exogenous increase in the maximum Pell awards.

To further examine the distinction between two- and four-year institutions, column 2 restricts the sample to two-year institutions. The enrollment effects associated with increases in the level of the maximum Pell award, while still positive, are insignificant for the sample of two-year institutions. In particular, the magnitude of the difference-in-difference coefficient declines by restricting

Table 7. Effect of the 1992 Higher Education Amendments on Institutional Pell Enrollments

Independent Variable	Full Sample (1)	Only Two-Year Institutions (2)
Post-1992 Higher Education Amendments X treatment group	0.053 (0.009)***	0.021 (0.013)
Log(lagged enrollment less Pell students)	0.030 (0.011)***	0.024 (0.012)**
Log(in-state tuition)	0.018 (0.013)	0.017 (0.015)
Log(state-based need aid expenditures)	0.052 (0.008)***	0.069 (0.011)***
Log(state-based merit aid expenditures)	0.060 (0.008)***	0.054 (0.011)***
Log(per capita disposable income in state)	−0.365 (0.190)*	−0.004 (0.247)
Lagged unemployment rate in state	0.032 (0.005)***	0.025 (0.006)***
Log(mean weekly manufacturing earnings in state)	−0.043 (0.145)	−0.164 (0.183)
Log(median home value in state)	−0.856 (0.069)***	−0.883 (0.083)***
Log(number of high school graduates in state)	0.176 (0.038)***	0.105 (0.049)**
Log(number of 18–19-year-olds in state)	−0.296 (0.096)***	−0.351 (0.118)***
Year fixed effect	Yes	Yes
Observations/number of institutions	6,377/724	4,713/539
R^2	0.43	0.46

Notes: In all specifications, the dependent variable is Log(number of Pell students). Coefficients are estimated while controlling for time-invariant unobserved heterogeneity specific to institutions. Standard errors are in parentheses. *significant at 10%; **significant at 5%; ***significant at 1%

the sample to two-year institutions, from 5.3 percent to 2.1 percent, which may be an indication of needy students who enroll in two-year (i.e., more locally oriented) institutions being less responsive to aid because they face additional constraints that require them to attend institutions close to home.[19] Alternatively, it may simply suggest that marginal price differences among two-year institutions do not affect the choice between competing two-year

19. To check the sensitivity of the control group, the specifications in table 7 are re-run including high-cost institutions in the control group that would be expected to be essentially unaffected by the rule change. The difference-in-difference coefficient remains significant and increases in magnitude from 5.3 to 11.5, reflecting the fact that the financial aid packages of students attending these institutions are not affected by the COA rule and that these institutions likely compete for a different set of students. Nonetheless, these results are not presented due to the concern that including these high-COA schools in the control group for the difference-in-difference analysis invalidates the identifying assumption that the control and treatment groups are similar in aspect other than that influenced by the target policy.

institutions but may well affect the choice between two-year institutions and low-cost, four-year institutions.

Thus the results in section 3 indicate that increases in the overall generosity of the Pell awards across all institutions may provide low-income students access to more selective institutions, whereas the results of section 4 suggest that low-cost institutions that experience a relative increase in the Pell award attract more low-income students than those that do not. Jointly, the empirical findings indicate that the college selection of low-income students responds to both the absolute and relative magnitude of Pell awards offered by higher education institutions.

5. DISCUSSION AND CONCLUDING REMARKS

In this article, we assess the impact of changing federal aid levels on institutional-level Pell revenues using institutional-level data on the number of Pell recipients and total Pell revenues from 1989 through 2002. We find that the three levers of Pell generosity—maximum Pell award, number of eligible students, and allocated level of federal funding—significantly affect Pell revenues earned by universities. For example, a 10 percent increase in the maximum Pell award is associated with a 15 percent increase in revenues received at the average institution. Nonetheless, we find significant asymmetries across schools of different selectivity in their sensitivity to Pell generosity in general and in the degree to which three different measures of generosity relate to institutional revenues. For example, two-year institutions are found to have a unitary revenue elasticity with respect to the maximum Pell award, whereas the least-selective four-year institutions are found to have a revenue elasticity of nearly two.

Scrutinizing the overall influence through separate analyses of award values and Pell enrollments reveals other important regularities in the data. For example, we find that, in addition to changes in maximum award values and overall federal appropriations affecting institutions differently according to selectivity, these effects need not even materialize through the two channels of Pell enrollments and average Pell awards in like fashion. For example, holding federal appropriations and the maximum potential award value constant, the benefits afforded to two-year institutions in response to increases in the number of eligible Pell recipients are roughly three times larger than those afforded to the most selective four-year institutions (i.e., an enrollment elasticity of 1.4 versus 0.4). On the other hand, revenues at two-year institutions are least sensitive to variation in the maximum award value (with an average revenue elasticity of 1.0), in particular with respect to the least selective and the middling four-year institutions, where there is evidence of a relatively strong

enrollment response to changes in the value of the maximum grant (with an average revenue elasticity of 1.8 and 1.6, respectively).

The apparent variation in the response of Pell recipients across the selectivity spectrum of institutions is compelling from an institutional policy standpoint because it suggests that changes in the various margins of generosity can have distinctly different impacts that vary with selectivity. While the available data do not permit an analysis of individual student choices with regard to enrollment decisions, exploiting the 1992 Higher Education Amendments to study whether differences in aid levels yield different enrollment patterns for comparable institutions reveals that student enrollments may be directly responsive to aid. In particular, the HEA removed the cost-of-attendance rule and therefore raised the institution-specific maximum Pell award at some, but not all, low-cost institutions. Around this margin, we demonstrate that the number of Pell recipients increased at those institutions that experienced an increase in their Pell award by over 5 percent relative to those that did not, suggesting that low-income students may in fact substitute toward those institutions with relatively generous need-based aid. Thus, although prior evidence suggests that Pell grants do not move students over the threshold from nonenrollment to enrollment, low-income students appear sensitive to the level of aid conditioned on the decision to enroll.

Overall, our results show that the Pell instrument levers the federal government chooses to use may affect the distribution of needy students across institutions and the federal money that they bring with them to the institution. In terms of our understanding recent need-related trends in postsecondary education and whether changes in policy instruments contribute to or mitigate the movement of needy students toward two-year degree programs and away from four-year schools, for example, our analysis is suggestive. For example, our analysis suggests that increases in the maximum available Pell award or changes to the calculation of family contributions that reduce the number of Pell-eligible students would relatively benefit four-year institutions.

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