# Money for Nothing? The Institutional Impact of Changes in Federal Financial Aid Policy

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Abstract: Using new institution-level data we assess the impact of changing federal aid levels on institution-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, and document significant asymmetries across institutional selectivity, both in terms of magnitude and in terms of which channel accounts for the measured sensitivity – Pell-award values or Pell enrollment. In the end, changes in the Federal Pell Grant program are found to correlate strongly with the distribution of needy students across institution quality.

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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 $12 billion in assistance to roughly one-quarter of all US undergraduates in 2003. Despite the size of the Pell program, however, the effect of its available policy instruments on post-secondary educational institutions is relatively unknown. This paper empirically examines whether changes in the generosity of the Pell program affect the distribution of Pell revenues across the quality spectrum of universities, which speaks to a primary interest of US 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 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 the institutional Pell revenue response relates both to changes in the average Pell award per student and enrollment at each institution, which provides some of the first formal evidence of the efficacy of the Pell program in influencing the composition and net distribution of needy students across US 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; Kane 1995; Heller 1997; McPherson and Schapiro 1998). This is particularly noteworthy in light of other forms of aid having been found

to have significant enrollment effects (e.g., Bound and Turner 2002; Dynarski 2003). The lack of clear and consistent enrollment effect suggests that Pell aid might not be sufficient for the broad population of needy students to clear 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 non-needy students. Similarly, Seftor and Turner (2002) exploit variation in the Pell eligibility formula in the late 1980s that particularly increased the generosity of the program for financially independent students to document increased access for non-traditional students who most often are part-time enrollees of two-year institutions. Thus, Pell aid, while not necessarily pushing a student over the enrollment margin, may well influence where a needy student chooses to enroll. Although four-year institutions that primarily serve traditional student populations still account for the majority of Pell revenues in our data, the empirical analysis examines whether changes in Pell generosity have differentially favored less-selective institutions.

Overall, we find significant increases in institutional Pell revenues with increased generosity. 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. 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

relatively expanded access at less selective institutions. Our empirical analysis examines whether changes in policy instruments contribute to or mitigate the movement of funds for needy students towards two-year degree programs and away from four-year schools, which highlights the political economy factors that underlie federal aid allocation mechanisms.

Our findings, along with those of prior work, are suggestive that changes in Pell generosity may affect the margin determining who among the needy apply for federal aid and where they enroll. Thus, while our analysis remains at the institution level, the ability to separate out Pell-enrollment patterns is complementary to existing efforts to document the efficacy of Pell. As institution-level data keep us from making strict inferences about the behavior of particular groups of students (i.e., needy students), we also investigate the aggregate enrollment patterns around a particular policy change. By following the enrollment decisions of low-income youth around the 1992 Higher Education Amendments (HEA) that removed tuition-based caps on maximum Pell awards, we identify how granting practices affect institutional choice. Measured against a group of slightly higher-cost but otherwise similar institutions, we report a significant increase in the enrollment of low-income students at low-cost institutions that experienced this exogenous increase in Pell generosity. In short, results are suggestive that student enrollment does respond to aid.

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 to 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.

After describing some of the mechanics of the federal Pell Grant program, the following section of the paper assesses the impact of changing federal aid levels on institution-level Pell revenues over the 1989 through 2002 period. Section 2 also contains the dissection of institutional revenue into enrollment and award value, offering estimates of the relative strength of these contributing factors. In Section 3, we separately test the efficacy of the 1992 HEA using a sub-sample of relatively low-cost institutions. We summarize the results in Section 4 and offer some additional discussion and concluding remarks regarding enrollment effects and average award values.

## 1. Total Institutional Pell Revenue and Variation in Pell Generosity

### *1.1 Data*

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 institution-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 period from 1989 through 2002. We supplement these data, with information drawn from the Integrated Post-Secondary Data System (IPEDS) and State-specific labor market and economic measures acquired from standard sources. We adopt Peterson's Guide to Four-Year Colleges as our metric for an institution's selectivity in 1989, which allows us to separate two-year institutions from four-year institutions classified as non-competitive, 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

non-profit institutions that offer at least an associates degree, excluding for-profit and trade schools.

*1.2 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 parent income and wealth (although home equity was removed from the formula in 1993) 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.[1]

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. However, given the potential contribution of an institution's COA to the grant determination, we restrict the sample of institutions in an attempt to alleviate concern for endogenous tuition responses to changes in Pell generosity. In particular, we report the results of our analysis only for the sample of institutions with COA sufficiently high such that

[1] The percentage-cap on Pell grants was 50 percent from 1973 through 1984 and 60 percent from 1985 through 1992. Following the 1992 Higher Education Amendments, the percentage cap was abolished. This exogenous variation is the natural experiment that forms the basis for the analysis in Section 3.

$MaxPell_{it} = MaxPell_t$ for all years in the sample period.[2] Specifically, we retain over 71 percent of the larger sample after discarding low-COA institutions. That such a large proportion of institutions have a sufficiently high COA as not to affect individual Pell-award values may explain the lack of support for what has become known as the Bennett hypothesis, which postulates universities might respond to more generous aid by raising their COA (e.g., Long 2004, Rizzo and Ehrenberg 2003). Of course, if Pell-award values for the remaining observations are actually independent of institutions' COA one would not expect that COA would rise with Pell at these institutions. As only Congress can change the maximum Pell grant each year, we exploit all remaining variation in $MaxPell_t$ as exogenous to institutions.

Beyond per-student maximum award values, two additional measures are key to controlling for federal policy as it relates to the generosity of the Pell program. In particular, 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 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.[3] Given that we can also control for variation in the typical-student-aged population, the number of Pell applicants deemed eligible based in family income, $Eligibles_t$, can also speak to the generosity of the Pell program. Summary statistics for the sample of 1,784 institutions that are never restricted by the COA rule are provided in Table 1.

[2] Actual room and board measures are incomplete in IPEDS for many schools. Thus, the COA at institution *i* is approximated by *i*'s in-state tuition plus the average statewide room and board for institutions of the same classification. As the average EFC is unobserved for each individual, calculations are based on students who have an EFC of zero. In 1994, approximately 59% of Pell recipients had and EFC of zero (1994-1995 Title IV / Federal Pell Program End-of-Year Report, Table 4).

[3] Further, the sum of actual award values would be prone to simultaneity bias, where federal appropriations would not.

*1.3 Empirical specification*

With all three measures being exogenous to individual institutions, we regress institution-specific Pell revenues on these attributes and a set of controls. Throughout the analysis, we adopt the Peterson's Guide to Four-Year Colleges ranking of institutions as our metric of institution selectivity. However, given the small cell size of the non-competitive 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 US 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 would also relate to those based on broad tuition categories.

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

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

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

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 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. In fact, 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.[4] Further, including both *MaxPell* and *FedApprop* may allow one to separately identify the effects of changing the upper tail of award values and changing where in the distribution of awards the mass falls.

As 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 assuming that enrollments and award values potentially vary with prices, institution characteristics and local market conditions. Specifically, we capture variation at the institution-level by including a measure of the direct cost of attendance (in-state tuition plus room and board) and a size measure (the lag of total enrollment minus of Pell enrollment), as reported in IPEDS. [5] Of course, selectivity is also held constant within specifications. State-level measures of 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.[6] A quadratic trend is also included to allow for unobserved time-dependence.

[4] Further, with both *MaxPell* and *FedApprop* depending on congressional authorization, the determination of *FedApprop* may in practice be the simple multiplying *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.

[5] Because a significant fraction of Pell recipients are non-traditional students who do not live on campus, we also estimate the model separating room and board costs from tuition, which yield results that are qualitatively identical to those presented.

[6] Unemployment rate and mean weekly manufacturing earning made available from the Bureau of Labor Statistics. Per-capita disposable income made available from the Bureau of Economic Analysis. Median home values made available from the US Census and Freddie-Mac. Number of high-school graduates made available from the Southern Regional Education Board. Population of 18-19 year-olds made available from the US Census.

In all specifications, we also 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. Of course, one concern with explaining institution-specific variation in Pell revenues using relatively few institution-specific controls and controls that exclusively vary over time is that the standard errors might be biased, possibly downwards. Nonetheless, the inclusion of institution 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 institution-level controls, the inclusion of institution fixed-effects alleviates concern over aggregation bias in standard errors.

*1.4 Results*

The results of [1] are presented in Table 2, by selectivity and for a pooled-sample of institutions. Overall, the empirical relationships with regard to the non-aid-related controls mostly confirm prior expectations. For example, total Pell revenues at an institution generally decline as income increases in the state suggesting that there are fewer Pell-eligible students, with the largest declines at two-year institutions, who are arguably more likely to be serving local markets.[7] Total Pell revenue increases with the number of high-school graduates in the

[7] 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 institution fixed effect, the interactions are largely insignificant. We report the more parsimonious specification as both specifications yield the same qualitative conclusions and in-state student counts are missing for 57 observations. In a subsequent sensitivity test, we also included annual U.S. GDP, which also reveals the robustness of the patterns in our variables of interest.

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.

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 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.[8] For example, pooled-sample estimates (in Column 5) 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 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 institution selectivity, both in terms of *MaxPell* and *FedApprop*. As selectivity increases among four-year institutions in our sample, point estimates suggest a

[8] Analysis of excluded institutions does reveal an elasticity measure significantly lower than unity, as would be implied by the reported elasticities in excess of unity. Of course, one might expect that such elasticities be greater than one as not all students at an institution receive the maximum available Pell award. Where 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, as many of the low-cost institutions are two-year or less-selective four-year institutions, this effect is more pronounced in such specifications.

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 non-competitive 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. Also among four-year institutions, as selectivity increases, 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 existing between two-year and non-competitive 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 graduating class and family income) increases in Pell generosity measured through changes in the number of Pell students in a given year appears 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 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.

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) 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.[9] Taking these estimates as given, our analysis suggests that were such numbers to be realized in aggregate, Pell revenue to institutions would decrease by 0.9 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 0.4 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 whether reduced Pell generosity might require more institutionally provided grants.

**2. Pell-Student Enrollment and Average Award Value**

As suggested earlier, any systematic relationship between institution 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

[9] "Department of Education's Update of the State and Other Tax Allowance for Student Aid Award Year 2005-2006," GAO-05-408R, page 2.

examine the proportional breakdown of the total-revenue estimates of Table 2 into these two contributing factors. In considering the underlying factors, one will recognize that the sensitivity of enrollment to changing generosity is itself non-trivial. In fact, as the generosity of the Federal Pell Program changes, there are four margins around which the number of Pell students enrolling in post-secondary 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.[10] 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 those above. 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, as 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

[10] 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.

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.

*2.1 Empirical specification*

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

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

and,

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

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 included in [1]. Re-writing [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 $\beta_{TR}$, then $\hat{\beta}_{AR} + \hat{\beta}_{PE}$ is also unbiased in predicting $\beta_{TR}$. This property is also made clear in the estimated coefficients reported in tables 3 and 4.

When considering the estimation of [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 institution-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 equivalent-valued awards 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.[11] 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 $PellEnroll_{it}$.[12] With our extensive list of controls, considering the proportional breakdown of the revenue-sensitivities of Section 1 into that derived from award values and that derived from enrollments we believe to be instructive. In the following sections, we therefore document separately the sensitivity of institution-level Pell enrollments and average award values to the time-varying measured of generosity: *MaxPell*, *FedApprop* and *Eligibles*.

*2.2 Results*

Tables 3 and 4 report the results of estimating equations [2] and [3] on institution-level Pell enrollments and institution-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 arise for two reasons. For example, it could simply 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 also consistent with increases in the

[11] 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 attendance 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 < *MaxPell* in 1992 (see Li 1999).

[12] Note that any bias in the estimation of *AR* and *PellEnroll* will be such as to maintain $\hat{\beta}_{AR}+\hat{\beta}_{PE}=\hat{\beta}_{TR}$.

maximum 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 one percent increase in *MaxPell* would necessarily increase the award value by more than one percent. Second, for a student already receiving the maximum Pell award, a one percent increase in *MaxPell* would increase the award value by exactly one 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 institution-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 point estimates 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, institution-level Pell enrollments are largely sensitive to only the number of eligibles (Table 3) and institution-level average award values are largely sensitive to only 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 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.[13] On the other hand, 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 also suggests

[13] 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 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 is 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 are properly thought of operating through Pell-award values.

that more-needy 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) than at the most-selective four-year schools (0.4). Moreover, proposed changes to the maximum Pell award ranging from $100 to $500 suggest that the enrollment of needy students would be most responsive at four-year institutions, with responses up to 3.3 times larger than at two-year institutions. Thus, our model suggests that these policy changes would jointly yield significantly greater benefits to four-year institutions.

### 3. 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 mitigates the potential endogeneity of Pell award-values in the sample as institutions with sufficiently low COA may find that increases in *MaxPell* afford them the opportunity, for example, to increase their tuition. While the potential endogeneity of *MaxPell* at these institutions made this sample restriction appropriate in the analysis of sections 1 and 2, this same sample of low-cost institutions may, in fact, contribute to our understanding of enrollment effects in particular. Specifically, it is this sample of institutions for which the 1993-removal of the cost-of-attendance 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 1 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 1 demonstrates that, prior to 1993, maximum Pell grants at institutions with relatively low costs of attendance (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 experience 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.

*** Insert Figure 1 approximately here. ***

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 examines 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.

*3.1 Empirical Model and Data*

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

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

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 in 1992, the year prior to the rule change.

Equation [4] is estimated using the same institutional-level dataset as in earlier sections, where the sample is restricted to contain all institutions that at any time between 1989 and 1992 had maximum Pell awards that were restricted, not by the Federal Maximum, but by the cost-of-attendance rule (i.e., institutions with a cost of attendance less than $COA_1$ in Figure 1). Given the definition of the treatment group above, the control group is all institutions which 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 1993 rule 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 of 1989 through 1997 are provided in Table 6.[14]

*3.2 Empirical Results*

Table 7 provides estimates of Eq.[4], which generally yield significant coefficient estimates that are qualitatively similar to the prior findings with regard to the control variables. Overall,

[14] Given educational amendments introduced in 1998, our sample is restricted to annual observations before 1998.

the difference-in-difference results confirm the prior finding that Pell enrollments decline at low-cost institutions coincident with increasing generosity. 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., $\beta_2 > 0$). This expectation is confirmed in the results presented in Column (1) for the full sample of low-cost institutions. Specifically, institutions which were restricted by the percent-of-cost rule in 1992 enrolled 5.2 percent more Pell recipients after the removal of the rule than those in the control group.[15]

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 the most institutions which were constrained by the percentage cost rule were two-year institutions. Nonetheless, although the removal of the percent-of-cost rule predominately affects two-year institutions, Table 6 shows that 12 percent of the control group and 40 percent of the treatment group are comprised of four-year institutions. Thus, four-year institutions appear to be over-represented 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 examine the possible importance of the distinction between two- and four-year institutions on the empirical results, Column (2) restricts the sample to two-year institutions. The

[15] 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.

positive enrollment effect from an increase in the level of the maximum Pell award is robust for an exclusive sample of two-year institutions. Specifically, cost of attendance restricted, two-year institutions experience a 2.6 percent increase in Pell recipients relative to those institutions not constrained by the cost-of-attendance rule in 1992. Nonetheless, it is also the case that the magnitude of the difference-in-difference coefficient declines by restricting the sample to two-year institutions (i.e., from 5.2 percent), which may suggest that needy students who enroll in two-year (locally oriented) institutions are less responsive to aid because they face additional constraints that require them to attend institutions close to home.

Thus, the results in Section 2 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 3 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 both to the absolute and relative magnitude of Pell awards offered by higher educational institutions.

## 4. Discussion and Concluding Remarks

In this paper, we assess the impact of changing federal aid levels on institution-level Pell revenues using institution-level data on the number of Pell recipients and total Pell revenues from 1989 through 2002. We report 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. Moreover, scrutinizing the overall influence through separate analyses of award values and Pell enrollments reveals other important regularities in the data.

Our analysis also suggests 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 sizably larger than that afforded to four-year institutions. On the other hand, revenues at two-year institutions are least sensitive to variation in the maximum award value, in particular with respect to middling four-year institutions where there is evidence of a relatively strong enrollment response to changes in the value of the maximum grant.

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 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 a student over the threshold from non-enrollment 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 universities and the federal money that they bring with them to the institution. In terms of our understanding recent need-related trends in post-secondary education and whether changes in policy instruments contribute to or mitigate the movement of needy students towards 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 jointly yield greater benefits to four-year institutions.

## Table 1: Sample Characteristics by Institution Selectivity

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 errors are in parentheses.

| | Two-year institutions | Non-competitive or Minimally difficult | Moderately difficult | Very difficult or Most difficult | Full Sample |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *MaxPell* | - | - | - | - | $2,315 |
| | | | | | (264) |
| *FedApprop* | - | - | - | - | $5,520 [a] |
| | | | | | (968) |
| *Eligibles* | - | - | - | - | 5,054,099 |
| | | | | | (474,516) |
| In-State tuition plus room and board | $6,462 | $8,707 | $11,508 | $17,306 | $10,207 |
| | (2,434) | (3,.019) | (4,335) | (5,699) | (4,896) |
| Lagged enrollment less Pell students | 3,077 | 1,295 | 3,041 | 3,684 | 2,778 |
| | (4,027) | (2,243) | (4,391) | (4,108) | (4,020) |
| Per-capita disposable income in State | $21,457 | $19,623 | $20,615 | $21,881 | $20,750 |
| | (3,026) | (2,757) | (2,895) | (3,003) | (2,994) |
| Unemployment Rate in State | 5.4 | 5.5 | 5.3 | 5.5 | 5.4 |
| | (1.5) | (1.5) | (1.5) | (1.5) | (1.5) |
| Mean weekly manufacturing earnings in State | $462 | $441 | $452 | $450 | $452 |
| | (54) | (57) | (51) | (40) | (52) |
| Median home value in State | $90,395 | $80.206 | $87,056 | $97,416 | $87,500 |
| | (23,800) | (26.296) | (29,182) | (27,956) | (27,645) |
| Number of high school graduates in State | 88,740 | 81,436 | 92,647 | 114,977 | 91,472 |
| | (57,110) | (70,057) | (74,383) | (79,592) | (7,0569) |
| Number of 18-19 year olds in State | 242,674 | 234,604 | 25,8961 | 323,661 | 255,826 |
| | (164,917) | (205,430) | (21,8917) | (237,561) | (207,068) |
| Observations / Number of institutions | 5,491 / 503 | 4,081 / 339 | 10,354 / 817 | 1,863 / 148 | 21,789 / 1,784 |

[a] Millions of dollars.

## Table 2: Effect of Changes in the Pell Generosity on Institutional Pell Revenue

In all specifications, the dependent variable is Log(Total institutional Pell revenue). Coefficients are estimated while controlling for institution-specific unobserved heterogeneity. Standard errors are in parentheses. Dependent variables means for columns (1) through (5) are $1,200,724, $1,071,024, $1,444,473, $1,280,246 and $1,299,059 respectively.

| Independent variable | Two-year institutions | Non-competitive or Minimally difficult | Moderately difficult | Very difficult or Most difficult | Full Sample |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Log(*MaxPell*) | 1.039 | 1.845 | 1.693 | 1.305 | 1.583 |
| | (0.136)*** | (0.149)*** | (0.073)*** | (0.134)*** | (0.058)*** |
| Log(*FedApprop*) | 0.094 | -0.074 | 0.042 | 0.072 | 0.027 |
| | (0.043)** | (0.049) | (0.024)* | (0.042)* | (0.019) |
| Log(*Eligibles*) | 1.425 | 0.780 | 0.381 | 0.279 | 0.653 |
| | (0.113)*** | (0.116)*** | (0.058)*** | (0.111)** | (0.047)*** |
| Log(In-State tuition plus room and board) | -0.075 | 0.167 | -0.006 | -0.099 | -0.034 |
| | (0.045)* | (0.061)*** | (0.031) | (0.052)* | (0.022) |
| Log(Lagged enrollment less Pell students) | 0.101 | 0.076 | 0.130 | 0.009 | 0.105 |
| | (0.011)*** | (0.012)*** | (0.010)*** | (0.038) | (0.006)*** |
| Log(Per-capita disposable income in State) | -0.685 | -0.012 | -0.442 | -0.527 | -0.502 |
| | (0.228)*** | (0.249) | (0.120)*** | (0.221)** | (0.096)*** |
| Lagged Unemployment Rate in State | 0.022 | 0.008 | 0.005 | 0.002 | 0.014 |
| | (0.006)*** | (0.006) | (0.003)* | (0.006) | (0.002)*** |
| Log(Mean weekly manufacturing earnings in State) | -0.542 | 0.354 | -0.066 | 0.572 | 0.005 |
| | (0.160)*** | (0.159)** | (0.082) | (0.165)*** | (0.066) |
| Log(Median home value in State) | -0.937 | -0.307 | -0.871 | -0.471 | -0.707 |
| | (0.084)*** | (0.091)*** | (0.044)*** | (0.079)*** | (0.035)*** |
| Log(Number of high school graduates in State) | 0.398 | 0.240 | 0.396 | 0.368 | 0.329 |
| | (0.058)*** | (0.069)*** | (0.030)*** | (0.055)*** | (0.025)*** |
| Log(Number of 18-19 year olds in State) | 0.145 | -0.196 | -0.463 | -0.520 | -0.272 |
| | (0.098) | (0.116)* | (0.052)*** | (0.092)*** | (0.042)*** |
| *t* (1989 = 1) | 0.031 | 0.045 | 0.044 | 0.055 | 0.046 |
| | (0.011)*** | (0.013)*** | (0.006)*** | (0.012)*** | (0.005)*** |
| $t^2$ | -0.001 | -0.004 | -0.002 | -0.003 | -0.002 |
| | (0.001) | (0.001)*** | (0.000)*** | (0.001)*** | (0.000)*** |
| Constant | -20.286 | -23.883 | -6.866 | -5.080 | -11.752 |
| | (3.230)*** | (3.350)*** | (1.694)*** | (3.181) | (1.350)*** |
| Observations / Number of institutions | 5,491 / 503 | 4,081 / 339 | 10,354 / 817 | 1,863 / 148 | 21,789 / 1,784 |
| R-squared | 0.39 | 0.35 | 0.44 | 0.43 | 0.38 |

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

## Table 3: Effect of Changes in the Pell Generosity on Institutional Pell Enrollment

In all specifications, the dependent variable is Log(Total institutional Pell enrollment). Coefficients are estimated while controlling for institution-specific unobserved heterogeneity. Standard errors are in parentheses. Dependent variables means for columns (1) through (5) are 925, 702, 966, 833 and 895 respectively.

| Independent variable | Two-year institutions | Non-competitive or Minimally difficult | Moderately difficult | Very difficult or Most difficult | Full Sample |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Log(*MaxPell*) | 0.264 | 0.867 | 0.749 | 0.430 | 0.679 |
| | (0.126)** | (0.143)*** | (0.070)*** | (0.129)*** | (0.055)*** |
| Log(*FedApprop*) | 0.017 | -0.079 | 0.025 | 0.048 | -0.003 |
| | (0.040) | (0.047)* | (0.023) | (0.040) | (0.018) |
| Log(*Eligibles*) | 1.462 | 0.781 | 0.412 | 0.255 | 0.685 |
| | (0.105)*** | (0.112)*** | (0.056)*** | (0.107)** | (0.044)*** |
| Log(In-State tuition plus room and board) | -0.101 | 0.203 | -0.003 | -0.123 | -0.026 |
| | (0.042)** | (0.059)*** | (0.030) | (0.050)** | (0.021) |
| Log(Lagged enrollment less Pell students) | 0.116 | 0.085 | 0.152 | 0.032 | 0.121 |
| | (0.011)*** | (0.012)*** | (0.009)*** | (0.036) | (0.006)*** |
| Log(Per-capita disposable income in State) | -0.328 | -0.000 | -0.328 | -0.603 | -0.369 |
| | (0.213) | (0.238) | (0.115)*** | (0.213)*** | (0.091)*** |
| Lagged Unemployment Rate in State | 0.017 | 0.009 | 0.003 | -0.000 | 0.011 |
| | (0.005)*** | (0.006) | (0.003) | (0.005) | (0.002)*** |
| Log(Mean weekly manufacturing earnings in State) | -0.507 | 0.243 | -0.015 | 0.592 | 0.017 |
| | (0.149)*** | (0.152) | (0.079) | (0.159)*** | (0.062) |
| Log(Median home value in State) | -0.910 | -0.222 | -0.844 | -0.447 | -0.674 |
| | (0.078)*** | (0.088)** | (0.042)*** | (0.076)*** | (0.033)*** |
| Log(Number of high school graduates in State) | 0.384 | 0.237 | 0.395 | 0.405 | 0.336 |
| | (0.054)*** | (0.066)*** | (0.028)*** | (0.053)*** | (0.023)*** |
| Log(Number of 18-19 year olds in State) | 0.039 | -0.112 | -0.419 | -0.494 | -0.246 |
| | (0.092) | (0.111) | (0.050)*** | (0.089)*** | (0.040)*** |
| $t$ (1989 = 1) | 0.016 | 0.046 | 0.044 | 0.056 | 0.043 |
| | (0.011) | (0.012)*** | (0.006)*** | (0.011)*** | (0.005)*** |
| $t^2$ | -0.000 | -0.004 | -0.002 | -0.003 | -0.002 |
| | (0.001) | (0.001)*** | (0.000)*** | (0.001)*** | (0.000)*** |
| Constant | -13.335 | -15.467 | -0.004 | 4.340 | -4.699 |
| | (3.009)*** | (3.214)*** | (1.620) | (3.065) | (1.281)*** |
| Observations / Number of institutions | 5,491 / 503 | 4,081 / 339 | 10,354 / 817 | 1,863 / 148 | 21,789 / 1,784 |
| R-squared | 0.27 | 0.15 | 0.24 | 0.27 | 0.20 |

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

## Table 4: Effect of Changes in the Pell Generosity on Mean Institutional Pell Awards

In all specifications, the dependent variable is Log(Mean institutional Pell revenue). Coefficients are estimated while controlling for institution-specific unobserved heterogeneity. Standard errors are in parentheses. Dependent variables means for columns (1) through (5) are $1,309, $1,507, $1,482, $1,525 and $1,446respectively.

| Independent variable | Two-year institutions | Non-competitive or Minimally difficult | Moderately difficult | Very difficult or Most difficult | Full Sample |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Log(*MaxPell*) | 0.775 | 0.978 | 0.944 | 0.875 | 0.905 |
| | (0.044)*** | (0.038)*** | (0.018)*** | (0.041)*** | (0.016)*** |
| Log(*FedApprop*) | 0.076 | 0.006 | 0.017 | 0.024 | 0.030 |
| | (0.014)*** | (0.013) | (0.006)*** | (0.013)* | (0.005)*** |
| Log(*Eligibles*) | -0.038 | -0.001 | -0.032 | 0.023 | -0.032 |
| | (0.037) | (0.030) | (0.015)** | (0.034) | (0.013)** |
| Log(In-State tuition plus room and board) | 0.026 | -0.036 | -0.003 | 0.024 | -0.008 |
| | (0.015)* | (0.016)** | (0.008) | (0.016) | (0.006) |
| Log(Lagged enrollment less Pell students) | -0.015 | -0.010 | -0.022 | -0.023 | -0.016 |
| | (0.004)*** | (0.003)*** | (0.002)*** | (0.012)** | (0.002)*** |
| Log(Per-capita disposable income in State) | -0.357 | -0.012 | -0.114 | 0.076 | -0.133 |
| | (0.074)*** | (0.064) | (0.030)*** | (0.068) | (0.027)*** |
| Lagged Unemployment Rate in State | 0.005 | -0.001 | 0.003 | 0.002 | 0.003 |
| | (0.002)*** | (0.002) | (0.001)*** | (0.002) | (0.001)*** |
| Log(Mean weekly manufacturing earnings in State) | -0.035 | 0.112 | -0.051 | -0.020 | -0.012 |
| | (0.052) | (0.041)*** | (0.021)** | (0.050) | (0.018) |
| Log(Median home value in State) | -0.027 | -0.084 | -0.027 | -0.024 | -0.033 |
| | (0.027) | (0.023)*** | (0.011)** | (0.024) | (0.010)*** |
| Log(Number of high school graduates in State) | 0.014 | 0.004 | 0.001 | -0.037 | -0.007 |
| | (0.019) | (0.018) | (0.007) | (0.017)** | (0.007) |
| Log(Number of 18-19 year olds in State) | 0.106 | -0.084 | -0.044 | -0.026 | -0.026 |
| | (0.032)*** | (0.030)*** | (0.013)*** | (0.028) | (0.012)** |
| *t* (1989 = 1) | 0.015 | -0.001 | -0.001 | -0.001 | 0.003 |
| | (0.004)*** | (0.003) | (0.002) | (0.004) | (0.001)* |
| $t^2$ | -0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000)* | (0.000) | (0.000)* | (0.000) | (0.000) |
| Constant | 2.259 | 0.794 | 2.349 | -0.209 | 2.157 |
| | (1.051)** | (0.860) | (0.427)*** | (0.972) | (0.379)*** |
| Observations / Number of institutions | 5,491 / 503 | 4,081 / 339 | 10,354 / 817 | 1,863 / 148 | 21,789 / 1,784 |
| R-squared | 0.55 | 0.73 | 0.82 | 0.85 | 0.72 |

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

**Table 5: Proportional Breakdown of the Sensitivity of Revenue to Generosity**

| | Two-year institutions | Non-competitive or Minimally difficult | Moderately difficult | Very difficult or Most difficult | Full Sample |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Log(*MaxPell*) | 1.039*** | 1.845*** | 1.693*** | 1.305*** | 1.583*** |
| Estimated contribution of Average Pell Award | .775 (75%) | .978 (53%) | .944 (56%) | .875 (67%) | .905 (57%) |
| Estimated contribution of Pell Enrollment | .264 (25%) | .867 (47%) | .749 (44%) | .430 (33%) | .679 (43%) |
| Log(*FedApprop*) | 0.094** | -0.074 | 0.042* | 0.072* | 0.027 |
| Estimated contribution of Average Pell Award | .076 (81%) | .006 (8%) | .017 (40%) | .024 (33%) | .030 (111%) |
| Estimated contribution of Pell Enrollment | .017 (19%) | -.079 (-108%) | .025 (60%) | .048 (67%) | -.003 (-11%) |
| Log(*Eligibles*) | 1.425*** | 0.780*** | 0.381*** | 0.279** | 0.653*** |
| Estimated contribution of Average Pell Award | -.038 (-3%) | -.001 (0%) | -.032 (-8%) | .023 (8%) | -.032 (-5%) |
| Estimated contribution of Pell Enrollment | 1.462 (103%) | .781 (100%) | .412 (108%) | .255 (92%) | .685 (105%) |
| Observations / Number of institutions | 5,491 / 503 | 4,081 / 339 | 10,354 / 817 | 1,863 / 148 | 21,789 / 1,784 |

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

## Table 6: Sample Characteristics: Treatment and Control Groups

Sample means (in 1990 dollars) are reported for the sample used in the estimation procedures reported in Table 7. Standard errors are in parentheses.

| | Treatment Group [a] | | Control Group | |
|---|---|---|---|---|
| | Pre 1992 HEA | Post 1992 HEA | Pre 1992 HEA | Post 1992 HEA |
| Number of Pell Recipients | 927 | 1,013 | 1,614 | 1,850 |
| | (1,068) | (1,171) | (1,576) | (1,747) |
| In-State tuition plus room and board | $3,186 | $3,372 | $3,970 | $4,317 |
| | (296) | (421) | (399) | (464) |
| Lagged enrollment less Pell students | 2,479 | 2,568 | 6279 | 6,221 |
| | (3,467) | (3,441) | (5,897) | (5,609) |
| Per-capita disposable income in State | $16,838 | $17,667 | $18,572 | $18,982 |
| | (1,441) | (1,434) | (23,970) | (1,902) |
| Unemployment Rate in State | 5.7 | 5.6 | 5.9 | 6.5 |
| | (1.5) | (1.2) | (1.3) | (1.7) |
| Mean weekly manufacturing earnings in State | $403 | $404 | $427 | $422 |
| | (44) | (38) | (51) | (47) |
| Median home value in State | $63,434 | $65,856 | $90,332 | $85,666 |
| | (29,480) | (27,673) | (42,967) | (31,291) |
| Number of high school graduates in State | 62,839 | 64,640 | 113,012 | 119,041 |
| | (51,005) | (57,864) | (99,994) | (107,644) |
| Number of 18-19 year olds in State | 198,471 | 195,640 | 36,0741 | 347,722 |
| | (169,557) | (174,736) | (327,587) | (310,030) |
| Four-year Institution | 0.40 | 0.40 | 0.12 | 0.12 |
| | 0.49 | 0.49 | 0.32 | 0.32 |
| Observations / Number of institutions | 1,356 / 357 | 1,742 / 357 | 1,453 / 367 | 1,826 / 367 |

[a] The treatment group consists of all institutions with maximum Pell awards which were restricted by the cost-of-attendance rule in 1992, the year prior to the Higher Education Amendments. The control group consists of all institutions which 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.

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

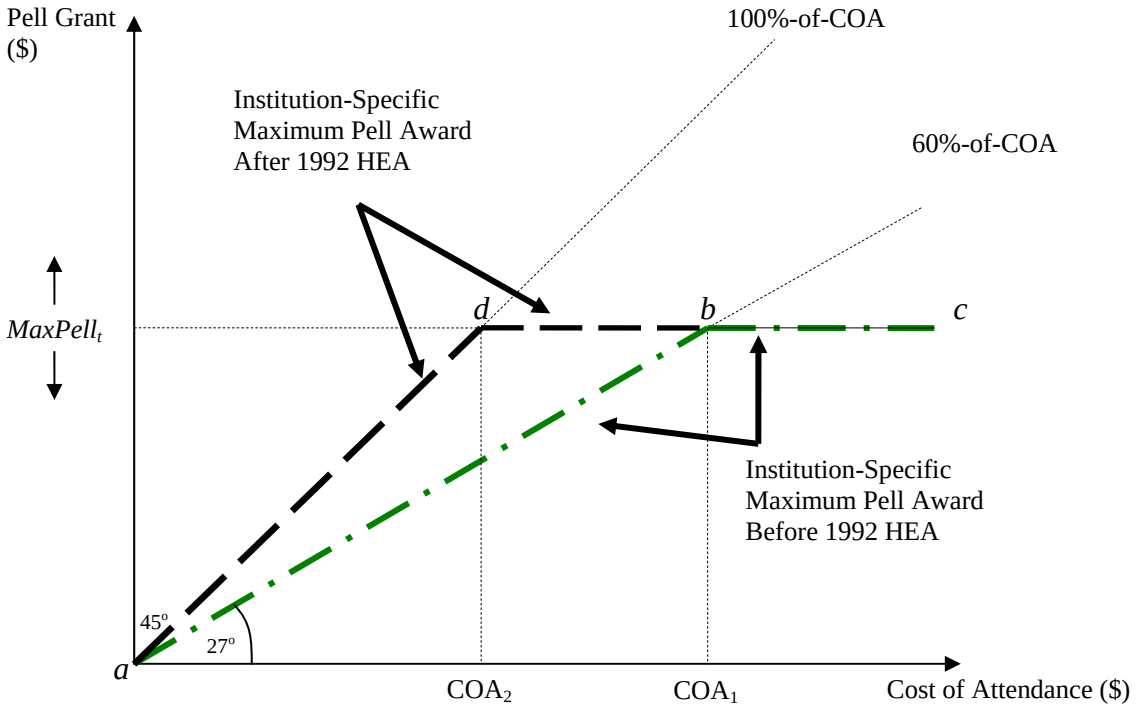
In all specifications, the dependent variable is Log(Number of Pell Students). Coefficients are estimated while controlling for institution-specific unobserved heterogeneity. Standard errors are in parentheses.

| Independent variable | Full sample | Only two-year institutions |
|---|---|---|
| | (1) | (2) |
| Post 1992 Higher Education Amendments X Treatment Group | 0.051 | 0.026 |
| | (0.010)*** | (0.013)* |
| Post 1992 Higher Education Amendments | -0.147 | -0.128 |
| | (0.012)*** | (0.016)*** |
| Log(In-State tuition plus room and board) | -0.156 | -0.139 |
| | (0.044)*** | (0.054)** |
| Log(Lagged enrollment less Pell students) | 0.027 | 0.021 |
| | (0.011)** | (0.012)* |
| Log(Per-capita disposable income in State) | -0.436 | -0.135 |
| | (0.173)** | (0.225) |
| Lagged Unemployment Rate in State | 0.041 | 0.037 |
| | (0.004)*** | (0.005)*** |
| Log(Mean weekly manufacturing earnings in State) | 0.160 | 0.137 |
| | (0.142) | (0.179) |
| Log(Median home value in State) | -0.724 | -0.719 |
| | (0.066)*** | (0.079)*** |
| Log(Number of high school graduates in State) | 0.254 | 0.206 |
| | (0.038)*** | (0.048)*** |
| Log(Number of 18-19 year olds in State) | -0.544 | -0.649 |
| | (0.090)*** | (0.111)*** |
| $t$ (1989 = 1) | 0.128 | 0.143 |
| | (0.008)*** | (0.010)*** |
| $t^2$ | -0.006 | -0.008 |
| | (0.001)*** | (0.001)*** |
| Constant | 17.386 | 16.135 |
| | (2.165)*** | (2.781)*** |
| Observations / Number of institutions | 6,377 / 724 | 4,713 / 539 |
| R-squared | 0.41 | 0.45 |

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

**Figure 1: Relationship Between the Maximum Institutional Pell Grant and Costs of Attendance**

The maximum Pell grant at any institution *i* with costs of attendance less than '$COA_1$' is constrained by 60 percent of that institutions cost of attendance, as illustrated by the line-segment *ab*. Maximum Pell grants at institutions with cost of attendance above '$COA_1$'are constrained by the Federal maximum ( $MaxPell_t$ ) as illustrated by the line-segment *bc*. With the 1993 removal of the explicit 60-percent cap, a student's Pell Grant of any institution *i* with costs of attendance less than '$COA_2$' will be constrained by institutions cost of attendance, and all institutions with cost of attendance greater that '$COA_2$' constrained by the Federal maximum as illustrated by the line-segment *adc*.



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