Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion

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The United States spends billions of dollars each year on financial aid for college students, but there is little evidence that these subsidies serve their goal of increasing college attendance and completion. Determining whether aid affects schooling decisions is an empirical challenge. The traditional approach has been to regress a person's educational attainment against covariates and the aid for which he is eligible and interpret the coefficient on aid as its casual effect. However, this is problematic, as aid eligibility is correlated with many observed and unobserved characteristics that affect schooling decisions. In order to identify the effect of aid, we need a source of variation in aid that is plausibly exogenous to unobservable attributes that influence college attendance. A shift in aid policy that affects some students but not others is one such source of exogenous variation.

In this paper, I analyze the impact on college attendance and completed schooling of the elimination of the Social Security Student Benefit Program in 1982. From 1965 to 1982, the Social Security Administration paid for millions of students to go to college. Under this program, the 18- to 22-year-old children of deceased, disabled, or retired Social Security beneficiaries received monthly payments while enrolled full time in college. The average annual payment in 1980 to the child of a deceased parent was $6,700. All dollar amounts are in real terms ($2,000). At the program's peak, 12 percent of full-time college students aged 18 to 21 were receiving Social Security student benefits.[1]

In 1981, Congress voted to eliminate the program. Enrollment sank rapidly (see Figure 1): by the 1984–1985 academic year, program spending had dropped by $3 billion. Except for the introduction of the Pell Grant program in the early 1970's, and the various G.I. bills, this is the largest and sharpest change in grant aid for college students that has ever occurred in the United States. The program's demise provides an opportunity to measure the incentive effects of financial aid. Using difference-in-differences methodology, and proxying for benefit eligibility with the death of a parent during an individual's childhood, I find that the elimination of the Social Security student benefit program reduced college attendance probabilities by more than a third. These estimates suggest that an offer of $1,000 in grant aid increases the probability of attending college by about 3.6 percentage points. Aid eligibility also appears to increase completed schooling.

I. Empirical Framework

We are interested in the effect of aid on a person's educational attainment. This relationship can be expressed with the following reduced-form equation:

$$S_i = \alpha + \beta AID_i + \chi \mathbf{X}_i + \varepsilon_i. \tag{1}$$

Here, S_i is some measure of an individual's schooling, such as college attendance or completed schooling, AID_i is the amount of student aid for which he is eligible, and $\mathbf{X}_i$ is a vector of

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[1] Statistics in this paragraph are drawn from Table 54 in Social Security Administration (1982), Table A in College Board (1998), and Table 174 in National Center for Education Statistics (1998).

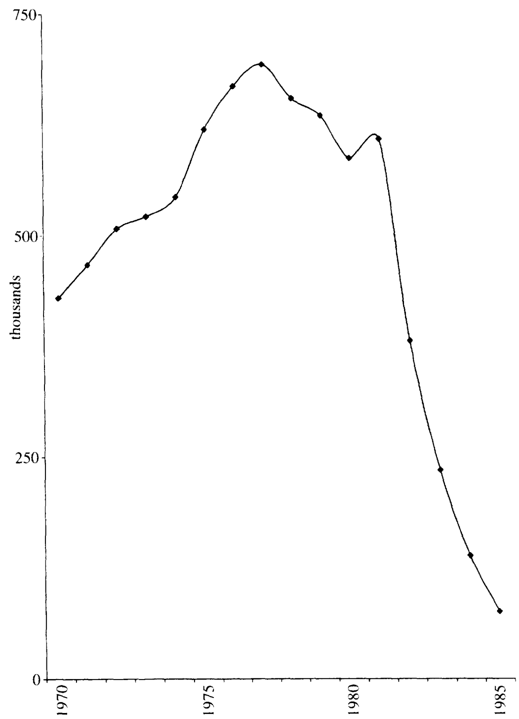

FIGURE 1. NUMBER OF SOCIAL SECURITY COLLEGE STUDENT BENEFICIARIES

Sources: Social Security Administration (1985, 1986). The moderate drop in the late 1970's is due to a national drop in enrollment rates and slowed growth in the college-age cohort (National Center for Education Statistics, 1998, Tables 6 and 15).

individual covariates. In order to obtain an unbiased estimate of the causal effect of aid, the analyst must include in the regression all of the elements of $\mathbf{X}_i$ that affect both schooling and aid eligibility. Unfortunately, many of these covariates are typically unavailable to the econometrician. A discrete shift in the rules governing aid eligibility can induce variation that is uncorrelated with these unobserved determinants of schooling.

Traditionally, the Social Security Administration has provided benefits to the children of deceased, disabled, and retired Social Security beneficiaries only until those children are 18.[2] But between 1965 and 1982, payments were extended to age 22 if the child remained enrolled full time in school.[3] Proponents of these payments argued that since parents generally subsidize children while they are in college so too should a program intended to replace the lost income of those parents. Note that, when this program was introduced, need-based federal aid for college students was negligible; the large-scale federal aid programs were not established until the 1970's. As shown in Figure 1, the program grew rapidly after its inception, peaking at 700,000 students in 1977.[4]

Social Security student benefits were distributed as monthly lump sums, without reference to actual schooling costs, much like the G.I. Bill benefits paid to college-going veterans. The benefit was determined by the earnings history of the parent whose death, disability, or retirement triggered Social Security payments.[5] The average annual benefit in 1980 for the child of a deceased parent was $6,700. Compared to other student aid, these benefits were extremely generous. In the same year, the average Pell Grant was $2,000 and the average guaranteed student loan $4,500. Benefits were sufficient to cover costs at public four-year colleges and universities, where tuition and fees averaged $1,900. Even costs at four-year private colleges, where tuition and fees averaged just $7,100, were nearly met by the average student benefit.[6]

[2] For background on the program see Committee on Ways and Means (1979, 1982), Office of the Comptroller General (1979), and Rebecca Luzadis (1983).

[3] Twenty-one percent of student beneficiaries were in high school (Phillip Springer, 1976). Since I am focusing on college-going behavior, I have excluded this group from the figures in the text. Further, I do not find an effect of student benefit eligibility on the high-school graduation rate (results available from author).

[4] Much of this growth was due to rising disability and retirement rates among prime-age men. Children of the disabled and retired were 25 percent of student beneficiaries in 1965 and 40 percent in 1980.

[5] Each child was eligible to receive 75 percent of the Primary Insurance Amount (PIA), a nonlinear function of the deceased parent's earning history. If the individual benefits of a family summed to more than 175 percent of the PIA, each benefit was proportionally reduced.

[6] Grant, loan, and tuition figures are from College Board (1983). The schooling costs of beneficiaries were further reduced by traditional student aid, which was only minimally offset by Social Security student benefits. In the federal aid formulas, a dollar in student benefits reduced other aid by five cents. Colleges may have treated student benefits less generously than this in calculating their own scholarships. Such "crowd-out" of institutional aid would bias the paper's estimates toward zero.

In 1981, Congress voted to eliminate the Social Security student benefit program. Those not enrolled in college as of May 1982 were ineligible for future subsidies, while those currently enrolled in college had their payments severely reduced. By the 1983–1984 academic year, program spending had dropped to \$0.38 billion. This sharp policy shift creates variation in aid eligibility that can be used to identify the effect of grant aid on schooling decisions.

I use difference-in-differences methodology to examine the effect of eligibility for Social Security student benefits on college attendance and completed schooling. The key estimating equation is the following:

$$(2)\quad y_i = \alpha + \beta(Father\ Deceased_i \times Before_i) + \delta Father\ Deceased_i + \theta Before_i + v_i$$

where the dependent variable is a measure of college attendance or completion and $Before_i$ is a binary variable that is set to one if a youth is a member of a cohort that graduated from high school before student benefits were eliminated. $Father\ Deceased_i$ is a binary variable set to one for those who, due to the death of their father, were potentially eligible for child survivor benefits.[7] I focus on eligibility due to the death of the parent because parental disability and retirement may be endogenously determined by the availability of student benefits. This endogenous selection into aid eligibility would bias upward the difference-in-differences estimator. I focus on fathers because 90 percent of student beneficiaries were entitled to benefits through their fathers.[8]

The reduced-form effect of Social Security student benefits is captured by β. The specification controls for changes over time in average college attendance rates and average differences in the college attendance of those with a deceased father and those with a living father. The key identifying assumption is that any relative shift in the attendance of the children of deceased fathers is attributable to the withdrawal of student benefits. Note that β captures the effect on schooling decisions of aid *eligibility* rather than aid *receipt*. In the language of the experimentalist literature, β captures the effect of the *intention to treat*. Since policy makers control the offer of aid, but not its take-up, β is the parameter of interest if we wish to predict the effect of altering aid policy.

The data used in the analysis are the National Longitudinal Survey of Youth (NLSY). See the Data Appendix for details. Survey respondents in their senior year of high school in the springs of 1982 and 1983 form the "after" cohorts, who were ineligible for any student benefits upon high-school graduation. Those who were seniors in the springs of 1979, 1980, or 1981 form the "before" cohorts.

Means, shown in Table 1, are presented separately for the periods before and after the policy change and for those with living and deceased fathers. Five percent of children had a male parent die before the child turned 18; this figure is consistent with mortality tables for the relevant age cohorts. Children with deceased fathers grow up in relatively low-income families and are more likely to live in single-parent households.[9] Children with deceased fathers are more likely to be black, due to the higher mortality rate of prime-age black men, and they have lower Armed Forces Qualification Test (AFQT) scores.[10] As the last column shows, these differences between the two groups are stable over time. During the period under analysis, there is no statistically significant change in the differences in background characteristics between children with living and dead fathers.

[7] Having a deceased father is an imperfect proxy for benefit eligibility, since some fathers did not work long enough in covered employment to generate survivor benefits. As I discuss in Section III, this mismeasurement of eligibility is minor and will bias the paper's estimates toward zero.

[8] See Social Security Administration (1982). Fathers are more likely to have a sufficient working history to generate survivor benefits upon death.

[9] Some wives remarry after the husband's death, and so not all deceased-father children live in a single-parent household. Children continue to be eligible for survivor benefits if their mothers remarry.

[10] Blacks are twice as likely to have a dead father (7.2 percent vs. 3.4 percent). This is consistent with the National Longitudinal Mortality Survey, which shows that the mortality rate for black men aged 25 to 50 is twice that of white men.

TABLE 1—NLSY SUMMARY STATISTICS

	High-school seniors 1979–1981		High-school seniors 1982–1983		
	Father not deceased	Father deceased	Father not deceased	Father deceased	Difference-in-differences
Household income ($2,000)	54,357	32,875	50,842	32,298	−2,938
	(537)	(1,839)	(788)	(2,958)	(4,816)
AFQT percentile	60.50	58.18	52.87	44.90	5.65
	(0.51)	(2.36)	(0.91)	(3.92)	(5.33)
Black	0.135	0.235	0.151	0.297	−0.046
	(0.007)	(0.036)	(0.011)	(0.063)	(0.068)
Hispanic	0.051	0.055	0.062	0.059	0.007
	(0.004)	(0.020)	(0.007)	(0.032)	(0.026)
Father attended college	0.331	0.184	0.299	0.158	−0.006
	(0.009)	(0.033)	(0.014)	(0.050)	(0.079)
Mother attended college	0.238	0.127	0.203	0.166	−0.074
	(0.008)	(0.029)	(0.012)	(0.050)	(0.085)
Single-parent household	0.153	0.787	0.194	0.837	−0.009
	(0.007)	(0.035)	(0.012)	(0.051)	(0.071)
Family size	4.77	4.40	4.71	4.34	0.00
	(0.03)	(0.18)	(0.05)	(0.27)	(0.31)
Age in 1988	25.95	25.92	23.95	23.95	−0.03
	(0.02)	(0.09)	(0.02)	(0.09)	(0.14)
Female	0.483	0.488	0.471	0.485	−0.009
	(0.010)	(0.043)	(0.015)	(0.069)	(0.097)
Attend college by 23	0.502	0.560	0.476	0.352	0.182
	(0.010)	(0.043)	(0.015)	(0.066)	(0.096)
Complete any college by 23	0.487	0.560	0.459	0.361	0.171
	(0.010)	(0.043)	(0.015)	(0.066)	(0.097)
Years of schooling at 23	13.41	13.44	13.25	12.90	0.380
	(0.03)	(0.13)	(0.05)	(0.20)	(0.296)
Number of observations	2,745	137	1,050	54	3,986

Notes: Means are of NLSY poverty and random samples, weighted by 1988 sample weights. Income and household composition measured during senior year of high school. AFQT is age adjusted; see Data Appendix. Standard errors are in parentheses. Standard errors in the difference-in-differences column adjusted for clustering at the household level.

Table 1 therefore provides suggestive evidence that the additivity assumption of difference-in-differences holds for this analysis.

I use ordinary least squares (OLS) to estimate equation (2). Standard errors in the tables are corrected for within-household correlation in error terms due to the presence of siblings in the data, as well as for heteroskedasticity due to the dichotomous dependent variable.[11] All regressions are weighted by the NLSY sample weights. The results are unchanged if I drop the poverty oversample and run unweighted regressions.

[11] Because the NLSY is a household survey, there are multiple sibling pairs in the sample.

II. Results

The first result is computed from the means in Table 1. The table shows probabilities of having entered college on a full-time basis at any time between the start of the survey and age 23, when everyone would have aged out of student benefit eligibility. For the cohort of students who were high-school seniors in 1979, 1980, and 1981, those with deceased fathers were more likely to attend college than their classmates: 56.0 percent had attended college by 1996, while 50.2 percent of seniors with living fathers had done so. For the younger cohort of students, seniors in 1982 and 1983, the pattern is reversed: only 35.2 percent of seniors whose fathers had died by the time they were 18 went to college, while 47.6 percent of their classmates attended. The probability of college enrollment dropped by more than a third for the group with deceased fathers (20.8 percentage points), while it barely dropped for other students (2.6 percentage points). The estimated effect of eligibility for Social Security student benefits on the probability of attending college is the difference in these two differences: 18.2 percentage points. This estimate is statistically significant at the 6-percent level.

I next use regression analysis in order to probe the robustness of this result. I include as covariates family size, income, parental education, and marital status of household head, all of which are measured when the youth is a high-school senior.[12] AFQT score and state-of-residence dummies are also included; these variables are measured in the first year of the survey. Additional covariates are age (as of the 1988 survey), race, and gender. Results are not sensitive to the functional form taken by age (linear, quadratic, or dummies). Further, I include two sets of interaction terms: (1) the interactions of the covariates just discussed with the "before" dummy and (2) their interactions with the deceased-father dummy.[13] The interaction terms will absorb bias caused by heterogeneity across time and eligibility status in the effect of the covariates. An example will clarify. A secular drop in the black college attendance rate coincides with the elimination of student benefits. Since youth with a deceased father are disproportionately black, this will bias upward the estimated effect of aid eligibility if the effect of race is constrained to be constant over time. Similarly, the college attendance of low-income youth may have been particularly affected by the 1981–1982 recession, inducing an interactive effect of time and income.[14]

TABLE 2—OLS, EFFECT OF ELIGIBILITY FOR STUDENT BENEFITS ON PROBABILITY OF ATTENDING COLLEGE BY AGE 23

	(1) Difference-in-differences	(2) Add covariates
Deceased father × before	0.182	0.219
	(0.096)	(0.102)
Deceased father	−0.123	Y
	(0.083)	
Before	0.026	Y
	(0.021)	
Senior-year family income/10,000 ($2,000)		Y
AFQT score		Y
Black		Y
Hispanic		Y
Father attended college		Y
Mother attended college		Y
Single-parent household		Y
Family size		Y
Female		Y
Age in 1988		Y
State dummies		Y
All covariates × before		Y
All covariates × deceased father		Y
R^2	0.002	0.339
Number of observations	3,986	3,986

Notes: Regressions weighted by 1988 sample weights. Standard errors adjusted for heteroskedasticity and multiple observations within households.

Table 2 presents results. The estimated effect of aid eligibility on attendance barely changes with the addition of this extensive set of covariates: it is 21.9 percentage points, with a standard error of 10.2 percentage points. However, the explanatory power of the regression rises dramatically, from 0.002 to 0.339. This regression clearly captures many of the key determinants of college attendance; an R^2 of 0.339 is especially high for a linear probability model. The robustness of the point estimate to the inclusion of this extensive set of covariates provides strong support for the identifying assumptions of the paper.

Bruce Meyer (1995) points out that difference-in-differences estimates can be sensitive to functional form. In particular, the difference-in-differences estimate can actually change sign if a nonlinear transformation, such as a log, is applied to the dependent variable. The present estimates are not vulnerable to this most severe form of functional-form sensitivity. As can be

[12] Family income is imputed if missing; see the Data Appendix. A dummy indicating these imputed values is included in the regression. Dummies indicating whether either mother's or father's education is missing are also included.

[13] The variables indicating missing data are also interacted with the before and deceased dummies.

[14] The repeal of the Middle Income Student Assistance Act (MISAA) may also induce an interaction between income and time. I have run a version of the specification that includes dummies corresponding to income eligibility cutoffs for need-based federal aid before and after the repeal of MISAA, along with their interaction with the "before" dummy. The results are unaffected.

seen in Table 1, the children of deceased fathers were *more* likely that their counterparts to attend college before the policy change but *less* likely to attend college afterward. Under these conditions, linear and nonlinear analysis will produce estimates of the same sign, though their magnitude may vary.

The results in Table 2 suggest that aid eligibility has a strong effect on college attendance. In the next section, I will put the magnitude of this effect in context. I first examine whether aid eligibility increased completed schooling in addition to college entry. These estimates are of interest because it is completed schooling that is rewarded by the labor market, rather than attempted schooling. If the marginal college entrant is not capable of completing even a year of college, then the attendance results discussed above will substantially overstate the social benefits of student aid. The estimates, based on the fully controlled specification of Table 2, are in Table 3. Eligibility for student benefits appears to increase the probability of completing at least a year of college by 14.5 percentage points and years of completed schooling by about half a year, though neither estimate is significant.

The positive effect of aid eligibility on attendance and completion could dissipate over time if student benefits induce students to simply accelerate, rather than increase, their schooling investments. I therefore examine schooling decisions as of age 28. There is attrition between age 23 and 28; nonrandom attrition has the potential to bias these estimates.[15] I test three alternative approaches to dealing with attrition, all of which yield similar results: I drop the attriters, I assign them their last observed value of the dependent variable, and I assign them values that provide a lower bound on the effect of aid eligibility in the presence of nonrandom attrition. I estimate this lower bound by imputing to attriters schooling values that would be produced by a *negative* correlation between the aid eligibility and schooling.[16] Results are in Table 3.

TABLE 3—OLS, EFFECT OF ELIGIBILITY FOR STUDENT BENEFITS ON SCHOOLING OUTCOMES BY AGE 23 AND AGE 28

	Attended college full time	Completed any years of college	Years of schooling
By age 23	0.219	0.145	0.564
	(0.102)	(0.123)	(0.379)
By age 28			
Lower bound	0.224	0.178	0.679
	(0.106)	(0.113)	(0.399)
Exclude attriters	0.248	0.191	0.754
	(0.111)	(0.118)	(0.408)
Assign last value	0.256	0.211	0.727
	(0.105)	(0.112)	(0.397)
Estimates Adjusted for Classification Error			
By age 23	0.243	0.161	0.626
By age 28			
Lower bound	0.249	0.198	0.754

Notes: Coefficients are those on deceased father × before in regressions in which the outcomes are those indicated in the columns. The regression specification is that of column (2) in Table 2. See text for explanation.

For none of the outcomes are the estimates at age 28 lower than those at age 23. This suggests that aid eligibility did not simply speed up investment in schooling but also raised its optimal level. The three approaches to handling attrition yield similar results. The lower-bound estimate is that eligibility for student benefits increases the probability of attending college by age 28 by 22.4 percentage points, which is almost identical to the effect estimated at age 23. If we examine the other schooling outcomes, the same pattern emerges: the lower bound of the effect at age 28 is just slightly above the effect at age 23, indicating that the effect of aid eligibility does not dissipate over time. If anything, it appears that the effect on completed schooling rises over time, though the size of the standard errors precludes any strong conclusions.

III. Discussion and Conclusions

The set of coefficients in Table 3 is consistent with aid eligibility increasing both college entry and persistence. Aid appears to induce into college about 22 percent of eligibles that would not

[15] Of those present in 1988, when questions were asked about parents' deaths, 5.3 percent exited the sample by age 28.

[16] For the control group, I assume that none of the before cohort but all of after cohort increases schooling after exiting the sample. For the deceased-father group, I assume the opposite: none of the before cohort but all of the after cohort increases schooling after attrition. These imputations will induce a relative *increase* in the schooling of the deceased-father group as of age 28, which works against finding a negative effect on schooling of the withdrawal of student benefits.

otherwise have entered. While many of these marginal entrants will complete just a few years of college, aid likely induces others who would have completed just a few years of college to instead finish their degrees. The effect of movement along these margins is a relative increase by age 28 in average schooling of 0.679 years.

Classification error will bias these estimates toward zero. Misclassification of the eligible group is minor: the Social Security Administration estimates that, in the early 1980's, 95 percent of children under 18 would have been eligible for survivor benefits had a working parent died. Misclassification of the control group is also minimal. The share of 17-year-olds in 1980 whose fathers were not dead but were eligible for Social Security due to the disability or retirement of a parent was 5.3 percent.[17] This degree of misclassification indicates that the estimates of Table 3 should be adjusted upward by 11 percent.[18] The bottom panel of Table 3 contains these adjusted estimates, which suggest that aid eligibility increases the probabilities of attending college by age 23 by 24.3 percentage points and of completing at least a year of college by 16.1 percentage points. The effect on completed schooling at age 28 is 0.754 years.

These are large effects, but so too was the financial incentive. The average student benefit for the child of a deceased parent was about $6,700 in 1980, more than enough to cover the $1,900 cost of tuition and fees at a public university. If we sum the direct and opportunity costs of college, the latter proxied by the annual wage of young high-school graduates ($18,500 in 1980) we obtain an elasticity of attendance with respect to schooling costs of about 1.5.[19] Each $1,000 of student benefits offered induces an increase of 3.6 percentage points in the share of high-school graduates attending college.[20]

The three-year phaseout of benefits that began in 1982 complicates the interpretation of the completion results. A beneficiary graduating from high school in 1981 would have expected four years of full student benefits, but would have instead received a subsidy that declined from $6,700 in the freshman year to $1,100 in the senior year. On average, members of the before cohorts who stayed in college for four years would have received annual benefits of $4,700. Therefore, each $1,000 of student benefits offered induced an increase of 0.16 years (=0.754/4.7) in the completed schooling of high-school graduates attending college.[21]

How do these results compare with previous estimates of the effect of aid on schooling decisions? Larry Leslie and Paul Brinkman (1988) review several dozen college attendance studies, which generally suggest that a $1,000 decrease in net price is associated with a 3- to 5-percentage-point increase in attendance. With few exceptions, discussed below, these estimates are vulnerable to the biases discussed earlier in the paper. Several studies that control for unobserved determinants of schooling have examined the effect of aid on college entry. By contrast, none have examined its effect on the completed schooling of young people, and so the present completion results, while imprecise, break new ground.[22]

Using within-state variation in public tuition costs, Thomas J. Kane (1994) concludes that a $1,000 drop in tuition produces a 3.7-percentage-point increase in college attendance. Dynarski

[17] The number of 17-year-olds is from the 1980 Census and the number of 17-year-olds with parents retired or on disability is from Table 54 in Social Security Administration (1982). Note that the NLSY contains no information about parents' retirement or disability status, so these individuals cannot be identified. See Dynarski (1999) for an analysis that uses father's age to proxy for benefit eligibility due to father's retirement status.

[18] See Dennis Aigner (1973) and Richard Freeman (1984) for the derivation of this correction for classification error.

[19] Annual wage is the average weekly wage of 19-year-old high-school graduates in the Merged Outgoing Rotation Group of the Current Population Survey multiplied by 50 and inflated to current dollars.

[20] The effect of aid may be nonlinear. In the presence of liquidity constraints, a threshold amount of aid may be needed to affect behavior, leading a large grant to have a larger per-dollar effect than a small grant. It is also plausible that the marginal effect of aid falls as aid rises.

[21] The interpretation of the attendance results is not complicated by the phaseout. The college entry decision of those affected by the phaseout occurred under the assumption of full benefits, since 85 percent who entered college by age 23 did so directly after high school.

[22] The only comparable completion studies have focused on the effect of the G.I. bills on the schooling of veterans, whose behavior is likely quite different from that of the typical young person. Joshua Angrist (1993), Marcus Stanley (2000), and John Bound and Sarah Turner (2002) all conclude that veterans' education benefits have a positive effect on completed schooling.

(2000) finds that Georgia's HOPE Scholarship, a merit-aid program, produced an increase in attendance of 4 percentage points per $1,000 of aid offered. W. Lee Hansen (1983) and Kane (1995) find that the introduction of the Pell Grant had no effect on college attendance.[23] Neil Seftor and Turner (2002) find an effect of Pell Grant eligibility on the attendance rate of older adults; they estimate an effect of 0.7 percentage points per $1,000 in aid.[24] In summary, with the exception of the Pell studies, estimates that do and do not account for unobservable differences across individuals reach similar conclusions: a $1,000 drop in schooling costs increases college attendance by 3 to 4 percentage points. This suggests that either the cross-sectional results are unbiased, or, as is the case in the return-to-schooling literature, competing biases cancel in a cross-sectional analysis.

Are the present estimates informative as to the effect of traditional aid, such as the Pell Grant? Are the student benefit program and the population it served sufficiently unique that the estimates' external validity is extremely constrained? The youth that are the focus of this paper are special in at least one way: their fathers are dead. This may make their families especially sensitive to the price of college, since they have only one parent's labor supply with which to buffer shocks to schooling costs. On other observable characteristics, the population eligible for student benefits closely resembles that served by need-based programs. In particular, both groups are disproportionately black and from low-income families.[25]

In its structure, the Social Security program is unusual in that benefits rise with the earnings of the deceased parent and its transaction costs are extremely low.[26] In traditional programs, the correlation between aid and parents' human capital is negative. The paper's estimates will provide a biased prediction of the effect of need-based aid if the response to aid is heterogeneous and correlated with parental human capital. If children from high-human-capital families are most sensitive to cost, the Social Security program will channel dollars to high-response individuals and the paper will overestimate the effect of traditional aid. The opposite will hold if, instead, children from high-human-capital families are *less* sensitive to cost.[27] The program's very low transaction costs unambiguously tend to make the present estimates provide an upper bound on the incentive effect of traditional student aid, which imposes substantial transaction costs on applicants.

From a student's perspective, this program made college the optimal choice at very low rates of return to schooling. With the student benefit, a year of college would pay for itself with a rate of return as low as 2.5 percent.[28] The program therefore likely induced into college some students with a very low *ex ante* payoff to schooling. But given the rapid rise in the return to schooling since the early 1980's, *ex post* returns for those induced into college by the student benefit were likely considerably higher.

Do the paper's high estimates of the elasticity of college attendance with respect to aid indicate the presence of liquidity constraints? Since grant aid reduces the cost of schooling and thereby increases its optimal level, a behavioral

[23] Sarah Turner (2000) suggests that schools may have "undone" the Pell Grant by lowering the institutional aid they offered low-income students, thereby explaining the zero program effects.

[24] While this effect is smaller than the present estimate, it is comparable in magnitude once the estimates are scaled by the baseline share of the relevant population that is in college.

[25] Twenty-six percent of deceased-father children and 21 percent of Pell Grant recipients are black (National Center for Education Statistics, 2000). The average family income in 1973 of student beneficiaries placed them in the bottom income quartile of families with children in college, while 90 percent of dependent Pell Grant recipients are from families with incomes below $40,000 (Bureau of the Census, 1974; Springer, 1976; National Center for Education Statistics, 1998).

[26] Child beneficiaries had to do little to obtain student benefits. The Social Security Administration sent form letters to child beneficiaries nearing age 18. To those responding that they would be continuing their schooling, SSA mailed separate, monthly benefit checks until the beneficiary left school, married or turned 22. Schools provided annual verification of enrollment to the government.

[27] Dynarski (2000) shows that, in a simple human-capital framework, the sign of the correlation is ambiguous. Kane (1994) presents results that indicate that low-income youth are most sensitive to tuition. By contrast, Stanley (2000) and Turner and Bound (forthcoming) find that the post-World War II G.I. Bill had the greatest impact on veterans from more privileged backgrounds.

[28] This calculation assumes costs consisting of tuition and fees at a public university ($1,900 in 1980–1981) and a year of forgone earnings ($18,500). I further assume a work life through age 65, annual real wage growth of 1 percent, and a real discount rate of 4 percent.

response does not, per se, indicate the presence of capital constraints. The policy experiment examined by this paper, while providing a plausibly identified estimate of the causal effect of aid, does not allow us to untangle its liquidity and subsidy effects.

Data Appendix

The National Longitudinal Survey of Youth was initiated in 1979 with a sample of 12,686 youth. I focus on the cross-section and poverty samples, which have been interviewed almost every year since 1979. I use the 1988 sample weights in the analysis, though the point estimates are similar when the regressions are limited to the random sample and not weighted.

The key variables of interest are whether a youth has attended college and how much schooling he has completed by age 23 or 28. In each survey, respondents indicate whether they are currently enrolled in college. I use these responses to code whether a youth attended college full time since his senior year of high school. The completion variables are obtained from the surveys in which the respondent is 23 or 28. Highest grade completed ranges from the 11th to the 20th. Since cohort is defined by the year one is a high-school senior, those who do not complete junior year are excluded from the analysis.

The Armed Forces Qualifications Test (AFQT) was administered to the NLSY sample in 1980. The respondents ranged in age from 14 to 22 when they took this test. Since age has been shown to affect AFQT score, I regress AFQT on age dummies and use within-age percentile scores in the analysis. I measure family income at the time a youth was a high-school senior; all values are inflated to $2,000. Family income is missing for about 20 percent of the sample. AFQT is missing for a handful of cases. For both variables, I calculate cohort-specific means separately for those with and without deceased fathers and assign these means to the missing values. A dummy is included in regressions to indicate these imputed values. Parental education, as measured in the 1979 survey, is used to create a set of variables that indicate whether each parent completed any college. Variables indicating whether education is missing for either parent are also included in the analysis.

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